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Workfare programs and their delivery system: Effectiveness of *Construyendo Perú*

VERÓNICA ESCUDERO

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Verónica Escudero[*]

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* Research Department, International Labour Organization (ILO). Email: escudero@ilo.org

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Abstract

This paper estimates the medium- to long-term effects of the workfare program *Construyendo Perú*, implemented in Peru from 2007 to 2011, to support unemployed populations in situations of poverty and extreme poverty. The paper finds that the intervention helped raise employment and reduce inactivity for certain groups of beneficiaries but at the cost of locking participants in lower quality jobs (i.e. informal and paid below the poverty line). Particularly, the program was not able to improve the perspectives of lower-educated participants in terms of job quality (although it was in terms of employment) and exacerbated the job quality perspectives of women, men, and higher-educated individuals. In terms of the mechanisms, it appears that the shift from infrastructure- to service-sector-related projects during the last two years—which were less costly, of shorter duration, and had no training component—exacerbated the effects of the program. The evaluation is carried out through a regression discontinuity approach, which exploits for the first time an interesting assignment rule of the program at the district level, namely, only districts above a certain level of poverty and development shortcomings were eligible to participate.

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1. Introduction

Public works programs are an increasingly popular policy tool in developing countries. During the last 10 to 15 years, massive public works have been implemented in developing countries with the aim of assisting vulnerable populations, providing people with income support as an insurance against shocks, and reducing poverty (Subbarao et al., 2013).[1] While in the developed world, public works programs are almost exclusively used to provide income support during times of crisis, in the developing world, these programs are not meant to provide short-term palliatives only. They aim to help individuals more assiduously by offering temporary employment to vulnerable households as a poverty alleviation measure (Del Ninno et al., 2009; ILO, 2016).[2] Although not equal to the magnitude of those in Asia and Africa, public works are also important in Latin America, where the number of programs (and budget) has increased during the last two decades.

Despite this increasingly important role of public works programs in developing countries and Latin America, the existing evidence with respect to their effectiveness is very much in its nascent phase and suffers from several gaps. Empirically, much of the evidence on the impact of public works and workfare programs in emerging and developing countries has focused either on the short-term income effects or the anti-poverty impacts, while very little is known regarding the labour market effects of these programs, especially the impacts after participation. This is particularly the case in Latin America, where only six impact evaluations have been carried out on public works programs, four of them focusing on the effects of beneficiaries during participation (Escudero et al., 2017).[3]

In this paper, I examine the medium- to long-term effects of the program *Construyendo Perú*, implemented in Peru in 2007 to support unemployed populations in situations of poverty and extreme poverty. The program provided access to temporary employment and skills development through the financing of public investment projects intensive in the use of unskilled labour. Interestingly, the program was introduced principally as a "workfare program" whose action was not limited to a recessionary event and whose aim was to address employability issues in addition to providing income support. In this respect, *Construyendo Perú* is not an exception. In developing countries, public works are more often implemented as workfare programs aimed to assist participants on a more permanent basis. Traditionally, this has been done either through the provision of longer lasting support than typical job creation measures or through the delivery of employability-enhancing components that can allow participants to find more permanent employment when the public program culminates. In Latin

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[1] Some examples include: the *Productive Safety Net Program (PSNP)* in Ethiopia, which helped around 7.6 million households withstand the impacts of the food crises within five years; the *Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS)* in India, the largest public works program to date, currently available to approximately 56 million households; and the Argentinian *Jefes y Jefas de Hogar* program, which expanded *Trabajar*, providing direct income support to poor families all over the country (Subbarao et al., 2013).

[2] Another objective of workfare programs in developing countries is community level development through the provision of public infrastructure. Although in some cases, the benefits associated with the public goods could exceed those of wage transfers (Gaiha and Imai, 2002; Ravallion and Datt, 1995), not enough evidence exists for this thesis to be conclusive, particularly since indirect effects of public goods, including their distributional effects, are difficult to quantify. The effects of public goods provided by workfare programs are thus beyond the scope of this paper.

[3] These evaluations comprise: (Hernani-Limarino et al., 2011; Jalan and Ravallion, 2003; Macroconsult S.A., 2012; Ronconi et al., 2006). Meanwhile, (Alik-Lagrange et al., 2017; Escudero et al., forthcoming) estimate the effects of public works programmes after participation.

America, *Construyendo Perú* is a case in point, as it is one of a series of workfare programs implemented that have been used as example for the design of similar government efforts in the region, although the post-participation effects of these programs have, until now, never been evaluated.

The potential positive impacts of well-designed workfare programs are numerous. Workfare programs can have an antipoverty effect arising from the direct transfers, at least during participation, provided that wages are set sufficiently high to outweigh the costs associated with participation (Subbarao, 1997). These programs can also have stabilization benefits and a consumption smoothing effect, particularly when they are implemented as safety nets to protect people against periods of economic slack (e.g. when labour demand is low) (O'Keefe, 2005). As such, even if wages are low, incomes provided as safety nets can protect households from unfavorable decisions often made by the most vulnerable during times of crises, such as selling productive assets (Subbarao, 1997). In the longer term, however, individual effects of workfare programs depend on their ability to raise participants' employability, enabling them to find sustainable employment after the program culminates (Hujer et al., 2004). At the macro level, workfare programs that are large enough can reduce poverty rates, and if these programs are able to influence private sector wages, they could have a positive effect on market wages or help enforce minimum wages (Dev, 1996).

Evidence shows that while workfare programs seem to provide effective income support to beneficiaries during participation, their impact on poverty reduction has not been conclusive. In Argentina, Colombia, and Peru, for example, working in a workfare program is associated with 25 to 40% higher wages than those typically earned by workers in the private sector (O'Keefe, 2005), although effects vary by program. In addition, these income gains were found to be progressive in some cases—i.e. gains are proportionally higher for the poorest quintiles (Murgai and Ravallion, 2005). This success could be explained partly by the fact that, prior to participation, workfare participants were already earning lower wages than those offered by the program, which were likely below the reservation wage for the non-poor population (Jalan and Ravallion, 2003). In terms of their anti-poverty effect, however, impact evaluations of workfare programs implemented in developing countries have shown mixed results on various fronts. Workfare programs have been deemed more effective than other public policies in reaching the poor (O'Keefe, 2005). Moreover, for particular programs, evaluations point to some positive anti-poverty effects, such as shifting the income distribution in a pro-poor manner or preventing beneficiaries from falling into extreme poverty.[4] However, even if the transfers have been found to be beneficial, for a number of programs, wage effects were not important (or sustainable) enough to raise participants and their families out of poverty (Ravallion and Datt, 1995).

This paper contributes to the literature in several respects. First, by estimating the medium- to long-term effects of *Construyendo Perú*, this paper aims to increase existing knowledge regarding the sustainability of workfare programs' effects after participation, a subject on which very little is currently known.[5] Second, while the scarce labour market evidence has focused only on the income and employment effects of interventions, this paper provides impacts on other aspects of labour market status (such as labour market participation, the formality or informality of found jobs, and the type of occupation of participants), working time (including excessive hours worked), and working poverty. Third, by studying particular treated groups, this paper aims to assess the heterogeneity of effects of the

[4] See, for example (Galasso and Ravallion, 2004) for an analysis of the *Jefes y Jefas* program.

[5] An evaluation of *Construyendo Perú* was carried out in 2012 to measure the effects of the program during participation (Macroconsult S.A., 2012). The study found that during participation, the program had a positive effect on wages, which was higher for women and in certain geographical areas.

program, particularly on women and on individuals with different levels of education. The choice of these societal groups for the analysis is not arbitrary. I have decided to pay special attention to female participants, because although these types of programs traditionally focus on men in the region, women actually represent the majority of participants, and the record of workfare programs in this respect is mixed (Del Ninno et al., 2009). Moreover, the micro-econometric literature in the developing and emerging world has seldom focused on the impact of programs on lower-educated individuals. Therefore, findings from this paper are novel in this respect. Finally, the paper takes the analysis one step further in trying to identify the mechanisms driving the effects, by assessing differences in impacts between different components of the program and different periods of implementation.

The empirical estimation strategy of this paper exploits (for the first time) a unique feature of *Construyendo Perú*'s assignment criteria that consisted in selecting beneficiary districts by ranking them according to a composite (poverty and development shortcomings) index FAD (*Factor de Asignación Distrital*). I use this discontinuity in the FAD index as a source of exogenous variation and resort to a fuzzy regression discontinuity approach (RD) to capture the causal effects of *Construyendo Perú* on a series of labour market variables. The findings illustrate that *Construyendo Perú* had a significant positive effect on labour participation and employment probabilities of the overall population, women, and lower-educated individuals. Unfortunately, alongside these positive effects, the program increased participants' probabilities of working informally and of being working poor. The program was not able to improve the perspectives of lower-educated participants in terms of finding a better-quality job (although it was in terms of employment) and exacerbated the job quality perspectives of women, men, and higher-educated individuals. In terms of the mechanisms explaining these results, the exposure that different groups of participants had to the different components provided by the program appear to explain the heterogeneity of effects between groups. Moreover, the shift from infrastructure- to service-sector-related projects during the last two years—which were less costly, of shorter duration, and had no training component—appear to have exacerbated the effects of the program. Finally, the paper also finds that the program suffered from multiple participation and overrepresentation of certain groups, which can be an indication of the need of better enforcement of targeting rules and eligibility criteria.

The remainder of this paper is organized as follows: Section 2 describes the main characteristics of *Construyendo Perú*, putting special emphasis on its targeting strategy. Section 3 discusses the data sources and the process followed: first, to reconstruct the assignment variable; and second, to estimate the probability of participation. Section 4 presents the empirical strategy, based on a fuzzy RD. Section 5 details the results of the impact evaluation on labour market status and work quality. Section 6 discusses an interpretation of these effects and explores the channels that may help explain them. Section 7 describes the robustness checks, and Section 8 concludes.

2. Policy description: the workfare program *Construyendo Perú*

Construyendo Perú was active from 2007 to 2011. It supplanted the program *A Trabajar Urbano*, in place from 2002 to 2007 (Figure 1), which aimed to generate temporary employment and provide some level of income support after the international economic crisis that affected Peru during the period 1998–2001. *A Trabajar Urbano* created projects with low wages,[6] in order to discourage those with more resources from participating in the program.[7] In June 2007, the program was replaced by *Construyendo Perú*, principally a workfare program, whose action was no longer limited to a recessionary event. In particular, the objective of *Construyendo Perú* was to support unemployed individuals, mainly heads of households, in situations of poverty and extreme poverty by (i) providing them access to temporary employment and skills development through the financing of public investment projects intensive in the use of unskilled labour; and (ii) improving the living conditions of the poorest segments of the population by providing or improving public infrastructure.[8]

Figure 1. *Construyendo Perú* and its preceding and succeeding programs

Construyendo Perú had four different modalities of intervention depending on the nature of the project: (i) a tender for projects, which included regular public investment projects (i.e. infrastructure works) and service-sector public investment projects (i.e. maintenance of public infrastructure), incorporated in 2009; (ii) special projects, tailored to areas officially declared in a state of emergency; (iii) rural interventions; and (iv) contingency projects. While all four modalities focused on providing financial support to short-term public investment projects intensive in the use of unskilled labour, their relative importance varied. The first modality (tender for projects) accounted for the bulk of the program's funds (between 80 and 85%), special projects accounted for around 10%, and contingency projects for 5%, leaving the remaining funds to be allocated to rural projects. In all cases, the role of the program was to finance and oversee the development of projects that were put in place by public and private implementing agencies.

Targeting was an important component in the planning of the different interventions, and it was done in three stages: geographical, self-targeting, and individual targeting. Geographical targeting was implemented first and aimed to prioritize and select districts in two ways: (i) all urban districts,

6 The maximum daily compensation was 14 PEN (10.8 USD, PPP), which kept monthly compensation at less than 300 PEN (231 USD, PPP) per month (Lizarzaburu Tesson, 2007).

7 The program was evaluated in 2003, showing during its first year since implementation positive, but not considerable, effects on beneficiaries' incomes—i.e. the average income gain of participants was around 25% of the wage provided by the program (Chacaltana, 2003).

8 (MEF, n.d.).

preferably those that were already part of the National Strategy *Crecer* and *Crecer Urbano,* were selected first;[9] (ii) out of these districts, beneficiary districts were carefully chosen by ranking them according to the composite index FAD.[10] Districts with a higher FAD were given priority and received higher shares of the budget allocated. Districts with a lower ranking received decreasing shares of the budget until the total allocated budget was exhausted.[11] Finally, when the ranking was completed, all districts receiving less than 200 thousand PEN, according to their FAD index, were removed from the beneficiary pool, and their allocations were shared equally among the remaining districts. It is worth noting that geographical targeting varied according to the modality of intervention of the program. While regular and service-sector public infrastructure projects (the large majority of the projects) used the FAD index for their geographical targeting, special projects used this index plus an additional indicator measuring the share of the population affected by the occurrence of a disaster in each district. For the other two modalities, the allocation of resources was discretionary. Once this geographical targeting was completed, the call for tender was put in place to choose the specific projects (by modality) to be implemented by the program in the selected districts.[12] Following the call for tenders for *Construyendo Perú's* projects, 380 urban districts received funding during the period 2007–10 (of the 605 districts with a population of more than 2500 inhabitants in Peru).[13]

The second stage, self-targeting, consisted of establishing wages at levels sufficiently low for the program to attract solely vulnerable individuals willing to participate for a low wage. This is a key step in public works programs aimed principally to reduce employment rationing, therefore improving targeting and reaching the poorest segments of the population. The program paid 16 PEN per day (11.4 USD, PPP) in all districts, which made for a monthly wage not higher than 352 PEN (252 USD, PPP) for 22 days of full-time work or 63.6% of the minimum wage from 2008 to 2010. Once the districts and the projects were determined, local offices of the program opened the registration process, allowing individuals interested to participate in the program to sign up.

The third and final stage was individual targeting, which selected beneficiaries from the pool of people registering to participate according to established criteria: notably, whether applicants were at least 18 years old, were unemployed heads of household, and who lived in poverty or extreme poverty. The poverty eligibility criteria were verified in two steps: all individuals who registered to participate in the program and were already part of the national household targeting system for the poor (*Sistema de Focalización de Hogares, SISFOH*[14]) were automatically retained as potential beneficiaries. For all other applicants, the program carried out a socioeconomic profiling to determine whether individuals

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9 INEI uses a 2500 urban inhabitants' limit as the lower bound to define urban districts.

10 The composite index FAD was constructed by the Planning Management Unit of the program until 2010. See Section 3 for additional information on the index and Table B1 of Appendix B for the definitions and sources of the variables.

11 This appears to imply that intensity of treatment varies along the FAD index. However, when looking at spending per participant, there is no evidence of larger shares allocated to participants in districts with a higher FAD. In fact, as discussed in Section 4.2 and shown by Figure 6, while districts with a higher FAD receive higher allowances, the intensity fades away when taking into account the number of potential beneficiaries of these funds.

12 Appendix A provides details on the selection of public investment projects. The program financed 11,300 projects during the period 2007–10, most of which were aimed to create pedestrian accesses, retaining walls, and educational and health infrastructure.

13 According to the Directorial Resolutions of the (MTPE, 2007a, 2007b, 2009).

14 The SISFOH enables the identification of individuals according to poverty levels, with views to facilitating the selection of beneficiaries into all public policies targeted to the poor.

were poor enough to participate (on the base of seven variables: housing with inadequate physical characteristics, overcrowding, housing without drain, households with children not attending school, households with high economic dependence, educational attainment of the household head, and number of employed individuals in the household). Once all eligible applicants were categorized, a public draw was done among applicants, prioritizing heads of household, particularly those with underage children.[15] In practice, some criteria were easier to verify (e.g. having children or being a household head) than others; therefore, individual targeting was focused on whether applicants had family burden (mostly children) and were living in poverty or extreme poverty. Based on the special survey carried out on participants, it can be observed that over 80% of participants were already carrying out a remunerated activity in 2007, and half had been working for over 6 months (close to a third had been in this activity for a year).

In terms of the support provided to participants, *Construyendo Perú* had two components. The first was the creation of temporary jobs in public investment projects such as pedestrian accesses, irrigation canals, retaining walls, etc. During the period 2007–10, the program created a little over 685 thousand temporary positions, varying considerably in length from a few weeks to 4 months (MTPE, 2007c).[16] As shown in Figure 2, the number of temporary jobs created was the highest in 2007, and then it decreased due to a reduction in the budget allocated to the program following the world financial crisis, hitting a trough in 2010. In spite of the reduction in financial allocations, the program suffered from a great deal of double participation (54% of beneficiaries participated more than once in the program), while 28% participated for a period exceeding the maximum 4 months.[17]

The second component entailed providing training to participants, of which there were two types, one general and one specific. The general type of training consisted of soft skills development, including social skills, empowerment, and a general knowledge of how to manage project implementation. The specific training component aimed at developing technical capabilities that would respond to the needs of the regional labour markets (rather than the project in question). Although the general training was mandatory, in practice it was not strictly enforced (that is why the number of people who completed the training was lower than the number of beneficiaries). Meanwhile, the more tailored training was voluntary, and therefore, due to self-selection, it was concentrated on higher-skilled persons. The program provided soft-skills training to close to 260 thousand individuals and more specific technical training to 27 thousand (Macroconsult S.A., 2012). Of these, 29% declared having attended practical courses, 30% attended illustrative courses, and the remaining 40% attended only informative sessions.[18] This illustrates the apparent lack of depth of the training component (even the specific one), discussed later in the paper. Importantly, the beneficiaries of the specific training were concentrated in the years

15 Unemployed heads of household with children younger than 18 years old were the first priority. According to the description of the program, this was done to target individuals who were actively looking for work, based on the assumption that chiefs of households would be actively searching to support their families. Second, up to a quarter of the available positions (per project) were reserved for youths (18 to 29 years) with dependents, even if they were childless; and a third (up to 5%) for individuals with disabilities.

16 This figure corresponds to 290 thousand full-time jobs (working 22 days) for a period of 4 months. The artificial assumption that each post had a duration of 4 months is made to allow comparisons in time and across programs (i.e. notional definition). In reality, some of the projects financed by *Construyendo Perú* had a duration of 4 months (regular projects), while other had a duration of one month (service projects), and a working month had 16 working days on average while the program was in place (Jaramillo et al., 2009). This means that various beneficiaries filled each notional "short-term job" in practice.

17 Own calculations using the special survey described below.

18 Ibid.

2007 and 2008. Since then, the number of participants started to fall until a seeming *de facto* elimination of the component in 2010.

Figure 2. Total number of participants of *Construyendo Perú* and its successor

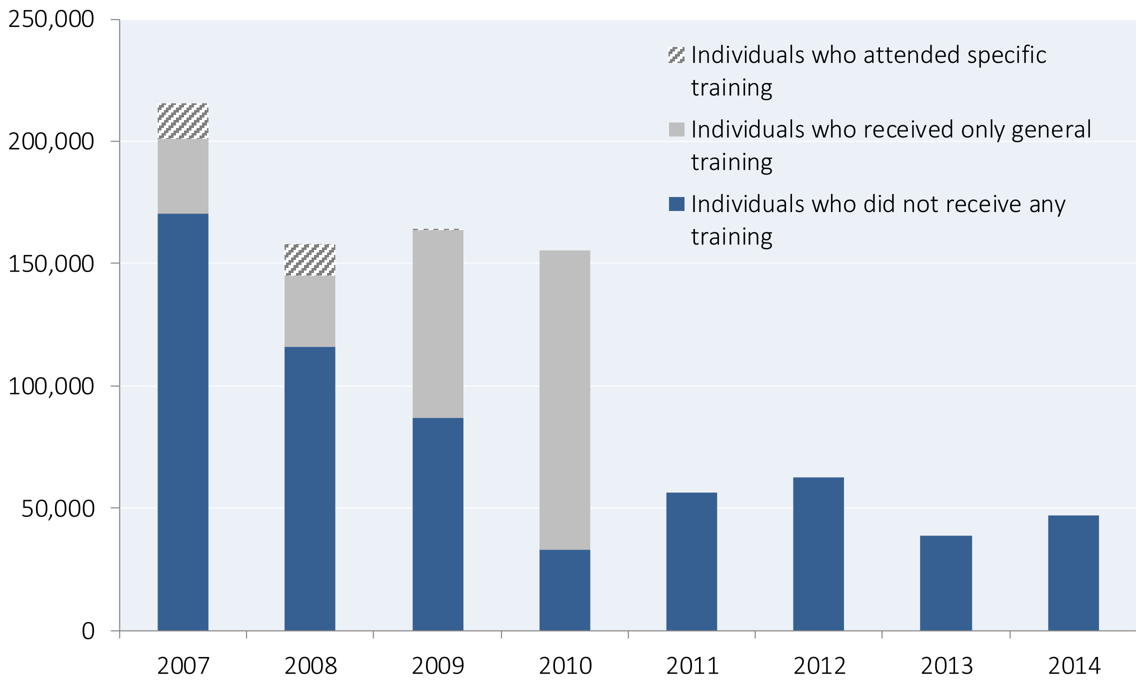

Fig. 2 illustrates the total number of individuals who participated in *Construyendo Perú* (2007–10) and its successor *Trabaja Perú* (2011–14). The figure also shows the shares of individuals who received general or specific training and those who did not receive any training.

Source: (MTPE, 2007c) and (MTPE, 2012), based on administrative data.

In 2011, the Government terminated *Construyendo Perú* and created the new program *Trabaja Perú* (Government of Peru, 2011). As with its predecessor, *Trabaja Perú* co-finances public investment projects that aim to create temporary jobs for the unemployed and underemployed with levels of income that fall within poverty or extreme poverty in both urban and rural areas. The aim of the program is to create jobs and develop productive capacities for the most vulnerable, thereby promoting sustained and quality employment for this segment of the population (Government of Peru, 2012). As such, *Trabaja Perú* assumes the full amount of functions of *Construyendo Perú* except for the training components, which were removed from the objectives of the program in 2012.[19]

[19] Supreme Decree No. 004-2012-TR (Government of Peru, 2012).

3. Data and descriptive statistics

3.1 Data sources

The analysis draws on three sources of information. The first one is a district level database that I created to reconstruct the FAD index, which is not publicly available. This additional effort represents a clear value added of the paper, since it allows to exploit the discontinuous assignment of *Construyendo Perú* at the district level for the first time, based on a threshold of poverty and development shortcomings.

The district level database includes information on rural, urban, and total population; poverty levels; human development indicators; and different district characteristics based on the Poverty Map and National Census of 2007. It also includes information on the participation of each district in the program, the year(s) of participation, the type of project for which the district applied, and the budget allocated. The variables, definitions, and sources of information are detailed in Table B1 of Appendix B. The FAD index was reconstructed on the basis of this newly created database by weighting three indicators equally: urban population, the index of human development shortcomings, and the poverty severity index FGT(2). I used the 2007 National Census for the recalculation of the FAD index, as this was the official source of information used while *Construyendo Perú* was active. Usually, the FAD index gets updated when a new census becomes available, but there was no update between 2007 and 2010. Given that there is enough detailed information on the calculation of the FAD index, the reconstruction I carried out in this paper does not seem to suffer from measurement error and should result in the exact FAD index used during the geographical targeting of the program.

The second and third sources of information include two surveys: the National Household Survey (*Encuesta Nacional de Hogares—ENAHO*) from 2007 to 2013, conducted by the Peruvian National Institute of Statistics and Information Technology (INEI); and a special survey carried out in March 2012 to *Construyendo Perú* participants covering participation during the period 2007 to 2010.

ENAHO's household survey has been conducted annually by INEI since 1995 and became a continuous survey in May 2003. It has national coverage and includes urban and rural areas of the 24 departments of the country plus the Constitutional Province of El Callao. Its sample consists of around 2,200 dwellings per month selected through a random assignment, which in 2013 made for approximately 32,000 dwellings and 115,000 individuals, around 60% in urban areas and 40% in rural ones. Interestingly, since 2007, ENAHO includes a partial rotation of sampled units, aimed to keep at least one fifth of the sample linked as a panel during five consecutive years and different panels to co-exist at all given times. ENAHO is a comprehensive survey targeting households and household members through 12 modules and 344 questions. Pertinent for this analysis, it provides information on personal characteristics of each individual in the sample (such as gender, age, marital status, and place of residence), as well as information about the composition of the individual's household and the dwelling's conditions. Moreover, it collects detailed information on individuals' education and labour characteristics, and it includes information about individuals' participation in food-related social programs; since 2012, it also records their participation in non-food related social programs, such as

Trabaja Perú.[20] This last module was critical to identify and exclude individuals who were *Trabaja Perú*'s beneficiaries when measuring outcomes (i.e. 2012) from the control group.

The special survey to participants of *Construyendo Perú* was conducted by (Macroconsult S.A., 2012) in consultation with INEI in 2012. The sample was selected randomly following a stratified probabilistic design. The inference levels were selected according to total population in urban areas and by whether the beneficiaries received the training component. In addition, the sample was selected with views to ensure the original shares of participants per year over the period 2007–10. (Macroconsult S.A., 2012) has assessed a sampling error equal to 3.1% for the overall sample, which they ensure guarantees the so needed representativity. However, it is important to note that given the difficult task of finding participants after the end of the program (i.e. information on addresses was outdated), the search for replacements established when randomly selecting the sample had to be modified and done in the field. To mitigate the practical consequences of this fact, it was decided to establish quotas (by department and year) in the sample selection, so that although the selection was not done in a random manner, it reasonably replicated the universe of participants.[21] The survey includes information on individuals' participation, such as dates of participation, types of works carried out, and whether participants received training and the type and length of training received; it also includes participants' perceptions about the program and their inclusion in it. It also provides information on beneficiaries' characteristics at the time when the survey was carried out, the characteristics of their household, their levels of education, their labour characteristics, and their income levels. All these questions are fully comparable with ENAHO, as they follow the same logic, definitions, and organization. Finally, the survey includes retrospective questions, including dwellings' conditions, income, and employment characteristics of beneficiaries in the year preceding participation. This special survey covers participation during the period 2007 to 2010 and includes 1200 beneficiaries (of which 1142 were retained for the analysis) and their families, which make for 3701 total observations.

While data from the participant survey was not used to estimate the effects of *Construyendo Perú*, the analysis could not have been carried out without the availability of this survey, which is the only source that allows to identify individuals in the treatment group and their characteristics. Indeed, as explained in more detail in Section 3.3, this survey was used, first, to calculate the probability of participation in the program based on baseline individual, district, and household characteristics of participants relative to comparable non-participants from the ENAHO survey.[22] Second, the special survey was particularly important to test the existence and robustness of the cutoff used for the analysis, as discussed in Section

[20] There is no consolidated version of ENAHO. Each module comes separately, and weighting is module specific, since it involves correction for non-response. As such, individual modules were first cleaned from invalid observations before merging them into a unique database. The author is grateful to ILO-SIALC for useful guidance in cleaning the modules.

[21] Tables 4 and 5, available on pp. 40 of (Macroconsult S.A., 2012), provide a distribution of this sample and the universe of participants by year, geographical distribution, and both the sex and age of participants, showing that the solution implemented to complete the gathering of the data was effective in replicating the main characteristics of the universe.

[22] This was achieved by integrating both surveys, which is certainly not the most common approach, but in this case, it was the only approach that could allow causal inference given existing data. Moreover, in this paper, this procedure is adequate, first, because the integrated database was used solely to calculate the probability of participation that would allow to estimate the out-of-sample predictions used as weights in the impact assessment. Second, since both surveys derive from the same institutional setting (i.e. questionnaires, sampling, and surveying methods), they are fully comparable; and third, because the share of non-identifiable treated individuals in ENAHO is small, accounting for less than 0.6% of the sample (Macroconsult S.A., 2012).

4.2. Meanwhile, once the probability of participation and the resulting out-of-sample predictions were assessed, ENAHO was used to estimate the causal effects of the program as explained below.

3.2 Individual level descriptive statistics

A central question for the analysis is how the characteristics of participants compare to those of adult individuals in the urban population sample of ENAHO from where the control group will be drawn. To assess this, Table 1 compares characteristics of individuals from the two samples for selected variables (a full set of descriptive statistics is provided in Table B2 of Appendix B). The sample from ENAHO includes comparable individuals based on selected criteria—i.e. adults, living in urban districts, and during the same period of analysis. The analysis shows that participants are very similar to the selected adult population in terms of age; on average, both are around 43 years old. They are also similar in terms of their likelihood to be married or widowed, but participants are more likely to be cohabiting or separated, although differences are not substantial. In terms of their status in the labour market, differences are not striking, either. Some 68% of participants were employed in 2012, and 22% were inactive; in the selected ENAHO population, these shares were 73% and 23%, respectively, for the same year. The difference in means for the share of unemployed individuals is, however, significant and higher for participants—7% compared to 3% for the ENAHO adult population.

The main difference arising from the analysis is that participation of women in the program is much higher than their share in the selected ENAHO population—around 78% compared with 53% of the urban population aged 18+. Interestingly, the program was not designed to target women. However, a field study carried out by the Ministry of Economy and Finance (MEF) (Jaramillo et al., 2009) suggests that the program was used by households to top-up family income—i.e. principal earners (generally men) kept their usual jobs, while women entered the program. This is reflected in the data, as half of participants were heads of households and the other half spouses of heads, while among the selected ENAHO population, half were heads but only around 28% were spouses of heads.

In addition, educational attainment of participants was lower than that of the ENAHO adult population. The share of participants who have not attained any level of education is around 8%, compared to 4% for all adults. Likewise, around half of participants have completed at most primary education (i.e. are lower-educated), while only 26% of all adults from ENAHO are lower educated. Results also show significant differences in means among people with an occupation, where most participants were either working as own-account (around 49%) or waged workers (34%). In comparison, a lower share of the selected adult population from ENAHO was own-account (36%) or waged worker (19%) in the same year, while a higher share was waged employee (27%).[23] Informal employment was considerably higher among participants (at over 90% of people with an occupation) than in the ENAHO sample (77%).

Both groups worked approximately the same number of hours (around 40 hours per week) in their main occupation (i.e. difference in means is non-significant). However, when all occupations are considered, it appears the selected adult population from ENAHO worked slightly more than participants. Despite these similarities, the share of people in time-related underemployment (i.e. employed individuals

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[23] According to the ENAHO, waged employees are individuals with a predominantly intellectual occupation in an institution or firm where they perceive a monthly or half-monthly remuneration or payment; and waged workers are those with a predominantly manual occupation in an enterprise or business where they perceive a daily, weekly, or half-monthly remuneration. As such, the difference between these two types of workers relates to the nature of the occupation and not to the sector where they work.

available and willing to work more) was considerably higher among participants (21% compared to 15%), and the share of working excessive hours (i.e. more than 48 hours per week) was considerably lower (32% compared to 41%). Finally, a higher share of participants was working poor.

Table 1. Descriptive statistics

	Urban population, ENAHO (18+) 2007		2012		**Participants (18+)** March 2012		**t-test** 2012
	Mean (A)	Std. Dev. (B)	Mean (C)	Std. Dev. (D)	Mean (E)	Std. Dev. (F)	Diff means (C) vs (E)
Individual characteristics							
Women	0.52	0.50	0.52	0.50	0.78	0.41	-17.32***
Age	40.5	16.8	42.8	17.6	43.5	12.5	-1.24
Marital Status							
Cohabiting	0.24	0.43	0.24	0.43	0.37	0.48	-10.21***
Married	0.35	0.48	0.33	0.47	0.30	0.46	2.61**
Widowed	0.05	0.22	0.06	0.24	0.07	0.25	-1.28
Divorced	0.00	0.07	0.01	0.08	0.00	0.04	1.88*
Separated	0.08	0.27	0.09	0.29	0.17	0.38	-8.60***
Single	0.28	0.45	0.27	0.45	0.10	0.30	13.14***
Kinship family							
Head	0.52	0.50	0.52	0.50	0.47	0.50	3.30***
Spouse	0.28	0.45	0.28	0.45	0.50	0.50	-16.04***
Son or daughter	0.20	0.40	0.20	0.40	0.04	0.19	13.73***
Educational attainment							
No education	0.05	0.21	0.04	0.21	0.08	0.26	-5.08***
At most primary education	0.28	0.45	0.26	0.44	0.47	0.50	-16.17***
Beyond primary education	0.73	0.45	0.74	0.44	0.53	0.50	16.09***
Household characteristics							
Household members	4.86	2.26	4.57	2.16	4.46	1.83	1.85*
Scales of monthly income (1 to 6)*	3.6	1.3	4.1	1.4	4.3	1.1	-4.56***
Labour characteristics							
Employed*	0.72	0.45	0.73	0.45	0.68	0.47	3.33***
Type of occupation							
Employer	0.05	0.21	0.05	0.21	0.00	0.04	7.11***
Own-account worker	0.26	0.44	0.27	0.44	0.33	0.47	-4.59***
Waged employee	0.20	0.40	0.20	0.40	0.05	0.22	12.45***
Waged worker	0.13	0.34	0.14	0.35	0.24	0.42	-8.91***
Non-paid family worker	0.08	0.26	0.07	0.25	0.02	0.13	7.00***
Domestic worker	0.03	0.16	0.02	0.13	0.04	0.21	-6.99***
Other	0.00	0.07	0.00	0.07	0.00	0.04	1.31
Informal employment*	0.59	0.49	0.55	0.50	0.62	0.49	-4.79***
Formal employment*	0.15	0.36	0.20	0.40	0.06	0.24	11.53***
Unemployed	0.04	0.18	0.03	0.16	0.07	0.25	-8.03***
Inactive*	0.22	0.41	0.23	0.42	0.22	0.41	1.02
Working time characteristics							
Working-poor*	0.47	0.50	0.36	0.48	0.41	0.49	-2.76***
Hours worked in main occupation	41.9	23.3	39.9	22.2	40.4	17.8	-0.55
Total usual hours worked*	48.1	22.2	45.8	21.2	43.7	16.4	2.72***
Excessive working time*	0.46	0.50	0.41	0.49	0.32	0.47	5.14***
Underemployed (time-related)*	0.26	0.44	0.15	0.36	0.21	0.41	-5.17***

Notes: *See Table B3 of Appendix B for the definitions of these variables.

Source: Author's calculations.

3.3 Assessing the probability of participation in *Construyendo Perú*

As mentioned above, while participants in the program can be observed using the special survey, comparable non-participants are only available from the ENAHO household survey of the country. There is no unique identifier to merge these two surveys, but even if there were, merging them risks leading to a composition of the new merged survey that is not representative of the overall population. To overcome this situation, I estimate a new treatment indicator based on the estimated probability of participation in *Construyendo Perú* by means of a probit model using a sample composed by individuals who either participated (special survey) or who could have participated given eligibility criteria of the program at the district and individual levels (i.e. whether they lived in a participant district, in an urban area, etc.). Then, I assign weights to the full sample of ENAHO to calculate the out-of-sample probability of participation in *Construyendo Perú*.

In order to estimate the probability to participate in *Construyendo Perú*, I include a set of variables Z_i that might affect programme participation and eventually the outcome of interest, but that are measured before the start of the programme (one year prior to participation for each individual). This includes (i) variables related to the eligibility criteria to participate in *Construyendo Perú*, and (ii) individual and household characteristics that might drive individuals to enroll in a public works programme. Importantly, both surveys' information allows to test selection based on a relatively wide set of characteristics, including: individual characteristics (gender, age, marital status, household structure, and kinship of the individual in the household), educational attainment, labour market information (status in employment), variables capturing socio-economic status, and regional fixed effects. Table C1 of Appendix C illustrates the results of the probit analysis, and Table C2 of the same appendix, the summary statistics of the estimated out-of-sample probability of participating in the program.

Using this estimated treatment variable brings about two particularities for the analysis. First, rather than having a binary treatment, I now have a continuous treatment.[24] Second, as the treatment variable assesses the probability of self-selection into the program among a pool of potential eligible participants, the analysis in practice answers the question of "what is the effect of being assigned to treatment", which is different from "what is the effect of treatment". These two aspects do not affect in any way the use and interpretation of the standard fuzzy RD explained below in Section 4.1.

24 A number of empirical analyses exist that apply RD designs using continuous treatments. Some recent salient examples include: (Clark and Royer, 2013; Pop-Eleches and Urquiola, 2013; Schmieder et al., 2012).

4. Empirical approach

4.1 Identification and empirical specification: a fuzzy regression discontinuity design

As explained above, the first phase of the targeting strategy (i.e. geographical targeting) consisted in excluding rural districts from the eligible pool and, out of the remaining districts, selecting the ones that will benefit from the program by ranking them according to the composite index FAD. This program assignment implies that participation is discontinuous at some point of the FAD index and that the assignment to treatment (D_i) is determined, totally or partially, by the value of a predictor being on either side of a cutoff point (x_0) (Imbens and Lemieux, 2008). I use this discontinuity in the FAD index as a source of exogenous variation and resort to a regression discontinuity approach (RD) to capture the causal effects of *Construyendo Perú* on a series of labour market variables. RD is an interesting strategy, since it offers a credible alternative to randomized experiments at the local level (i.e. in the vicinity of the discontinuity) (Bargain and Doorley, 2011; Cattaneo et al., 2018), and is an especially powerful, yet flexible (particularly fuzzy RD), research design (Angrist and Lavy, 1999).

The literature distinguishes between two types of RD designs: (i) the sharp design, in which treatment status is a deterministic function of the running variable; and (ii) the fuzzy design, which exploits discontinuities in the probability of treatment conditional on crossing the cutoff point. In this paper, given that the probability of receiving treatment does not change from 0 to 1 when the cutoff is crossed (Figure 4), my estimates are based on a fuzzy RD. The result is an empirical specification where participation in *Construyendo Perú* is not exclusively determined by the FAD index (X), but where additional unobserved factors may be determining assignment to treatment (Hahn et al., 2001). As such, the discontinuity becomes an instrumental variable for participation in the program, rather than determining this participation, provided the eligibility threshold is exogenously determined by the program and highly correlated with treatment (something that is tested and discussed later in this section).

To estimate the effects that participating in the workfare program has on labour market outcomes, I use both a parametric and a nonparametric estimator. Let, D_i be the treatment status, X the FAD index, x_i the FAD index for district i, x_0 the cutoff point and $g_i(x_i)$ a function that captures the relationship between the running variable and treatment status for district *i* or individual *j* in a field away from the threshold. Parametrically, the effects of the program can be estimated through a two-stage least square (2SLS) strategy, where the first- and second-stage equations will be given by 4.1 and 4.2, respectively:

$$D_i = \gamma_0 + \gamma_1 T_i + g(x_i) + \varepsilon_{1i} \quad \textbf{(1)}$$

$$Y_i = \beta_0 + \beta_1 \widehat{D}_i + g(x_i) + \varepsilon_{2i} \quad \textbf{(2)}$$

where, T_i is an indicator function taking the value of 1 when x_i is above the cutoff point (x_0) and 0 otherwise, Y_i includes different measures of labour market status and job quality, and $\widehat{D}_i$ is the predicted value of participation from Equation (1).

Meanwhile, to estimate the fuzzy RD nonparametrically, I use an IV estimator in the vicinity of the discontinuity (Angrist and Pischke, 2009). In principle, it would be possible to use any nonparametric estimator; however, in practice, it has been shown that some estimators are more efficient than others, given that the function to be estimated is at a boundary. The standard solution to reduce bias is to use a local linear nonparametric regression (LLR), which amounts to estimating linear regression functions within a window ("local") on both sides of the discontinuity. These are weighted regressions, where weights decrease smoothly as the distance from the cutoff point increases (Imbens and Lemieux, 2008). Thus, if T_i is used as an instrument for D_i in an δ-nneighborhood of x_0, the effect of treatment (which needs to be estimated using the same estimator and bandwidth) (Angrist and Pischke, 2009) equals to:

$$\lim_{\delta \to 0} \frac{E[Y_i|x_0 < x_i < x_0 + \delta] - E[Y_i|x_0 - \delta < x_i < x_0]}{E[D_i|x_0 < x_i < x_0 + \delta] - E[D_i|x_0 - \delta < x_i < x_0]} = \rho \qquad \textbf{(3)}$$

In other words, the causal effect of treatment will be determined dividing the jump in the outcome-rating relationship by the jump in the relationship between treatment status and rating (Jacob et al., 2012). This will provide an unbiased estimate of the LATE (local average treatment effect), where the Wald estimand for fuzzy RD captures the causal effect on compliers (i.e. individuals whose treatment status changes depending on whether they are just to the left or to the right of x_0). It is important to note that while the average treatment effect around the discontinuity (LATE) is the most relevant effect for the assessment of *Construyendo Perú*, it cannot be obtained in general in a fuzzy RD setting. Given the imperfect compliance that characterizes fuzzy RD designs, the effect often recovered is that of being assigned to treatment, which differs from the effect of receiving the treatment (Cattaneo et al., 2018). In other words, when some units are non-compliers, RD captures the average intention-to-treat (ITT) effect. The average effect of receiving treatment can still be captured in a fuzzy RD setting, but additional assumptions, such as monotonicity and continuity (Hahn et al., 2001), must hold.

While estimating this in a given window of width h around the cutoff is straightforward, it is more difficult to choose the bandwidth (there is a trade-off between bias and efficiency). In this analysis, I use an "optimal" bandwidth based on the standard (Imbens and Kalyanaraman, 2012) procedure, which is designed to minimize MSE (mean squared error, i.e. squared bias plus variance). In addition, I use two alternative bandwidths, calculated as twice and as half the optimal bandwidth, as an informal sensitivity test (Nichols, 2007).

While impacts in the vicinity of the cutoff point are nonparametrically identified in RD designs (Cattaneo et al., 2018), the applied literature frequently uses the parametric alternative (Cattaneo et al., 2018; Ravallion, 2008). Although this method uses data that is far away from the cutoff to estimate the f(X) function, the parametric RD could allow for the possibility to extrapolate, albeit not without a cost in terms of precision. A combination of both alternatives might be a way to ensure consistency (Hahn et al., 2001). Thus, in the empirical analysis below, I estimate the causal effects of *Construyendo Perú* in the vicinity of the discontinuity through the LLR (nonparametrical) method and complement this local estimation with the global 2SLS described above.

4.2 Testing the validity of the research design

The validity of this paper's identification strategy relies on the assumption of continuity of the FAD index around the cutoff, before the program started. This assumption is first tested by looking at the geographical distribution of the 380 districts participating in *Construyendo Perú* (out of the 605 districts with a population of more than 2500 inhabitants, and 1880 total districts) during the period 2007–10 (Figure 3). The map shows that participant and non-participant districts are scattered across the territory and are equally distributed among smaller and larger districts. Given the regional assignment of the program, we would also expect districts' characteristics to be smooth around the discontinuity, as a discontinuity would imply some sorting of districts around the threshold. To test this hypothesis, Figure D1 of Appendix D presents the graphical RD estimation with baseline covariates as outcome variables. Panels A--L plot the probability of a change in baseline characteristics, conditional on districts having a FAD index above or below the cutoff point of 0.125. They show that all relevant variables appear to be smooth around the cutoff that determines participation before the program started (this analysis holds when a smaller bandwidth is considered). This is also the case for the share of people without health insurance (panel B), the size of the manufacturing sector (panels E), the share of individuals working in micro-firms (panel G), and the share of own account workers (panel K), all of which are key variables for this paper's analysis, as they suggest smoothness in the availability of formal and informal jobs between beneficiary and non-beneficiary districts around the discontinuity at the baseline. This provides reassurance that districts around the discontinuity were similar at the baseline in all aspects but participation.

To complement the district level analysis, I also test the validity of the continuity assumption at the individual level, to assess whether individuals' observable characteristics were, on average, similar on both sides of the FAD index cutoff, before the program started. This hypothesis is tested by replacing dependent variables in equations (1) and (2) with each of the observed baseline characteristics (Lee and Lemieux, 2010). Results, presented in Table D1 of Appendix D, show that observable characteristics (unrelated to the FAD index) are, on average, well-balanced on both sides of the cutoff, with few exceptions including the incidence of individuals aged fifty or more and the educational levels (which are significant at the 5% level). In contrast, household variables related to the construction of the running variable are less continuous at the cutoff (although only the variable 'sanitary system' is significant at the 1% level), which is to be expected.[25] The lack of significant jumps in relevant observable characteristics along the discontinuity confirms the validity of the continuity assumption at the individual level.

A specific potential concern with programs assigned geographically is that people could migrate to beneficiary districts to participate in the program, invalidating the continuity assumption. This does not appear to be the case with *Construyendo Perú.* Panel C of Figure D1 of Appendix D suggests that no important differences exist among districts on one side and the other of the cutoff regarding the share of migrant population during the last 5 years. To complement this analysis, I looked at migration among beneficiaries of the program to see whether people moved to beneficiary districts, which is not the case,

[25] This does not represent a threat to the validity of the RD design, since these observed characteristics are unrelated to the labour market outcomes of interest (van der Klaauw, 2008).

as 98.1% of participants were living in the same district one year before participating in the program.[26]

Figure 3. Districts that participated in *Construyendo Perú* during the period 2007–10

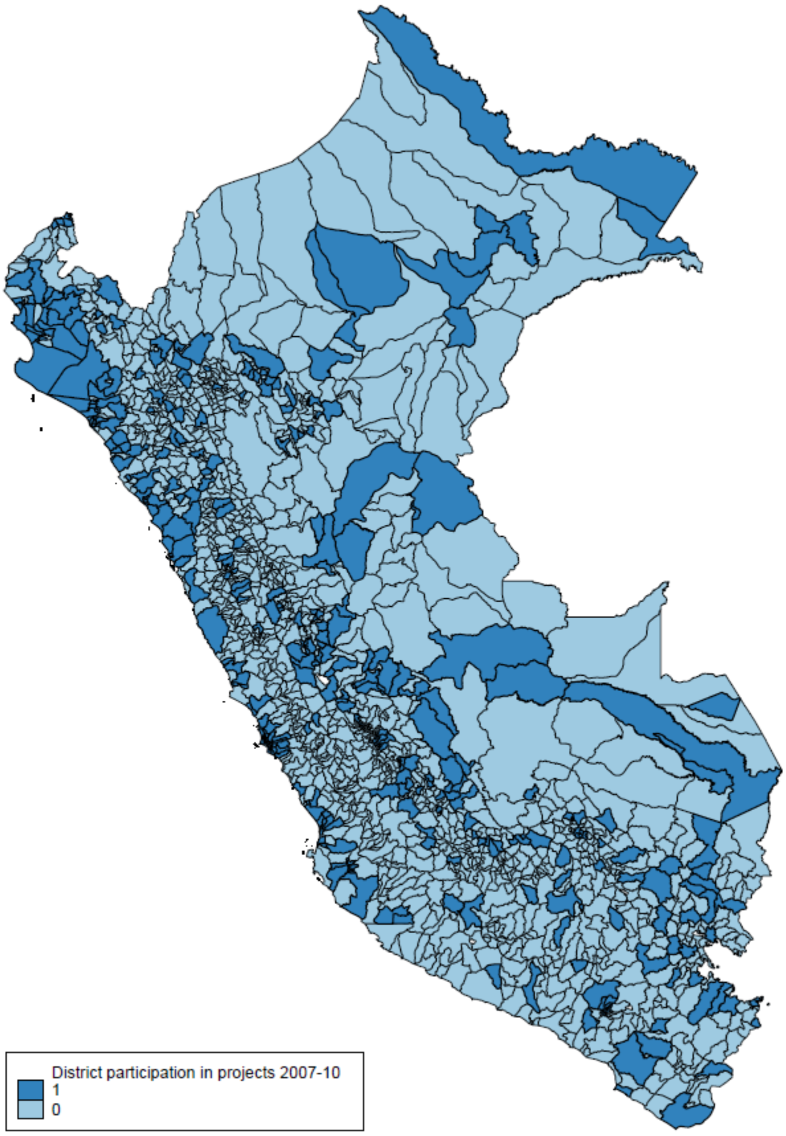

Fig. 3 shows the geographical distribution of the 380 districts that participated in *Construyendo Perú* (out of the 605 districts with a population of more than 2500 inhabitants, and 1880 total districts) during the period 2007–10. Districts that participated (D=1) have been shaded in dark blue, and those that did not participate (D=0) in light blue. A district is marked as having participated when it was selected following the FAD index and participated in the call for tender to choose the specific projects to be implemented by the program.

Source: Author's calculations based on the Poverty Map and National Census 2007 (INEI).

[26] It would have been ideal to complement these results with an estimation using migration as an outcome variable to assess whether the program had an effect on migration of participants to other districts. However, the only available information on migration comes from the 2007 National Census (INEI), and thus, I cannot measure effects post-participation.

The validity of the RD strategy also critically relies on the assumption that the running variable was not caused or influenced by treatment and that the cutoff point was determined independently of the running variable. Even when the continuity assumption holds, the manipulation by districts or individuals of the running variable would invalidate the RD design. On the contrary, if these agents cannot "precisely" manipulate X, the variation in treatment near the cutoff would be randomized as though from an experiment (Lee and Lemieux, 2010). These two conditions are satisfied in the analysis by construction. Although the FAD index was designed by the program's administration, it is based upon three indicators that are calculated by government institutions independently from the program. Moreover, their definitions predate the establishment of the program and did not change throughout its duration. The cutoff point in the FAD index was determined by the availability of government funds for this particular program per year[27]—i.e. independently from the construction of the running variable. Finally, given this regional assignment of the program, it is unlikely that individuals could have manipulated these indicators to participate in the program.

In addition, it is important to verify whether there really is a discontinuity in the probability of participation and that any observed discontinuity in mean outcome Y_i should result exclusively from this discontinuity in the participation rate (i.e. exclusion restriction). Checking for all the discontinuities in the running variable was fundamental for the estimation strategy of this paper. In fact, a baseline analysis was needed to identify the discontinuity related to the FAD index, since neither the running variable (FAD index) nor the cutoff point were publicly available. The cutoff point was determined at the district level by plotting the FAD index against the mean participation of urban districts drawing on the individual-level database created for this paper. The analysis found a unique and clear cutoff at the 0.125 level.[28] Figure 4 (panel A) illustrates this, with a figure showing a clearly observable fuzzy jump in the participation of districts (during the period 2007–10) according to whether they have a FAD index above or below the 0.125 level. Following (Hahn et al., 2001), the figure has been constructed using nonparametric methods, where the relationship between the two variables is estimated without assuming a functional form.[29] Panel B of Figure 4 displays this same analysis, but at the individual level, where the x axis illustrates the probability of participation of individuals during the period 2007–10 (see Section 3.3). This is a graphical representation of the first stage of the fuzzy RD specification, which captures "the average effect at the cutoff of being assigned to the treatment on receiving the treatment" (Cattaneo et al., 2018). Finding this discontinuity is another test of the validity of the estimation strategy.

[27] This, however, did not result in a change in the cutoff point. See Footnote 40 for more details.

[28] After having reconstructed the FAD index based on the database at the regional level, this database was merged with the individual level data—namely, the participants' database and the sample selected from the ENAHO—to create a comprehensive individual-level database for the analysis.

[29] Rather than plotting all individual information, the literature suggests presenting smoothed plots, where the conditional mean is drawn on the base of equal-sized intervals (bins) of the running variable (Cattaneo et al., 2018; Jacob et al., 2012). This strategy makes for a cleaner graphical analysis, as it reduces noise. This same strategy is used throughout the whole graphical analysis presented in this paper.

These graphical analyses also suggest that there is no discontinuity in the mean probability of districts participating in the program, other than the cutoff point.[30] This assumption was tested through a careful inspection of other possible discontinuities (necessary to unveil where the actual discontinuity lay). Panels A, B, and C of Figure 5 show that no other discontinuity can be detected from the overall dispersion of the data, other than the one used for the analysis. Panel D provides further proof. Using the special survey only, this panel shows that the minimum FAD level of participants is effectively 0.125.

Figure 4. Discontinuity in districts' and individuals' participation (2007–10), conditional to their situation along the FAD index

Panel A. District participation | Panel B. Individual participation

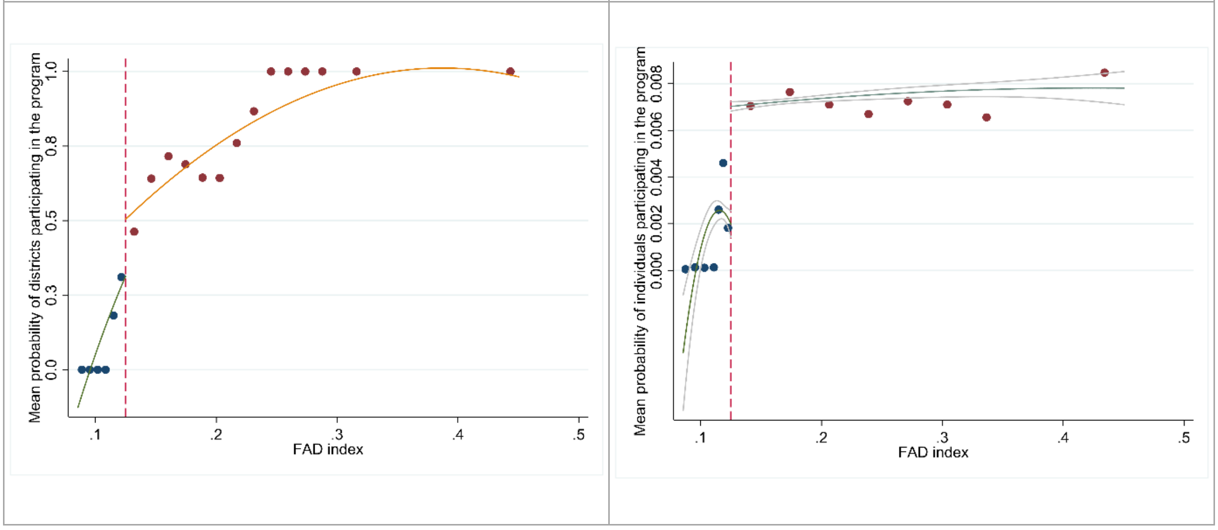

Fig. 4 plots the mean probability of districts (panel A) and individuals (panel B) participating in the program according to the FAD index along with the 95% level confidence bounds. The conditional mean is drawn on the base of equal-sized bins (i.e. each dot in graph corresponds to a bin; see Footnote 29). The fit used was suggested by the graphical analysis carried out using Lowess fit. While panel A is based on the district-level database constructed for this analysis, panel B is based on the ENAHO survey, where individuals have been reweighted based on the out-of-sample predictions of a probit model estimated using individual, district, and household characteristics of participants (from the special survey) and comparable non-participants (from ENAHO) at the baseline (see Section 3.3 for details on this estimation). After calculating the probit model and reweighing individuals from ENAHO, the special survey was dropped to carry out the analysis.

Source: Author's calculations

[30] The precision of the cutoff point was first analyzed at the district level by testing other points at close range with no other conclusive result. There are 23 districts with a FAD index between the 0.12 and 0.125 cutoff, of which 7 participated in the programme. This analysis also suggests very minor or no change in the cutoff point each year (see Footnote 40).

Figure 5. Individuals' probability of participation according to their situation along the FAD index at various cutoff points

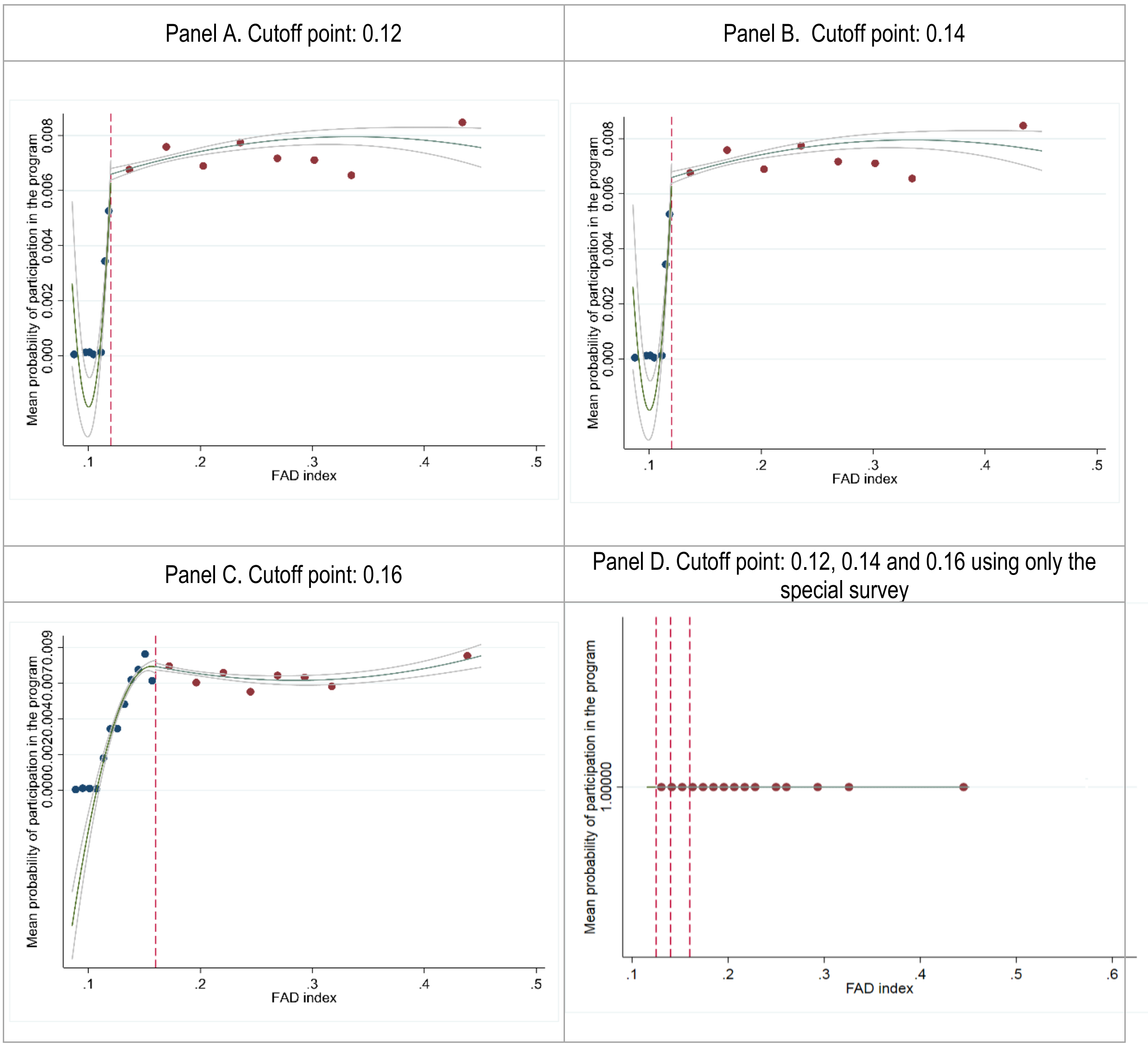

Fig. 5 plots the mean probability of individuals participating in the program according to the FAD index using cutoff points at 0.12, 0.14, and 0.16, along with the 95% level confidence bounds. The conditional mean is drawn on the base of equal-sized bins (i.e. each dot in graph corresponds to a bin, see Footnote 29). The fit used was suggested by the graphical analysis carried out using Lowess fit. Panel D uses only the special survey showing that participation effectively starts from cutoff of 0.125.

Source: Author's calculations

Finally, this paper's analysis contains an additional potential concern. The assignment of the program is such that the FAD index is set, first, to define which districts participate in *Construyendo Perú*, and second, how much funds are allocated to the program, i.e. a measure of treatment intensity. If intensity of treatment varies along the FAD index, the treatment effect would be the effect of no treatment versus minimum intensity treatment, which would represent a very particular type of effect. However, while districts with a higher FAD receive higher allowances, the intensity fades away when we examine the number of potential beneficiaries of these funds. Figure 6 shows two discontinuity graphs: the first portrays the mean share of funds channeled by the program to each district divided by the population of each district, and the second depicts this same average allocation of funds but as a share of the number of participants per department.[31] Both figures show that when taking into account the number of potential beneficiaries, the intensity of treatment appears to disappear.

Figure 6. Mean share of funds channeled by the program by the population and number of participants, according to their situation along the FAD index

Panel A. As a share of the district population	Panel B. As a share of the number of participants

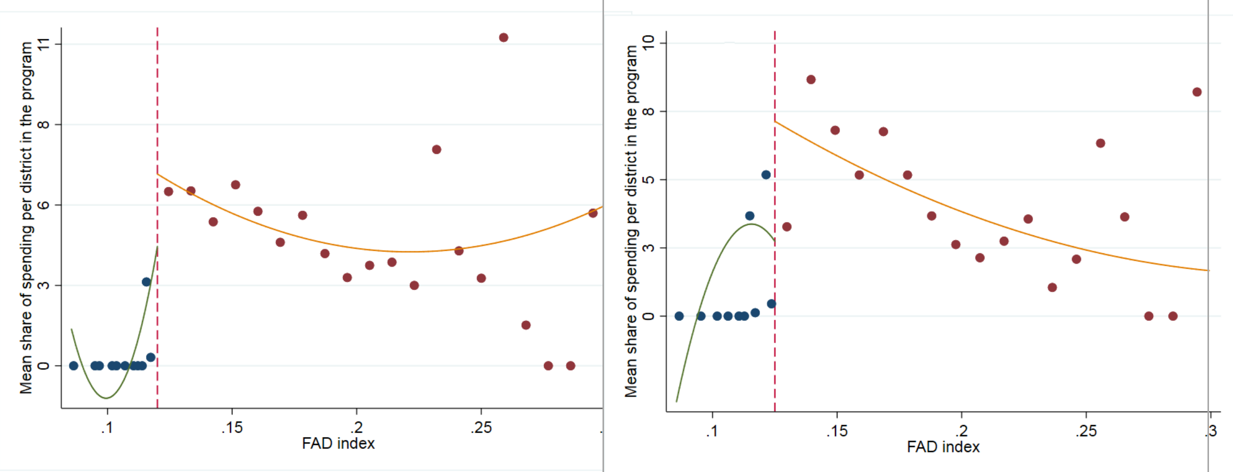

Fig. 6 plots the mean probability of districts participating in the program according to the FAD index. The conditional mean is drawn on the base of equal-sized bins (i.e. each dot in graph corresponds to a bin; see Footnote 29). The fit used was suggested by the graphical analysis carried out using Lowess fit. Panel A x-axis portrays the amount of funds channeled by the program to each district as a share of the population. Meanwhile, panel B x-axis depicts the amount of funds channeled by the program to each district as a share of the number of participants per department.[32] Both panels are based on the district-level database constructed for this analysis, using all districts during 2012.

Source: Author's calculations

[31] It would have been preferred to calculate this variable at the district level; here, however, the share of participants is not available. It also must be noted that information on total investment and number of participants by department is only available from 2008 onwards.

[32] Ibid.

5. Estimated results

Panel B of Figure 4 illustrated the first-stage relationship between the probability that being assigned to the program has on individuals participating in the program and the FAD index, at their relevant cutoff. The figure shows a clear and fuzzy discontinuity, as already discussed. Table 2 provides the estimates of Equation (1), confirming the statistical significance of the discontinuity showed in Figure 4. The table provides six sets of estimates, one for each group analyzed. The focus on women and men separately, and on individuals with different levels of education, is not arbitrary. As mentioned in the Introduction, I have decided to pay special attention to female participants, because although public works programs traditionally focus on men in the region, women represent, in fact, the majority of participants on these programs (e.g. *Construyendo Perú*), and the record in terms of effects is mixed (Del Ninno et al., 2009). Moreover, the micro-econometric literature in the developing and emerging world has seldom focused on the impact of programs on lower-educated individuals. Findings from this paper, therefore, aim to bring new light into the effects for these groups. The table shows, for the overall population, that individuals leaving in districts with a FAD index above the cutoff are around 5 percentage points more likely to participate in *Construyendo Perú*. Because we know that an average of only 3% of the urban population in Peru is involved in the program, the discontinuity explains a large part of the probability of participation.

Table 2. First-stage estimates

Sample used for the analysis on:	All	Women	Men	Lower educated*	Higher educated*	Urban departments	Total obs.
Employment status	0.441***	0.660***	0.195***	0.720***	0.305***	0.449***	
	(0.110)	(0.166)	(0.055)	(0.233)	(0.086)	(0.110)	43,741
F-value	19.09	18.53	19.27	4.40	20.86	20.98	
Hourly wage	0.473***	0.742***	0.210***	0.697**	0.328***	0.483***	
	(0.133)	(0.228)	(0.065)	(0.340)	(0.102)	(0.134)	32,702
F-value	13.19	11.65	15.17	1.85	15.29	15.88	
Working poverty	0.484***	0.770***	0.214***	0.680**	0.339***	0.493***	
	(0.134)	(0.234)	(0.065)	(0.347)	(0.102)	(0.134)	31,736
F-value	14.35	12.86	15.99	1.79	16.39	16.76	
Working time	0.473***	0.742***	0.210***	0.697**	0.328***	0.483***	
	(0.133)	(0.228)	(0.065)	(0.340)	(0.102)	(0.134)	32,702
F-value	13.69	11.65	15.17	1.85	15.29	15.88	
Group observations	42,963	22,952	20,789	11,388	32,353	36,303	

Notes: *In this analysis, I consider lower-educated individuals as those who have completed at most primary education (0-7 years of schooling) and higher educated as those beyond that level of education (8 years or more). Tab. 2 reports 2SLS estimates of the effect of the FAD index cutoff of 0.125 on the probability of participating in the program for the six groups studied. All effects have been calculated including all districts. Standard errors are in parentheses. Significance levels: *significant at 10%; **significant at 5%; ***significant at 1%.

Source: Author's calculations.

In what follows, I present the effects of the program on the different outcome variables. Appendix E shows the graphical effect of the program based on this discontinuity. The different figures of the appendix plot the probability of having a certain labour market status, of working time, and of being working poor,[33] conditional on participants living in districts with a FAD index greater than 0.125. All graphical effects have been measured nonparametrically using a standard kernel estimator. Given that RD is a local estimator, the analysis has been performed both in the overall window and in the neighborhood of the discontinuity for each output variable estimated. These effects are discussed below along with their corresponding RD estimates. As suggested in section 4.1, two different estimators have been used to assess the effect of *Construyendo Perú*: a parametric 2SLS setup and a nonparametric LLR with three different bandwidths.[34] The estimated results are shown in Tables 3 and 4, which corroborate the results from the graphical analysis. A comparison between the different bandwidths is presented in Section 7.

5.1 Effects of the program on participants' labour market status

As discussed above, one of the program's final objectives was to enhance the employability of individuals living in poverty and extreme poverty so they can find sustainable employment after the program culminates. Table 3 and panels A-G of Appendix E illustrate the effects of *Construyendo Perú* for the first seven outcome variables analyzed, two to five years after individuals participated in the program,[35] for the different groups of participants.

Estimates show that, in general, assignment to the program did have a positive effect on the probability of participants being employed and being active in the labour market for the overall population; the assignment had even clearer effects for women and the lower educated (i.e. individuals with primary schooling at most[36]), for whom coefficients are higher and more significant. In terms of the size of effects, assignment to the program increased the probability of women being employed by between 1 and 2 percentage points,[37] and reduced the probability of being inactive by roughly the same amount. These positive labour market effects are stronger for lower-educated individuals (between 2 and 3 percentage points).

Alongside these effects, the program increased participants' probability of being employed informally and decreased the probability of working formally, although of lower magnitude. Effects of being assigned to the program by status in employment show an increased probability of working as own-account workers and a decreased probability of working as waged employees. These results may provide some insights into the negative informal employment effects. Effects are again statistically significant for female participants and the overall population, but unlike previous results, also for higher-educated individuals and for men (although sometimes effects are less precisely estimated for

33 See Table B3 of Appendix B for the definitions and sources of all output variables.
34 The "optimal" bandwidth is selected using the standard (Imbens and Kalyanaraman, 2012) procedure, which is designed to minimize MSE (i.e. squared bias plus variance) (Nichols, 2007). The choice of the two alternative bandwidths is also standard and includes half and twice the optimal bandwidth. Considering the overall sample, the optimal bandwidth contains 12,077 participants and 181 districts; the half bandwidth, 5,244 participants and 97 districts; and the double bandwidth, 23,509 participants and 309 districts.
35 Since this evaluation assesses the effects of the program in 2012 for individuals who participated between 2007 and 2010.
36 For details on the definition of higher- and lower-educated individuals, see Tables 2, 3, and 4.
37 In other words, the difference in mean probability of being employed between individuals living in districts with a FAD index that falls on one side and the other of the cutoff point.

this latter group). In comparison, the effects on the probability of working informally are non-significant for lower-educated individuals. It is important to note that, whereas the sample by sex is almost perfectly balanced (around half of the sample has completed at most primary education), this is not the case by level of education. As such, the lack of statistically significant results for lower-educated individuals could be driven by the lack of statistical power resulting from a small sample. The significance of these effects is robust to alternative estimators and different bandwidths (see Section 7). The exception is the variable *waged worker*, for which only LLR estimates are statistically significant.

A final sub-group analysis was carried out to assess the particular effects of the program on departments with a higher proportion of urban inhabitants. Results remain roughly unchanged to those found for the overall population, not surprising given that these departments account for most program participants. This analysis suggests that the detrimental effects of the program on informal employment are not related to the unavailability of formal-sector jobs in departments with a higher proportion of rural inhabitants.

5.2 Effects of the program on working poverty and working time

The persistence of informal employment can also have detrimental effects on poverty, potentially endangering one of the primary objectives of the program. Table 4 and panels H-J of Appendix E illustrate the effects of *Construyendo Perú* for the remaining three outcome variables analyzed, confirming this concern. A higher probability of participation in the program increases participants' odds of being working poor for the overall group, for women, men, higher-educated individuals, and individuals living in urban districts; it is worth noting, though, that effects for men are not consistent across specifications. In terms of the size of the estimated coefficients, overall assignment to the program appears to increase the probability of being working poor by between 2 and 3 percentage points. In contrast, the effect is non-statistically significant and close to zero for the lower educated. The effect on working excessive hours is also positive for most groups, but it is often less precisely estimated and not necessarily consistent across specifications. Finally, no consistently significant effects are found in terms of the number of hours worked.

Table 3. Estimated effect of *Construyendo Perú* on labour market status

	All		Women		Men		Lower educated*		Higher educated*		Urban departments	
	2SLS	LLR	2SLS	LLR	2SLS	LLR	2SLS	LLR	2SLS	LLR	2SLS	LLR
Employed	0.10** (0.05)	0.14* (0.08)	0.11** (0.04)	0.18** (0.09)	0.10 (0.14)	0.11 (0.21)	0.23*** (0.09)	0.26** (0.13)	0.12 (0.07)	0.16 (0.11)	0.09* (0.05)	0.14 (0.09)
Inactive	-0.11* (0.06)	-0.13 (0.08)	-0.11** (0.05)	-0.20** (0.09)	-0.11 (0.14)	0.03 (0.20)	-0.23** (0.10)	-0.31*** (0.12)	-0.13 (0.08)	-0.16 (0.10)	-0.10 (0.06)	-0.15* (0.08)
Employed informally	0.25*** (0.08)	0.34*** (0.07)	0.19*** (0.06)	0.28*** (0.08)	0.51*** (0.20)	0.61** (0.22)	0.16* (0.09)	0.15 (0.11)	0.34*** (0.12)	0.48*** (0.12)	0.23*** (0.08)	0.35*** (0.08)
Employed formally	-0.13** (0.06)	-0.17*** (0.06)	-0.07** (0.03)	-0.11* (0.06)	-0.36* (0.21)	-0.44* (0.25)	0.06* (0.03)	0.09* (0.05)	-0.18* (0.10)	-0.30*** (0.11)	-0.12** (0.06)	-0.16*** (0.06)
Own-account worker	0.17*** (0.05)	0.22*** (0.07)	0.14*** (0.04)	0.18*** (0.06)	0.30* (0.17)	0.32* (0.19)	0.11 (0.08)	0.11 (0.11)	0.18** (0.07)	0.26*** (0.09)	0.15*** (0.05)	0.22*** (0.07)
Waged worker	-0.02 (0.06)	0.10** (0.05)	-0.00 (0.02)	0.08* (0.04)	-0.04 (0.23)	0.36* (0.22)	0.02 (0.06)	0.02 (0.09)	-0.02 (0.09)	0.14** (0.06)	0.01 (0.06)	0.14** (0.06)
Waged employee	-0.11** (0.06)	-0.27*** (0.08)	-0.07* (0.04)	-0.14* (0.08)	-0.30* (0.17)	-0.70** (0.27)	0.01 (0.01)	0.02 (0.03)	-0.12 (0.08)	-0.34*** (0.12)	-0.12** (0.06)	-0.28*** (0.08)
Observations	43,741	43,741	22,952	22,952	20,789	20,789	11,388	11,388	32,353	32,353	36,303	36,303

Notes: *For the purpose of this analysis, I consider lower-educated individuals those who have completed at most primary education (0-7 years of schooling), and higher educated, those beyond that level of education (8 years or more). Table 3 reports estimated effects of assignment to treatment to the program *Construyendo Perú*, conditional on crossing the FAD index cutoff point of 0.125, for the six groups studied. For each group, the first column reports 2SLS estimates (where standard errors have been clustered at district level) and the second column LLR estimates obtained using a triangular kernel regression model on both sides of the cutoff for the optimal bandwidth (see Footnote 34 for a discussion of the different bandwidths used and Section 7 for a comparison of effects among bandwidths). Standard errors are in parentheses. Significance levels: *significant at 10%; **significant at 5%; ***significant at 1%.

Source: Author's calculations.

Table 4. Estimated effects of *Construyendo Perú* on participants' income and working time

	All		**Women**		**Men**		**Lower educated***		**Higher educated***		**Urban departments**	
	2SLS	**LLR**	**2SLS**	**LLR**	**2SLS**	**LLR**	**2SLS**	**LLR**	**2SLS**	**LLR**	**2SLS**	**LLR**
Working poor	0.29***	0.23***	0.22***	0.24***	0.55**	0.29	0.06	0.01	0.35***	0.26**	0.25***	0.23***
	(0.09)	(0.08)	(0.06)	(0.09)	(0.23)	(0.23)	(0.12)	(0.17)	(0.13)	(0.10)	(0.08)	(0.08)
Logarithm of hours worked	0.05	0.33**	0.05	0.24*	0.14	0.60*	0.39	0.66**	0.03	0.35**	0.08	0.33**
	(0.09)	(0.13)	(0.09)	(0.14)	(0.22)	(0.31)	(0.26)	(0.34)	(0.13)	(0.17)	(0.08)	(0.13)
Excessive working time	0.07*	0.22**	0.05	0.15**	0.20*	0.46*	0.07	0.13	0.11*	0.29**	0.10**	0.22**
	(0.04)	(0.08)	(0.05)	(0.07)	(0.12)	(0.26)	(0.07)	(0.16)	(0.06)	(0.11)	(0.04)	(0.09)
Observations	31,736	31,736	14,601	14,601	17,135	17,135	7,368	7,368	24,368	24,368	26,067	26,067

Notes: * For the purpose of this analysis, I consider lower-educated individuals those who have completed at most primary education (0-7 years of schooling), and higher educated, those beyond that level of education (8 years or more). Table 4 reports estimated treatment effects of assignment to treatment to the program *Construyendo Perú*, conditional on crossing the FAD index cutoff point of 0.125, for the six groups studied. For each group, the first column reports 2SLS estimates (where standard errors have been clustered at district level) and the second column LLR estimates obtained using a triangular kernel regression model on both sides of the cutoff for the optimal bandwidth (see Footnote 34 for a discussion of the different bandwidths used and Section 7 for a comparison of effects among bandwidths). The sample has not been restricted to employed individuals only for the assessment of work quality. Standard errors are in parentheses. Significance levels: *significant at 10%; **significant at 5%; ***significant at 1%.

Source: Author's calculations.

6. Interpretation: What can we learn from the heterogeneity of effects?

As discussed in the previous section, *Construyendo Perú* had heterogeneous effects by sex and educational attainment; interpreting these effects can then raise a number of questions. In this section, I first focus on whether these heterogeneous effects can be related to the exposure that different groups of participants had to the program's components. Second, I delve into the changes in the budget allocated to the program, not only to further explore the implications of those changes in terms of the types of public investment projects selected, but also to rule out the presence of additional institutional factors (i.e. latent variables) that might be affecting both participation and outcomes. Third, I examine issues related to the nature and implementation of these public investment projects to see whether the heterogeneity of effects is driven by differences in the characteristics of those projects.

6.1 Exposure of different groups of participants to the different components offered by the program

As mentioned above, although public works programs have traditionally focused on men in the region, women actually represent the majority of participants on these programs. This is also the case in *Construyendo Perú*, where women's participation was disproportionately higher compared to the median distribution in the household survey. As pointed out by the field study carried out by MEF (Jaramillo et al., 2009), this difference might be explained by the low take-up rates for men, and could be behind the clearer and more robust effects of the program on women.

Qualitative evidence from the MEF field study (Jaramillo et al., 2009) shows that female participants have unstable labour patterns (e.g. multiple entries and exits from the labour market, usually working in temporary jobs). Thus, the detrimental effects of the program on women's employment status and working poverty may be related to the inability of the program to sustainably raise their employability and to change their labour patterns. For example, women in Peru suffer disproportionally from informal employment (while the urban informal employment rate for men was around 72% during the period 2007–13, for women, it stood at 83%). Hence, in the absence of components particularly targeted to raise their employability (e.g. specific type of training), the program may have simply perpetuated the informal and low-pay labour market trends of women. Existing literature on the effectiveness of ALMPs specifically targeted to vulnerable groups argues that in the absence of specific components aimed to raise employability, programs could have negative effects, due to stigma- and lock-in effects during participation (Hujer et al., 2004). Although the program included a training component (which was officially eliminated only in 2010), the monitoring of the program carried out by the MEF notes that already in 2009, no specific training had been provided. In addition, even when provided, the reach of the specific training in terms of number of participants treated remained low (e.g. one third of sampled participants affirmed having received specific training),[38] and the quality and depth of the courses were uneven among participants and between districts (e.g. specific training consisted only of informative sessions for 40% of the beneficiaries of this training).

Likewise, the difference in effects between higher- and lower-educated participants could also be linked to their participation in these different components of the program. Since participation in specific

[38] And only 6.6% of participants were certified after the training culminated (i.e. meaning they attended at least 70% of the training and validated the training) (Jaramillo et al., 2009).

training was voluntary, some purposive selection of more driven participants into this training is to be expected. In fact, as explained by the field study carried out by the MEF, some of these participants used the specific training to establish productive microenterprises that were likely located in the informal sector (ILO, 2016). The results of the impact evaluation seem to confirm this analysis: the program increased the probability of higher-educated participants of being self-employed and decreased their probability of being waged employees. This may explain why the program had a negative effect on the probability of higher-educated participants of having a better-quality job (e.g. formal, better paid, not working excessive long hours), while it had no effect on the probability of having a job. Meanwhile, for lower-educated participants (less likely to participate in this training and less exposed to open their small businesses), the program did improve their odds of being employed, possibly even in a formal job.

Finally, since the poorest sections of the Peruvian population are burdened disproportionately by informal employment, it can be argued that the effects of the program on working poverty are linked to its detrimental effects on the probability of working informally. The ENAHO shows, for example, that most working poor (around 90%) worked informally during 2007–13, mostly as own account workers (close to 60%). These figures are considerably higher than those for the non-working poor, of whom 77% worked informally during this period and a little over 35% as own-account workers. Moreover, relative to the whole population, a higher proportion of working poor had an occupation as unpaid family worker (close to 13%) but, interestingly, also as employer (over 10%). In addition, working poor have lower incomes (40% lower) for the same number of hours worked. They are not substantially less educated than the overall occupied sample (on average, they have completed over 9 years of schooling compared to 11 for the overall sample), and the proportion of women is only slightly higher. In summary, the informal working status is mainly what separates the working poor from the rest of the population.

Regarding the program's possible effects on working time, participation does not show a clear change on the total hours worked or on the probability of working excessive hours. Although estimates for hours worked are positive across the board (showing an increase of 33%, or 15 hours per week, for the overall population), only local treatment effects are significant (except for the lower educated) and often only at the 10% level. As to the probability of working excessive hours, positive effects are not systematically significant or consistent across estimators.

The lack of robustness and/or significance of these effects may be explained by the longer hours that Peruvians spend working in more formal jobs and in occupations less common among *Construyendo Perú*'s participants. For example, while individuals working formally reported an average of 50 hours per week (in all occupations confounded) during the period, those who worked informally reported an average of 45 hours. Consequently, the share of individuals working excessive hours was also higher among formal workers than informal ones (around 47% compared to 42%, respectively). Likewise, by occupation, employers reported the highest number of hours worked, with close to 53 hours per week (in all occupations confounded); they were followed by waged workers, with around 50 hours; and finally, waged employees and own-account workers, with 47 hours per week. Employers also had the highest share of individuals working excessive hours (over 56%), while this share was close to 47% for each of waged workers and own-account workers.

On balance, it appears that heterogeneous effects by sex and educational attainment are related to the exposure that different groups of participants had to the components provided by the program, possibly through the different occupations they ended up with following program participation.

6.2 Changes in the budget allocated to the types of public investment projects selected

As mentioned in Section 3, the budget allocated to the program varied from one year to the next depending on the fiscal space available by the central government, which assigned funds in a more or less discretionary manner. Indeed, during the period 2007–10, *Construyendo Perú* spent close to 648 million PEN (425 million USD, PPP), which benefited around 685,000 individuals, through temporary positions varying considerably in length from a few weeks to 4 months.[39] The program also financed over 11,300 projects, most of which were aimed to create pedestrian accesses, retaining walls, and educational and health infrastructure. These figures, however, vary drastically from one year to the next. From 2007, total expenditure and the number of standardized-duration jobs decreased, then hit a trough in 2009, and increased again in 2010, although not to their initial peak (Figure 7).[40] This reduction in the budget allocated to the program is explained by a fall in transfers from the central government due to the world financial crisis that hit Peru in 2009. Meanwhile, short-term jobs fell in 2008 but remained relatively stable from then-on,[41] and the number of projects fell gradually until 2009 and then bounced back to their highest level in 2010. The difference between the trend in short-term jobs and that in the number of projects is related to a dramatic change in the type of projects financed by the program during the last two years. While during the period 2007–08, most projects (58%) were related to public infrastructure, during the period 2009–10, over 80% of projects were related to the services sector (Macroconsult S.A., 2012).

[39] When looking at the comparable notional four-month short-term job, *Construyendo Perú* created 72,700 four-month jobs per year on average (or close to 291,000 during the period 2007–10).

[40] It is important to note that the change in the yearly allocation of funds could have implications in terms of a change in the yearly cutoff point. An analysis (available upon request) was therefore carried out to test different cutoff points by year, suggesting very minor or no changes in the cutoff point from one year to the next. In fact, the analysis shows that districts with the highest and lowest FAD index remained in the treatment sample every year. As such, increases and reductions in budget allocation affected districts in the middle of the distribution but did not affect the cutoff point.

[41] This is the variable for short-term jobs illustrated in Figure 2.

Figure 6. Total budget of *Construyendo Perú*, number of jobs created, and number of projects executed

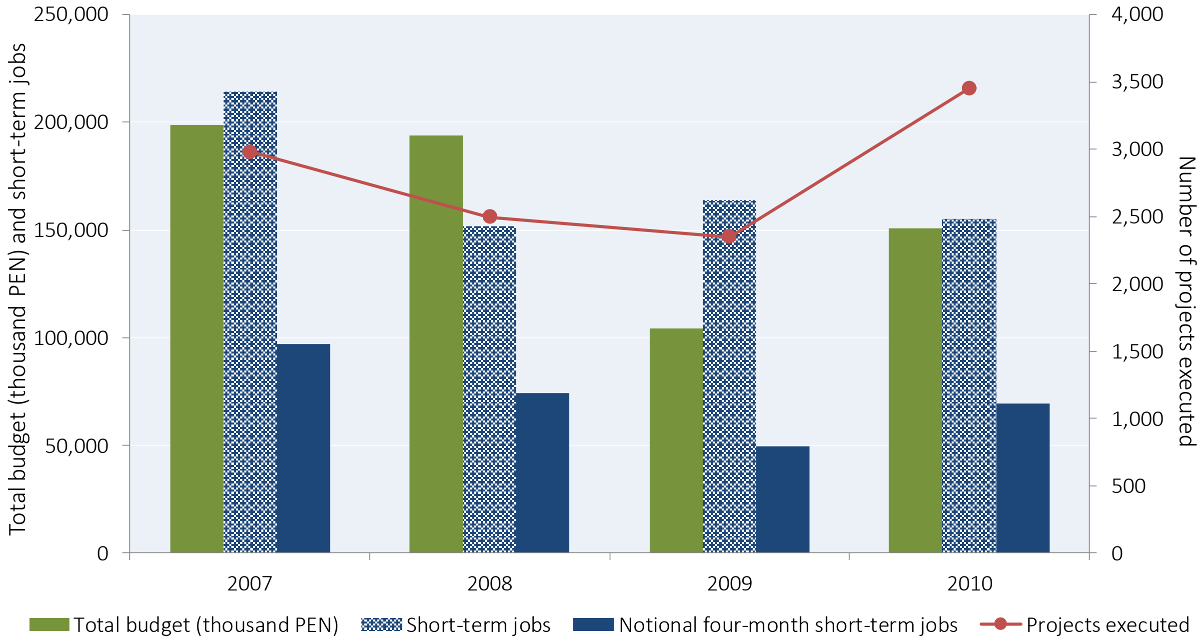

Fig. 7 illustrates the total budget of *Construyendo Perú* during the period 2007–10, along with the number of jobs created and number of projects executed. In terms of the number of jobs created, the figure shows the total number of short-term jobs, which amounts to the total number of beneficiaries. Moreover, given that jobs varied considerably in length, the figure also displays the number of notional four-month short term jobs (see Footnote 39), so there is comparability between years.

Source: Author's calculations based on MTPE (2007–11).

Two main factors seem to explain this radical move from infrastructure- to service-related projects. First, projects implemented during the first two years were of longer duration and more costly, so in an effort to maintain a more or less stable number of beneficiaries at a lower cost, service-sector projects were used more prominently from 2009 onwards (Macroconsult S.A., 2012). Second, as explained by former program officials, this second period coincided with an increased focus (and spending) by the government on public infrastructure, which reduced the need to use public work programs to address deficits in public services.

Thus, the question that emerges is whether this move from infrastructure- to service-related projects had an effect on district participation and whether this effect was randomly distributed. One could argue, for example, that budget constraints combined with this increased focus on service-related projects could have opened the door for picking districts (within the FAD index cutoff point), depending on their sectorial specialization or political orientation. A thorough analysis of the selection of public investment projects (Appendix A), however—including several meetings with former government officials—suggests an apparent complete independence between the agents selecting the public investment projects (composed of representatives of different governmental and non-governmental groups at the national and local levels) and those involved in the assignment of the program. As such, it seems we can be confident that no institutional factor biased participation of districts based on the selection of public investment projects, and the changes in the budget allocated by type of project affected districts randomly.

6.3 Differences in the nature and characteristics of the public investment projects

As gathered from the above discussion, the implementation of *Construyendo Perú* was done in two very distinct phases. The first phase ran from 2007 to 2008 and was characterized by a more generous budget, the financing of public infrastructure-type projects, and the provision of the two different training components, a general and a technical one. During the last two years of activity (2009–10), the program's implementation was determined by the world economic crisis, which brought about a reduction in the program's budget. Moreover, the program was executed through the financing of service-sector related projects of shorter duration, and both training components were abandoned.

Given these drastic changes, effects are likely to be different between the two periods of the program's operation. To test this hypothesis, I estimate the effects of *Construyendo Perú* on all outcomes during the three periods 2007–10, 2007–08, and 2009–10, for the overall group of participants and for women (which account for the bigger samples). For this analysis, I use a slightly different strategy than the one discussed in Section 3.3, taking participants from the special survey and their comparable controls from ENAHO, so I can identify individuals who received treatment during the different periods. Given the caveats of estimating effects based on this integrated two-different sample database (see Section 3.1), the assessment below needs to be taken as suggestive evidence only. I thus focus only on the sign, significance, and relative magnitude of coefficients, rather on the point estimates themselves, with views of gaining additional insights into the reasons behind the effects of the program. Appendix F illustrates the 2SLS estimates, the only possible estimator in this case, as the sample resulting from splitting participants by years and subgroups is not sufficiently large to have stable LLR estimates, particularly for the period 2009–10.[42]

Estimates for the overall population are similar in terms of direction and significance between the three periods analyzed, but the size of effects is consistently higher during 2009–10. Beneficiaries of the program during the last two years appear to have a higher probability of being employed informally than those participating during the first two years, as well as a higher probability of working as own-account workers and a lower probability of working as waged employees. Finally, the probability of being working poor also increases during the period 2009–10. 'Frontier effects' could be influencing the results, since individuals who benefited during the first phase of implementation are observed further away (4 to 5 years after participation) than those who benefited during the second face of implementation (2 to 3 years after participation). To ensure this is not a problem, I compare effects between 2008 and 2009 in columns 4 and 5 of the table. Effects remain clearly stronger during 2009, which means that they are not being influenced by the distance to participation. In terms of the estimates for women, effects on their probability to be employed and active in the labour market seem to be driven by their participation during the last phase of the program.

The question is therefore what is driving these more detrimental effects on employment quality during the second phase of the program's implementation. I first examine whether these differences are related to changes in the economic cycle, particularly, the global crisis that hit Peru in 2009–10. The analysis suggests otherwise. In fact, the labour market effects of the global crisis never materialized in Peru. The differences in the share of people in employment and unemployment between the two periods are very small (0.8 and 0.5 percentage points, respectively), and the difference in the share of people outside the

[42] The complete set of results, including effects for all groups of participants, is available from the author on request.

labour market is insignificant.

Second, I examine whether participants' characteristics are different between the two implementation phases. Few observable differences are indeed significant, including a slightly higher share of women among participants during the second phase (81% compared to 77% during the first phase) and a higher monthly income. Differences between the two periods in terms of family composition or educational attainment, on the contrary, are not significant.

Third, I examine the effect of differences in the nature of the public investment projects used to execute the program during the two phases. To assess this, I estimate the effects of *Construyendo Perú* separately for beneficiaries working in infrastructure-related public investment projects (categorized in the database as construction projects) and service-sector-related ones (categorized in the database as maintenance and rehabilitation projects).[43] Table 5 presents the 2SLS estimates, which should again be considered as suggestive evidence only. I calculated two types of estimates for each type of project. Columns 2 and 3 illustrate the effects of the program for individuals who participated *mainly* in one type of project but who could have participated eventually in the other type, as well. Columns 4 and 5 illustrate the effects of the program for individuals who participated *solely* in each type of project. Estimating these latter effects implied losing about half of the participants in each case.

The table illustrates that detrimental effects are stronger for individuals who participated in service-related projects. Given that the main difference between infrastructure- and service-sector-related public investment projects is their duration and training provision, it could be assumed that the detrimental impacts on job quality are driven by these components. In other words, longer-duration projects—which involve higher public investment, including the provision of training—produce lower informality and working poverty.[44]

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[43] In the special survey, participants were asked how many times they participated in construction, rehabilitation, and maintenance projects. For the purpose of this analysis, construction projects are categorized as infrastructure-related projects, and maintenance and rehabilitation projects are categorized as service-sector-related projects.

[44] It is difficult to know whether different coefficients for all the periods and groups analyzed are significant (i.e. small variation between coefficients combined with relatively large standard errors). Since the size of standard errors in this analysis is driven by the small sample size resulting from separately estimating the effects of the program for various periods and groups, it could be the case that coefficients are indeed different, and effects are more significant than what they appear to be (i.e. effectiveness might be penalized by the mechanically high standard errors). It is telling that despite the sample size limitations, the different analyses produce results that go consistently in the same direction, giving confidence to the coherence of the results.

Table 5. Effects of *Construyendo Perú* by type of public investment project

	Overall effects	The effects of participation in:			
		Mainly construction	Mainly maintenance	Only construction	Only maintenance
A. Employment status					
Employed	2.1 (1.5)	3.5 (2.3)	3.8 (-0.0)	5.1 (3.5)	5.9 (4.5)
Inactive	-2.3 (1.7)	-3.7 (2.5)	-4.0 (3.0)	-5.3 (3.7)	-6.2 (4.8)
Employed informally	5.5** (2.4)	8.4** (3.8)	9.3* (4.9)	12.3** (5.6)	14.5* (8.3)
Employed formally	-3.0** (1.4)	-4.2** (2.1)	-4.7* (2.8)	-6.2** (3.1)	-7.4 (4.7)
Own-account worker	3.6** (1.5)	5.5** (2.3)	6.2** (3.1)	8.1** (3.5)	9.7* (5.2)
Waged worker	-0.02 (1.3)	-0.3 (2.0)	-0.5 (2.3)	-0.6 (2.9)	-0.8 (3.6)
Waged employee	-2.8** (1.4)	-3.8* (2.1)	-4.3* (2.6)	-5.6* (3.1)	-6.8 (4.3)
B. Income and working time					
Monthly income scales	-16.8* (9.2)	-24.7** (12.2)	-30.5 (18.9)	-40.7** (20.1)	-59.1 (49.9)
Working-poor	7.6*** (2.7)	9.9*** (3.2)	13.6** (6.9)	15.9*** (5.1)	28.8 (23.8)
Number of hours worked	1.5 (2.4)	1.4 (3.0)	2.1 (4.3)	2.4 (4.8)	4.5 (9.8)
Excessive working time	1.7 (1.3)	2.4 (1.8)	3.3 (2.5)	4.0 (2.8)	7.3 (6.6)
Obs. employment status	46,664	44,305	44,346	43,989	44,030
Obs. working time	34,635	33,075	33,076	32,844	32,845
Number of participants	1142	710	753	388	431

Notes: Table 5 reports estimated treatment effects of the program *Construyendo Perú* conditional on crossing the FAD index cutoff point of 0.125, for the overall pool of participants, for beneficiaries of infrastructure- (i.e. construction), and service-sector-related (i.e. maintenance) projects. The table reports 2SLS estimates, clustered at the district level. All effects have been calculated including all districts. Standard errors are in parentheses. Significance levels: *significant at 10%; **significant at 5%; ***significant at 1%.

Source: Author's calculations.

This section will present the main results of the macroeconomic analysis on the impact of overall spending in active and passive labour market policies (i.e. without differentiating by type of intervention) on labour market indicators. In particular, Section 4.1 will present the results of our preferred specification (both for the overall sample and splitting the countries according to their development status); while Section 4.2 reports a large set of robustness tests aimed at exploring the extent to which our results are sensitive to slight changes in the identification strategy.

Although an additional assessment of the effectiveness of the training components would have been desired, this analysis is not possible, given sample size and unavailability of detailed information on the participation by type of training component. However, I believe that examining the underlying nature and characteristics of the public investment projects has provided new insights. This analysis suggests that changes in the project's characteristics from one phase to the other of *Construyendo Perú*'s implementation seem to have negatively shaped the program's effects. Given the projects' characteristics during the latter phase of the program's implementation (e.g. low budget and short duration), it is unlikely than any training component could have had strong positive effects.

7. Robustness checks

Three different informal sensitivity tests were carried out to check how changes in the estimation strategy affect results.[45] First, the use of different estimation methods inherently constitutes a first test. As discussed above, estimated treatment effects are generally robust to the use of different parametric and nonparametric estimation methods. Indeed, results using the parametric 2SLS setup are similar to those calculated through the nonparametric LLR using the optimal and larger bandwidths. Yet, the size of effects is smaller when using the parametric method.

Second, as suggested by (Nichols, 2007), an additional informal sensitivity test while using the nonparametric LLR consists of estimating the effects of the program using twice and half the optimal bandwidth. Estimates, presented in Appendix G, show overall consistent results in terms of significance using the different bandwidths (the size of effects is, in most cases, larger using narrower bandwidths).

Third, different estimations have also been carried out including and excluding districts with an urban population of fewer than 2500 inhabitants (i.e. first eligibility criteria during geographical targeting). Results using the 2SLS specification and LLR with the optimal, double, and half bandwidths are consistent between the two samples. 2SLS estimates excluding smaller districts are systematically (yet slightly) smaller in magnitude and few times of lower significance.

I also carried out falsification tests to assess whether non-targeted groups (or less targeted ones) have been affected by the program. Similar effects on non-participants would mean that other measures could be generating the observed impacts, invalidating the causality of effects. Three particular non-participant groups are inspected. The first consists of districts not targeted by *Construyendo Perú*, namely those with an urban population below 2500 individuals. The second and third are composed of individuals who should not normally be affected by the program, namely individuals having completed higher education (i.e. individuals with a university degree and beyond) and the wealthiest individuals (i.e. highest decile of annual per capita income).

Panels A, B, and C of Figure 8 illustrate non-significant effects across the board. First, there is no clear discontinuity in the FAD index for individuals living in small districts (panel A), having completed higher education (panel B), or being in the highest decile of annual per capita income (panel C). Second, RD estimates for these groups (available upon request) illustrate non-significant treatment effects, regardless of the size of the bandwidth.

[45] Partial equilibrium effects were also assessed given the variability in the number of participants between districts. However, given the low shares of participants in the total population per district (0.74% in average), and the low variance among districts shares (std. dev=1.4), I conclude that partial equilibrium effects are not affecting results.

Figure 7. Discontinuity in the FAD index for specific non-targeted groups

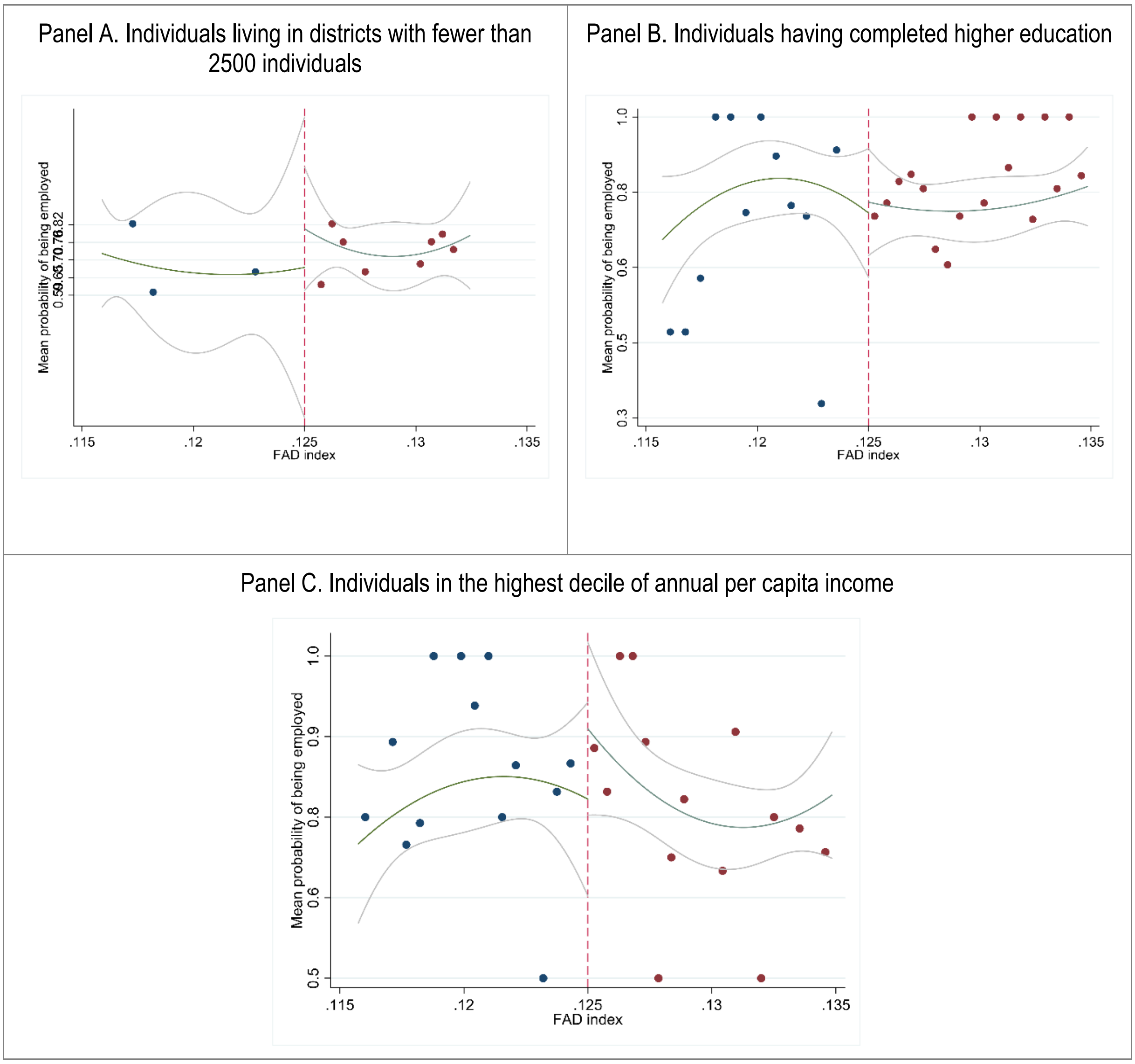

Fig. 8 plots the mean probability of being employed according to the FAD index, along with the 95% level confidence bounds, for individuals in three categories. The conditional mean is drawn on the base of equal-sized bins (i.e. each dot in graph corresponds to a bin; see Footnote 29). The fit used was suggested by the graphical analysis carried out using Lowess fit.

Source: Author's calculations.

Finally, a potential concern is whether the effects of the program assessed in this paper have been underestimated or overestimated, since they have not considered potential indirect effects on non-participant individuals within treated districts. The existing literature on the indirect effects of policy programs in developing countries indeed points to the importance of these locality-wide effects (Angelucci and De Giorgi, 2009). In the case of *Construyendo Perú*, the setting at the individual level does not provide a clean identification strategy to assess these indirect effects,[46] but the allocation of the program at the district level could provide a good approximation. Higher or lower effects at the district level than those found at the individual level would point to the existence of indirect effects. However, as the district-level database does not have sufficient statistical power (not enough

[46] Figure 8 suggested, however, that there were no indirect effects with respect to some groups of individuals, such as the highest educated and the highest earners. It also suggests that the program did not have indirect effects on households in non-eligible districts.

observations), the analysis should be taken as suggestive only. Table 6 shows the effects of the program on employment status and working time for districts according to the FAD index. While point estimates are not precisely estimated across the board, they have the same sign and similar magnitude of the individual-level effects discussed in Section 5, pointing to the absence of indirect effects. The two exceptions include the effect of the program on the probability of working as own-account, which is clearly smaller at the district level; and the effect on working poor, which is larger at the district level.

Table 6. Effects of *Construyendo Perú* on employment status and working time at the district level

	Employed	Inactive	Informal	Formal	Own-account worker	Waged worker	Waged employee	Working poor	Hours worked	Excessive time
2SLS estimator	0.063	-0.058	0.242	-0.145	0.048	-0.040	-0.111	0.382*	0.229	0.109
	(0.084)	(0.092)	(0.159)	(0.114)	(0.087)	(0.095)	(0.117)	(0.220)	(0.230)	(0.107)

Notes: Table 6 reports estimated treatment effects of the program *Construyendo Perú* at the district level conditional on crossing the FAD index cutoff point of 0.125. For each group, the first column reports 2SLS estimates (where standard errors have been clustered at district level). All effects have been calculated including eligible (urban) districts. Standard errors are in parentheses. Significance levels: *significant at 10%; **significant at 5%; ***significant at 1%. Number of districts: 467.

Source: Author's calculations.

8. Conclusions

In this paper, I exploit a unique feature of *Construyendo Perú*'s assignment criteria, namely, the fact that districts are ranked according to a composite index (FAD), and those below a threshold are not eligible to participate. A fuzzy RD approach is therefore used, drawing upon three distinct sources of information: (i) a district level database (created for this study); (ii) a special survey given to program participants in March 2012 (Macroconsult S.A., 2012); and (iii) the ENAHO household survey from 2007 to 2013. The evaluation assesses the effects of the program in 2012 for individuals who participated between 2007 and 2010. While the intervention helps raise employment and reduce inactivity for particular groups, it does so at a cost of locking participants in lower quality jobs (i.e. informal, paid below the poverty line, and working excessive hours).

Specifically, the program increased the probability of the overall population, women, and lower-educated participants of being employed and attached to the labour market, while for men and higher-educated individuals, the program had no effect on employment and inactivity. The lack of employment effects for certain groups may be related to deadweight losses (i.e. participants would have found a job in the absence of the program), as most participants were already engaged in a remunerated activity before the program started. Another possibility is that the program did have positive short-term effects, but that they faded away with time (i.e. especially given that effects in this paper are measured over the medium- to long-terms). This hypothesis was, however, challenged by the analysis of 'frontier effects', carried out by comparing effects on beneficiaries who participated in the program in 2008 and 2009.

Alongside these labour market effects, the program increased the probability of participants being employed informally and being working poor. These effects are again statistically significant for women and the overall group of participants, but unlike previous results, also for men and for higher-educated participants. The effects seem to be related to the impact of the program on the status in employment—i.e. the program increases the probabilities of participants working as own-account workers and decreases their probability working as waged employees. In other words, the program increases the odds of participants working in occupations characterized by having lower job quality. Given that in Peru, the poorest sections of the population are disproportionately burdened by informal employment, it can be argued that the effects of the program on working poverty are linked to those on informality.

The key question, therefore, is what is driving these detrimental effects of the program on work quality? First, qualitative evidence (on the program's implementation and the characteristics of the different groups of participants) suggests that heterogeneous effects are related to the participants' exposure to the different components provided by the program. Second, changes in political priorities and availability of resources in 2009 appear to have driven a radical move from infrastructure- to service-sector-related public investment projects between the first and last two years of the program's implementation, with detrimental effects of the program's results. Third, an analysis of the nature and implementation of these two types of public investment projects suggests that the heterogeneity of program effects is indeed related to the differences in projects' characteristics, namely the duration of the projects and the provision of training. It appears that the detrimental effects in terms of job quality are worsened for individuals participating in service-sector-related projects, which could be attributed to the cut in public investment that reduced the duration of projects and eliminated the provision of training.

These results are in line with findings from the empirical literature that have extensively argued that the success of workfare programs hinges on their design and implementation characteristics (Subbarao et al., 2013). As this paper points out, *Construyendo Perú* does not appear to be an exception in this regard. The paper also finds that in addition to the challenges posed by the selection and characteristics of public investment projects, the program suffered from multiple participation and overrepresentation of particular groups, which can be an indication of the need for better enforcement of targeting rules and eligibility criteria. Although *Construyendo Perú* no longer exists under this name, many developing countries continue to implement workfare programs with similar characteristics, making the results of this paper all the more relevant. This is the case for Peru, where the program was supplanted by the similar *Trabaja Perú*, which remains active today.

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Appendices

Appendix A: Selection of public investment projects

As mentioned in the text, the program provided access to temporary employment and skills development through the financing of public investment projects intensive in the use of unskilled labour. The program was organized as follows: Once the geographical targeting (i.e. first step of the program assignment) was completed, the selection of regular public infrastructure and service-sector projects (large majority of projects accounting for around 85% of the total budget allocated to the program) was launched by the local offices through a call for tender aimed to decide which specific projects would be implemented in the selected districts. At this point, proponent organizations, which could include civil society organizations, private, or public institutions, prepared and presented their projects.

The local offices then verified that projects complied with a number of pre-established eligibility criteria: that projects responded to the districts' needs, that they were labour intensive, and that the complexity of their execution did not require the full-time presence of a specialist (i.e. engineer). Importantly, these criteria did not change during the whole duration of the program.

Eligible projects were then prioritized according to the same criteria by the interinstitutional district committees (*comités interinstitucionales distritales*), composed of two representatives from civil society, one from the local government, another from the central government, and one representative from the program, to decide on the projects that could be financed depending on the available budget. While the first three actors decided on the selection of projects, the program representative's role was to observe and oversee the selection process. Finally, on this final list of projects, the program checked the viability, taking into account technical, financial, and documentary aspects.

Once the selection of the projects was done and validated, agreements were signed between the program and the executing agencies and, if applicable, the co-financers. After this, the program entered the second and third targeting strategies discussed in Section 2 (i.e. self-targeting and individual targeting), and once participants were selected, the execution of the projects could take place. During this project implementation phase, the role of *Construyendo Perú* was to finance and oversee the development of the projects that were put in place by public and private implementing agencies.

Appendix B: Definitions of variables and descriptive statistics

Table B1. Definitions and sources of variables of the district-level database

Variable	Definition	Source
Urban population	Population living in areas of a district with 100 or more dwellings laid out contiguously forming urban centers. Districts may be comprised of one or more populated urban centers.	(INEI, 2007)
Poverty severity index (FGT2)	The FGT(2), or Squared Poverty Gap Index, is one of the indexes of the Foster, Greer, Thorbecke family of poverty measures, which measures the severity of poverty and gives a greater weight to individuals who fall far below the poverty line than to those closer to it.	(INEI, 2009)
Human development index (HDI)	A summary measure of average achievement in key dimensions of human development, namely: a long and healthy life, being knowledgeable, and having a decent standard of living. The HDI is the geometric mean of normalized indices for each of the three dimensions.	(UNDP, 2009)
Index of human development shortcomings	An index calculated by FONCODES as 1-HDI of UNDP and called officially *Índice de carencia (IC). IC* measures the level of deprivation of the population in the access to basic services and the level of vulnerability in terms of illiteracy and children's malnutrition. Values closer to 1 represent districts with higher deprivation and vulnerabilities, and therefore, districts with higher priority in terms of social investment.	(De la Torre, 2005)
Districts participating by year (2007–2010)	Districts that have received funding to participate in the program "Construyendo Perú".	MTPE (2009, 2007a, 2007b)
Allocation factor at the district level (*Factor de Asignación Distrital*, FAD)	A composite index constructed by the Planning Management Unit of the program until 2010 on the basis of three indicators weighted equally: (i) urban population, (ii) the index of human development shortcomings, and (iii) the poverty severity index FGT(2).	Author's calculations based on (Jaramillo et al., 2009)

Table B2. Descriptive statistics

	Total urban population (18+)						Participants (18+)		
	2007			2012			March 2012		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Individual characteristics									
Male	39279	0.480	0.500	43826	0.475	0.499	1142	0.217	0.412
Age	39279	40.49	16.83	43826	42.84	17.58	1142	43.49	12.53
Household members	39279	4.863	2.260	43826	4.57	2.16	1142	4.46	1.83
Marital Status									
Cohabiting	39279	0.242	0.428	43826	0.236	0.425	1142	0.367	0.482
Married	39279	0.346	0.476	43826	0.332	0.471	1142	0.295	0.456
Widowed	39279	0.052	0.223	43826	0.058	0.235	1142	0.067	0.251
Divorced	39279	0.004	0.067	43826	0.006	0.078	1142	0.002	0.042
Separated	39279	0.077	0.267	43826	0.094	0.292	1142	0.170	0.376
Single	39279	0.277	0.448	43826	0.273	0.446	1142	0.099	0.299
Kinship family									
Head	39279	0.517	0.500	43826	0.516	0.500	1142	0.467	0.499
Spouse	39279	0.284	0.451	43826	0.279	0.449	1142	0.496	0.500
Son or daughter	39279	0.195	0.397	43826	0.201	0.401	1142	0.038	0.190
School attendance	39279	0.076	0.265	43826	0.078	0.268	1142	0.003	0.051
Educational attainment									
No education	39279	0.046	0.209	43826	0.044	0.205	1142	0.075	0.264
Initial education	39279	0.000	0.007	43826	0.000	0.021	1142	0.003	0.051
Incomplete primary	39279	0.117	0.322	43826	0.111	0.314	1142	0.220	0.414
Primary education	39279	0.111	0.314	43826	0.105	0.307	1142	0.176	0.381
Incomplete secondary	39279	0.132	0.339	43826	0.119	0.323	1142	0.187	0.390
Secondary education	39279	0.272	0.445	43826	0.268	0.443	1142	0.257	0.437
Incomplete post-secondary	39279	0.053	0.224	43826	0.053	0.224	1142	0.024	0.152
Post-secondary education	39279	0.101	0.301	43826	0.107	0.309	1142	0.035	0.184
Incomplete tertiary	39279	0.069	0.253	43826	0.086	0.280	1142	0.015	0.121
Tertiary education	39279	0.083	0.276	43826	0.088	0.284	1142	0.009	0.093
Post-tertiary education	39279	0.014	0.117	43826	0.018	0.132	1142	0	0
Department									
Amazonas	39279	0.025	0.156	43826	0.024	0.151	1142	0.025	0.155
Ancash	39279	0.037	0.188	43826	0.040	0.196	1142	0.035	0.184
Apurímac	39279	0.017	0.128	43826	0.016	0.125	1142	0.032	0.177
Arequipa	39279	0.050	0.217	43826	0.049	0.216	1142	0.035	0.184
Ayacucho	39279	0.029	0.168	43826	0.027	0.163	1142	0.035	0.184
Cajamarca	39279	0.022	0.147	43826	0.018	0.134	1142	0.035	0.184
Cusco	39279	0.027	0.163	43826	0.029	0.168	1142	0.032	0.175
Huancavelica	39279	0.017	0.128	43826	0.016	0.124	1142	0.027	0.163
Huánuco	39279	0.025	0.156	43826	0.023	0.149	1142	0.035	0.184
Ica	39279	0.048	0.213	43826	0.058	0.233	1142	0.035	0.184
Junín	39279	0.040	0.196	43826	0.042	0.201	1142	0.033	0.179
La Libertad	39279	0.041	0.199	43826	0.043	0.203	1142	0.032	0.175
Lampayeque	39279	0.046	0.210	43826	0.049	0.217	1142	0.036	0.186
Lima y Callao	39279	0.237	0.425	43826	0.220	0.414	1142	0.217	0.412
Loreto	39279	0.044	0.205	43826	0.043	0.204	1142	0.035	0.184
Madre de Dios	39279	0.026	0.158	43826	0.022	0.148	1142	0.027	0.163
Moquegua	39279	0.031	0.173	43826	0.034	0.181	1142	0.030	0.170
Pasco	39279	0.026	0.159	43826	0.029	0.169	1142	0.034	0.182
Piura	39279	0.052	0.221	43826	0.051	0.220	1142	0.034	0.182
Puno	39279	0.024	0.154	43826	0.019	0.135	1142	0.069	0.254
San Martín	39279	0.036	0.187	43826	0.036	0.187	1142	0.032	0.177
Tacna	39279	0.033	0.179	43826	0.037	0.188	1142	0.034	0.182
Tumbes	39279	0.035	0.183	43826	0.037	0.188	1142	0.034	0.182
Ucayali	39279	0.033	0.179	43826	0.039	0.193	1142	0.026	0.160

Household characteristics									
Annual household income	39279	10390.1	14677.9	43826	13916.0	17421.1	1142	8510.1	9534.1
Annual household income per capita	39279	2363.7	4134.1	43826	3208.2	4583.1	1142	1976.9	2252.8
Monthly income in main occupation	39279	502.9	1024.9	43826	713.1	1229.4	1142	364.9	108.8
Labour characteristics									
Employed	39279	0.720	0.449	43826	0.725	0.447	1142	0.680	0.467
Type of occupation									
Employer	39279	0.048	0.214	43826	0.046	0.209	1142	0.002	0.042
Own-account worker	39279	0.262	0.440	43826	0.270	0.444	1142	0.331	0.471
Waged employee	39279	0.195	0.396	43826	0.201	0.401	1142	0.053	0.223
Waged worker	39279	0.132	0.338	43826	0.141	0.348	1142	0.235	0.424
Unpaid family worker	39279	0.076	0.264	43826	0.070	0.254	1142	0.017	0.128
Domestic worker	39279	0.025	0.155	43826	0.017	0.129	1142	0.045	0.207
Other	39279	0.004	0.066	43826	0.004	0.066	1142	0.002	0.042
Type of contract									
Permanent contract	39279	0.069	0.253	43826	0.073	0.260	1142	0.009	0.093
Fixed-term contract	39279	0.088	0.284	43826	0.104	0.305	1142	0.103	0.305
Probation period	39279	0.000	0.019	43826	0.001	0.026	1142	0.002	0.042
Youth training agreement	39279	0.002	0.050	43826	0.002	0.044	1142	0.001	0.030
Apprenticeship program	39279	0.000	0.016	43826	0.019	0.137	1142	0	0
Service provider	39279	0.018	0.134	43826	0.010	0.100	1142	0.007	0.083
Working without contract	39279	0.245	0.430	43826	0.219	0.413	1142	0.233	0.423
Unemployed	39279	0.035	0.184	43826	0.027	0.163	1142	0.067	0.251
Duration of unemployment									
Less than 1 month	39279	0.031	0.173	43826	0.025	0.156	1142	0.046	0.210
From 1 to 3 months	39279	0.003	0.057	43826	0.002	0.048	1142	0.013	0.114
From 3 to 6 months	39279	0.000	0.020	43826	0.000	0.017	1142	0.003	0.051
More than 6 months	39279	0.000	0.016	43826	0.000	0.005	1142	0	0
Actively looking for a job	39279	0.032	0.176	43826	0.025	0.156	1142	0.057	0.232
Inactive	39279	0.215	0.411	43826	0.233	0.423	1142	0.220	0.414
Variables related to job quality									
Employed informally	39279	0.590	0.492	43826	0.550	0.497	1142	0.622	0.485
In the informal sector	39279	0.156	0.362	43826	0.170	0.376	1142	0.204	0.403
In the formal sector	39279	0.435	0.496	43826	0.380	0.486	1142	0.418	0.493
Employed formally	39279	0.150	0.357	43826	0.198	0.398	1142	0.061	0.240
Discouraged	39279	0.028	0.165	43826	0.014	0.116	1142	0.032	0.177
Working poor	28292	0.466	0.499	31774	0.359	0.480	777	0.407	0.492
Hours worked in main job	28660	41.87	23.32	32799	39.99	22.21	780	40.43	17.80
Total usual hours worked	28653	48.05	22.16	32740	45.75	21.20	780	43.67	16.42
Excessive working time	28653	0.458	0.498	32740	0.414	0.492	780	0.322	0.467
Underemployed (time-related)	39279	0.259	0.438	43826	0.154	0.361	1142	0.210	0.408
Less than 1 month	39279	0.448	0.497	43826	0.410	0.492	1142	0.502	0.500
From 1 to 5 months	39279	0.201	0.401	43826	0.229	0.420	1142	0.148	0.355
From 6 to 11 months	39279	0.091	0.287	43826	0.109	0.312	1142	0.033	0.179
From 1 to 4 years	39279	0.198	0.398	43826	0.204	0.403	1142	0.317	0.466
From 5 to 10 years	39279	0.136	0.343	43826	0.120	0.325	1142	0.130	0.337
More than 10 years	39279	0.177	0.382	43826	0.177	0.381	1142	0.088	0.284

Table B3. Definitions and sources of labor market output variables

Variable	Definition	Source
Labor market status		
Employed	Individuals who had an occupation during the week of reference, remunerated or not, but were working more than 14 hours.	ENAHO
Inactive	Individuals who were not in the economic active population during the week of reference. This includes individuals not in employment or unemployment, and individuals who had an occupation as unpaid family workers or "other" but were working less than 15 hours per week.	ENAHO
Informal worker	Individuals whose main occupation is in informal employment. Includes: (i) individuals working in the informal sector;[47] (ii) non-remunerated family workers; (iii) and individuals working in the formal sector who are not affiliated to any pension system. The pension insurance system has been used as a proxy for health insurance, since it is the only social protection information available in ENAHO.	ENAHO based on ILO definition. Definition has been adapted according to data availability in the survey.
Formal worker	Individuals whose main occupation is in formal employment. Includes those working in the formal sector who are affiliated with a pension system. The pension insurance system has been used as a proxy for health insurance, since it is the only social protection information available in ENAHO.	ENAHO based on ILO definition. Definition has been adapted according to data availability in the survey.
Informal sector	Own account workers or employers who have not registered their activities in SUNAT (*Superintendencia Nacional de Aduanas y de Administración Tributaria*), who have no accounting system, and who have 5 or fewer employees.	ENAHO based on ILO definition. Definition has been adapted according to data availability in the survey.
Occupation	There are six different occupations in ENAHO: waged employee, waged worker, own-account worker, employer, domestic worker, and unpaid family worker. The main occupations analyzed in this paper are: Waged employees: individuals with a predominantly intellectual occupation in an institution or firm where they perceive a monthly or half-monthly remuneration or payment; waged workers: have a predominantly manual occupation in an enterprise or business where they perceive a daily, weekly, or half-monthly remuneration; own-account workers: can exercise a profession or operate their own business but without having dependent employees.	ENAHO
Income		
Working poor	Employed individuals living in households in which per-capita income/ expenditure of the household is below the USD1.25 international poverty line. The international poverty line has been converted to the national currency using the INEI exchange rate at the end of 2011.	ENAHO based on ILO definition (ILO, 2012).[48]
Scales of income	Scales of the monthly household income, going from 1 (no income) to 6 (more than PEN 700). Monthly household income includes all incomes, monetary and other, in the main occupation. For participants, this measure of income corresponds to year 2011 but post-participation.	ENAHO and special participants' survey
Annual net household income per capita	Annual net household income divided by the number of individuals living in the household.	ENAHO

[47] The informal sector is defined as all employers or enterprises with fewer than 5 employees and not registered in the Peru internal revenue service (SUNAT).

[48] ILO (2012), pp. 68-69.

Hours worked		
Total hours worked	Total number of hours usually worked per week in all occupations.	ENAHO
Excessive hours	Employed individuals working more than 48 hours per week.	ENAHO based on ILO definition (ILO, 2012)
Underemployed	Employed individuals who, during the week of reference, were available and willing to work more hours than those usually worked.	ENAHO

Appendix C: Assessing the probability of participation in *Construyendo Perú*

Table C1. Results of probit model assessing the determinants of participation in *Construyendo Perú* (baseline)

	(1)	(2)	(3)	(4)	(5)	(6)
Man	-0.40*** (0.03)	-0.36*** (0.03)	-0.42*** (0.03)	-0.50*** (0.04)	-0.50*** (0.04)	-0.49*** (0.04)
Age brackets (ref. aged between 18 and 29 years)						
Aged between 30 and 39 years	0.18*** (0.03)	0.17*** (0.03)	0.14*** (0.03)	0.08** (0.03)	0.09** (0.04)	0.09*** (0.04)
Aged between 40 and 49 years	0.15*** (0.04)	0.10** (0.04)	0.05 (0.04)	-0.05 (0.04)	-0.05 (0.04)	-0.05 (0.04)
Aged more than 50 years	-0.07 (0.040	-0.23*** (0.05)	-0.22*** (0.05)	-0.35*** (0.05)	-0.35*** (0.05)	-0.35*** (0.05)
Marital status (ref. cohabiting)						
Married	-0.22*** (0.03)	-0.17*** (0.03)	-0.17*** (0.03)	-0.17*** (0.03)	-0.15*** (0.03)	-0.18*** (0.03)
Widowed	-0.16*** (0.06)	-0.19*** (0.06)	-0.16*** (0.06)	-0.31*** (0.07)	-0.30*** (0.07)	-0.32*** (0.07)
Divorced	-0.40** (0.20)	-0.20 (0.20)	-0.21 (0.21)	-0.36* (0.21)	-0.37* (0.22)	-0.37* (0.22)
Separated	-0.06 (0.04)	-0.02 (0.04)	-0.04 (0.04)	-0.20*** (0.05)	-0.21*** (0.05)	-0.20*** (0.05)
Single	-0.45*** (0.04)	-0.33*** (0.04)	-0.33*** (0.04)	-0.42*** (0.04)	-0.41*** (0.04)	-0.43*** (0.06)
Educational attainment (ref. completed secondary education)						
Has not approved any level of education		0.68*** (0.25)	0.69*** (0.25)	0.70*** (0.25)	0.71*** (0.26)	0.67** (0.26)
Initial education		1.64*** (0.40)	1.62*** (0.40)	1.68*** (0.41)	1.62*** (0.42)	1.60*** (0.42)
Incomplete primary education		0.55*** (0.17)	0.55*** (0.17)	0.56*** (0.17)	0.56*** (0.18)	0.55*** (0.18)
Completed primary education		0.41*** (0.20)	0.41*** (0.20)	0.42*** (0.20)	0.43*** (0.12)	0.44*** (0.12)
Incomplete secondary education		0.20*** (0.06)	0.20*** (0.06)	0.21*** (0.06)	0.21*** (0.06)	0.22*** (0.06)
Incomplete tertiary (non-university) education		-0.23*** (0.08)	-0.25*** (0.08)	-0.25*** (0.08)	-0.25*** (0.08)	-0.25*** (0.09)
Completed tertiary (non-university) education		-0.47*** (0.10)	-0.49*** (0.11)	-0.49*** (0. 11)	-0.49*** (0. 11)	-0.50*** (0. 11)
Incomplete university education		-0.39*** (0.10)	-0.38*** (0.10)	-0.38*** (0.10)	-0.39*** (0.11)	-0.41*** (0.11)
Completed university education		-0.82*** (0.17)	-0.85*** (0.17)	-0.85*** (0.17)	-0.85*** (0.17)	-0.88*** (0.18)
Number of years in education		0.03 (0.02)	0.03 (0.02)	0.03 (0.02)	0.02 (0.02)	0.02 (0.02)
Employed (previous week)			0.33*** (0.03)	0.32*** (0.03)	0.35*** (0.03)	0.33*** (0.03)
Household characteristics (ref. no children in the household, not head of household)						
There is a child in the household				0.22*** (0. 03)	0.20*** (0. 03)	0.20*** (0.03)
Head of household				0.14*** (0.04)	0.14*** (0.04)	0.13*** (0.04)
Number of people living in the household				-0.07*** (0.01)	-0.07*** (0.01)	-0.07*** (0.01)
Lives in a district with socio-economic conditions to participate in Construyendo Perú					1.23*** (0.11)	1.30*** (0.11)

Constant	-2.18*** (0.03)	-2.60*** (0.26)	-2.81*** (0.26)	-2.54*** (0.26)	-3.64*** (0.30)	-3.69*** (0.30)
Observations	151,045	151,040	151,040	151,040	151,040	151,040
Department FE	NO	NO	NO	NO	NO	YES
Pseudo R2	0.0501	0.0798	0.0895	0.101	0.133	0.145

Notes: Tab. C1 illustrates the results from a probit model, where the outcome variable is a dummy variable equal to one if an individual participated in *Construyendo Perú* and zero otherwise. The sample includes individuals who participated in the program (special survey) or who could have participated given eligibility criteria of the program at the district and individual levels (ENAHO). Each column refers to a separate probit regression. All explanatory variables are pre-determined characteristics at baseline (one year before participation). They are defined as dummy variables, with the exception of number of years of education and number of people living in the household. Reference categories for the dummy variables are: woman, aged between 18 and 29 years, cohabiting, completed secondary education, unemployed or inactive in the labor market, no children in the household, not a head of household, and does not live in a district with socio-economic conditions to participate in the program. Based on Column (5), I predict the out-of-sample probabilities of program participation used when carrying out the RD analysis. Robust standard errors are in parentheses. Significance levels: *significant at 10%; **significant at 5%; ***significant at 1%.

Source: Author's calculations.

Table C2. Results of estimated out-of-sample probability of participation in *Construyendo Perú*, 2012

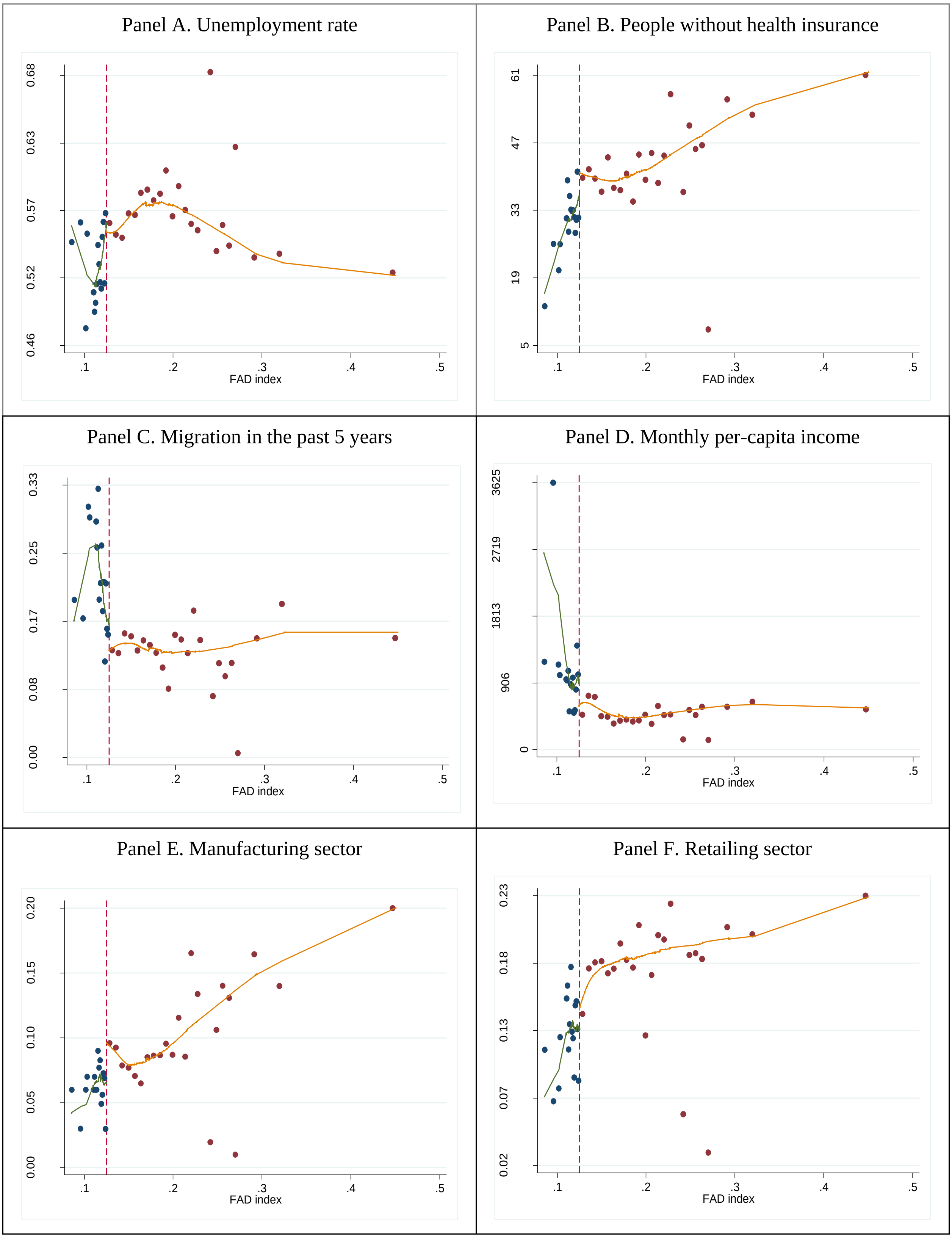
	Percentiles	Smallest	Stats	
1%	0.00000218	1.61E-08	Obs.	43,791
5%	0.0000254	1.93E-08	Sum of Wgt.	43,791
10%	0.0000914	2.93E-08	Mean	0.007076
25%	0.000614	5.02E-08	Std. Dev.	0.010419
50%	0.0030024	**Largest**	Variance	0.000109
75%	0.0089846	0.1040046	Skewness	2.858141
90%	0.0196202	0.1131205	Kurtosis	14.93641
95%	0.0280739	0.1503452		
99%	0.0501875	0.1610977		

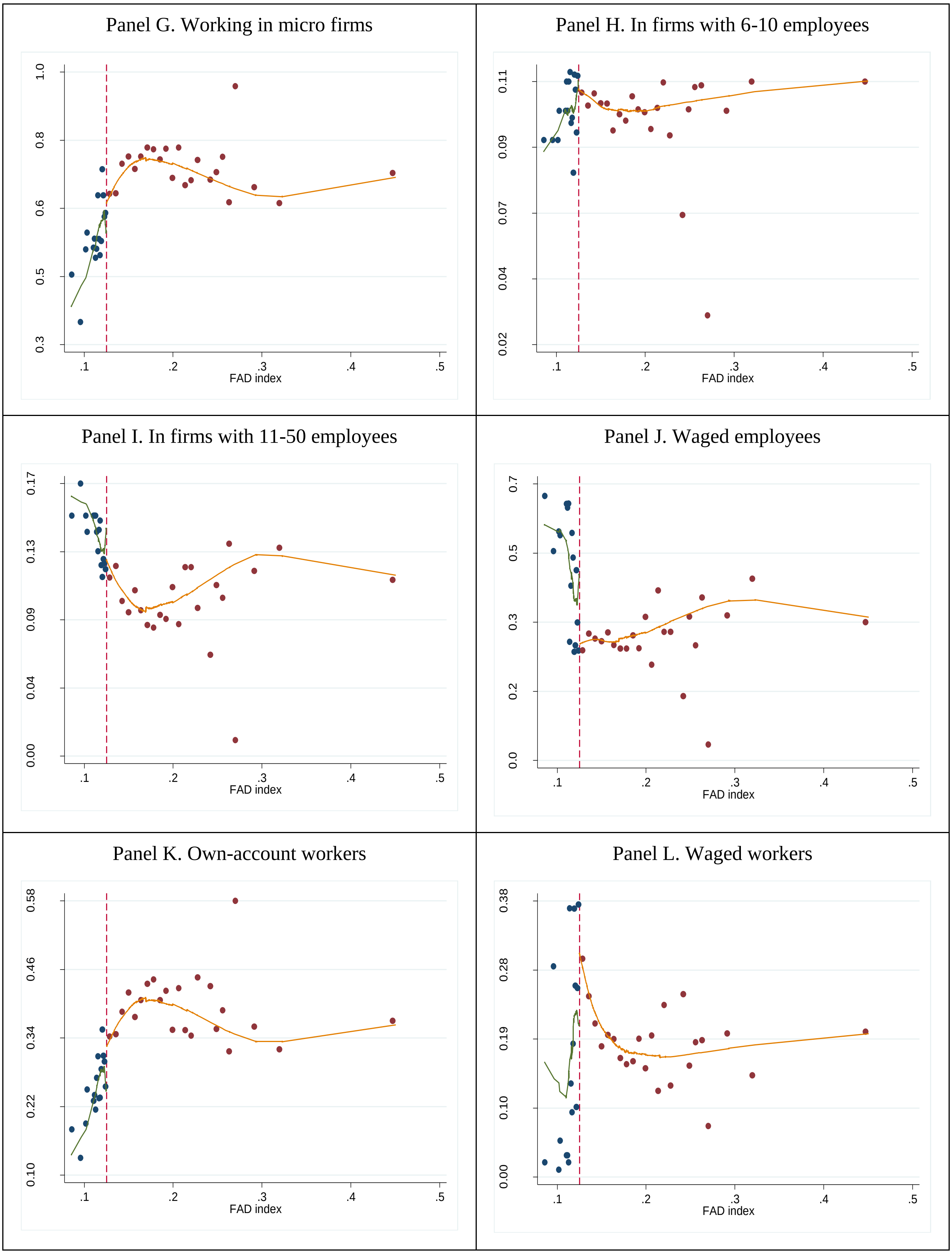
Notes: Table C2 presents the detailed summary statistics of the estimated out-of-sample probability of participation in the program. Statistics are presented solely for the sample used for the analysis, although the probabilities were calculated for the whole database. The sample includes all adult population from ENAHO living in urban districts. Since the post-participation analysis is carried out in 2012, only observations for this year are included in this table and the analysis.

Source: Author's calculations.

Appendix D: Smoothness in districts' characteristics around the discontinuity

Figure D1. Graphical analysis of district discontinuities in baseline characteristics, 2007





Notes: Fig. D1 plots the mean probability of a change in baseline districts' characteristics (measured at the individual level, i.e. individuals living in districts that participated in the program during the period 2007–10), conditional to the districts' FAD index levels along with the 95% level confidence bounds, using the Lowess fit. The conditional mean is based on equal-sized bins (i.e. each dot in graph corresponds to a bin, see Footnote 29). The analysis includes all urban districts. Variables are measured as shares at the district level and were drawn from the 2007 National Census (INEI).

Source: Author's calculations.

Figure D2. Estimated Discontinuities in Baseline Characteristics, 2007

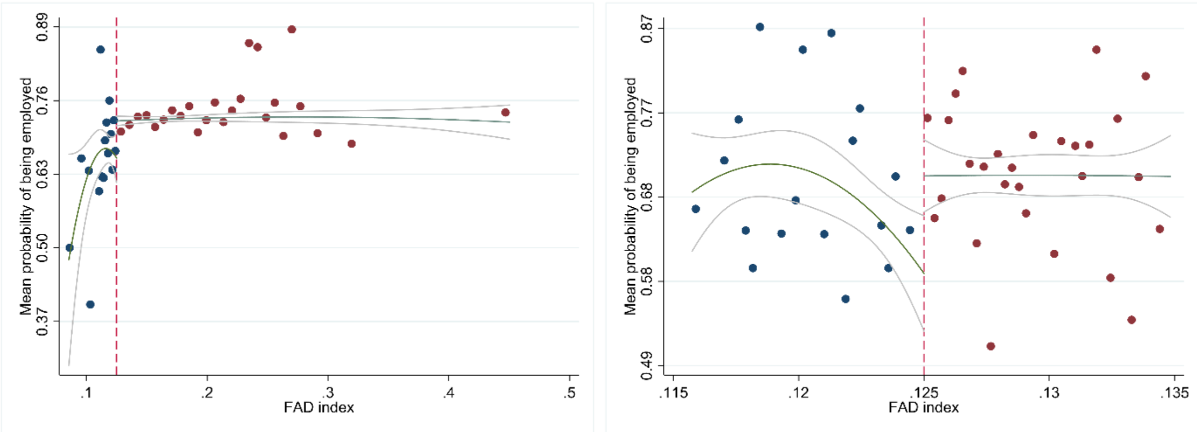
Cutoff: FAD Index = 0.125	
Female	0.363 (0.792)
Age groups	
Age (18-25)	0.542 (0.714)
Age (26-49)	1.621 (1.177)
Age (50 and more)	-2.735** (1.291)
Marital status	
Cohabitating/Married	2.324 (1.714)
Divorced/Separated	0.715* (0.417)
Single/Widowed	-3.039* (1.715)
Educational attainment	
No education	1.006** (0.449)
Initial education	-0.0327 (0.0636)
Primary incomplete	2.415*** (0.917)
Primary	1.565** (0.624)
Secondary incomplete	1.215 (0.780)
Secondary	-1.921 (1.515)
Post-secondary education incomplete	-0.430 (0.518)
Post-secondary education	-2.423** (0.943)
University incomplete	-0.996 (0.817)
University or post-graduate	-0.295 (1.497)
Housing characteristics	
Light	-0.305* (0.171)
Drinkable water	-0.830 (1.621)
Sanitary system	2.661*** (0.116)
Number of rooms	-23.38* (12.28)
Household members	**9.041** **(11.69)**

Notes: Each coefficient comes from a different regression using the 2SLS method. Results are estimated by replacing the dependent variable in Eqs. (1) and (2).

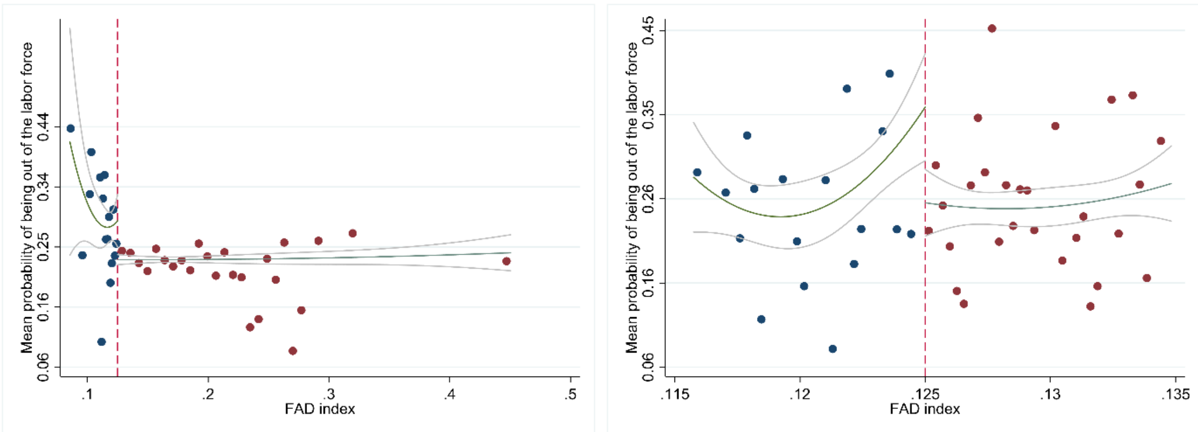
Source: Author's calculations.

Appendix E: Graphical analysis of the effects of the program, 2012*

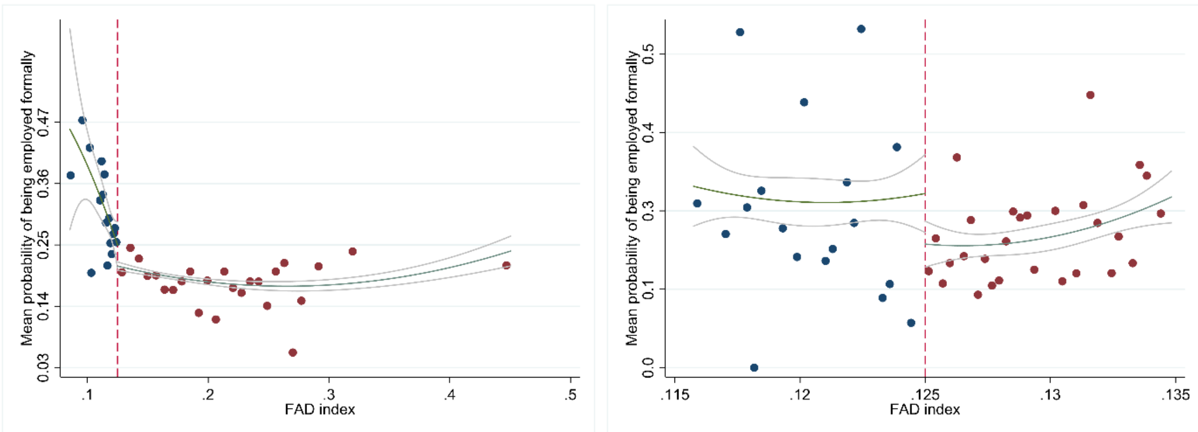
Panel A. Probability of being employed (overall window and smaller bandwidth)



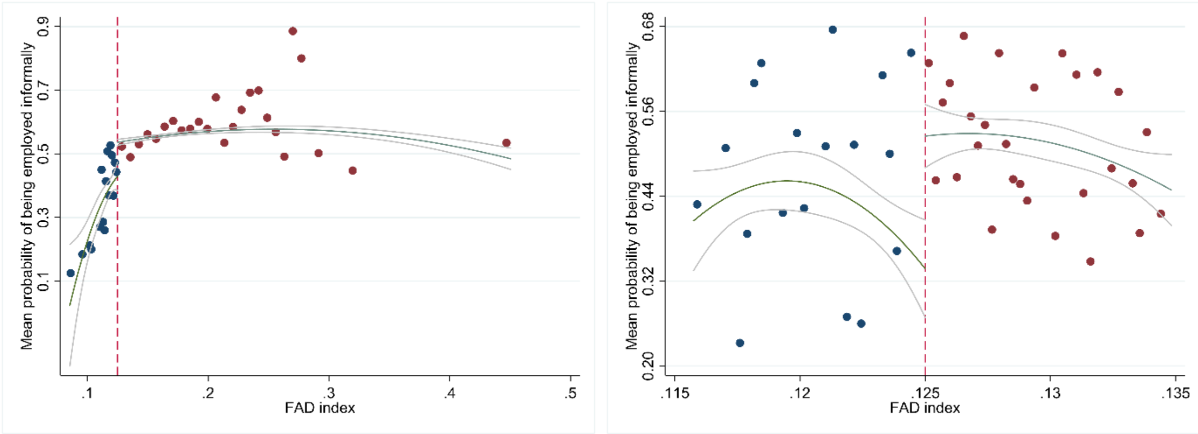
Panel B. Probability of being inactive (overall window and smaller bandwidth)



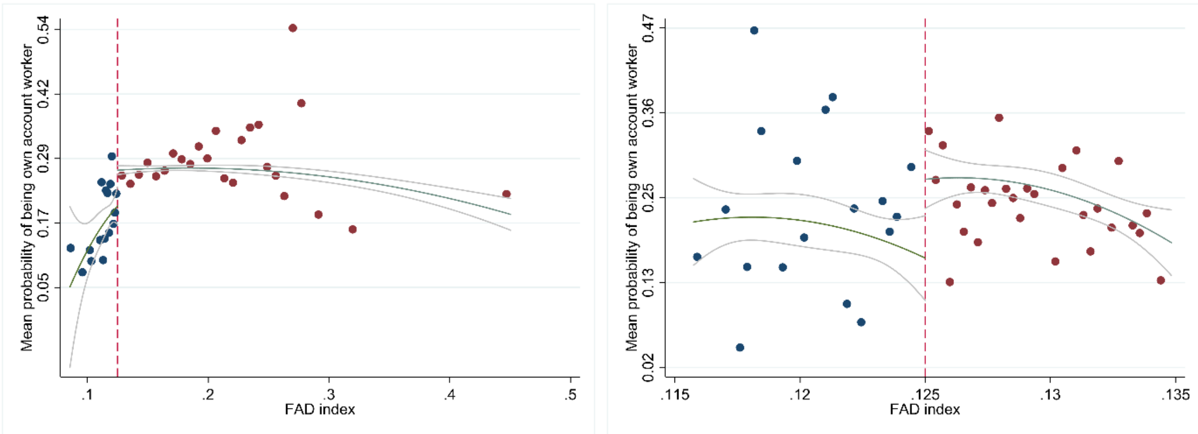
Panel C. Probability of being employed formally (overall window and smaller bandwidth)



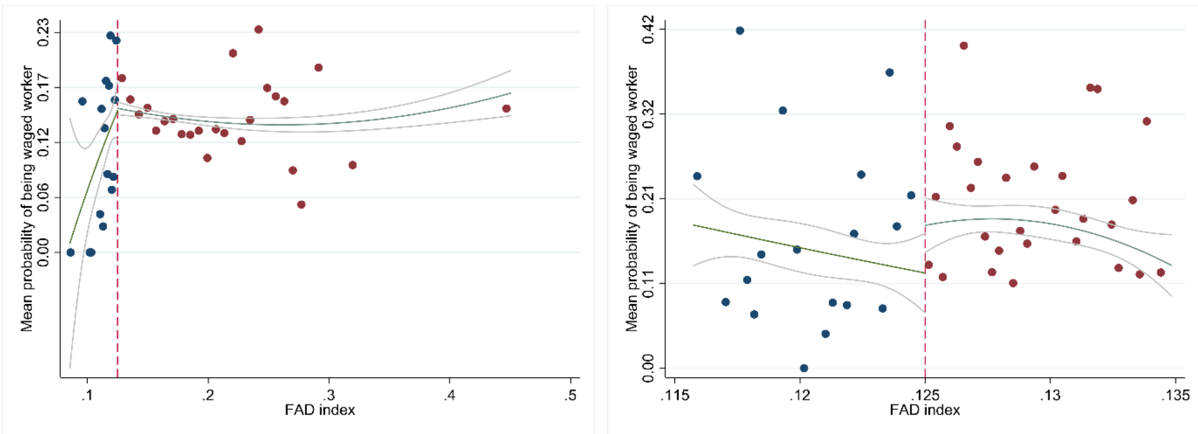
Panel D. Probability of being employed informally (overall window and smaller bandwidth)



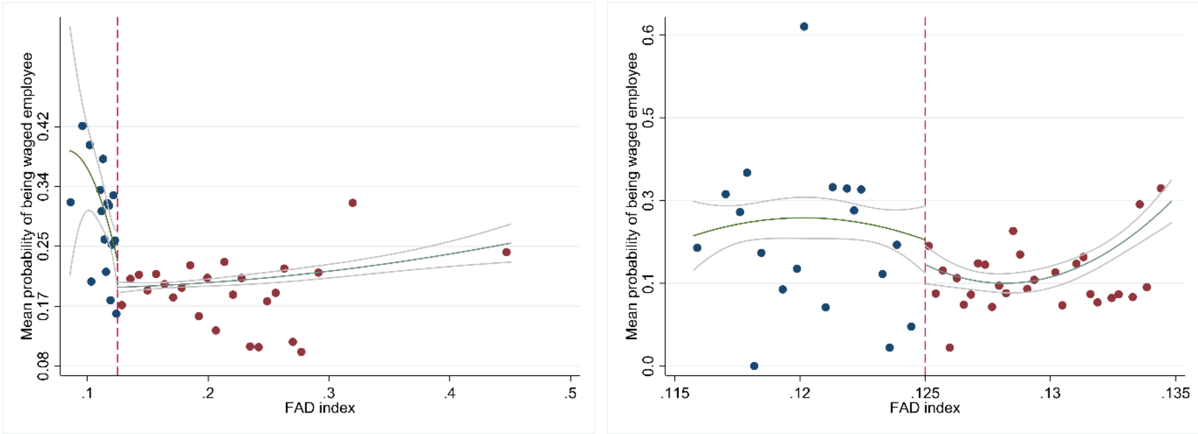
Panel E. Probability of being own-account worker (overall window and smaller bandwidth)



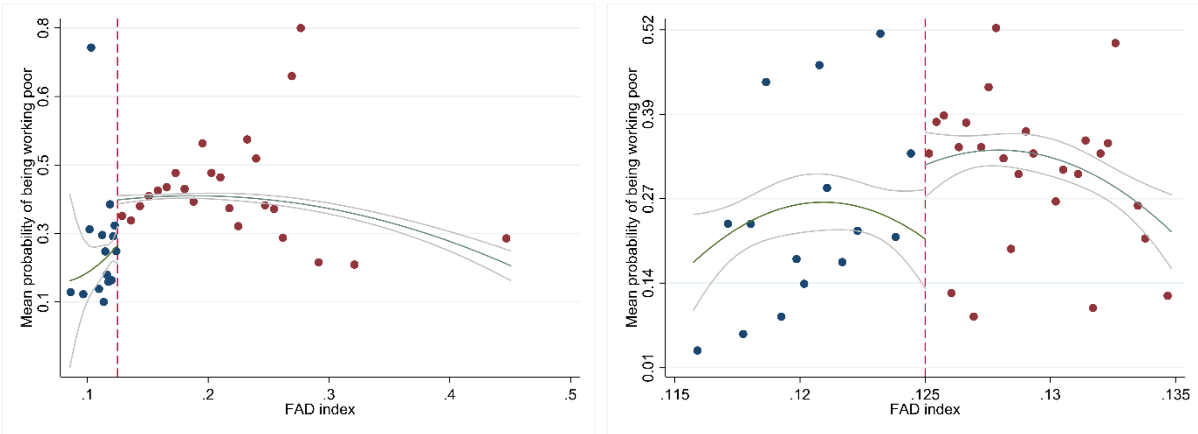
Panel F. Probability of being waged worker (overall window and smaller bandwidth)



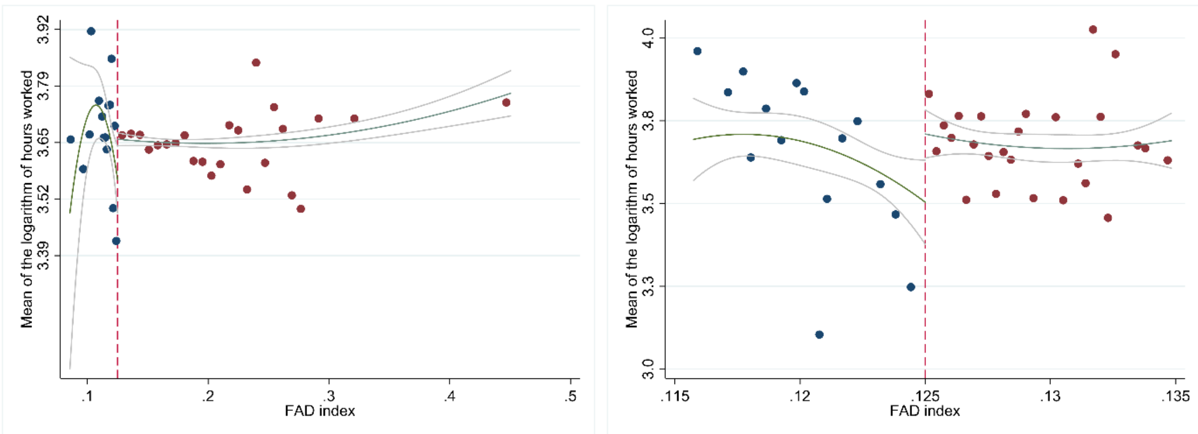
Panel G. Probability of being waged employee (overall window and smaller bandwidth)



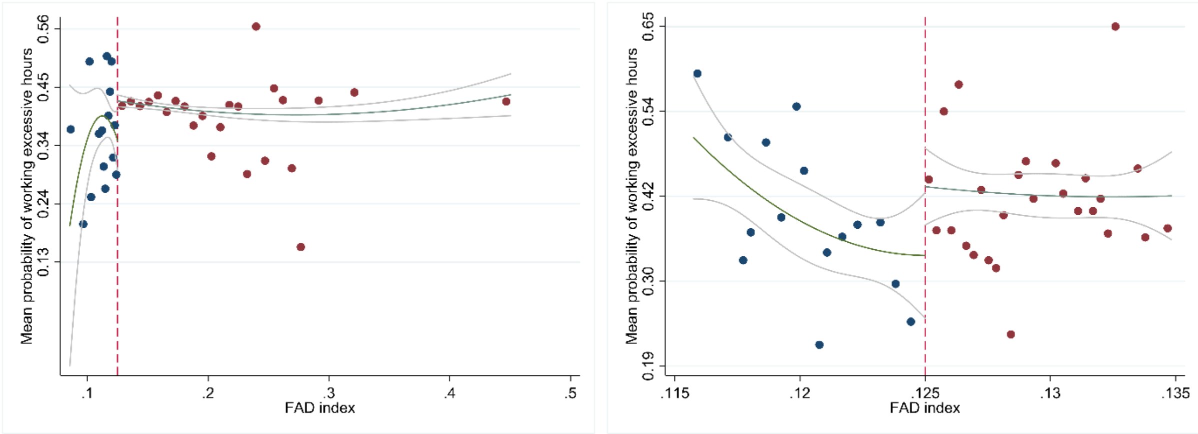
Panel H. Probability of being working-poor (overall window and smaller bandwidth)



Panel I. Logarithm of total hours worked per week (overall window and smaller bandwidth)



Panel J. Probability of working excessive hours (overall window and smaller bandwidth)



Notes: Appendix E plots the mean probability of having a certain employment status, working poverty, and working time conditional to the districts' FAD index levels, along with the 95% level confidence bounds. The conditional mean is drawn on the base of equal-sized bins (i.e. each dot in graph corresponds to a bin, see Footnote 29). The fit used was suggested by the graphical analysis carried out using the Lowess fit. The analysis includes all urban districts.

Source: Author's calculations.

Appendix F: Estimates of *Construyendo Perú* on employment status and job quality for different periods—parametric 2SLS method

	All					Women				
Periods analyzed	2007–10	2007–08	2009–10	2008	2009	2007–10	2007–08	2009–10	2008	2009
A. Employment status										
Employed	2.1 (1.5)	3.7 (2.8)	5.1 (3.5)	7.8 (5.3)	11.0 (7.5)	2.3* (1.2)	4.2 (2.6)	5.1* (2.6)	8.3* (4.4)	11.9* (6.2)
Inactive	-2.3 (1.7)	-4.0 (3.01)	-5.3 (3.7)	-8.1 (5.7)	-11.5 (8.0)	-2.5* (1.4)	-4.4 (2.8)	-5.3* (2.9)	-8.7* (4.8)	-12.4* (6.7)
Employed informally	5.5** (2.4)	9.3* (4.9)	12.7** (5.4)	19.0** (8.4)	26.9** (11.7)	3.9** (1.9)	7.1* (3.9)	8.7** (3.9)	14.1** (6.5)	20.1** (9.1)
Employed formally	-3.0** (1.4)	-4.8* (2.7)	-6.6** (3.1)	-9.7** (4.8)	-13.6** (6.7)	-1.5* (0.9)	-2.5 (1.7)	-3.1* (3.9)	-4.9* (2.9)	-6.9* (4.1)
Own-account worker	3.6** (1.5)	6.1** (3.0)	8.3** (3.2)	12.6** (5.2)	17.8** (7.1)	2.8*** (1.0)	5.0** (2.4)	6.2*** (2.0)	10.1*** (3.6)	14.4*** (4.9)
Waged worker	-0.02 (1.3)	-.3 (2.28)	-0.5 (3.0)	-1.0 (4.5)	-1.4 (6.4)	0.115 (0.518)	0.1 (0.9)	-0.04 (1.1)	-0.2 (1.8)	-0.3 (2.6)
Waged employee	-2.8** (1.4)	-4.4* (2.6)	-6.0** (3.0)	-8.7* (4.7)	-12.5* (6.6)	-1.6* (0.8)	-2.6 (1.6)	-3.2* (1.7)	-5.1* (2.7)	-7.3* (3.9)
B. Income and working time										
Monthly income scales	-16.8* (9.2)	-28.4 (17.6)	-42.7** (20.4)	-58.5** (28.3)	-91.0** (44.3)	-10.4* (5.9)	-18.6 (12.3)	-24.8** (12.4)	-36.2** (18.2)	-57.4* (29.4)
Working-poor	7.6*** (2.7)	12.2** (5.6)	18.7*** (5.6)	22.2*** (7.1)	40.2*** (13.1)	5.6*** (1.9)	9.4** (4.4)	13.2*** (3.8)	16.5*** (4.9)	31.9*** (10.7)
Number of hours worked	1.5 (2.4)	2.2 (3.9)	3.0 (5.7)	3.3 (6.9)	6.3 (12.2)	1.313 (2.385)	2.1 (4.1)	2.1 (4.1)	3.1 (6.9)	6.1 (13.2)
Excessive working time	1.7 (1.3)	2.8 (2.2)	4.7 (3.3)	5.6 (4.0)	10.2 (7.1)	1.057 (1.307)	1.8 (2.2)	1.8 (2.2)	3.3 (3.8)	6.6 (7.4)
Observations A	46,664	45,692	44,573	44,451	44,046	24,427	23,912	23,336	23,268	23,052
Observations B	34,635	33,994	32,214	33,146	31,908	16,107	15,119	15,484	14,751	14,630
No. participants	1142	778	364	317	172	894	598	296	245	130

Notes: Appendix F reports estimated treatment effects of the program *Construyendo Perú* conditional on crossing the FAD index cutoff point of 0.125, for the full duration of the program (2007–10), for beneficiaries of the first two years and for beneficiaries of the latter two years, as well as for beneficiaries who participated only in 2008 and 2009. The table reports 2SLS estimates, clustered at the district level. All effects have been calculated including all districts. Standard errors are in parentheses. Significance levels: *significant at 10%; **significant at 5%; ***significant at 1%.Source: Author's calculations.

Appendix G: LLR estimates of the effect of *Construyendo Perú* on participants' labor market status, income, and working time, using three different bandwidths

	All			Women			Men		
	Half	Opt.	Double	Half	Opt.	Double	Half	Opt.	Double
Employed	0.16*** (0.06)	0.14* (0.08)	0.13* (0.07)	0.14*** (0.06)	0.18** (0.09)	0.17*** (0.06)	0.19 (0.15)	0.11 (0.21)	0.01 (0.19)
Inactive	-0.15*** (0.06)	-0.13 (0.08)	-0.12** (0.06)	-0.17*** (0.06)	-0.20** (0.09)	-0.18*** (0.06)	-0.09 (0.15)	0.03 (0.20)	-0.01 (0.15)
Employed informally	0.54*** (0.10)	0.34*** (0.07)	0.30*** (0.09)	0.42*** (0.099)	0.28*** (0.08)	0.23*** (0.07)	0.90*** (0.32)	0.61** (0.22)	0.40 (0.28)
Employed formally	-0.24*** (0.09)	-0.17*** (0.06)	-0.19** (0.08)	-0.16** (0.08)	-0.11* (0.06)	-0.07 (0.06)	-0.39** (0.18)	-0.44* (0.25)	-0.16 (0.22)
Own-account worker	0.20*** (0.05)	0.22*** (0.07)	0.15** (0.06)	0.19*** (0.06)	0.18*** (0.06)	0.14*** (0.04)	0.25 (0.15)	0.32* (0.19)	0.20 (0.23)
Waged worker	0.14** (0.06)	0.10** (0.05)	0.05 (0.03)	0.07** (0.03)	0.08* (0.04)	0.05* (0.03)	0.43*** (0.16)	0.36* (0.22)	0.27 (0.18)
Waged employee	-0.21*** (0.06)	-0.27*** (0.08)	-0.16*** (0.06)	-0.13** (0.05)	-0.14* (0.08)	-0.07 (0.05)	-0.54*** (0.19)	-0.70** (0.27)	-0.42** (0.19)
Obs.	43,741	43,741	43,741	22,952	22,952	22,952	20,789	20,789	20,789
Working poor	0.17*** (0.06)	0.23*** (0.08)	0.18** (0.06)	0.19*** (0.06)	0.24*** (0.09)	0.17*** (0.06)	0.13 (0.16)	0.29 (0.23)	0.23 (0.20)
Logarithm of hours worked	0.27*** (0.10)	0.33** (0.13)	0.29** (0.12)	0.34* (0.19)	0.24* (0.14)	0.36** (0.16)	0.60*** (0.23)	0.60* (0.31)	0.39 (0.28)
Excessive working time	0.18*** (0.06)	0.22** (0.08)	0.17** (0.08)	0.23** (0.10)	0.15** (0.07)	0.17* (0.09)	0.37** (0.19)	0.46* (0.26)	0.33 (0.23)
Obs.	31,736	31,736	31,736	14,601	14,601	14,601	17,135	17,135	17,135

	Lower educated*			Higher educated*			Urban departments		
	Half	Opt.	Double	Half	Opt.	Double	Half	Opt.	Double
Employed	0.48** (0.19)	0.26** (0.13)	0.25** (0.12)	0.18** (0.08)	0.16 (0.11)	0.14 (0.08)	0.15** (0.07)	0.14 (0.09)	0.11* (0.06)
Inactive	-0.57*** (0.18)	-0.31*** (0.12)	-0.28** (0.13)	-0.16 (0.10)	-0.16 (0.10)	-0.13* (0.07)	-0.15** (0.07)	-0.15* (0.08)	-0.12** (0.05)
Employed informally	0.28 (0.17)	0.15 (0.11)	0.08 (0.13)	0.71*** (0.15)	0.48*** (0.12)	0.38*** (0.12)	0.52*** (0.10)	0.35*** (0.08)	0.30*** (0.09)
Employed formally	0.15** (0.07)	0.09* (0.05)	0.13** (0.06)	-0.24*** (0.08)	-0.30*** (0.11)	-0.18* (0.10)	-0.23*** (0.09)	-0.16*** (0.06)	-0.20** (0.08)
Own-account worker	0.29* (0.15)	0.11 (0.11)	0.08 (0.11)	0.23*** (0.07)	0.26*** (0.09)	0.19** (0.09)	0.20*** (0.05)	0.22*** (0.07)	0.18*** (0.06)
Waged worker	0.04 (0.12)	0.02 (0.09)	0.03 (0.08)	0.23*** (0.09)	0.14** (0.06)	0.07 (0.05)	0.14*** (0.05)	0.14** (0.06)	0.07* (0.04)
Waged employee	0.02 (0.03)	0.02 (0.03)	0.01 (0.02)	-0.30*** (0.09)	-0.34*** (0.12)	-0.19** (0.08)	-0.22*** (0.06)	-0.28*** (0.08)	-0.16*** (0.06)
Obs.	11,388	11,388	11,388	32,353	32,353	32,353	36,303	36,303	36,303
Working poor	0.14 (0.11)	0.01 (0.17)	-0.08 (0.15)	0.21** (0.09)	0.26** (0.10)	0.22*** (0.07)	0.17*** (0.06)	0.23*** (0.08)	0.22*** (0.07)
Logarithm of hours worked	0.51* (0.27)	0.66** (0.34)	0.45** (0.20)	0.31** (0.13)	0.35** (0.17)	0.27 (0.17)	0.28*** (0.10)	0.33** (0.13)	0.30** (0.13)
Excessive working time	0.07 (0.12)	0.13 (0.16)	0.10 (0.11)	0.41*** (0.15)	0.29** (0.11)	0.22* (0.12)	0.18*** (0.06)	0.22** (0.09)	0.16** (0.08)
Obs.	7,368	7,368	7,368	24,368	24,368	24,368	26,067	26,067	26,067

Notes: *For the purpose of this analysis, I consider lower-educated individuals those who have completed at most primary education (0-7 years of schooling) and higher educated those beyond that level of education (8 years or more). Appendix G reports estimated effects of assignment to treatment to the program *Construyendo Perú* conditional on crossing the FAD index cutoff point of 0.125 for the six groups studied. These are LLR estimates obtained using a triangular kernel regression model on both sides of the cutoff for half, optimal, and double bandwidths. All effects have been calculated including all districts. Standard errors are in parentheses. Significance levels: *significant at 10%; **significant at 5%; ***significant at 1%.

Source: Author's calculations.