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Workfare programmes and their impact on the labour market: Effectiveness of *Construyendo Perú*

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Abstract

This paper estimates the medium-term effects of the workfare programme *Construyendo Perú* implemented in Peru to support unemployed populations in situations of poverty and extreme poverty from 2007 to 2011. I find that the intervention helps raising employment and reducing inactivity for particular groups of beneficiaries, yet at a cost of locking participants in lower quality jobs (i.e. informal, paid below the poverty line and working excessive hours). Particularly, the programme 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 perspectives of women and higher-educated individuals. The evaluation is carried out through a regression discontinuity approach, which exploits for the first time an interesting assignment rule the programme has at the district level, namely, that only districts above a certain level of poverty and development shortcomings are eligible to participate.

Keywords: workfare programme, direct job creation, work quality, impact evaluation, Peru, Latin America, regression discontinuity

JEL codes: J21, J48, I38, H53

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1. Introduction

Public works programmes 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] Although not to the magnitude of those in Asia and Africa, public works are also important in Latin America where the number of programmes (and budget) has increased during the last two decades. In spite of this, the existing evidence with respect to the effectiveness of these programmes is very much in its nascent phase and suffers from a number of gaps. This is particularly the case in Latin America, where only four impact evaluations have been carried out on public works programmes and three of them focusing on the effects of beneficiaries during participation (Kluve, 2016).

This paper contributes to filling this void by examining the medium-term effects of the programme *Construyendo Perú* implemented in Peru in 2007 to support unemployed populations in situations of poverty and extreme poverty. The programme provided access to temporary employment and skills development through the financing of public investment projects intensive in the use of unskilled labour. Interestingly, the programme was introduced principally as a "workfare programme" whose action was not limited to a recessionary event and whose aim was addressing 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 programmes, many of which are 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 the delivery of employability enhancing components that can allow participants to find more permanent employment when the public programme culminates.

The potential impacts of well-designed workfare programmes are numerous. Workfare programmes can have an antipoverty effect arising from the direct transfers, at least during participation, provided wages are set sufficiently high to outweigh the costs associated with participation (Subbarao, 1997). These programmes 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). In this case, even if wages are low, incomes provided as safety nets can protect households from unfavourable decisions that are often taken among the most vulnerable during crises times, such as selling productive assets (Subbarao, 1997). In the longer term, however, individual effects of workfare programmes depend on their ability to raise participants' employability so they can find sustainable employment after the programme culminates (Hujer et al. 2004). At the macro level, workfare programmes that are large enough can reduce poverty rates and if these programmes are able to influence private sector wages and jobs, they could have a positive effect on market wages or help enforce minimum wages (Dev, 1996).

Empirically, much of the evidence on the impact of public works and workfare programmes in emerging and developing countries has focused either on the short-term income effects or the anti-poverty impacts. This is not surprising since in these countries programmes of this sort have traditionally been focused on their role as a safety net strategy (through the provision of incomes during shocks) and as a poverty

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

alleviation measure (by offering temporary employment to vulnerable households) (Del Ninno et al., 2009; ILO, forthcoming).[2]

Evidence shows that while workfare programmes seem to provide effective income support to beneficiaries during participation, their impact on poverty reduction has not been conclusive. For example, in Argentina, Colombia and Peru working in a workfare programme is associated with 25 to 40 per cent higher wages than those typically earned by participants in the private sector (O'Keefe, 2005), although effects vary per programme. In addition, in some cases these income gains were found to be progressive – i.e. gains are proportionally higher for poorest quintiles than for richer ones (Murgai and Ravallion, 2005). This success could be explained, in part, by the fact that prior to participation workfare participants were already earning lower wages than those offered by the programme, which were likely below the reservation wage for the non-poor population (Jalan and Ravallion, 2003). In terms of their anti-poverty effect, impact evaluations of workfare programmes implemented in developing countries have shown mixed results on various fronts. Workfare programmes have been found to be more effective than other public policies in reaching the poor (O'Keefe, 2005). Moreover, for particular programmes, 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.[3] However, even if the transfers have been found to be beneficial, for a number of programmes wage effects were not important (or sustainable) enough for raising participants and their families out of poverty (Ravallion and Datt, 1995).

Unfortunately, very little is known regarding the labour market effects of workfare programmes, particularly the impacts after participation. The evaluation carried out in this paper helps bridging this gap, first, by estimating the medium-term effects of *Construyendo Perú* (the first to be estimated for this particular programme).[4] Second, while the scarce labour market evidence has focused only on the employment effects of interventions, this paper provides impacts on other aspects of labour market status (such as labour market participation, whether jobs found were formal or informal and the type of occupation of participants), working time (including excessive hours worked), working poverty and incomes. Third, by studying particular treated groups, this paper aims to assess the heterogeneity of effects of the programme, particularly on women and on individuals with different levels of education. Although some evidence exists on the effect of workfare programmes on female participants, the record of workfare programmes in this respect is mixed (Del Ninno et al. 2009). Moreover, the literature has not often focused on the impacts of programmes on higher or lower-educated individuals and therefore findings from this paper are an added value to what exists.

My findings illustrate that *Construyendo Perú* had a positive effect on labour participation and employment probabilities of women and lower-educated individuals. Unfortunately, alongside these positive effects the programme increased participants' probabilities of working informally, during excessively long hours and of being working poor. By particular group, the programme has not been able to improve the perspectives of lower-educated participants in terms of finding a better quality job (although it has in terms of employment) and has exacerbated the perspectives of women and higher-educated individuals. Finally, from the implementation point of view, the analysis shows that the programme attracts mainly women who

[2] Another objective of workfare programmes in developing countries is community level development through the provision of public infrastructure. Although the benefits associated with the public goods could exceed in some cases those of wage transfers (Ravallion and Datt, 1995; Gaiha, 2002), 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 programmes are beyond the scope of this paper.

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

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

are not necessarily heads of household and that the programme suffers from double participation. These two latter results may be suggesting that there are implementation problems limiting the labour market impacts of the programme.

The paper is organised as follows. Section 2 describes the main characteristics of *Construyendo Perú* putting special emphasis on its targeting strategy. Section 3 presents the data used in the analysis and provides descriptive statistics. Section 4 discusses the evaluation strategy and presents graphical and estimated results, as well as an interpretation of the effects. Section 5, discusses the plausibility of the identifying assumption and provides the results of sensitivity tests and robustness checks. Finally, Section 6 concludes with an overall appraisal of the results.

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

Construyendo Perú was active from 2007 to 2011. It supplanted the programme *A Trabajar Urbano*, in place from 2002 until 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,[5] in order to discourage those with more resources from participating in the programme.[6] In June 2007, the programme was replaced by *Construyendo Perú*, principally a workfare programme, whose action was no longer limited to a recessionary event. In particular, the objective of *Construyendo Perú* was to support unemployed individuals, mainly unemployed 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.[7]

Construyendo Perú had four different modalities of intervention depending on the nature of the project: (i) 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), included in 2009; (ii) special projects, tailored to areas officially declared in an estate 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 funds provided by the programme (between 80 and 85 per cent). Out of the other categories, special projects accounted for around 10 per cent and contingency projects for 5 per cent, leaving the remaining of the funds to be allocated to rural projects. In all cases, the role of the programme 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 the first stage and aimed to prioritize districts in two ways: (i) all urban districts, preferably those that were already part of the National Strategy *Crecer* and *Crecer Urbano,* were selected first;[8] (ii) out of these districts, beneficiary districts were carefully chosen by ranking them according to the composite index FAD (*Factor de Asignación Distrital*). Districts with a higher FAD were given priority and received higher shares of the

[5] 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).

[6] The programme 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 per cent of the wage provided by the programme (Chacaltana, 2003).

[7] MEF (no date).

[8] INEI uses a 2500 urban inhabitant's limit as the lower bound to define urban districts.

budget allocated. Districts ranking lower received decreasing shares of the budget until the total budget allocated was exhausted. 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. The composite index FAD was constructed by the Planning Management Unit of the programme until 2010 on the basis of three indicators weighted equally:[9] urban population, the index of human development shortcomings, and the poverty severity index FGT(2).[10] Importantly, geographical targeting varied according to the modality of intervention of the programme. While regular and service-sector public infrastructure projects (large majority of the projects) used FAD for their geographical targeting, special projects used FAD 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, there was a call for tender to choose the specific projects (by modality) to be implemented by the programme in the selected districts.[11]

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

The third and final stage was individual targeting, which consisted in selecting beneficiaries from the pool of people that registered to participate according to established criteria, notably whether applicants were at least 18 years old, unemployed heads of household and lived in poverty or extreme poverty. The poverty eligibility criteria were verified in two steps: all individuals that registered to participate in the programme and were already part of the national household targeting system for the poor (*Sistema de Focalización de Hogares, SISFOH*), were automatically retained as potential beneficiaries. For all other applicants, the programme carried out a socioeconomic profiling to determine whether individuals were sufficiently poor 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 taking into account the following priorities: (i) unemployed chiefs of household with children younger than 18 years old were the first priority;[12] (ii) 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 (iii) up to 5 per cent for individuals with disabilities. In practice, some criteria were easier to verify (e.g. having children or being

[9] This equal weighting has been criticized for prioritizing districts that are more populated even though they might be less poor and underdeveloped (Jaramillo et al. 2009).
[10] See Section 3 for more information on the index and Appendix 1 for the definitions and sources of information of the variables.
[11] According to the Directorial Resolutions of the MTPE (2009–2010, 2007–2010, 2007) on the results of the call for tenders for *Construyendo Perú*'s projects, 380 urban districts received funding during the period 2007–2010 (of the 605 districts with a population of more than 2500 inhabitants in Peru).
[12] According to the description of the programme, this was done to target individuals that were actively looking for work, based on the assumption that chiefs of households would be actively searching, given the need to support their families.

a household head) than other, and so in practice individual targeting was focused on whether applicants had family burden (mostly children) and were living in poverty or extreme poverty.[13]

In terms of the support provided to participants, *Construyendo Perú* had two components.[14] The first one was the creation of temporary jobs in public investment projects such as pedestrian accesses, irrigation canals, post-harvest infrastructure, retaining walls, as well as educational and health infrastructure, etc. In this respect, the programme created a little over 685 thousand temporary positions, varying considerably in length from a few weeks to 4 months (MTPE, 2007–2011).[15] The second component entailed providing training to participants, of which there were two types, one general and one specific. The more general type of training consisted of soft skills development including social skills, empowerment and a general knowledge of how to manage project implementation. The second training component aimed to develop 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 enforced strictly (that is why the number of people that 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 persons with higher skills. The programme provided soft-skills training to close to 260 thousand individuals and more specific technical training to 27 thousand (Macroconsult S.A., 2012). Importantly, the beneficiaries of specific training were concentrated in the years 2007 and 2008. Since then, the number of participants started to fall until a seeming *de facto* elimination of the component in 2010.

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

In 2011, the Government terminated *Construyendo Perú* and created the new programme *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

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[13] In fact, based on the special survey carried out on participants, it can be observed that over 80 per cent of participants were already carrying out a remunerated activity in 2007 and half of them had been working for over 6 months (in fact, close to a third of them had been in this activity for a year).

[14] The development of social and productive infrastructures was considered an additional benefit of the programme, although this was not quantified. The programme financed 11,300 projects during the period 2007–2010, most of which were aimed to create pedestrian accesses, retaining walls and educational and health infrastructure.

[15] 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 programmes (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 in average while the programme was in place (Jaramillo et al. 2009). This means that various beneficiaries filled each notional "short-term job" in practice.

within poverty or extreme poverty in both urban and rural areas. The aim of the programme 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* with the exception of the training components, which were removed from the objectives of the programme in 2012.[16] Moreover, unlike its predecessor, the funding for *Trabaja Perú* depends on the fulfilment of previously established targets.

3. Data and descriptive statistics

The analysis draws on three sources of information. The first one is a database at the district level created for the purpose of this paper to reconstruct the FAD index and identify the related discontinuity in district participation, since this information was not publicly available. This additional analysis represents a clear value added of the paper, since it allows for the first time to exploit an interesting assignment rule that *Construyendo Perú* has at the district level, namely, that only districts above a certain level of poverty and development shortcomings are eligible to participate in the programme.

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 programme, 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 Appendix 1. The FAD index was reconstructed on the basis of this database by weighting equally three indicators: urban population, the index of human development shortcomings[17], and the poverty severity index FGT(2).[18] According to official documents of the programme (Jaramillo et al. 2009), the FAD index is constructed on the basis of data from the latest national census and updated when new information becomes available. The index used for the assignment of *Construyendo Perú* was therefore based on the 2007 census and calculated only once for the whole period during which the programme was active. Given the existence of detailed information regarding the sources of information and the calculation of the FAD index, the reconstruction carried out in this paper should result in the exact FAD index used during the geographical targeting of the programme. Details about the use of the FAD index for the analysis are discussed in more detail in Section 4.2.

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. While data from the participant survey was used to identify individuals in the treatment group, data from ENAHO was used to identify individuals in the control group.

ENAHO 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

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

[17] Calculated by FONCODES (*Fondo de Cooperación para el Desarrollo Social*) as 1 – HDI (Human Development Index calculated by UNDP) and called officially *índice de carencias (IC)*. This index 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 sectors with higher deprivation and vulnerabilities and therefore sectors with higher priority in terms of social investment (Días Álvarez, 2006).

[18] The FGT(2) or Squared Poverty Gap Index, is one of the indexes of the Foster, Greer, Thorbecke family of poverty measures. The index measures the severity of poverty giving a greater weight to individuals that fall far below the poverty line than to those that are closer to it (CIESIN, no date).

through a random assignment, which in 2013 made from approximately 32,000 dwellings and 115,000 individuals, around 60 per cent in urban areas and 40 per cent 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 household survey targeting questions to households and household members. It is a comprehensive survey, including 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 the individual's household and the dwelling's conditions. In addition, ENAHO collects information on individuals' education such as literacy levels, school attendance and levels of educational attainment. It also provides information on the individual's labour characteristics, such as employment status, occupation, industry, hours worked and monthly earnings in the case of employed individuals, or cause and duration of unemployment, among others, in the case of unemployed individuals. Finally, it collects information about individuals' participation in food related social programmes; and since 2012 about their participation in non-food related social programmes, such as *Trabaja Perú*.[19] This last module was critical to identify and exclude individuals that were *Trabaja Perú*'s beneficiaries from the control group at the time of measuring outcomes (i.e. 2012, as explained in more detail below).

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. The survey includes information on individuals' participation, such as dates of participation, types of works carried out, whether participants received training and the type and length of training received, as well as perceptions of participants about the programme and their participation. 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 provides information about participation during the period 2007 to 2010 and includes 1200 beneficiaries (of which 1142 were retained for the analysis) and their families, which in total make for 3701 observations.

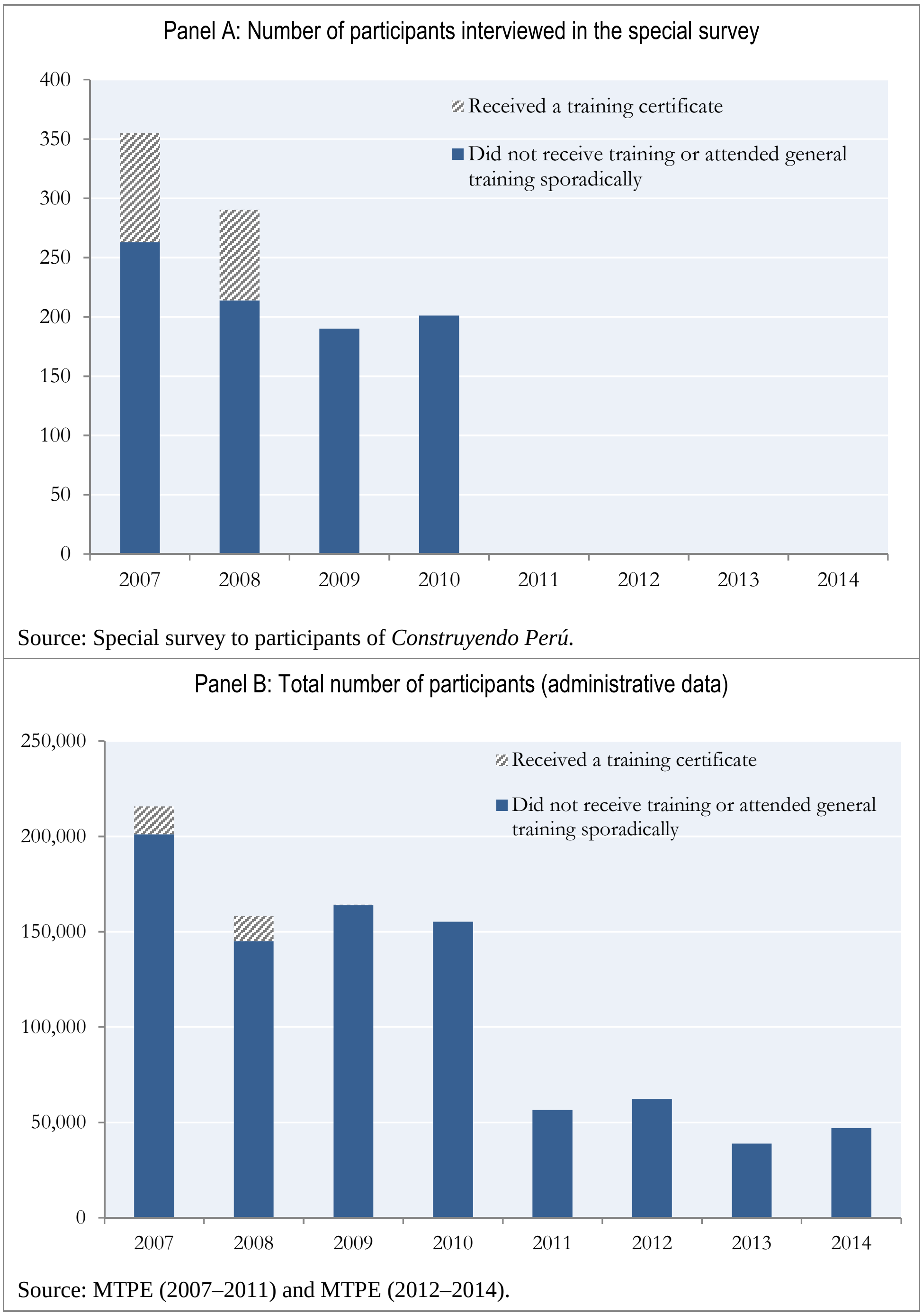
Figure 2 provides information on the evolution of the number of participants during the period. According to the special survey, the number of participants was the highest in 2007 and then it decreased to hit the lowest participation in 2010 (Figure 2, panel A). This fall in participation is in line with administrative data gathered from INEI (Figure 2, panel B) and is explained by a reduction in the budget allocated to the programme following the world financial crisis. In spite of the fall in funds allocated and number of participants, the programme suffered from a great deal of double participation. Indeed, data from the special survey shows that more than half of the beneficiaries (54 per cent) have participated more than once in the programme, while 28 per cent participated exceeding the maximum time of permanence of 4 months.

In terms of the training component, although over 90 per cent of beneficiaries interviewed affirm having received the training provided, only one third received a certificate after completion of specific training.

[19] 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.

Of these, only 29 per cent declared having assisted to practical courses, 30 per cent attended illustrative courses, and the remaining 40 per cent assisted only informative sessions. This illustrates the apparent lack of depth of the training component (even the specific one), discussed later in the paper.[20]

Figure 2. Number of participants of *Construyendo Perú* and its successor, 2007–2014



[20] Importantly, the difference in the share of participants that received a training certificate between panels A and B results from the choice of sampling technique used in the special survey, where individuals who received training were oversampled to ensure a sufficiently large sample size for the analysis (Macroconsult S.A., 2012).

A relevant 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 Appendix 2). 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, as both are on average around 43 years old. They are also similar in terms of their likelihood to be married, 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. While 68 per cent of participants were employed in 2012 and 22 per cent were in inactivity; in the selected ENAHO population these shares were 73 and 23 per cent respectively, the same year. The share of unemployed individuals is, however, higher for participants – 7 per cent compared to 3 per cent for the ENAHO adult population.

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

In addition, educational attainment of participants is lower than that of the ENAHO adult population. The share of participants who have not approved any level of education is around 8 per cent, compared to 4 per cent for all adults. Likewise, around half of participants has completed at most primary education (from here on, lower-educated individuals), while only 26 per cent of all adults from ENAHO are lower educated.

Among people with an occupation, most participants where either working as own-account (around 49 per cent) or waged workers (34 per cent). In comparison, a lower share of the selected adult population from ENAHO was own-account (36 per cent) or waged worker (19 per cent) in the same year, while a higher share was waged employee (27 per cent).[21] Moreover, at over 90 per cent of people with an occupation, informal employment was considerably higher among participants than in the ENAHO sample (77 per cent).

Both groups worked approximately the same number of hours (around 40 hours per week) in their main occupation. However, when all occupations are taken into account, it appears the selected adult population from ENAHO worked slightly more than participants. In spite of these similarities, the share of people in time-related underemployment (i.e. employed individuals available and willing to work more) was considerably higher among participants (21 per cent compared to 15 per cent) and the share working excessive long hours (i.e. more than 48 hours per week) was considerably lower (32 per cent compared to 41 per cent). Finally, a higher share of participants was working poor.

[21] 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.

Table 1. Descriptive statistics

	Urban population, ENAHO (18+)				Participants (18+)	
	2007		2012		March 2012	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Individual characteristics:						
Women	0.52	0.50	0.53	0.50	0.78	0.41
Age	40.5	16.8	42.8	17.6	43.5	12.5
Marital Status						
Cohabiting	0.24	0.43	0.24	0.43	0.37	0.48
Married	0.35	0.48	0.33	0.47	0.30	0.46
Widowed	0.05	0.22	0.06	0.24	0.07	0.25
Divorced	0.00	0.07	0.01	0.08	0.00	0.04
Separated	0.08	0.27	0.09	0.29	0.17	0.38
Single	0.28	0.45	0.27	0.45	0.10	0.30
Kinship family						
Head	0.52	0.50	0.52	0.50	0.47	0.50
Spouse	0.28	0.45	0.28	0.45	0.45	0.50
Son or daughter	0.20	0.40	0.20	0.40	0.04	0.19
Educational attainment						
No education	0.05	0.21	0.05	0.21	0.08	0.26
At most primary education	0.28	0.45	0.26	0.44	0.47	0.50
Beyond primary education	0.73	0.45	0.74	0.44	0.53	0.50
Household characteristics:						
Household members	4.86	2.26	4.57	2.16	4.46	1.83
Scales of monthly income (1 to 6)*	3.6	1.3	4.1	1.4	4.3	1.1
Labour characteristics:						
Employed*	0.72	0.45	0.73	0.45	0.68	0.47
Type of occupation						
Employer	0.05	0.21	0.05	0.21	0.00	0.04
Own-account worker	0.26	0.44	0.27	0.44	0.33	0.47
Waged employee	0.20	0.40	0.20	0.40	0.05	0.22
Waged worker	0.13	0.34	0.14	0.35	0.24	0.42
Non-paid family worker	0.08	0.26	0.07	0.25	0.02	0.13
Domestic worker	0.03	0.16	0.02	0.13	0.05	0.21
Other	0.00	0.07	0.00	0.07	0.00	0.04
Informal employment*	0.59	0.49	0.55	0.50	0.62	0.49
Formal employment*	0.15	0.36	0.20	0.40	0.06	0.24
Unemployed	0.04	0.18	0.03	0.16	0.07	0.25
Inactive*	0.22	0.41	0.23	0.42	0.22	0.41
Working time characteristics:						
Working-poor*	0.47	0.50	0.36	0.48	0.41	0.49
Hours worked in main occupation	41.9	23.3	39.9	22.2	40.4	17.8
Total usual hours worked*	48.1	22.2	45.8	21.2	43.7	16.4
Excessive working time*	0.46	0.50	0.41	0.49	0.32	0.47
Underemployed (time-related)*	0.26	0.44	0.15	0.36	0.21	0.41

Note: *See Appendix 3 for the definitions of these variables.

4. A regression discontinuity analysis

As explained above, the first phase of the targeting strategy (i.e. geographically targeting) was implemented by excluding rural districts from the eligible pool and, out of the remaining districts, selecting the benefiting districts by ranking them according to the composite index FAD. This type of programme assignment implies that participation is discontinuous at some point of the FAD index. Under these conditions, a regression discontinuity approach (RD) can be applied to capture the causal effects of the programme by using FAD (i.e. the running variable) as the potential source of identification of impacts. This is an interesting strategy as RD estimates can offer a credible alternative to randomized experiments at the local level (i.e. in the vicinity of the discontinuity) given that discontinuities provide a natural source of randomization (Bargain and Doorley, 2011).

4.1. Empirical specification: a fuzzy discontinuity design

As with any other microeconometric evaluation, the aim of the econometric implementation of this paper is to: (i) overcome the archetypal evaluation problem arising from the fact that individuals either receive treatment or do not but cannot be observed in both states at the same time; (ii) and tackle this problem of missing data all while addressing the possible occurrence of selection bias. As such, constructing a counterfactual that allows to estimate outcomes of participants had they not participated, in a convincing manner, is the key element of this evaluation.

In a non-experimental setting, such as the one where *Construyendo Perú* was implemented, some methods exist that can properly tackle the evaluation problem and address selection bias. Interestingly, certain non-experimental policy designs can even provide a natural source of randomization that allow estimating treatment under weaker assumptions (Blundell and Costa Dias, 2009; Smith and Todd, 2005). Regression discontinuity (RD) is one special case of this, which can be exploited when treatment changes discontinuously with some continuous variable, called the running variable (X). RD is based on the idea that assignment to treatment (D_i) is determined, totally or partially, by the value of a predictor being on either side of a fixed threshold called cut-off point (x_0) (Imbens and Lemieux, 2008).

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 cut-off point (e.g. under this approach the probability of receiving treatment need not change from 0 to 1). In practice there is inevitably some degree of fuzziness in the application of this approach, and the particular case discussed in this paper is no exception.[22] The result is an empirical specification where treatment is not determined by X_i, but there are additional unobserved factors that determine assignment to treatment (Hahn et al. 2001). Identification would therefore be possible by comparing individuals in the vicinity of the discontinuity – this is required for fuzzy RD to closely reproduce its sharp counterpart (Blundell and Costa Dias, 2009). As such, fuzzy RD relies on a local mean independent assumption to identify a local treatment effect, restricting external validity. This restriction constitutes the most important limitation of RD designs. The advantage of RD compared to other non-experimental estimators that may have more external validity is that: (i) comparatively RD has stronger internal validity (Imbens and Lemieux, 2008), and (ii) RD (specially the fuzzy type) is an especially powerful, yet flexible research design (Angrist and Lavy, 1999).

The key identification assumption of the RD approach is that treatment is a discontinuous function of x_i since regardless how close x_i approaches x_0, treatment will be unchanged until $x_i = x_0$. In the case of

[22] This fuzziness may occur, for example, when eligibility rules are not strictly observed or when only certain zones are targeted but mobility across regions occurs.

fuzzy RD, this assumption is somewhat relaxed. Treatment is no longer deterministically related to crossing a threshold, but there is a jump in the probability of treatment (i.e. $g_0(x_i)$ if $x_i < x_0$ and $g_1(x_i)$ if $x_i \geq x_0$) at x_0. It is assumed that $g_1(x_0) > g_0(x_0)$, so $x_i \geq x_0$ makes treatment more likely (Angrist and Pischke, 2009). This is called the continuity assumption (Hahn et al. 2001). Moreover, the exclusion restriction has to be satisfied, meaning that any observed discontinuity in mean outcome Y_i should result exclusively from the discontinuity in the participation rate. In other words, nothing other than participation is discontinuous in the analysis interval. In addition, the validity of RD is based on the premise that the running variable has not been caused or influenced by treatment and that the cut-off point has been determined independently of the running variable.

Particularly for the policy evaluated in this paper, given that participation is no longer deterministically related to crossing a threshold (i.e. there are participants and non-participants at both sides of the threshold), the probability of treatment jumps at the cut-off point x_0. Following Hahn et al. (2001) the conditional probability of treatment given x_i could be written as:

$$E[D_i|x_i] = P[D_i = 1|x_i] = \begin{cases} g_1(x_i) \ if \ x_i \geq x_0 \\ g_0(x_i) \ if \ x_i < x_0 \end{cases} \qquad 4.1$$

where, D_i is treatment status, X is the running variable, x_0 the cut-off point and $g_i(x_i)$ the relationship between the running variable and treatment status for individual *i*. It is assumed that $g_1(x_i) \neq g_0(x_i)$.

The relationship between the probability of treatment and x_i can be written as:

$$P[D_i = 1|x_i] = g_0(x_i) + [g_1(x_i) - g_0(x_i)]\, T_i \qquad 4.2$$

where treatment, $T_i = 1(x_i \geq x_0)$.

There are two ways to estimate these effects, through a polynomial function or a nonparametric estimator. Following Angrist and Pischke (2009) polynomials could be used to model $g_1(x_i)$ and $g_0(x_i)$:

$$E[D_i|x_i] = \gamma_{00} + \gamma_{01}x_i + \gamma_{02}x_i^2 + \ldots + \gamma_{0p}x_i^p + [\pi + \gamma_1^* x_i + \gamma_2^* x_i^2 + \ldots + \gamma_0^* x_i^p]T_i \qquad 4.3$$

where γ^*'s are the coefficients of the polynomial interactions with treatment. If the eligibility threshold is exogenously determined by the programme and highly correlated with treatment, the discontinuity becomes an instrumental variable for treatment status, which can be estimated through a two-stage least square (2SLS) strategy. Using T_i, as well as the interaction terms $\{x_iT_i + x_i^2T_i + \ldots + x_i^pT_i\}$ as instruments for D_i, I obtain $f(x_i)$ as:

$$Y_i = \alpha + \beta_1 x_i + \beta_2 x_i^2 + \ldots + \beta_p x_i^p + \rho D_i + \eta_i \qquad 4.4$$

where $D_i = \pi T_i$. Note that the behaviour of $E[Y_{0i}|x_i]$ and $E[Y_{1i}|x_i]$ may differ and that $\tilde{x}_i \equiv x_i - x_0$ centres the polynomials at x_0. Substituting $E[Y_i|x_i] = E[Y_{0i}|x_i] + E[Y_{1i} - Y_{0i}|x_i]D_i$, I obtain:

$$Y_i = \alpha + \beta_{01}\tilde{x}_i + \beta_{02}\tilde{x}_i^2 + \ldots + \beta_{0p}\tilde{x}_i^p + \rho D_i + \beta_1^* D_i \tilde{x}_i + \beta_2^* D_i \tilde{x}_i^2 + \ldots + \beta_p^* D_i \tilde{x}_i^p + \eta_i \qquad 4.5$$

The interacted model would generate the following conditional effects:

$$E[Y_{1i} - Y_{0i}|x_i] = \rho + \beta_1^* \tilde{x}_i + \beta_2^* \tilde{x}_i^2 + \ldots + \beta_p^* \tilde{x}_i^p$$

The second method to estimate a fuzzy RD is nonparametrically, using an IV estimator in the vicinity of the discontinuity (Angrist and Pischke, 2009). In principle it would be possible to use any nonparametric estimator to estimate the $f(x_i)$; in practice, however, 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 cut-off point increases (Imbens and Lemieux, 2008).

Specifically, the objective of the LLR is to find α_0 and β_0, as well as α_1 and β_1 that minimize:

$$\sum_i k_h(\tilde{x}_i)1[\tilde{x}_i < 0](Y_i - \alpha_0 - \beta_0\tilde{x}_i)^2 \text{ and } \sum_i k_h(\tilde{x}_i)1[\tilde{x}_i > 0](Y_i - \alpha_1 - \beta_1\tilde{x}_i)^2$$

In the fuzzy case, T_i (which equals 1 when $x_i \geq x_0$) is used as an instrument for D_i in an δ-neighbourhood of x_0. Thus, the effect of treatment (which needs to be estimated using the same estimator and bandwidth – Angrist and Pischke, 2009) equals to:

$$\lim_{\delta \to 0} \frac{E[Y_i|x_0 < x_i < x_0 + \delta] - E[Y_i|x_0 - \delta < x_i < x_0]}{E[D_i|x_0 < x_i < x_0 + \delta] - E[D_i|x_0 - \delta < x_i < x_0]} = \rho \tag{4.5}$$

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 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). While estimating this in a given window of width h around the cut-off is straightforward, it is more difficult to choose the bandwidth. There is essentially a trade-off between bias and efficiency.

Numerically, as noted by Hahn et al. (2001), when using a uniform kernel with the same bandwidth for the estimation of both the numerator and the denominator and no additional covariates, the estimate ρ is equivalent to that of a 2SLS estimator. However, inference based on uniform kernel estimators and LLR (4.5) will be different since the former will continue to be asymptotically biased given the poor boundary properties.[23] As such, LLR has two main advantages in the case of fuzzy RD: first, it is more rate-efficient since there is a smaller bias associated to LLR relative to traditional kernel methods; second, the bias does not depend on the design density of the data (Hahn et al., 2001).

While impacts in the vicinity of the cut-off point are nonparametrically identified for RD, the applied literature has frequently used the parametric alternative (Ravallion, 2008). The problem with this method is that it uses data that is far away from the cut-off to estimate the *f*(X) function. The equivalent of choosing the right bandwidth for the polynomial method is to use the right order of polynomial. However, 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.

4.2. Graphical discontinuity

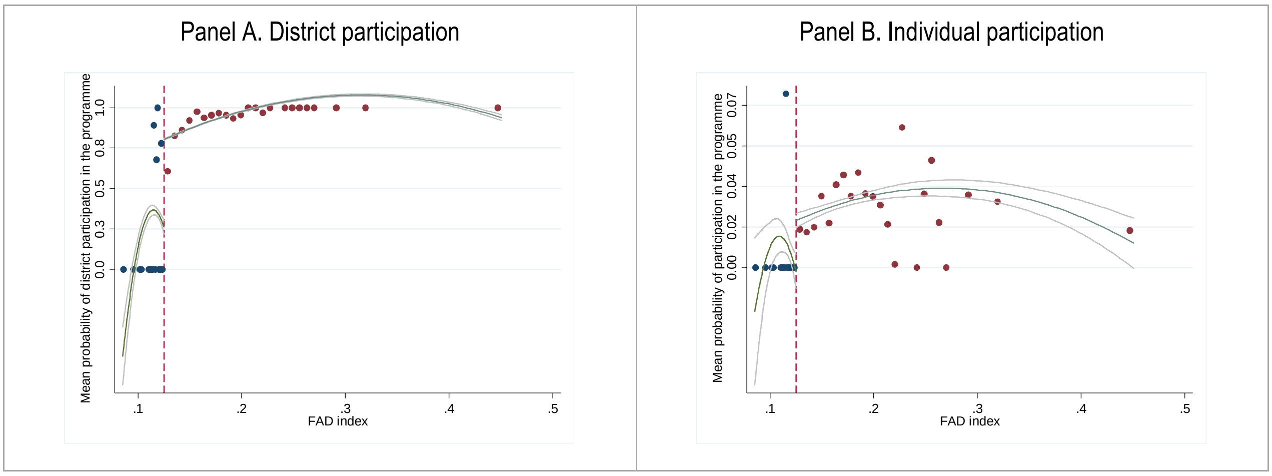
Before discussing the estimation results, I present the graphical analysis of the discontinuity discussed in previous sections. This graphical analysis, as argued by Imbens and Lemieux (2008), is an integral part of any RD analysis and is critical to ensure the robustness of the more sophisticated statistical assessment that follows.

[23] Imbens and Lemieux (2008) extended this work to show that the equality still holds when a LLR is computed by 2SLS using additional interaction terms included as exogenous controls. These additional covariates allow for changes in the slope on either side of the cut-off, eliminating small sample biases and improving precision.

As explained above, a baseline analysis was needed to identify the discontinuity related to the FAD index since neither the running variable (FAD index) nor the cut-off point were publicly available. After having reconstructed the FAD index based on the database at the regional level, the cut-off point was determined by a graphical examination of the data. Both the graphical and the statistical analyses are based on a comprehensive individual-level database including the sample selected from the ENAHO, the participants' database and the district-level database. To determine the cut-off point at the district level, the FAD index was plotted against the mean participation of urban districts, drawing on this comprehensive database. Figure 3 (panel A) illustrates a clearly observable fuzzy discontinuity in the participation of districts (measured at the individual level, i.e. individuals living in districts that participated in the programme during the period 2007–2010) according to the FAD index. Given this is a fuzzy RD design, the figure exhibits the mean probability of districts participating in the programme conditional on crossing the running variable's (FAD index) cut-off point of 0.125. 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.[24,25] This graphical analysis also shows that there is no discontinuity in the mean probability of districts participating in the programme, other than the cut-off point.

Panel B of Figure 3 displays this same analysis but at the individual level of participation (i.e. individuals that participated in the programme during the period 2007–2010). Finding a discontinuity in the relationship between individual participation and the running variable is an important step in the analysis of fuzzy discontinuity designs. In fact, the first-stage of the specification (e.g. participation as a function of the running variable and the probability of being beyond the cut-off point) depends on whether this discontinuity exists. The figure illustrates that there is indeed a discontinuity in the mean probability of individuals participating in the programme conditional on them living in districts with a level of FAD at one side or the other of the cut-off point (0.125).

Figure 3. Discontinuity in districts' and individuals' participation (2007–2010), conditional to the FAD index



Note: Fig. 3 plots the mean probability of districts (panel A) and individuals (panel B) participating in the programme according to the FAD index along with the 95 per cent level confidence bounds. The conditional mean is drawn on the base of equal-sized bins. The fit used was suggested by the graphical analysis carried out using lowess fit.

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[24] 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 (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.

[25] The quadratic fit used in the graphical representation was suggested by the analysis carried out using the lowess fit.

Appendix 4 illustrates the RD estimates of the impact of the programme based on this discontinuity. The different figures of the appendix plot the probability of having a certain employment status, income and working time,[26] 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 neighbourhood of the discontinuity for each output variable estimated. With regards to the larger sample (left figures of all panels), the graphical analysis suggests that participating in the programme has a small but positive effect on the probability of being employed (panel A), a negative effect on the probability of being inactive (panel B), a positive effect on the probability of being employed informally (panel D) (and a mirror effect on formal employment). It also has a positive effect on the probabilities of being own-account worker (panel E) and waged worker (although this last effect is not as clear) (panel F), and an unclear effect on the probability of being waged employee (panel G). Moreover, there seems to be no effect on the probability of participants moving up or down in their income scales[27] (panel H) and an increased probability of being working-poor (panel I). Regarding hours worked, participation seems to have a positive effect on the total number of hours worked (panel J) but also a positive effect on the probability of working excessive hours (panel K). Effects are consistent when analysing local effects (right figures of all panels), with the exception of the impact in the probability of working excessive hours where the effects seem to lose significance.

4.3. Estimated results

Now I turn to the statistical results of the effect of individuals' participation in the programme. This section examines whether the graphical effects hold using more sophisticated techniques and if these effects are robust to different specifications. As suggested in section 4.1, two different estimators have been used: a parametric 2SLS setup and a nonparametric LLR with three different bandwidths.[28] The estimated results are shown in tables 2 and 3, which corroborate the results from the graphical analysis presented above.

Effects of the programme on participants' labour market status

As discussed above, the programme was created with the final objective of enhancing the employability of individuals living in poverty and extreme poverty so they can find sustainable employment after the programme culminates, and improving their living conditions by providing or improving public infrastructure.

Estimates show that the programme had indeed a positive effect on the probability of participants of being employed and being active in the labour market (Table 2). Effects are, however, not significantly different from zero for all specifications and for all groups. Particularly, these labour market effects are statistically significant for women and the lower educated in the sample (i.e. individuals with at most primary schooling[29]), for whom the programme increases the probability of being employed and reduces the probability of being inactive. In comparison, these effects are non-statistically significant for men and higher-educated individuals. For this latter group, however, the impact on inactivity is statistically significant and negative under some specifications. It is important to note that whereas the sample by level of education is almost perfectly balanced (around half of the sample has completed at most primary

[26] See Appendix 3 for the definitions and sources of all output variables.
[27] Income scale categories go from 1 (no income) to 6 (highest income).
[28] 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.
[29] For details on the definition of higher- and lower-educated individuals, see tables 2 and 3.

education), the opposite is true by sex. As such, the lack of statistically significant results for men could be driven by the lack of statistical power resulting of an insufficiently large sample.

In terms of the size of effects, the programme increases the probability of women of being in employment by around 4.5 to 7 percentage points[30] depending on the bandwidth used; and reduces the probability of being inactive by around 5 to 8 percentage points. The significance of these effects is robust to different bandwidths and alternative estimators and their magnitude increases with smaller bandwidths.

Alongside these positive effects, the programme brought about an increase in the probability of participants of being employed informally (and a decrease in the probability of working formally, although of lower magnitude). Interestingly, effects of the programme by status in employment show increased probabilities of participants working as own-account and waged workers and a decreased probability of them working as waged employees. These results may provide some insights into the negative informal employment effects. Effects are again statistically significant for female participants, but unlike previous results also for higher-educated individuals. In comparison, the effects of the programme on the probability of working informally are non-significant for men and for lower-educated individuals.

A final sub-group analysis was carried out to assess the effects of the programme particularly on departments that have a higher proportion of urban inhabitants. Results remain unchanged to those found for the overall population, which is not surprising given that these departments account as well for the majority of programme participants. This analysis confirms that the detrimental effects of the programme on informal employment are not related to the unavailability of formal-sector jobs in departments with a higher proportion of rural inhabitants.

Table 2. Estimates of the effect of *Construyendo Perú* on labour market status

PANEL A	Parametric 2SLS (two stage least square) method					
	All	Women	Men	Lower educated*	Higher educated*	Urban departments
Employed	ns	2.28* (1.24)	ns	4.67* (2.69)	ns	ns
Inactive	ns	-2.46* (1.36)	ns	-4.73* (2.75)	ns	ns
Employed informally	5.53** (2.44)	3.97** (1.85)	11.73** (5.45)	ns	6.64** (2.90)	5.95* (3.06)
Employed formally	-3.01** (1.42)	-1.49* (0.85)	-8.63* (4.82)	ns	-3.95** (1.92)	-3.42* (1.81)
Own-account worker	3.57** (1.47)	2.79*** (1.02)	ns	ns	3.53** (1.43)	3.98** (1.88)
Waged worker	ns	ns	ns	ns	ns	ns
Waged employee	-2.76** (1.36)	-1.56** (0.79)	-7.37* (4.26)	ns	-2.79* (1.63)	-3.56** (1.78)

[30] 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 cut-off point ranges between 4.5 and 7 percentage points.

Table 2 (continued)

PANEL B		Non parametric LLR (local linear regression) method					
	Bandwidth	All	Women	Men	Lower educated*	Higher educated*	Urban departments
Employed	*Optimal*	ns	4.49* (2.54)	ns	ns	ns	ns
	Half	8.85** (3.59)	6.99** (3.34)	ns	20.93** (10.55)	6.81** (3.45)	8.31** (3.69)
	Double	4.52* (2.72)	6.08** (2.94)	ns	ns	ns	4.58* (2.62)
Inactive	*Optimal*	ns	-4.94* (2.53)	ns	ns	ns	ns
	Half	-8.64** (3.72)	-8.25** (3.53)	ns	ns	-5.26* (3.13)	-6.97** (3.29)
	Double	-5.85* (3.28)	-6.71** (2.97)	ns	ns	-6.35* (3.86)	-5.35* (2.75)
Employed informally	*Optimal*	15.77*** (4.94)	7.53** (3.07)	ns	ns	15.21*** (5.14)	14.48*** (4.69)
	Half	20.87*** (5.30)	11.23*** (3.70)	ns	ns	-20.24*** (6.21)	-17.37*** (5.95)
	Double	8.13*** (2.95)	6.72** (2.87)	ns	ns	8.10*** (3.11)	6.96*** (2.39)
Employed formally	*Optimal*	-8.43** (3.44)	ns	ns	ns	-9.44** (4.01)	-8.39** (3.47)
	Half	-8.54** (3.42)	-3.89* (2.09)	ns	ns	-8.84*** (3.38)	7.63** (3.25)
	Double	-4.20* (2.18)	ns	ns	ns	-4.34* (2.56)	-6.24** (2.76)
Own-account worker	*Optimal*	5.47** (2.21)	4.54** (2.01)	ns	ns	7.15** (3.09)	7.86*** (2.95)
	Half	10.22*** (3.55)	7.05** (2.80)	ns	ns	9.19*** (3.36)	10.91*** (3.35)
	Double	5.78** (2.76)	4.74** (1.87)	ns	ns	6.57* (3.56)	5.06** (2.07)
Waged worker	*Optimal*	3.55** (1.76)	2.09* (1.22)	ns	ns	4.30** (1.99)	3.55** (1.65)
	Half	6.28** (2.72)	2.40* (1.31)	ns	ns	7.71*** (2.94)	5.37** (2.42)
	Double	3.49* (2.01)	ns	ns	ns	5.31* (3.07)	2.90* (1.53)
Waged employee	*Optimal*	-7.52*** (2.67)	-3.18* 1.87	ns	ns	-8.93** (4.34)	-8.92*** (3.09)
	Half	-10.97*** (3.77)	-5.40** 2.68	ns	ns	-10.09*** (3.84)	-10.48*** (3.59)
	Double	-7.40** (3.32)	ns	ns	ns	-5.25** (2.40)	-6.78*** (2.58)
Observations		46,664	24,427	22,237	12,374	34,256	38,440

* For the purpose of this analysis, I consider lower-educated individuals those that have completed at most primary education (0-7 years of schooling) and higher educated those beyond that level of education (8 years or more).

Notes: Tab. 2 reports estimated treatment effects of the programme *Construyendo Perú* conditional on crossing the FAD index cut-off point of 0.125. Panel A reports estimates obtained using the parametric 2SLS method. Panel B reports estimates obtained using a triangular kernel regression model on both sides of the cut-off for three different bandwidths (see footnote 29 for a discussion of the different bandwidths used). All effects have been calculated including all districts. Standard errors are in parentheses. Significance levels: *significant at 10%; **significant at 5%; ***significant at 1%; *ns* is non-statistically significant.

Effects of the programme on income and working time

The persistence of informal employment can also have detrimental effects on poverty, potentially endangering one of the primary objectives of the programme. The effects of the programme confirm this concern. The programme increases participants' probability of being working poor for the overall group, for women and for higher-educated individuals (Table 3). In contrast, the effect is non-statistically significant for men and the lower educated. Effects are robust to different specifications and different bandwidths. Moreover, the programme shows non statistically-significant effects on the probability of participants moving upwards or downwards their income scales.

Given that in Peru, the poorest sectors of the population are burdened disproportionately by informal employment, it can be argued that the effects of the programme on working poverty are linked to its detrimental effects on the probability of working informally. The ENAHO shows, for example, that the majority of working poor (around 90 per cent) worked informally during 2007–2013, mostly as own account workers (close to 60 per cent). These figures are considerably higher than those for non-working poor, of whom 77 per cent worked informally during this period and a little over 35 per cent 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 per cent) but, interestingly, also as employer (over 10 per cent). In addition, working poor have lower incomes (40 per cent lower) but working the same number of hours. Interestingly, they are not substantially less educated than the overall occupied sample (they have cursed in average over 9 years of schooling compared to 11 for the overall sample) and the proportion of women is only slightly higher. In sum, what sets apart working poor from the rest of the population is mainly their informal working status.

Regarding the effect of the programme on working time, participation has a positive effect on the total number of hours worked (an increase of 24 per cent or 11 hours per week) and a positive effect on the probability of working excessive hours (of around 14 percentage points). However, (and consistent with the graphical analysis) these results are only statistically significant (at the 10 per cent level) for the overall treated group and for the higher skilled, and effects are not robust to the alternative specification.

The lack of robustness and/or significance of these effects may be related to the fact that in Peru, longer hours are worked in formal jobs and in occupations that might be less common among *Construyendo Perú*'s participants, such as employers. For example, while individuals working formally reported having worked more than 50 hours per week (in all occupations confounded) during the period, those who worked informally reported working 45 hours. Consequently, the share of individuals working excessive hours was also higher among formal workers than informal ones (around 47 per cent 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), followed by waged workers with around 50 hours and waged employees and own-account workers with 47 hours worked per week. Consequently, employers also had the highest share of individuals working excessive hours (over 56 per cent), while this share was close to 47 per cent for each of waged workers and own-account workers.

Table 3. Estimates of the effect of *Construyendo Perú* on participants' income and working time

PANEL A	Parametric 2SLS (two stage least square) method					
	All	Women	Men	Lower educated*	Higher educated*	Urban departments
Monthly income scales	-16.8* (9.17)	-10.39* (5.99)	-36.97* (21.45)	ns	ns	ns
Working poor	7.60*** (2.72)	5.62*** (1.95)	13.5** (5.88)	ns	8.86*** (3.21)	9.51** (4.26)
Logarithm of hours worked	ns	ns	ns	8.13** (3.97)	ns	ns
Excessive working time	ns	ns	ns	ns	ns	3.59* (1.94)

PANEL B		Non parametric LLR (local linear regression) method					
	Bandwidth	All	Women	Men	Lower educated*	Higher educated*	Urban departments
Monthly income scales	*Optimal*	ns	ns	ns	ns	ns	ns
	Half	ns	ns	ns	ns	ns	ns
	Double	ns	ns	ns	ns	ns	ns
Working poor	*Optimal*	13.9* (7.58)	10.24* (5.78)	ns	ns	7.72** (3.85)	14.45* (7.66)
	Half	15.2** (6.45)	16.23* (8.40)	ns	ns	10.55* (6.04)	13.54** (5.74)
	Double	10.6** (5.28)	ns	ns	ns	Ns	8.67** (3.70)
Logarithm of hours worked	*Optimal*	23.6* (13.50)	ns	ns	ns	18.85* (11.41)	23.99* (13.57)
	Half	21.3** (9.36)	ns	ns	ns	15.94* (8.06)	ns
	Double	13.9* (7.18)	15.97* (9.44)	ns	ns	ns	11.53** (5.45)
Excessive working time	*Optimal*	14.0* (7.78)	ns	ns	ns	16.12* (8.47)	15.77* (8.83)
	Half	17.5** (7.94)	ns	ns	ns	-12.62** (5.69)	13.75** (5.99)
	Double	11.1* (6.69)	ns	ns	ns	7.61* (4.36)	7.12* (3.69)
Observations[31]		34,635	16,107	18,528	8,361	26,273	28,053

* 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).

Tab. 3 reports estimated treatment effects of the programme *Construyendo Perú* conditional on crossing the FAD index cut-off point of 0.125. Panel A reports estimates obtained using the parametric 2SLS method. Panel B reports estimates obtained using a triangular kernel regression model on both sides of the cut-off for three different bandwidths (see footnote 29 for a discussion of the different bandwidths used). All effects have been calculated including all districts. Standard errors are in parentheses. Significance levels: *significant at 10%; **significant at 5%; ***significant at 1%; *ns* is non-statistically significant.

[31] The estimation for the working-poor is based upon 33,666 observations for the full sample, 15,402 for women, 18,264 for men, 8,002 for the lower educated, 25,663 for the higher educated and 27,386 for urban districts. The monthly income scales estimation is based upon 45,801 observations for the full sample, 23,894 for women, 21,907 for men, 12,666 for the lower educated, 33,081 for the higher educated and 37,768 for urban districts.

4.4. Interpretation of results

Some hypotheses can be made to interpret these effects. Clearer and more robust effects of the programme on women may be influenced by the fact that, as discussed above, women participation in the programme was disproportionately higher compared to the median distribution in the household survey. This, as pointed out by the field study carried out by MEF (Jaramillo et al. 2009) might be explained by the low take-up rates of men.

Moreover, the detrimental effects of the programme on women's employment status and incomes may be related to the inability of the programme to sustainably raise their employability. For example, qualitative evidence from the MEF field study (Jaramillo et al., 2009) shows that female participants have unstable labour patterns (e.g. multiple entries and exists from the labour market usually working in temporary jobs). One of these labour market challenges is informal employment, which hits women disproportionally in Peru – while the urban informal employment rate for men was around 72 per cent during the period 2007–2013, for women it stood at 83 per cent. Given the disproportionately high participation of women and their unstable labour patterns, the *de facto* absence of components to raise their employability (e.g. specific type of training) may have 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, programmes could have negative effects (due to stigma- and lock-in effects during participation – Hujer et al. 2004). Although the programme counted with a training component (which was officially eliminated only in 2010), the monitoring of the programme carried out by MEF notes that already in 2009 no specific training had been provided by the programme. 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)[32] and the quality and depth of the courses uneven among participants and between districts (e.g. specific training consisted only of informative sessions for 40 per cent of the beneficiaries of this training).

In addition, the difference in effects between higher- and lower-educated participants seems also to be related to the provision of the training component. Since participation in specific training was voluntary, some purposive selection of more driven participants is to be expected into this training. In fact, as explain by the field study carried out by the MEF some of the specific training provided gave rise to the establishment of productive microenterprises by some of the most driven participants that completed the course, which were likely located in the informal sector (ILO, forthcoming). The results of the impact evaluation seem to confirm this analysis, i.e. the programme increased the probability of higher-educated participants of being self-employed and decreased their probability of being waged employees. This may explain why the programme 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 therefore less exposed to employability-enhancing components), the programme did not improve their odds of having a better quality job.

[32] And only 6.6 per cent of participants were certified after the training culminated (i.e. meaning they assisted to at least 70 per cent of the training and validated the training) (Jaramillo et al. 2009).

5. Robustness checks and additional results

This section provides, first, a number of sensitivity tests to the change in estimation strategy to check whether estimation results hinge on the choice of estimator. Second, it provides a thorough robustness analysis to ensure that no threat to the validity of assumptions remains.

5.1. Sensitivity tests

Three different informal sensitivity tests were carried out to check how changes in the estimation strategy affect results. First, the use of different estimation methods constitutes in-of-itself 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 on estimating the effects of the programme using twice and half the optimal bandwidth. Estimates, presented in the last two columns of tables 2 and 3, show overall consistent results in terms of significance using the different bandwidths (the size of effects is in the majority of cases larger using narrower bandwidths).

Third, different estimations have also been carried out including and excluding districts with an urban population of less than 2500 inhabitants (i.e. first eligibility criteria during geographical targeting). Results using the 2SLS specification and LLR with optimal bandwidth are consistent between the two samples. Findings from the LLR estimation using the larger bandwidth are broadly consistent too, with a slight loss of significance when some districts are excluded (e.g. the sample is reduced). When using half the preferred bandwidth, however, some results switch signs in the sample that excludes smaller districts – i.e. the effect on the probability of finding formal employment becomes positive and the effect on the probability of being employed as own-account worker becomes negative.

5.2. Threats to validity

Ensuring that agents cannot manipulate the running variable in a discontinuous manner

As discussed in section 4.1, the validity of RD is based on the premise that the running variable has not been caused or influenced by treatment and that the cut-off point has been determined independently of the running variable. These two conditions are satisfied in the analysis by construction. Although the FAD index was designed by the programme's administration, it is based upon three indicators that are calculated by government institutions independently from the programme. Moreover, their definitions predate the establishment of the programme and did not change throughout its duration. Finally, the cut-off point in the FAD index was determined by the availability of government funds for this particular programme per year – i.e. independently from the construction of the running variable.

Checking for other discontinuities in the running variable

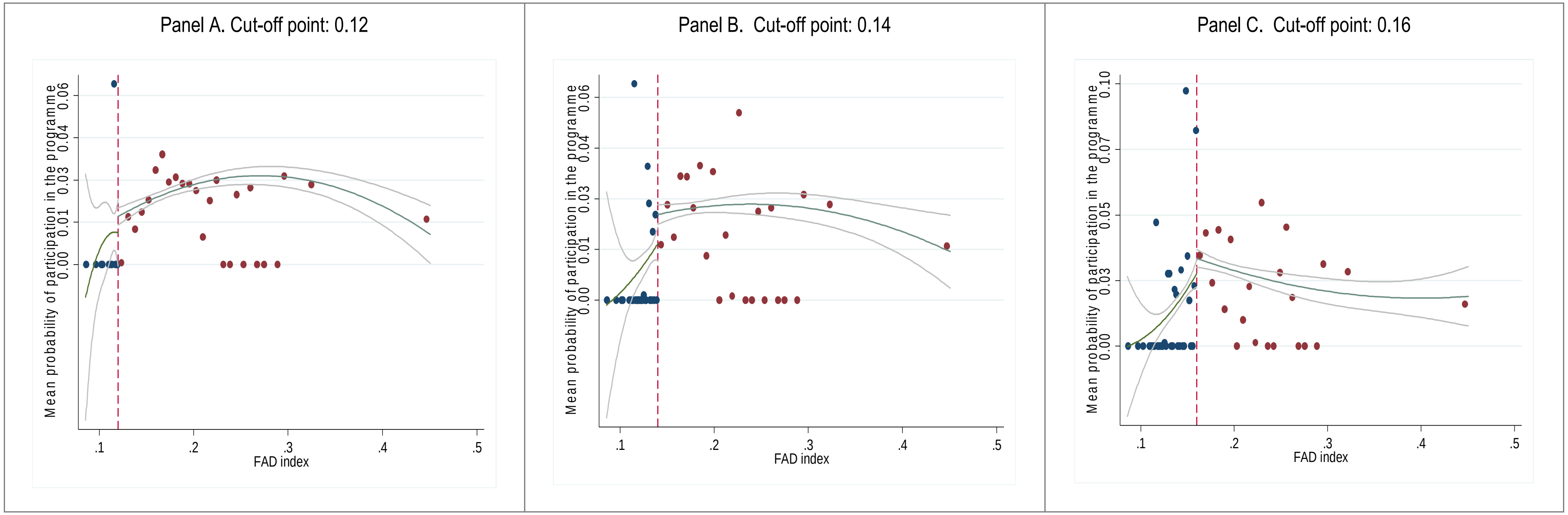
As discussed during the graphical analysis, checking for other discontinuities in the running variable was fundamental for the estimation strategy. Given that the FAD index was used for assignment at the district (rather than individual) level, a careful analysis is carried out earlier in this paper to determine whether there was a jump in participation at the individual level. This close scrutiny of the running variable included an inspection of other possible discontinuities (necessary to unveil where the actual discontinuity lied). Findings from this graphical analysis (Figure 4) show no other discontinuity that can be detected from the overall dispersion of the data, other than the one used for the analysis (see Figure 3, panel B).

Falsification tests and a look to non-eligible districts and non-eligible groups

Falsification tests in this paper assess whether non-targeted groups (or less targeted ones) have been affected by the programme. Similar effects of the analysis on non-participants would mean that other programmes or policies 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 that should not normally be affected by the programme, 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).

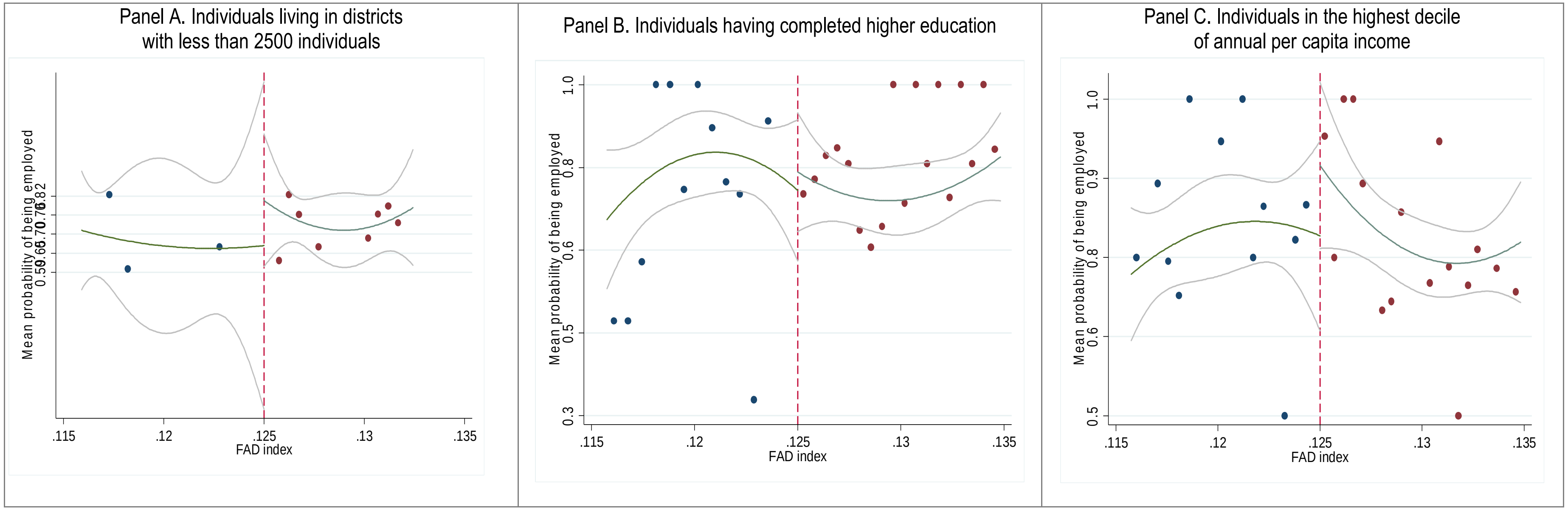
Panels A, B and C of Figure 5 show a clear difference in results between the findings of the evaluation and these falsification tests on selected variables. First, there is no clear discontinuity in the FAD index for individuals: living in small districts (panel A), having completed higher education (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.

Figure 4. Individuals' participation according to the FAD index at various cut-off points



Note: Fig. 4 plots the mean probability of individuals participating in the programme according to the FAD index using cut-off points at 0.12, 0.14 and 0.16, along with the 95% level confidence bounds. The fit used was suggested by the graphical analysis carried out using lowess fit.

Figure 5. Discontinuity in the FAD index for specific non-targeted groups



Note: Fig. 5 plots the mean probability of being employed according to the FAD index, along with the 95% level confidence bounds, for individuals in three particular categories. The fit used was suggested by the graphical analysis carried out using lowess fit.

6. 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 the purpose of this evaluation) including information on district characteristics as well as on the participation of districts in the programme; (ii) a special survey carried out to programme participants in March 2012 (Macroconsult S.A., 2012); and (iii) the ENAHO from 2007 to 2013. The evaluation assesses the effects of the programme in 2012 for individuals that participated during the period 2007–2010, finding mixed effects. The intervention helps raising employment and reducing inactivity for particular groups of beneficiaries, yet at a cost of locking participants in lower quality jobs (i.e. informal, paid below the poverty line and working excessive hours).

In more detail, the programme raises the probability of women and lower-educated participants of being employed and attached to the labour market. For higher-educated individuals the programme reduces the probability of being inactive but has no effect on employment. Finally, the programme has no effects for men (which may be due to insufficient sample size). The lack of employment effects for certain groups is not surprising given that the majority of participants where already engaged in a remunerated activity before the programme started. Lack of employment effects could thus imply large deadweight loss (i.e. participants would have found a job in the absence of the programme). Another explanation is that the programme had short-term effects but that they faded away with time (i.e. especially given that effects in this paper are measured over the medium-term). This hypothesis is in line with the existing literature on the employment effects of activation measures in Latin America, which points to a greater effectiveness of programmes on the very short term (Kluve, 2016). Moreover, clearer and more robust effects of the programme on women could be explained by their considerably higher participation in the programme.

Alongside these labour market effects, the programme increased the probability of participants of being employed informally and of being working poor. These effects are again statistically significant for women, but unlike previous results also for the overall group of participants and for the higher-educated ones. The effects seem to be related to the impact of the programme by status in employment – i.e. programme increases the probabilities of participants of working as own-account and waged workers and decreases their probability of working as waged employees. In other words, the programme increases the odds of participants of working in occupations characterized by having lower job quality. Meanwhile, given that in Peru the poorest sectors of the population are burdened disproportionately by informal employment, it can be argued that the effects of the programme on working poverty are linked to those on informality. Finally, the programme had a positive effect on the number of hours worked, but only for the overall group of participants. For particular groups, this effect is non-significant, which is not surprising given that in Peru, longer hours are worked in formal jobs and in occupations that are not common among *Construyendo Perú*'s participants (i.e. employers). Participation also increased the probability of working excessive hours; an effect that is again particularly relevant for women and for higher-educated individuals.

It is argued in this paper that the detrimental effects of the programme on work quality may be related to the inability of the programme to sustainably raise the employability of participants (e.g. ineffectiveness or the *de facto* absence of training, particularly the specific type). Indeed, existing literature notes that ALMPs specifically targeted to vulnerable groups could have detrimental effects in the absence of specific components aimed to raise employability (Hujer et al. 2004). In particular, given the disproportionately high participation of women in the programme and their unstable labour patterns, the absence of measures to favour their employability may have perpetuated the informal and low-pay labour market trends of women. Moreover, different effects of the programme among higher- and lower-educated participants can also be explained by the provision of an employability-enhancing component. Particularly, the self-selection of more driven participants into the specific training (where participation was voluntary) seems to have given rise to the establishment of microenterprises, likely located in the informal sector (Jaramillo et al., 2009). This may explain why the programme 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 therefore less exposed to employability-enhancing components, the programme did not improve their odds of having a better quality job.

Importantly, it is well known that the success of these programmes hinges on their particular design and implementation characteristics, which in developing countries has not been invariable positive (Subbarao et al. 2013). *Construyendo Perú* is no exception in this regard. The evaluation also finds that the programme suffered from multiple participation and overrepresentation of particular groups, which can be an indication of the need of better enforcement of targeting rules and eligibility criteria or even of lack of demand for this type of programme. It is essential to consider the evidence of this paper in light of this.

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Appendix 1: 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 centres. Districts may be comprised of one or more populated urban centres.	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 giving a greater weight to individuals that fall far below the poverty line than to those that are 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 districs with higher priority in terms of social investment.	De la Torre, R. (2005)
Districts participating by year (2007–2010)	Districts that have received funding to participate in the programme "Construyendo Perú".	MTPE (2009–2010, 2007–2010, 2007).
Allocation factor at the district level (*Factor de Asignación Distrital*, FAD)	A composite index constructed by the Planning Management Unit of the programme 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).

Appendix 2: Full set of 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:									
Household annual income	39279	10390.1	14677.9	43826	13916.0	17421.1	1142	8510.1	9534.1
Household annual 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
Scales of the monthly household income (1 to 6)	39279	3.6	1.3	43826	4.1	1.4	882	4.3	1.1
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 programme	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

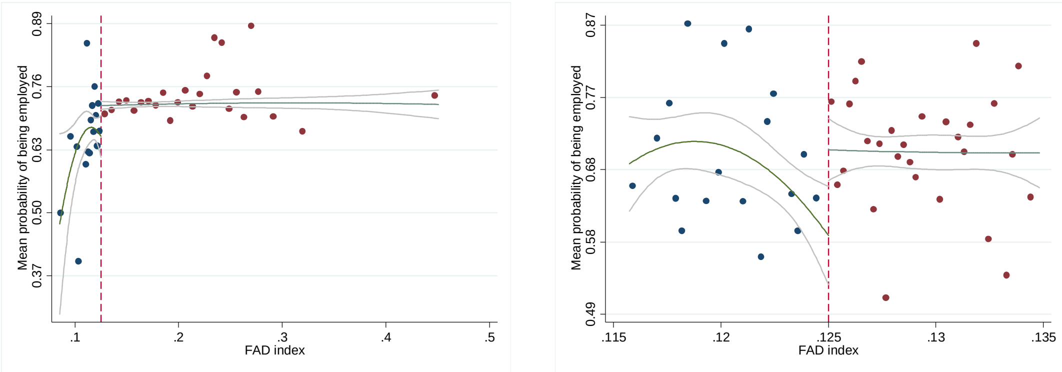
Appendix 3: Definitions and sources of labour market variables

Variable	Definition	Source
Labour market status:		
Employed	Individuals that had an occupation during the week of reference, remunerated or not, but working more than 14 hours.	ENAHO
Inactive	Individuals that were not in the economic active population during the week of reference. This includes individuals not in employment or unemployment, and individuals that had an occupation as unpaid family workers or "other", but 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[33], (ii) non-remunerated family workers; (iii) and individuals that working in the formal sector 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 that are affiliated to 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 that have not registered their activities in SUNAT (*Superintendencia Nacional de Aduanas y de Administración Tributaria*), that have no accounting system and that have 5 or less 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 analysed in these paper are: <u>Waged employees</u>: individuals with a predominantly intellectual occupation in an institution or firm where they perceive a monthly or half-monthly remuneration or payment; <u>waged workers:</u> have a predominantly manual occupation in an enterprise or business where they perceive a daily, weekly or half-monthly remuneration; <u>own-account workers:</u> can exercise a profession or operate their own business but without having dependant employees.	ENAHO
Income:		
Working poor	Employed individuals living in households in which per-capita income/ expenditure 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).[34]
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
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 that during the week of reference were available and willing to work more hours than those usually worked.	ENAHO

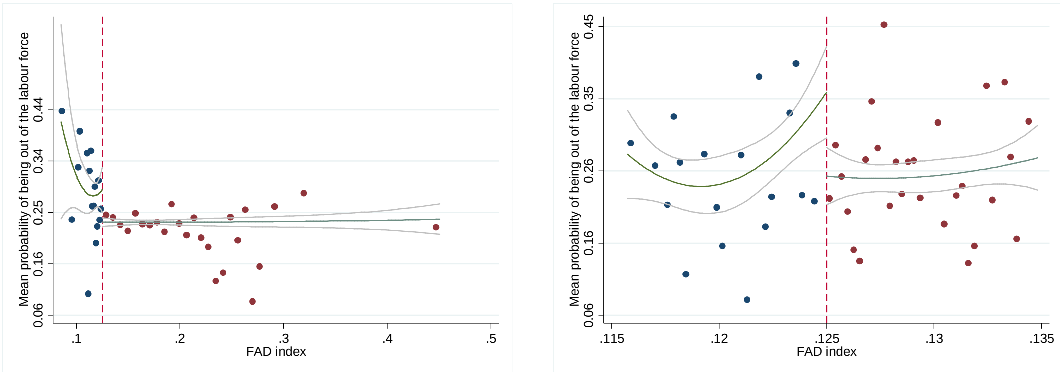
[33] The informal sector is defined as all employers or enterprises with less than 5 employees and not registered in the Peru internal revenue service (SUNAT).
[34] ILO (2012), pp. 68-69.

Appendix 4: Graphical analysis of the effects of the programme, 2012*

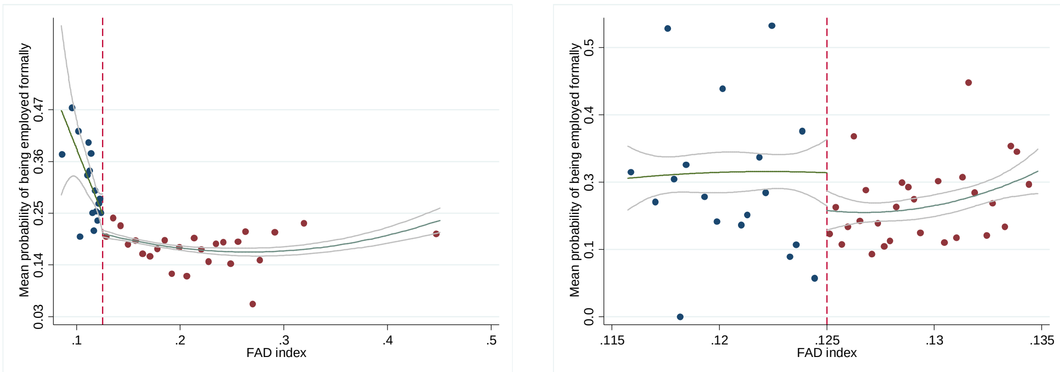
Panel A. Probability of being employed (overall window and smaller bandwidth)



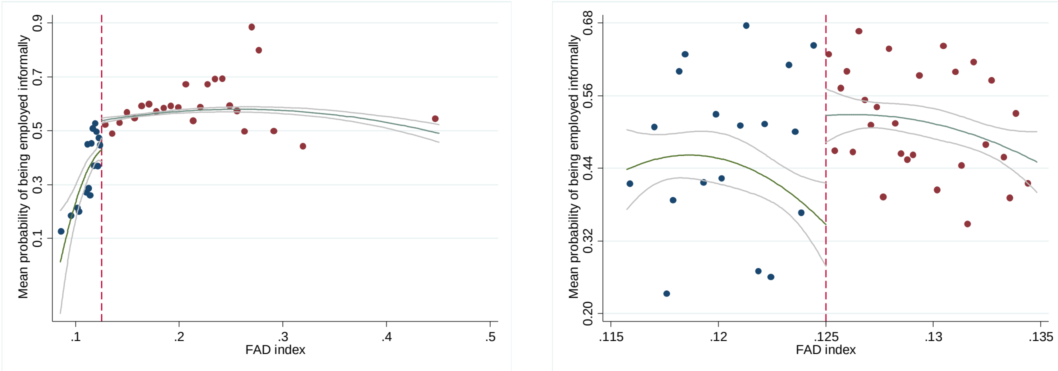
Panel B. Probability of being inactive (overall window and smaller bandwidth)



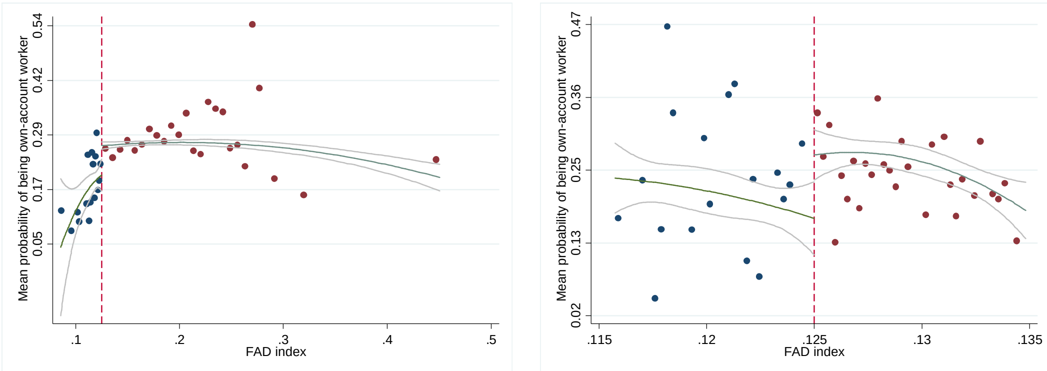
Panel C. Probability of being employed formally (overall window and smaller bandwidth)



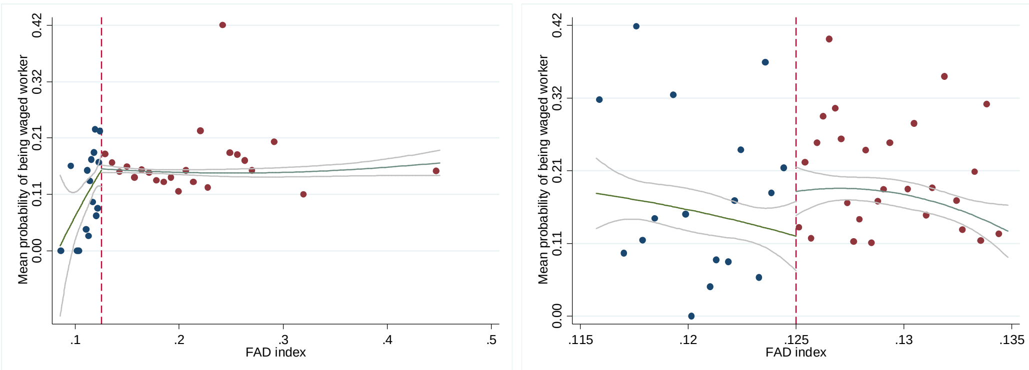
Panel D. Probability of being employed informally (overall window and smaller bandwidth)



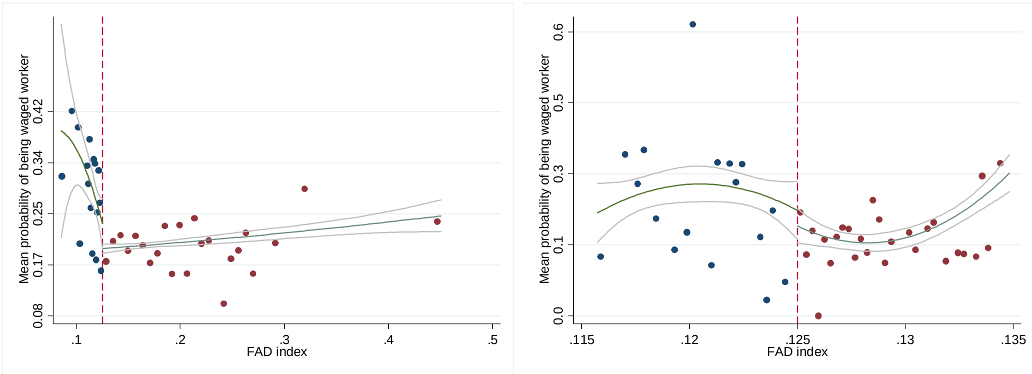
Panel E. Probability of being own-account worker (overall window and smaller bandwidth)



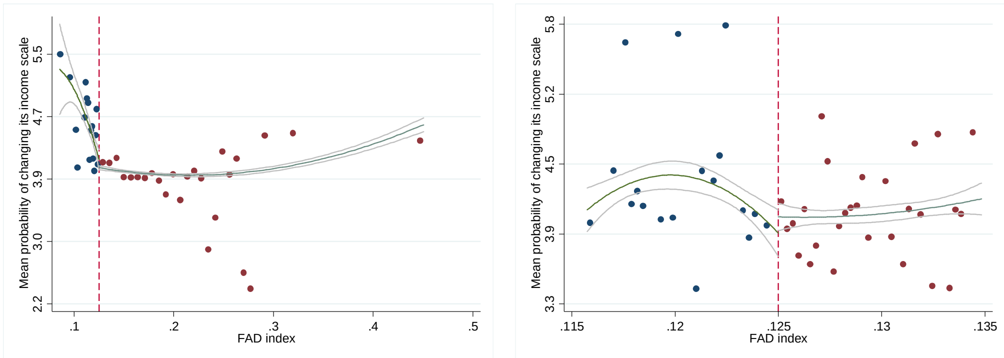
Panel F. Probability of being waged worker (overall window and smaller bandwidth)



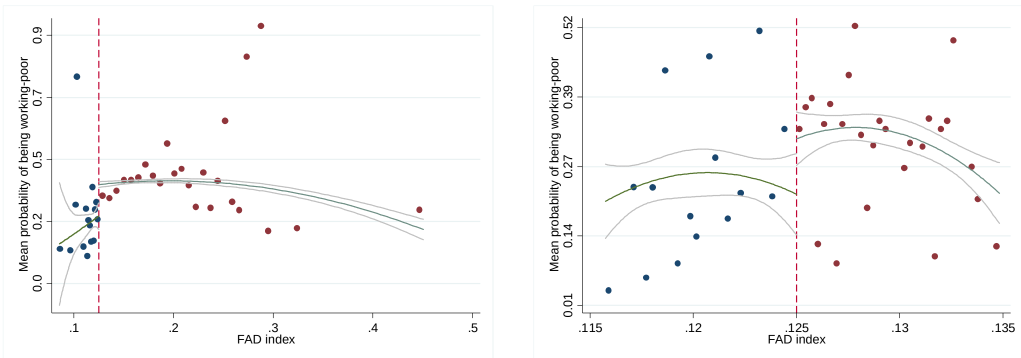
Panel G. Probability of being waged employee (overall window and smaller bandwidth)



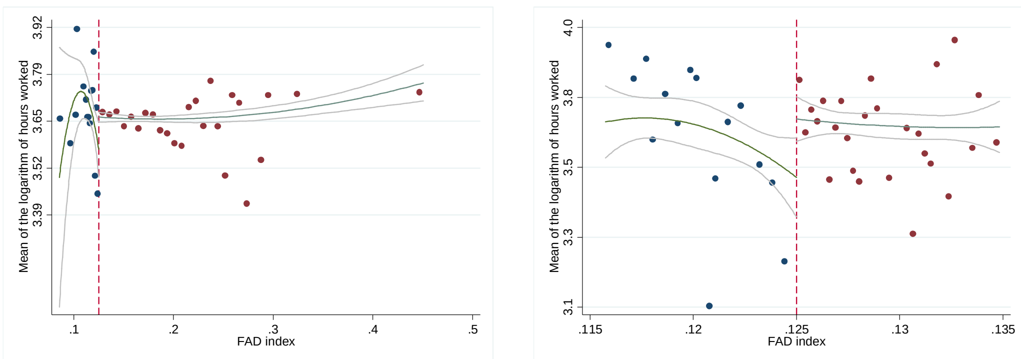
Panel H. Probability of a jump in their monthly income scale (overall window and smaller bandwidth)*



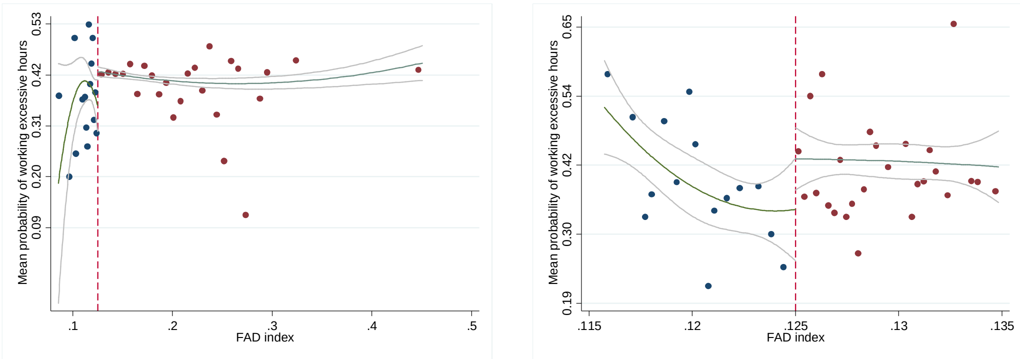
Panel I. Probability of being working-poor (overall window and smaller bandwidth)



Panel J. Total hours worked per week (overall window and smaller bandwidth)



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



* Monthly income scales of participants are only available for 2011 in the special survey. As such, the effect of the programme on the probability of jumping income scales has also been estimated for 2011.

Note: Fig. 4 plots the mean probability of having a certain employment status, income 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. The fit used was suggested by the graphical analysis carried out using the lowess fit. The analysis includes all urban districts.