Labour Market Effects of Ethiopia's Social Safety Net

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Abstract

This paper assesses the effects of a large social protection programme in rural Ethiopia on local labour markets. The programme targeted food-insecure households to provide them with food or cash transfers, as compensation for public works participation or unconditionally. Using repeated cross-sections of the Ethiopian Labour Force Survey, I show that workers shifted from agricultural to non-agricultural self-employment. I also find that the programme did not change employment rates or wages in this rural economy. I find similar results complementing my analysis with data from the Ethiopian Socio-Economic Survey. My findings contrast with previous work on the labour market effects of social protection programmes due to the thinness of rural wage markets in Ethiopia.

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1 Equilibrium effects of social protection programmes

Policies aiming to increase the welfare of individuals living in poverty can also affect non-participants by shifting the labour market equilibrium (Bandiera et al., 2017; Mobarak and Rosenzweig, 2014; Bryan et al., 2014; Egger et al., 2022; Imbert and Papp, 2015; Muralidharan et al., 2023). For example, programmes offering skill and asset transfers, rainfall insurance, resettlement, cash transfers, or guaranteed employment schemes may affect labour decisions and wages of individuals not directly targeted by the interventions. A common theme across these studies is that landless workers bear most of the general equilibrium effects of these interventions. However, there is limited evidence on the labour market effects of transfer programmes in markets where the agricultural workforce comprises mostly small landowners and few landless workers. In markets with relatively few landless workers, the general equilibrium effects of these programmes may be diminished.

This paper examines how a large social protection programme can affect non-beneficiaries through changes in local labour markets. I focus on Ethiopia's Productive Safety Net Programme (PSNP), which provides cash or food transfers conditional on public works participation to over ten million beneficiaries annually. It is one of the largest rural social protection systems in Africa reaching almost 10% of the population (Gilligan et al., 2009). Its impressive scale has contributed to making it a frequently used reference in international comparisons of similar programmes in policy circles.[1] As such, rigorous evaluations of this programme can provide insights that are of significance both within and outside the Ethiopian context. By analysing the district-level exposure to the programme, I aim to provide a first assessment of how this programme affects labour markets, moving beyond the individual-level effects that have so far been the focus of previous evaluations (Subbarao et al., 2013).

To identify the main effects of the programme on local labour markets, I estimate a difference-in-differences model. I investigate whether the programme has affected employment participation, occupational categories, hours worked and wages in the targeted districts, relative to those that did not receive the programme. I use a unique geo-referenced dataset combining three cross-sections of the National Labour Force Survey, observing over 400,000 individuals in all regions of Ethiopia

[1] See, for example, Alderman and Yemtsov (2012), Grosh et al. (2008), McCord (2013), and Subbarao et al. (2013).

and spanning from 1999 to 2013.[2] I complement this main source of information with other geo-referenced datasets: village-level census data, climatic variables and the district-level historical frequency of aid receipts. To disentangle the effects within treated districts, I also employ the Ethiopian Socio-Economic Surveys (ESS), which include household and community data on PSNP participation.[3]

I have two main results on how the programme affects labour market outcomes. First, I find a reallocation of the workforce towards non-agricultural self-employment (5 percentage point increase) in targeted districts.[4] The reallocation towards non-agricultural self-employment is driven by women in my sample. To unpack this result, I descriptively compare the labour market outcomes of non-beneficiaries in targeted districts with individuals in untargeted communities within those districts. Using the ESS, I describe that non-beneficiaries in untargeted communities in PSNP districts experienced a larger shift away from agriculture towards other forms of self-employment. Second, I find no impact on the extensive and intensive margins of labour supply or wages in rural districts targeted by the programme. These results holds across several robustness checks. Conducting a placebo test with pre-programme data shows parallel trends in outcomes between targeted and untargeted districts prior to the program's start, supporting the validity of the findings. Moreover, including additional demographic controls does not alter the results. Overall, these results are consistent with the idea that the PSNP stimulated demand for local goods and market access.

This paper contributes to the literature on the impacts of public works programmes on rural economies.[5] My study is most closely related to the work of Imbert and Papp (2015), who also use a difference-in-differences model to estimate the effect of India's Mahatma Gandhi National Rural Employment Guarantee Act (NREGA) on wages and employment.[6] More recently, Muralidharan et al. (2023) document the substantial general equilibrium effects of NREGA. In contrast with the evidence from India, my findings suggest that wages of private sector labourers do not

[2] Since I only focus on two periods for my main analysis, my identification strategy is not affected by the potential biases of staggered difference-in-differences models with more periods (de Chaisemartin and D'Haultfoeuille, 2022).

[3] The ESS allows me to identify the communities targeted by the programme and combine it with district-level data, but it covers fewer districts and was not collected before the programme started.

[4] Throughout the article I interchangeably refer to districts as *woredas*.

[5] See Bagga et al. (2023) for a review on the effects of workfare programmes. Besley and Coate (1992), Ravallion (1991), and Basu (2013) also provide theoretical treatments of workfare programmes.

[6] Other recent examples of papers estimating the labour market impacts of NREGA are Berg et al. (2018), Zimmermann (2020), Fetzer (2020) and Santangelo (2019).

seem to respond significantly to the presence of public works programmes. The difference is likely due to factors such as programme design or structural differences in the labour markets analysed. Importantly, unlike NREGA, the PSNP transfers were set below the prevailing market wage. This decision was made in order to minimise the risk of creating a disincentive for participation in other productive activities. Wages in the Indian employment guarantee scheme are generally above the private sector wage for casual labourers (Subbarao et al., 2013).

This paper also contributes to our understanding of the broader impacts of the PSNP. Previous studies on the PSNP focused on estimating the impact of the programme only on individual beneficiaries, collecting information exclusively in targeted districts (Berhane et al., 2011, 2014; Gilligan et al., 2009, 2011; Hoddinott et al., 2011, 2012). But, as McCord and Slater (2013) point out, enlarging the unit of analysis beyond the beneficiary-level is of particular relevance to a programme like the PNSP, which aims to benefit the whole community. For example, one of the aims of the programme is to increase resilience and agricultural productivity within the whole community targeted so as to stimulate production and local market activities of food and non-food products (World Bank, 2014). Studies that explore effects beyond the individual-level, to analyse the district-level impacts of the programme, remain scant. Filipski et al. (2016) documented that "new income created by PSNP benefits households that do not receive cash transfers; these non-beneficiaries benefit [...] through local and national markets", which is consistent with how I interpret my results. In contrast to earlier work, this paper unpacks some of those general-equilibrium patterns to further document the potential micro-level spillovers of the programme, focusing on labour markets.

Two recent studies on the wider effects of the PSNP are complementary to my analysis. First, Gazeaud and Stephane (2023) find little evidence on the effectiveness of the PSNP public works in improving the agricultural productivity in districts targeted by the PSNP. Abebe et al. (2021) study the effects of Ethiopia's Urban Productive Safety Net Program, which provides employment on local public works to the urban poor, and was rolled out randomly across neighbourhoods of Addis Ababa. They find that the programme increased public employment, reduced private labour supply among beneficiaries, and increased overall private wages by 18%. My work complements both of these studies since I focus on the *rural* PSNP, rather than its *urban* counterpart, and because I focus on the labour market and household-level decisions, rather than aggregate yields data.

In a broader sense, this paper contributes to the literature studying the functioning of rural labour markets in low- and middle-income countries (Behrman, 1999). It aims to enhance our understanding of how labour markets in low- and middle-income countries respond to in-kind and cash transfer programmes. Recent papers have shown that households change their labour supply decisions in response to the provision of different in-kind assets, such as land-titles, better housing conditions, roads, electrification, and agricultural inputs (Field, 2007; Dinkelman, 2011; Franklin, 2020; Asher and Novosad, 2020; Moneke, 2020; Diop, 2023). Aid or cash transfers have been found to have null or positive effects on the labour supply of recipients.[7]

This paper is organised as follows. Section 2 provides details on the targeting of the programme, the main dataset and outcome variables analysed, presents summary statistics, and outlines the main empirical strategy. Section 3 presents and discusses the main results, along with several robustness checks. Section 4 concludes.

2 Background, data, and empirical strategy

This section briefly describes how Productive Safety Net Programme (PSNP) participants are targeted, the data employed, the main empirical strategy, and descriptive statistics. My primary analysis leverages repeated cross-sections of the Ethiopian National Labour Force Survey (LFS). I match district identifiers across rounds of the LFS to construct my main outcome and control variables. Further, I combine additional data sources to construct district-level covariates on: (i) the geographical assignment of the programme, (ii) the frequency of relief assistance received prior to the PSNP, (iii) rainfall, (iv) temperature, and (v) population density. In a secondary complementary analysis, I employ a panel dataset from the Ethiopian Socio-Economic Surveys (ESS). Appendix B provides more institutional details about the programme. Appendix C further describes all data sources and the strategy used to match districts across waves of the LFS.

[7] For example, Egger et al. (2022) find that an unconditional cash transfer programme in Western Kenya that injected about 15% of GDP had a positive effect on labour demand. Banerjee et al. (2017) find no evidence of disincentive effects among transfer recipients by combining datasets from seven randomised controlled trials from different countries. In Ethiopia previous food aid programmes were found not to disincentive work among recipients (Abdulai et al., 2005; Quisumbing and Yohannes, 2005).

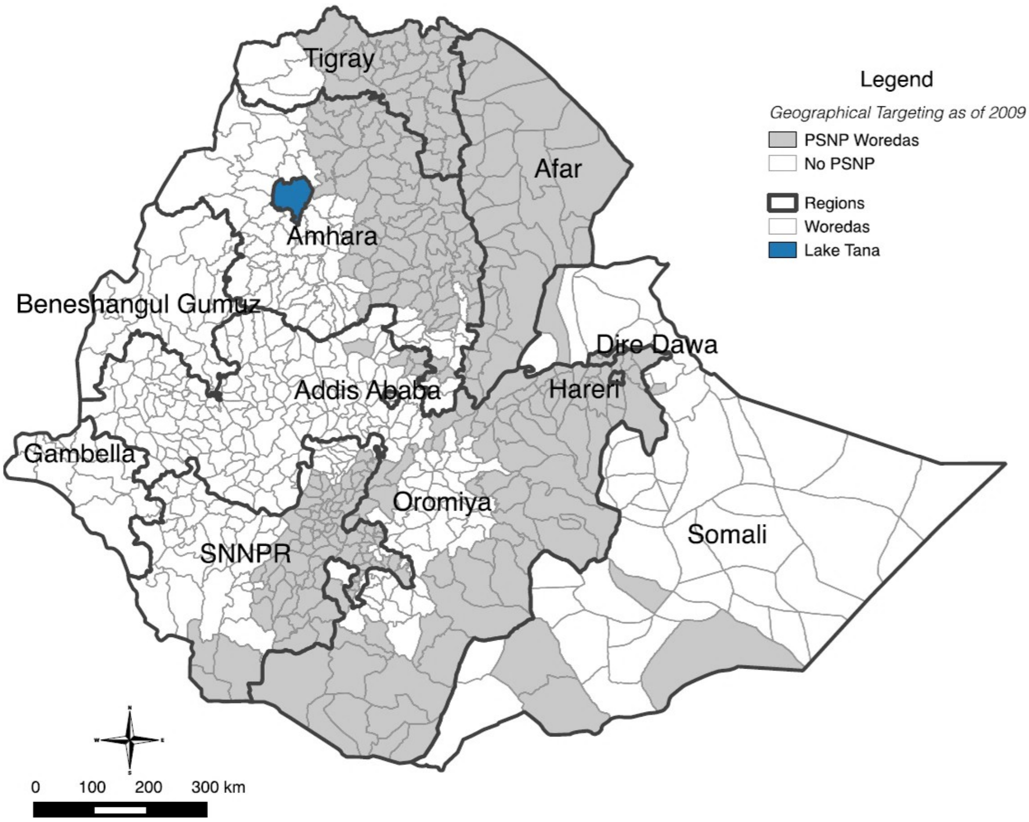

Figure 1: Productive Safety Net geographic targeting[a]

[a] *Notes:* PSNP assignment of 290 districts (*woredas*), as of the end of PSNP Phase II (2006-2009).

2.1 PSNP targeting

The PSNP targets districts based on the historical allocation of food aid prior to 2005.[8] Within targeted districts, local officials and community leaders select beneficiaries by constructing lists of eligible households for each community within the district. The main eligible beneficiaries of the programme are chronically food insecure households. Targeted beneficiaries participate in public works in exchange for cash or food transfers, while some receive unconditional support based on their circumstances. In 2009, cash or food transfers conditional on public works participation comprised 84% of the total transfer to beneficiaries (World Bank, 2010b).

The PSNP was initially introduced in 192 rural districts (*woredas*) in 2005 and expanded to 290 districts by the end of 2009, forming the treated sample for my analysis. Figure 1 displays the geographical distribution of these targeted districts, sourced from the programme reports (World Bank, 2010a).

2.2 Data and main outcomes

For the main analysis, I combine three nationally representative cross-sections of the Labour Force Surveys, conducted in 1999, 2005, and 2013. I restrict my sample to individuals living in rural regions, since the PSNP only targeted rural areas in this period.[9] Figure 2 provides a timeline of the programme phases during the years I analyse. The primary sample analysed consists of individual-level observations from a balanced panel of 453 rural districts from the 2005 and 2013 rounds, illustrated in Appendix Figure 3.

The main outcomes are measures of employment, on the intensive and extensive margins, wages, and occupational categories. First, I categorise individuals aged 17 to 65 as currently employed, unemployed, or inactive.[10] Currently employed individuals are those who reported engaging in productive activities for at least one hour in the week before the interview, following the ILO definition (Hussmanns, 2007). For unemployment, I consider individuals as unemployed if they are not

[8] Specifically, the 2006 Project Implementation Manual states that a *woreda* was eligible for the programme if it was: '[i] in one of 8 regions (Tigray, Amhara, Oromiya, SNNPR, Afar, Somali, rural Harari and Dire Dawa), and [ii] has been a recipient of food aid for a significant period, generally for at least each of the last three years' (GFDRE, 2006, pp.3). The same criterion is reiterated in the 2010 revised version of the PIM, which also adds that in 2004 eligibility was defined more broadly, but was later revised. The previous broader eligibility criteria would have deemed *woredas* eligible based on 'the frequency with which they required food assistance in the ten years preceding the design of the PSNP (the ten years up to 2004)' (GFDRE, 2010, pp.7). It is not clear how many years were deemed enough in the broader criterion, and to what extent the revised one was followed.

[9] The CSA defines as non-rural all (enumeration) areas with a population of more 1000 individuals, and any administrative capitals (regional, zonal or district capitals) regardless of population. More information on the survey design is available on `http://tinyurl.com/csa-nlfs2013`, visited on the 14/04/2016.

[10] The age cutoffs were chosen based on the Programme Implementation Manual specification that individuals below 17 years of age should not participate in public works, which is in line with findings from the recent programme evaluation Berhane et al. (2011). The manual also specifies that elderly should not participate in the programme, without specifying an age. Thus, the upper age cutoff is chosen so as to follow previous studies of the labour supply responses of food aid programmes in Ethiopia (e.g. Abdulai et al. (2005) and Quisumbing and Yohannes (2005)).

PSNP Phases	Phase I	Phase II				Phase III			
PSNP geographic expansion	PSNP launched in 192 woredas	262 PSNP woredas, as district split	290 PSNP woredas, as Afar added			319 PSNP woredas, as Somali added			
Year	**2005**	**2006**	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**	**2013**
Main data sources	Labour Force Survey 2005 Round		2007 Census						Labour Force Survey 2013 Round

Figure 2: Timeline of the PSNP and data sources[a]

[a] *Notes:* The Productive Safety Net Programme (PSNP) was launched in 2005, in a testing phase in 192 districts (*woredas*). In the second phase (2006-2009), the number of districts targeted reached 290, which I refer as the main sample of treated districts. In 2010, 49 districts from the Somali region were added to the programme.

currently employed but are available for work and willing to take up a job opportunity, even if they have not actively searched for work in the last three months, as in Franklin (2014) and Broussar and Tekleselassie (2012). Second, I construct individual indicators for those employed, such as hours worked in the last seven days, engagement in additional working activities, and willingness to work more hours. Third, I also create a measure of real monthly wages for manual labourers, using regional deflators from Headey et al. (2012). Fourth, I create main occupation categories grouping the International Organization for Standardization (ISO) codes available in the LFS.[11] These occupational categories allow me to estimate transitions related to the primary source of livelihood, but they do not fully capture the diverse range of activities that individuals in rural Ethiopia may be engaged in beyond their main employment activity (Dercon and Krishnan, 1996).

2.3 Summary statistics

Table 1 shows the means of the main covariates used in the analysis for both PSNP districts (Column 1) and non-targeted districts (Column 2). Column 3 displays the p-value from a t-test of equality of means between the two groups.

Panel A shows some differences in labour market conditions between PSNP and

[11] The ISO codes can be found on `http://tinyurl.com/csa-isco08`, accessed on 09/05/2016.

non-PSNP districts in the baseline year of comparison (2005). PSNP districts show lower fractions of workers engaged in agriculture and seasonally unemployed individuals, but higher fractions of manual labourers and public sector workers. Observable demographic characteristics are balanced between the two groups of districts. Measures of human capital and household demographics show no significant differences. The frequency of relief assistance prior to 2005 is higher in PSNP districts, as expected, since this variable was used in targeting the programme at the district-level. Weather conditions also differ, with PSNP districts experiencing less rainfall on average.

Table 2 shows that despite differences in control variables, the outcomes of interest are balanced between districts targeted by the PSNP and other districts in 2005. Most individuals (around 82%) are employed, with the majority being self-employed in agriculture (crop, livestock, mixed-farming, or forestry). Of those employed, 10-13% are in self-employment outside of agriculture, usually working in trade or crafts work.[12] Public and private sector labourers constitute a relatively small category of employment. Labourers undertake relatively low-skill tasks usually in agriculture or construction work. Public labourers may include PSNP participants, as well as labourers in other publicly funded projects. The additional outcome variables related to the intensive margin of labour supply are also balanced between the two groups of districts.

2.4 Empirical strategy

My main identification strategy compares changes over time in targeted districts with changes in other districts. To improve identification, I include as controls the variables in Panel A of Table 1 to account for differential dynamics across districts. These controls encompass the frequency of aid receipts and district-level labour market conditions, which are interacted with a time varying indicator, and time-varying district controls related to rainfall and temperature.[13] As an additional specification, I also include individual-specific controls to improve efficiency, though since treatment is a the district-level those controls are not needed for iden-

[12] This occupation is more common among women, with 22% of working women engaged in non-agricultural activities, while the proportion of working men in this category is 6%.

[13] The district-level controls of labour market conditions are obtained averaging the individual-level observations from the Labour Force Survey at the round-specific district-level. Temperature, rainfall, and frequency of aid between 1995 and 2004 are only observed at the district-level. More details on these variables are in Appendix Section C.

Table 1: Summary statistics — Mean balance of district controls in 2005

	PSNP (1)	Control (2)	p-value (3)	Source (4)	Time-Varying? (5)
Panel A: District-level controls					
Female labour force participation rate	0.77	0.78	0.491	2005 LFS	No
Male labour force participation rate	0.92	0.92	0.964	2005 LFS	No
Literacy rate	0.27	0.27	0.820	2005 LFS	No
Fraction in-migrants	0.04	0.04	0.482	2005 LFS	No
Fraction disabled	0.02	0.03	0.348	2005 LFS	No
Fraction female headed household	0.16	0.16	0.561	2005 LFS	No
Fraction working in agriculture	0.73	0.77	0.018	2005 LFS	No
Fraction of workers seasonally not at work	0.02	0.03	0.001	2005 LFS	No
Fraction public employees	0.03	0.01	0.003	2005 LFS	No
Fraction private employees	0.02	0.03	0.483	2005 LFS	No
Fraction labourers	0.03	0.01	0.057	2005 LFS	No
Cumulative Belg season rainfall (standardized)	0.11	0.46	0.000	GPCC	Yes
Cumulative Meher season rainfall (standardized)	-0.44	-0.16	0.000	GPCC	Yes
Average Belg season temperature (standardized)	0.26	0.38	0.011	UDel_AirT	Yes
Average Meher season temperature (standardized)	0.07	-0.04	0.001	UDel_AirT	Yes
Years of emergency assistance (1995-2004)	7.68	1.69	0.000	NDRMC	No
Panel B: Individual-level controls					
Age	34	33	0.569	2005 LFS	Yes
Fraction female	0.52	0.53	0.941	2005 LFS	Yes
Fraction with some schooling	0.15	0.15	0.947	2005 LFS	Yes
Fraction with primary schooling	0.03	0.03	0.893	2005 LFS	Yes
Fraction with some secondary schooling	0.06	0.07	0.973	2005 LFS	Yes
Fraction with secondary schooling or more	0.01	0.02	0.821	2005 LFS	Yes
Fraction married	0.72	0.72	0.969	2005 LFS	Yes
Fraction of households with no children below age 5	0.02	0.03	0.923	2005 LFS	Yes
Fraction of households with elderly above age 70	0.05	0.05	0.936	2005 LFS	Yes
Fraction of households with individuals with a disability	0.09	0.11	0.664	2005 LFS	Yes
Fraction of household heads	0.44	0.44	0.964	2005 LFS	Yes
Fraction of female household heads	0.16	0.16	0.928	2005 LFS	Yes
Fraction of household heads with primary education, or more	0.10	0.11	0.735	2005 LFS	Yes
Fraction of household heads with some schooling, below primary	0.19	0.19	0.932	2005 LFS	Yes
District Observations	215	238			
Individual Observations	31574	26805			

Notes: Panel A presents means of the district-level controls used in the main regression model for different samples. Column 1 includes controls for districts that were targeted by the PSNP. Column 2 includes district controls for districts that were not targeted by the PSNP (which form the control group). Column 3 presents the *p*-values of the student's *t* *t*-test of equality of means. Standard errors for the student's *t*-test of equality of means are computed assuming correlation of individual observations within each district in a given year. The LFS controls are computed using the 2005 Labour Force Survey round, with sampling weights adjusted for boundary changes. The sample is restricted to individuals of ages between 17-65, using information from the usual activity reported. Cumulative rainfall is expressed as the standardized deviation from the 1979-2014 mean cumulative rainfall during the rain seasons for the *Meher* harvest (June-October) and Belg harvest season (February-May). Temperature is calculated as the standardized deviation from the 1979-2014 monthly averages for the respective pre-harvest rainy season. Years of assistance refers to the frequency in years between 1994-2004, of emergency assistance received by district.

Panel B presents means of the individual-level means. Apart from age, all controls are indicator variables. The omitted category is a male individual with no schooling, unmarried, who is not a household head, and living in a male-headed household, where the household head has no schooling, there are children aged below 5, and no member of the household is above 70 years of age, or has a disability

Table 2: Summary statistics — Mean balance of district controls in 2005

	PSNP (1)	Control (2)	p-value (3)
Main Outcome Variables			
Employed (%)	81.8	83.1	0.731
Self-employed in ag. (%)	81.8	86.4	0.185
Self-employed not in ag. (%)	13.1	10.2	0.338
Public sector labourers (%)	1.0	0.1	0.175
Private sector labourers (%)	0.9	1.2	0.766
Unemployed (%)	1.6	1.8	0.852
Inactive (%)	16.6	15.1	0.671
Additional Outcome Variables			
Total hours worked in main occupation in the last 7 days	27.4	26.6	0.619
Underemployed (%)	30.0	28.2	0.676
Has more than one productive activity (%)	22.3	18.9	0.386
Total hours worked in the last 7 days	30.1	28.5	0.342
Private sector labourers' monthly real wage	350.0	347.4	0.950
In-migrants (%)	5.6	7.6	0.403
Household size	5	5	0.700
District observations	215	238	
Individual observations	31574	26805	

Notes: This table presents means of the outcome variables for different samples. All samples are restricted to persons aged 17 to 65. Column 1 only includes districts that were targeted by the PSNP. Column 2 only includes districts that were not targeted by the PSNP (which form the control group). Column 3 presents the p-values of the student's t-test of equality of means in columns 1 and 2. Standard errors for the student's t-test are computed assuming correlation of individual observations within each district.

tification. District-level averages of the individual-level controls are presented in Panel B of Table 1.[14]

For my main analysis, I estimate a difference-in-differences specification across two periods, with the 2005 wave being the pre-treatment period and the 2013 wave being the post-treatment period. Since I only focus on two periods, I am not concerned with potential biases of staggered difference-in-differences models with more periods (de Chaisemartin and D'Haultfoeuille, 2022). Hence, I estimate the following linear specification:

[14] Individual-level controls comprise: age; indicators for whether the individual is female, their level of education (omitting no schooling), married, or the household head; indicators for whether the individual's household has someone aged five years of age or below, someone aged 70 or above, someone with a disability, a female household head, or a household head with any schooling.

$$
\begin{aligned}
Y_{idt} =& \beta \times (\mathbb{1}_{(PSNP=1)} \times \mathbb{1}_{(t=2013)}) + (\mathbf{C}_d \times \mathbb{1}_{(t=2013)})'\delta + \\
\mathbf{X}'_{dt}\theta + & \mathbf{H}'_i\zeta + \eta_d + \gamma \times \mathbb{1}_{(t=2013)} + \epsilon_{1,idt}
\end{aligned} \tag{1}
$$

where Y_{idt} is the outcome for individual i in district d in year t. $\mathbb{1}_{(PSNP)}$ is an indicator equal to one if the district is targeted by the PSNP. $\mathbf{C}_d$ and $\mathbf{X}_{dt}$ are vectors of time-invariant and time-varying district controls, respectively. The indicator $\mathbb{1}_{(t=2013)}$ is equal to one for the year 2013 (the first LFS round after the start of the programme), which accounts for any aggregate-level factors affecting all districts in that specific year. $\mathbf{H}_i$ is a vector of individual controls. η_d is a district-specific fixed effect capturing unobserved characteristics of districts that do not change over time. The unobserved idiosyncratic component is denoted by $\epsilon_{1,idt}$. The coefficient β quantifies the effect of the PSNP. I present estimates of β with and without the individual-level controls. I cluster standard errors at the district level, following Bertrand et al. (2004).

To interpret the estimates at the district-level, I adjust individual observations using sampling weights to account for potential under-representation of larger districts in the data, following Imbert and Papp (2015).[15]

3 Results

This section presents the estimated effect of the PSNP on the labour market outcomes of individuals living in districts that were targeted by the programme. I find that the programme shifted self-employed individuals from agricultural to non-agricultural occupations. However, I do not find a significant impact on wages or employment.

[15] These weights, provided by the Central Statistical Authority (CSA), reflect the inverse probability of being sampled. The weighting strategy ensures that the sum of all weights within a district-year is constant over time for each district and proportional to the sampling weight of the rural population within that district. Additionally, I present unweighted estimates for robustness, but the results remain unaffected by the weighting strategy.

3.1 Labour supply (extensive margin) and sectoral occupation

Table 3 presents the main results on the effect of the PSNP on employment and occupational categories. The coefficients are presented as percentage changes in the fraction of workers in each category.[16] I find no significant impact of the PSNP on labour market participation in targeted districts between 2005 and 2013. Adding individual-level controls reduces standard errors but does not change the overall results (Panel B). Finding no effect on the extensive margin of labour supply is unlikely to be spurious given the size of the program. The 90% confidence interval for the PSNP's effect on employment rate ranges from -4.3 to 3.2 percentage points, indicating little to no impact.

In the last four columns of Table 3, the results show changes in the composition of the labour force in PSNP-targeted districts. Specifically, there is a considerable increase of around 5 percentage points (p-value: 0.01) in the share of workers engaged in non-agricultural self-employment, from a baseline of 13 percentage points in the control group. Moreover, I also find a 0.29 percentage points increase in public sector labourers in targeted districts, statistically significant at the 10% level, consistent with a potential increase in PSNP participants engaged in public works.

To better understand the sectoral shifts, Appendix Table 8 reports the estimates for men and women, separately. This analysis shows that the increase in the share of workers engaging in non-agricultural activities is driven by women. All estimated coefficients for men are not statistically significant, except for a potential increase in unemployment rates, which is only significant at the 10% level (Panel B).

3.2 Demographic structure and labour supply (intensive margin)

Table 4 shows that the impact of the PSNP on the demographic composition and the intensive margin of labour supply is minimal and not statistically significant. The programme's effects on household in-migration rates and household size are small in magnitude and never close to statistical significance (Columns 1-3).[17] This finding implies that changes in the targeted districts' demographics are not likely

[16] To improve the readability of the tables, the indicator variables are multiplied by 100.

[17] Household size is the integer number of household members. The second and third column report indicator variables (multiplied by 100) on whether the household has had at least one in-migrant in the last five or ten years.

Table 3: Effects on employment participation and sectoral composition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. No individual controls							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
	-0.575	0.978	-0.403	-6.359**	5.471**	0.018	0.292*
	(2.276)	(0.659)	(2.061)	(2.617)	(2.149)	(0.433)	(0.167)
Mean Dep. Var.	83.18	1.7	15.12	84.25	11.54	1.33	0.49
Observations	105,323	105,323	105,323	86,779	86,779	86,779	86,779
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	No	No	No	No	No	No
Panel B. Individual controls added							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
	-0.16	0.936	-0.776	-5.826**	5.286**	-0.008	0.310*
	(2.277)	(0.655)	(2.066)	(2.427)	(2.122)	(0.434)	(0.168)
Mean Dep. Var.	83.18	1.7	15.12	84.25	11.54	1.33	0.49
Observations	105,323	105,323	105,323	86,779	86,779	86,779	86,779
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each cell reports an estimate of β for different dependent variables; standard errors in parenthesis are clustered at the district level. Each column has a different dependent variable. In Panel A, each model includes district fixed effects and district controls. In Panel B, each model includes district fixed effects, district controls and individual controls. The sample consists of individuals aged 17-65, pooling data from the 2005 and 2013 LFS rounds. Columns (4)-(7) restrict the sample only to those that are currently employed. Individual observations are weighted by sampling weights that are proportional to district population. All models are estimated using ordinary least squares. The means of district-level and individual-level controls are shown in Table 1. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

to have confounded any labour market effects of the program.

When focusing on the currently employed individuals (Columns 4-7), I find no significant effect on different measures of the intensive margin of labour supply. I find no change in underemployment, changes in working hours during the lean agricultural season (at the time the survey took place), or engagement in more than one form of employment. The estimated effects are small in magnitude (below 10% of the untreated district mean) and not statistically significant. Overall, these findings do not provide evidence that the programme crowded out alternative forms of employment at the district-level.

3.3 Effects on the wage of private sector labourers

In Table 5, I focus on the wages of private sector labourers as they could theoretically be most influenced by the programme's general equilibrium effects. The coefficient in column 1 indicates a 31% reduction in wages in PSNP districts compared to control districts, but this effect is not statistically significant at the 10% level.[18]

It is challenging to determine if the observed private labourers' wages are representative of rural wages in Ethiopia, as wage employment may be more prevalent than official statistics suggest (Rizzo, 2011). The wage data is only observed for 1% of the sample, making it indicative rather than representative of rural markets in Ethiopia, as I do not observe wages for those self-employed. Due to potential selection bias and a small sample size, these results are illustrative, and no causal relationship is claimed. With these caveats in mind, my analysis finds no significant changes in private sector labourers' wages. To partly address the potential selection bias within the labourers' sub-sample, I show estimates of the programme's effect on various outcomes in columns 2 to 8 for this sub-sample. The sub-sample of labourers does not seem to be significantly affected by the programme across different measures of employment, although the effect sizes differ from the rest of the sample.

[18] I use Kennedy (1981)'s transformation and interpret the estimated percentage effect on continuous variable measured in logs when a district switches from control to treatment as $100 \times [\exp(\hat{\beta} - 0.5 \times \hat{V}(\hat{\beta})) - 1]$, assuming normality of the errors.

Table 4: Effects on demographic composition and intensive margin of labour supply and unemployment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. No individual controls							
dependent variable:	Household Size	In-migrant (last 5 years)	In-migrant (last 10 years)	Underemployment	Has more than one activity	Hours worked in main activity	Hours worked in all activities
	0.015	-0.030	0.927	-4.004	-0.618	-0.328	-0.633
	(0.125)	(0.872)	(1.276)	(3.380)	(2.921)	(0.965)	(0.952)
Mean Dep. Var.	5.232	3.771	6.518	37.04	27.05	30.98	39.79
Observations	105,323	105,323	105,323	86,779	86,779	86,779	86,779
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	No	No	No	No	No	No
Panel B. Individual controls added							
dependent variable:	Household Size	In-migrant (last 5 years)	In-migrant (last 10 years)	Underemployment	Has more than one activity	Hours worked in main activity	Hours worked in all activities
	0.012	-0.348	0.639	-3.881	-0.574	-0.242	-0.560
	(0.095)	(0.800)	(1.194)	(3.375)	(2.908)	(0.962)	(0.941)
Mean Dep. Var.	5.232	3.771	6.518	37.04	27.05	30.98	39.79
Observations	105,323	105,323	105,323	159,902	159,902	159,902	116,321
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each cell reports an estimate of β; standard errors in parenthesis are clustered at the district level. Each column reports an estimate for a different dependent variable. Household size indicates the number of individuals normally residing in an household. In-migrant is an indicator variable equal to one if the individual has migrated into the district in the last 5 years (Column 2), or the last 10 years (Column 3). Columns (4)-(7) are conditional on being employed: the dependent variable in column (5) is an indicator variable equal to one if the individual has reported willingness to work more hours. The dependent variable in column (6) is a dependent variable equal to one if the individual has engaged in more than productive activity in the last seven days. The dependent variable in column (6) and (7) are in levels. In Panel A, each model includes district fixed effects and district controls. In Panel B, each model includes district fixed effects, district controls, and individual controls. The sample consists of individuals aged 17-65, pooling data from the 2005 and 2013 LFS rounds, sampled in 453 districts in each round. Individual observations are weighted by sampling weights that are proportional to district population. All models are estimated using ordinary least squares. The means of district-level and individual-level controls is shown in Table 1.* denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 5: Effects on private sector wage labourers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
dependent variable:	(log) Real monthly wage	Household Size	In-migrant (last 5 years)	In-migrant (last 10 years)	Underemployment	Has more than one activity	Hours worked in main activity	Hours worked in all activities
	-0.289	0.571	-19.526	-3.294	-14.809	-19.797	-1.173	-4.148
	(0.416)	(0.744)	(12.030)	(15.879)	(18.944)	(17.516)	(7.244)	(6.453)
Mean Dep. Var.	5.447	5.390	19.29	25.04	41.40	29.10	39.79	42.97
Observations	932	932	932	932	932	932	932	932
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each cell reports an estimate of β; standard errors in parenthesis are clustered at the district level. Each column reports an estimate for a different dependent variable. (log) Real monthly wage is computed is deflated to 2011 real prices using CSA regional deflators. Household size indicates the number of individuals residing in an household. In-migrant is an indicator variable equal to one if the individual has migrated into the district in the last 5 years (Column 3), or the last 10 years (Column 4). Columns The dependent variable in column (5) is an indicator variable equal to one if the individual has reported willingness to work more hours. The dependent variable in column (6) is a dependent variable equal to one if the individual has engaged in more than productive activity in the last seven days. The dependent variable in column (7) and (8) are in levels. The sample is restricted to private sector labourers aged 17-65, pooling data from the 2005 and 2013 LFS rounds, sampled in 453 districts in each round. There are only 81 districts where private sector labourer's are observed in both rounds. Individual observations are weighted by sampling weights that are proportional to district population. The means of district-level and individual-level controls is shown in Table 1.* denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

3.4 Discussion

There are two take-aways from my analysis. First, I find that a considerable proportion of workers, especially women, tend to transition from agriculture to non-agricultural self-employment activities as a result of the programme. Second, there is no significant effect of the programme on the local labour supply, considering both intensive and extensive margin.

I speculate that the observed shift towards non-agricultural self-employment in PSNP districts could be attributed to improved market access facilitated by the community assets built by the public works component of the programme, such as rural roads. This explanation aligns with previous research highlighting the importance of rural roads on labour market participation in rural areas (Dercon et al., 2009; Asher and Novosad, 2020). Based on this interpretation, the effect of the programme does not seem to distort the labour market or crowd out other employment opportunities but instead stem from the positive externalities generated by the assets created through the public works component.

There are at least two possible explanations for the lack of labour supply response. First, the programme's design aims to support food-insecure households to become self-sufficient rather than creating new job opportunities. The capped days of employment for participants ensure time for other potential productive activities without replacing existing livelihood sources. Second, the labour supply in rural areas may be almost perfectly inelastic, in line with the experimental findings of Goldberg (2016) in Malawi. This interpretation is in stark contrast to the anecdotal assumption that this elasticity can be assumed to be infinite (Lewis, 1954). An inelastic labour supply could explain the muted response in terms of work's extensive margin, which would occur even if the PSNP increased the reservation wages among some rural workers.[19] My findings suggest that effects of social programmes into the labour market, like those found in NREGA, are less likely to be observed in the Ethiopian rural context where wage markets are thinner.

[19] In addition to this conceptual discussion, Appendix A sketches a more formal theoretical framework borrowed from Imbert and Papp (2015) and minimally adapted.

3.5 Unpacking within-district heterogeneity

Finally, I investigate whether the district-level effects of the programme are driven by changes within-targeted communities (*kebeles*) or whether these effect are due to changes in untargeted *kebeles* within targeted districts. To do so, I complement my results with a descriptive analysis of three waves of the Ethiopian Socio-Economic Surveys (ESS) collected in 2011, 2013, and 2015, combined with my district-level covariates. This additional panel of households allows me to observe variation in PSNP beneficiaries within targeted districts, at both the community-level and individual-level.[20] I can still control for the targeting rule variables, to account for the selection at the *woreda*-level, but I cannot estimate my difference-in-differences specification as no wave of this survey was collected before the programme started. I therefore estimate two descriptive specifications that mimic my difference-in-differences analysis:

$$Y_{irwkt} = b \times \mathbb{1}_{\{PSNP=1\},w} + \mathbf{X}'_w \theta + \eta_r + \gamma_t + \epsilon_{1,irwkt} \tag{2}$$

where Y_{irwkt} is the outcome of interest for individual (or household) i, in the region r, in *woreda* w, in survey round t. $\mathbb{1}_{\{PSNP=1\},w}$ is an indicator equal to one if the *woreda* is targeted by the PSNP, $\mathbf{X}_w$ is a vector of time-invariant controls accounting for the geographic targeting rule of the programme; η_r and γ_t are region and survey-round fixed-effects. Finally, $\epsilon_{1,idt}$ is the unobserved idiosyncratic component, while b remains the main coefficient of interest.

Second, to study how the programme effects on the labour market decisions in untargeted communities inside targeted *woredas*, I extend the specification in Equation 2 to include an indicator for whether the *kebele* participated in the PSNP according to the community-level respondent.

$$\begin{aligned} Y_{irwkt} = & b_1 \times \mathbb{1}_{\{PSNP=1\},w} + b_2 \times \mathbb{1}_{\{PSNP=1\},k} + b_3 \times \mathbb{1}_{\{PSNP=1\},i} \\ & + \mathbf{X}'_w \theta + \eta_r + \gamma_t + \epsilon_{2,irwkt} \end{aligned} \tag{3}$$

In Appendix Tables 9-11, Panel A reports estimates of b from estimating Equation 2 across different outcomes using the ESS. Each column presents this partial corre-

[20] Appendix Figure 5 shows districts by their exposure to the PSNP and on whether they were sampled in the Ethiopian Socio-Economic Survey. Appendix C provides more details on the within-district distribution of PSNP targeting according to the survey data.

lation for different outcome. Whereas Panel B reports estimates of b_1, b_2, b_3 from estimating Equation 3. Panel B unpacks whether any differences between targeted and untargeted *woredas* are stronger in targeted or untargeted *kebeles*. Moreover, the difference between b_1 and b_2 is of interest, because it indicates the relative difference of outcomes among untargeted households within targeted woredas, but across different *kebeles*. This difference can be interpreted as a descriptive non-causal spillover effect. The interpretation is not causal, since *kebeles* within targeted districts were selected based on the list of food insecure households compiled by the local administrators. The p-value testing the equality of these two coefficients is reported below Panel B.

The descriptive results are consistent with the main analysis. There is no significant differences in the extensive labor supply or the share of workers engaged in different occupations among non-beneficiaries in targeted and untargeted districts. The sign of the effects is consistent with the main analysis, but the magnitude is smaller.[21] However, there are some differences in the intensive labor supply. There are increases in the hours spent working in self-employment outside of agricultural for individuals in untargeted communities in PSNP districts. While non-beneficiaries in PSNP communities report spending more hours working in farming compared to non-beneficiaries in untargeted communities within the same district.[22] Non-beneficiaries in targeted communities demand more days of unpaid labour and report lower wages compared to both untargeted communities within the same district and individuals in untargeted districts (Column 2 and 3, Appendix Table 11). Individuals in untargeted communities in targeted districts report higher wages compared to those in untargeted districts, though this difference is not statistically significant. The descriptive analysis supports the notion that sectoral shifts may not be a direct effect of the programme but may be driven indirectly from untargeted communities in PSNP districts. Overall, the findings align with the patterns identified earlier in the main analysis.

[21] In particular, there is a 1.5 percentage point higher proportion of workers in non-agricultural self-employment in untargeted communities in PSNP-districts compared to non-PSNP ones (Column 3, Appendix Table 9).

[22] The difference is not statistically significant at the 10% level (p-value: 0.138), but it is non-negligible in magnitude, corresponding to 128 fewer hours (in a whole year) spent farming. Appendix Table 10 Panel B shows a 12% negative difference in the hours spent working in farming in untargeted communities in PSNP-districts compared to non-PSNP ones and a 15% positive difference in the hours spent working in agriculture among non-beneficaries in targeted communities.

3.6 Robustness checks

To validate the results in Table 3 (in particular that the PSNP increased non-agricultural self-employment by about 5 percentage points) I employ four strategies: First, I run a placebo test replacing the indicator variable $\mathbf{1}(t = 2013)$ with $\mathbf{1}(t = 2005)$ and using the data from the 1999 and 2005 LFS rounds. Second, I include population density from the 2007 census (interacted with a dummy for the year 2013) as an additional control, although this variable might be considered a "bad control" since it is measured post-implementation. Third, I study heterogeneity of the main effects by whether districts experienced a pre-programme shocks. Finally, I present estimates of the main effects without district-level controls, without weights, and on the unbalanced panel of districts. None of my conclusions are affected by these robustness checks.

3.6.1 Placebo test

The first robustness check (Appendix Table 12) examines changes in employment and occupational categories before the PSNP started using data from 1999 and 2005 LFS rounds. The results show no significant changes in labour supply or self-employment activities. However, I do find an increase of 0.9 percentage points in public sector labourers in targeted districts, which is large (relative to the overall sample mean of 0.7%) and statistically significant at the 5% level. This effect may be consistent with the start of the first implementation phase of the PSNP public works by 2005.[23] Nonetheless, I regard 2005 as a baseline pre-programme year because, as the World Bank (2010b, pp.1) states, the first phase of the programme (between 2005 and 2006) 'focused on testing and strengthening institutional arrangements and delivery systems', and facilitated the transition from the previous emergency system. Since 2007, the programme was seen to consolidate the changes and operate at a much larger scale. Hence, it is unlikely that within the first few months of the programme there would have been enough participants to strongly attenuate any market-level impacts of the programme by 2013. However, to be precise, my estimates should be seen as the additional effect of the programme relative to its initial adjustment phase. This placebo test is based on a sub-sample of the 391 districts due the challanges matching across the first two LFS rounds,

[23] A higher share of government employees is also plausibly due to the political and institutional factors related to the historical disbursements of aid in those districts, where sufficient administrative capacity had to be in place to monitor the transfers during times of emergency.

further described in Appendix C.

3.6.2 Adding population density as a control

The reduced availability of land, due to increased population growth, has been identified as one of the factors contributing to the reduction of productive assets in rural Ethiopia (World Bank, 2010a). To account for this dynamic, Appendix Table 13 presents the results adding population density as a control, a variable taken from the 2007 census. Before adding population density as a control, I drop observations from the Somali region in Panel A. I remove these observations because the 2007 census did not cover this region. Hence, the estimates of β, after including population density as a control (Panel B), should be compared to Panel A. After removing observations from the Somali region, the coefficients in Panel A are similar to the main results in Table 3. However, after including population density as a control in Panel B, the magnitude and significance of the coefficients decrease, particularly in columns 4 and 5. This suggests that the baseline controls were not fully accounting for the influence of population density. While the results remain consistent with the main analysis, this robustness check indicates that the magnitude of the effects may be about 1 percentage point smaller when accounting for changes in population dynamics.

3.6.3 Testing for heterogenous effects due to pre-programme shocks

As the third robustness check, I investigate whether pre-programme shocks affected the labour market outcomes differently in PSNP districts. I add to my previous specification a district-specific variable, W_d, likely correlated with pre-2005 shocks. I use two measures of pre-programme shocks, W_d: standardized cumulative rainfall for the 2002 *Belg* season and a dummy variable indicating continued relief assistance in 2005.[24]

[24] The first measure is based on the observation that districts affected by the widespread 2003 drought generally received limited rainfall during the 2002 *Belg* season (Gill, 2010). The second measure accounts for the possibility that PSNP-targeted districts that required emergency assistance in 2005 may have been more susceptible to experiencing negative shocks before the programme's implementation.

$$\begin{aligned} Y_{idt} =& \beta \times (\mathbb{1}_{(PSNP=1)} \times \mathbb{1}_{(t=2013)}) + (\mathbf{C}_d \times \mathbb{1}_{(t=2013)})'\delta + \mathbf{X}'_{dt}\theta + \\ & \mathbf{H}'_i\zeta + \eta_d + \gamma \times \mathbb{1}_{(t=2013)} + \\ & \beta_2 \times (\mathbb{1}_{(PSNP=1)} \times \mathbb{1}_{(t=2013)} \times W_d) + \gamma_2 \times (\mathbb{1}_{(t=2013)} \times W_d) + \epsilon_{3,idt} \end{aligned} \tag{4}$$

Appendix Table 14 presents the estimates of β and β_2 from estimating Equation 4. The results indicate that including the interaction terms does not significantly alter the estimates relative to the main results. The estimates of β increase slightly in magnitude relative to the main results. This pattern suggests that pre-programme shocks could have attenuated the effects of the programme on the labour market outcomes considered, rather than bias them upwards. These estimates do not imply a failure of the parallel trends assumptions because of pre-programme shocks.

3.6.4 Removing weights and district-level controls

In Appendix Table 15, the results remain similar to the main estimates even when not using weights or when expanding the sample to include all individual observations in the 601 districts sampled in either the 2005 or 2013 LFS rounds. Although removing weights decreases standard errors, adding more districts does not change the main estimates but improves the precision of the control variables.[25] In Appendix Table 16, the effect of the PSNP on the main outcomes remains significant even without additional controls in the basic difference-in-differences model, demonstrating the overall robustness of the results presented in Table 3.

4 Conclusion

This paper examines the impact of the Productive Safety Net Programme (PSNP) on rural labour markets in Ethiopia using a difference-in-differences approach. The results indicate that the programme did not significantly affect the extensive and intensive labour supply in targeted districts. However, the PSNP led to a higher share of self-employed individuals engaging in non-agricultural activities in these districts. The PSNP primarily serves its main objective of ensuring food security

[25] Solon et al. (2015) note that weighting may harm precision if the intra-group (district) correlation makes up a large proportion of the variance of the error term.

for its beneficiaries rather than increasing overall employment. The programme does not appear to crowd out private sector activities. The results are consistent with the programme's productive assets having improved market access, leading to shifts in non-agricultural self-employment activities. The analysis of equilibrium wages remains limited due to the nature of the Ethiopian rural labor market data. Future research could explore how the rural-urban wage equilibrium may have been affected by both the rural and urban PSNP (which was launched in 2015).

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Appendix

This appendix has four sections. In Section A I report a conceptual framework that motivates the analysis of public works on equilibrium wages. Section B provides additional institutional details about the programme. Section C provides additional details about the dataset construction and sources of the covariates. Finally, Section D reports additional analytical checks described in main text.

A Theoretical appendix

The exposition here follows Imbert and Papp (2015). This model illustrates theoretically how changes in public works can affect the labor market equilibrium.

A.1 A model of household labour supply and demand

Time is static. Households are indexed by i. D_i denotes household labour demand. Households operate a production function:

$$F_i(D_i) = A_i G(D_i) \tag{5}$$

where $A_i \in [\underline{A}, \overline{A}]$ are exogenous productive factors owned by the household. $G'(.) > 0$ and $G''(.) < 0$, i.e. the production function exhibits decreasing marginal returns to scale.

Households choose consumption (c_i), labour supply (L_i^s) and demand (D_i) to solve:

$$\max_{c_i, L_i^s, D_i} u(c_i, T - L_i^s) \quad \text{subject to}$$

$$\begin{aligned} c_i &= y_i + \tilde{W}_i L_i^s \\ y_i &= \pi_i \\ &= A_i G(D_i) - \tilde{W}_i D_i \end{aligned} \tag{6}$$

where y_i is non-labour (non-wage) income, π_i is profits from home production, $\tilde{W}_i$ is the shadow wage, which is the price of labour for the household that could be lower than the market wage W. Deriving first order conditions for Equation 6, given separability of consumption and production decisions, households will set the marginal product of labour equal to the shadow wage:

$$A_i G'(D^*) = \tilde{W}_i \tag{7}$$

A.2 Equilibrium with competitive labour markets

Suppose that labour markets are competitive, such that: $\tilde{W}_i = W$, the shadow wage that measures the opportunity cost of time is equal to the market wage for all households. If so, then $A_i G'(D^*) = W$.

If A_i is low, then $G'(D^*)$ will be high and because of $G''(.) < 0$ then D^* will be low for low-productivity households. In particular, low productivity households will be net-sellers of labour $D^* < L_i^{*s}$. Conversely, if A_i is high, then $G'(D^*)$ will be low and because of $G''(.) < 0$ then D^* will be high for high-productivity households. In particular, high productivity households will be net-buyers of labour $D^* > L_i^{*s}$.

A.3 Equilibrium with frictions

Suppose that due to labour market frictions (e.g. job search costs), there is a wedge $p \in [0,1]$ between the returns to one unit of labour for workers (pW) and its costs for employers (W). High-productivity households that are net-buyers of labour will then price the marginal value of labour according to $A_i G'(D^*) = W$. Low-productivity households that are net-sellers of labour will then price the marginal value of labour according to $A_i G'(D^*) = pW$. Households with intermediate productivity levels do not participate in the market and set $A_i G'(D^*) \in [pW, W]$. Denote with $\phi(W)$ the value of the productivity factor A_i such that labour supply is equal to labour demand for household i.

A.4 The effect of public works on labour market equilibrium

Suppose the government starts hiring labour at a wage W_g. Total labour hired in public works is $L^g = \int_i L_i^g di$ Then the households' non-labour income earned outside of labour markets (i.e. in own-farm agriculture or through public works) is:

$$y_i = \pi_i + (W_g - \tilde{W}_i)L_i^g \tag{8}$$

Define the labour market clearing condition that sets the total labour supply by net-sellers of labour equal to the total labour demanded by net-buyers of labour:

$$\underbrace{p\int_{\underline{A}}^{\phi(pW)} [L_i^s(pW) - D_i(pW) - L_i^g]dA_i}_{\text{low-productivity suppliers}} = \underbrace{\int_{\phi(W)}^{\overline{A}} [D_i(W) - L_i^s(W) + L_i^g]dA_i}_{\text{high-productivity buyers}} \tag{9}$$

To understand the equilibrium effects of public works, we would want to totally differentiate Equation (9) with respect to L_g. Note that we implicitly define the market wage W to be a function of L_g.

After applying Leibniz rule to differentiate an integral and several steps to simplify the algebra, we can define the total effect on market wage of public works as follows:

$$\frac{dW}{dL^g} = \frac{E_1 - E_2}{-E_3 + E_4} \tag{10}$$

where:

$$E_1 = p\int_{\underline{A}}^{\phi(pW)} \frac{dL_i^g}{dL^g}dA_i + \int_{\phi(W)}^{\overline{A}} \frac{dL_i^g}{dL^g}dA_i > 0 \tag{11}$$

E_1 is the crowding out of public employment from other sources of employment. The other sources of employment are wage labour, for the least productive households, and self-employment, for the more productive households.

$$E_2 = p\int_{\underline{A}}^{\phi(pW)} \frac{dL_i^s}{dy_i}(W_g - pW)\frac{dL_i^g}{dL^g}dA_i + \int_{\phi(W)}^{\overline{A}} \frac{dL_i^s}{dy_i}(W_g - W)\frac{dL_i^g}{dL^g}dA_i \tag{12}$$

E_2 is the effect on aggregate labour supply through non-labour income. This effect can be interpreted as the change in the total labour supply occurring from individuals shifting out of the wage market since they are getting an income directly through public works. $E_2 < 0$ if:

(i). $\frac{dL_i^s}{dy_i} < 0$ because of an income effect.

(ii). $(W_g - W) > 0$ by assumption of the programme, but this assumption is not valid in the PSNP case, where $(W_g - pW) \leq 0$.

Hence, the numerator of Equation (10) is generally positive, so long as (i). and (ii). are true.

If $E_1 > 0$ and $E_2 \geq 0$, then $E_1 - E_2$ can be ambiguous. In particular, if the income effect is larger than the crowding-out effect, i.e. $E_2 > E_1$, public works may reduce employment. Otherwise, if the income effect is small, then $E_1 > E_2$ will still make the numerator of Equation (10) positive, but smaller relative to the case where $E_2 < 0$. The latter scenario may occur if the public works wage is set to be below or equal to the market wage.

$$E_3 = p^2\int_{\underline{A}}^{\phi(pW)} D'(pW)dA_i + \int_{\phi(W)}^{\overline{A}} D'(W)dA_i \tag{13}$$

E_3 is the effect on aggregate labour demand, which will generally be negative, based on the slope of the demand curve.

$$E_4 = p^2\int_{\underline{A}}^{\phi(pW)} [\frac{dL_i^s}{dW}|_u + \frac{dL_i^s}{dy_i}(L_i^s - D_i - L_i^g)]dA_i + \int_{\phi(W)}^{\overline{A}} [\frac{dL_i^s}{dW}|_u + \frac{dL_i^s}{dy_i}(L_i^s - D_i - L_i^g)]dA_i \tag{14}$$

E_4 is the effect on aggregate labour supply via the wage equilibrium changes. If leisure is not a luxury good (which you consume a higher share of as you get richer), then $E_4 > 0$, which makes the denominator of Equation (10) also positive.

The effect on the wage of an increase in public works will be larger if $-E_3$ is small (i.e. aggregate demand is inelastic to the wage), or if E_4 is small because the labour supply is inelastic to the wage.

B Programme details

B.1 Weather shocks and safety nets

Despite being one of Africa's fastest growing economies, Ethiopia's poverty rate remains high. While poverty reduction is one of the main objectives of the Ethiopian government, the number of individuals consuming less than US$1.25 per day (in purchasing power parity terms) was estimated to be 29.6% in 2010/11 (GFDRE, 2013). Food security is an unavoidable policy concern that Ethiopia has to address in pursuing poverty reduction.

To counter seasonal food shortages, Ethiopia has been receiving relief food aid from abroad, with amounts varying from year to year over the last 30 years. Until the establishment of the PSNP in 2005, the government resorted to annual appeals to the international community in order to secure assistance.[26] The emergency response system in place prior to 2005 had saved many lives, but was seen as not having protected the livelihoods of those affected by shocks (Kehler, 2004).

Following the 2003 drought, the GFDRE and a consortium of Development Partners[27] developed a Food Security Programme that aimed to overhaul the relief aid system, turning it into a more reliable safety net. The programme developers anticipated that the new system would allow both recipients and donors to plan support ahead of emergencies, rather than organising relief responses on a nearly annual ad-hoc basis. In particular, they argued that the provision of transfers over multiple years would allow recipients to curb the depletion of their own assets in times of need.[28] The PSNP was allocated the lion's share of the Food Security Programme's budget, and is the flagship component of this new strategy to counter food insecurity.[29] Drawing from existing studies and reports, I next provide an overview of how the programme is designed.

B.2 Overview of the PSNP

The PSNP aims to alleviate the incidence of food insecurity and avoid asset depletion among historically vulnerable rural communities. It primarily seeks to achieve this through timely and

[26] The annual appeal system was considered unreliable, because food deliveries were often untimely and irregular, and unsustainable, because of instability in the global food marketing regime and uncertainties regarding donor pledges following the appeals (Rahmato, 2013). It could have taken up to three months after the outbreak of a food crisis for relief to reach those in need.

[27] The Development Partners comprise multilateral agencies such as the World Bank, the World Food Program, the European Union, and bilateral partners, such as USAID, the UK Department for International Development, Irish Aid, the Canadian International Development Agency and the Swedish International Development Agency.

[28] Short-selling of livestock in bearish market conditions is an example of a short-term coping mechanisms taken by households during food shortages. However, this practice may only contribute to less than a third of income smoothing after a drought (Fafchamps et al., 1998). Another short-term coping strategy is the deforestation of hill-sides for the production of charcoal. The PSNP seems to have had a modest positive impact on forest stock (Andersson et al., 2011), reducing environmental degradation of the agroecological conditions.

[29] The other components of the Food Security Programme were complementary to the PSNP, and were implemented in some, but not all, of the districts where the PSNP operated.

appropriate food and/or cash transfers, and the creation of productive community assets that can contribute to environmental rehabilitation, increase household productivity, and improve access to infrastructure and services (GFDRE, 2006).

The programme is managed by the GFDRE, but remains mostly donor-funded.[30] It has grown significantly in terms of budget requirements as the number of targeted beneficiaries has expanded. The fourth and latest phase of the programme, running from 2015 until 2019, has a budget requirement of US$3.6 billion, towards which the GFDRE has committed US$500 million, with the remainder financed by its Development Partners (World Bank, 2014).

After the first year, which was intended to test the administrative and logistic capacity to deal with the deployment of such a large programme, the number of districts went up to 262. However, the increase in the number of districts was mostly due to large districts splitting, shortly after the 2005 elections. These administrative splits were partly justified on the grounds that large *woredas* were harder to administer and lacked sufficient governance.[31] Hence, the actual number of targeted districts, relative to the 2005 administrative boundaries, had not actually increased by 2006.

B.3 PSNP beneficiaries

The demographic characteristics of beneficiaries are relevant in choosing the appropriate labour market to focus on, and potential control variables for the analysis. The main beneficiaries of the PSNP transfers are chronically food insecure households, which the Programme Implementation Manual (PIM) defines as 'households that have been unable to meet their food needs for a period of three months or more in the last three years' (GFDRE, 2006, pp.4). In addition to chronically insecure households, the programme aims to provide transfers to households that are temporarily unable to meet their minimum food consumption requirements due to a negative shock, and households that have no means of support, such as remittances.

Eligible beneficiaries, who are able-bodied and above 16 years of age, receive transfers in return for participation in public works. In 2009, transfers conditional on public works participation comprised 84% of the total transfer to beneficiaries (World Bank, 2010b). Other eligible households, who cannot supply labour (either temporarily or permanently), receive an unconditional transfer (referred to as Direct support). Direct support beneficiaries include, but are not limited to, orphans, pregnant and breast-feeding women, the elderly, people with disabilities, and female-headed households with young children (GFDRE, 2006).

B.4 Public works

The main feature of the PSNP operations is its public works component. The public works supported under the PSNP are small-scale, labour-intensive community projects designed to provide unskilled, temporary employment for eligible households with able-bodied members. For all sub-projects in a district, the ratio of total labour inputs to total costs should be at least 80% (GFDRE, 2010). Annually

[30] The World Food Programme covers implementation in the Somali region.
[31] I refer to the conversation I had with World Bank Officials in January 2016.

around 46,000 public works sub-projects are undertaken (World Bank, 2009). The nature of the projects vary depending on the local environmental conditions and community needs. Most projects involve soil and water conservation activities aimed at fostering the local watershed development. Other PSNP-funded projects involve the construction of local roads, schools or health posts. The potential productivity effects of the infrastructure generated by these projects is what motivated the first "P" in the programme's acronym. These productivity gains can plausibly be the factor driving changes in the local labour market. However, because of a lack of a spatial database for public works program activities, it has been hard to accurately evaluate their impact (Subbarao et al., 2013).

The timing of public works is key. Public works run for 6 months each year, usually from January to June, to coincide with the agricultural slack season. The project's timing aims not to interfere with agricultural labour needs.[32] Participants usually work for eight hours a day for around 5 days/month. The actual days of individual employment vary depending on the household circumstances, as able-bodied members are expected to fulfil the workfare requirements (up to a maximum of 15 days/month) other household members that also receive transfers, but who do not participate in public works. The individual cap of 15 days/month was implemented for two reasons: budgetary constraints; and to enable participants to have sufficient time to engage in other productive activities outside of the programme. As such, the programme was designed in a way that would not distort the intensive and extensive margin of the labour supply of participants.

In 2009, the World Bank estimated that the PSNP provided 190 million days of public works employment to 1.27 million households (World Bank, 2009). An additional 242,000 households were estimated to be direct support beneficiaries. The average household employed in public works received 129 days of employment in 2009, with some variation in this average across regions (Berhane et al., 2011). Administrative data on individual participation to the PSNP has been hard to find, even for the authors involved in the official impact evaluation of the programme (*Ibid* pp.131). As such, aside from the estimates of the independent evaluation and the official statistics, I am unable to observe directly whether individuals have taken up participation in the programme.[33]

B.5 Cash and food transfers

PSNP beneficiaries are remunerated with a daily payment in either cash or food, depending on their location. Overall, 60% of transfers are provided in cash, with factors such as local market conditions, beneficiaries' preferences and logistical constraints influencing which of the two is used.

The cash wage was meant to enable households to purchase the equivalent food transfers from

[32] One may worry that the public works were not operating at the time in which the survey used in the analysis were collected. Luckily, the surveys were collected in March and June. I further elaborate on this point when discussing the potential limitations of my dataset.

[33] This is a limitation of my study, if one worries about the potential institutional malfunctioning that could hinder the implementation of the programme. However, the high degree of scrutiny from the Development Partners, along with the fact that the evaluations of the programme were independent of the government, should provide some reassurance that the programme was operating.

the local market.[34] By design, this level is below the usual market wage for unskilled labourers (Subbarao et al., 2013). Currently, the wage rate is on average ETB23/day of work across all regions receiving cash transfers. In 2009, the estimated value of (annual) wages earned per average household recipient was US$137 (World Bank, 2009).

The parity of cash and food transfers has eroded over the years, with food becoming more expensive and cash transfers not adjusting fast enough. This disparity was particularly accentuated during the food price spike in 2008-2009, but the share of cash transfers never went below 50%. Economic theory suggests that if the public works wage is set above the market wage, then private labour supply may be crowded-out by public employment, raising the equilibrium wage for workers in the private sectors (Ravallion, 1991). The erosion in purchasing power of the wages offered by the programme, coupled with the fact that rates were intentionally set below market wages, could potentially reduce any aggregate effect of the programme occurring through changes in the demand for labour.[35]

[34] Food transfers are in general 3kg of cereals per day worked. In 2008, the rate was first increased from 6 to 8 birr/day to take into account the soar in food prices, with subsequent raises following roughly every 2 years. Until 2011, a uniform wage rate was employed across all recipient *woredas*, but, in 2012, it was decided to allow districts to change the wage rate so as to take into account the geographic heterogeneity in food availability and prices. In 2015, US$1 was exchanged for approximately 20 Ethiopian Birr (ETB).

[35] The reasoning is analogous to the introduction of a minimum wage above the market wage in a competitive market, which results in a higher equilibrium wage and lower employment.

C Data appendix

C.1 Constructing a panel of districts

While the CSA made a big effort to cover both rural and urban areas in all regions of the country, its objective was not to cover all districts. There are only a few zones (and the districts within them) that are systematically omitted from the sampling frame. Appendix Figure 3 shows how the 2005 and 2013 round differ in their coverage of districts and what that means for the size of my balanced panel of districts.

I have to drop observations from the Gambella region and most of the Afar region, which make up about 0.7% and 1.5% of the total rural population of all districts sampled, respectively.[36] This is because rural districts in these regions were not included in the sampling frame in 2005. Aside from these cases, the sampling method was similar across survey rounds. Hence, the reason why a given district is not sampled in a round is (presumably) due to the realisation of the random draw of districts from the same population that were chosen to be sampled, except for those zones that were ex-ante excluded from the sampling frame. I do not expect there to be a bias in my estimates due to sample selection because of the survey design.

To merge the datasets, I follow this procedure: First, I construct a district identifier for the 2013 round of the LFS, which I match with the 2007 census. To create unique district identifiers across districts, I concatenate three numbers: an integer for the region, an integer defining the zone within a particular region, and an integer for the district within a particular zone. The CSA, which also carries out the census, did not change its maps since the 2007 census, so district identifiers are consistent between the 2013 LFS round and the 2007 census. This is how I obtain a list of district names in the 2013 LFS round, which was missing and is crucial for what follows next.

Second, I digitalize the 2005 LFS district geographic identifiers, which were only available as a scanned file. As noted in the identification section, many new districts were formed following the 2005 election, by splitting large districts into two or more new ones. About 200 new districts were formed between 2005 and 2006. There are only a few instances in which two (pre-2006) districts were divided to jointly form a new district; I treat these few cases as if the new district was formed from a part of either of the two old ones. My challenge consisted in finding out which districts had split, and then assigning to each old district an identifier that was consistent with the 2013 round. I used the district names to identify which districts had split, using the information from two sources: recent administrative maps of Ethiopia[37], and the map plotting years of assistance, which was originally drawn using pre-2007 boundaries (before I converted it to post-2007 boundaries). Google searches were also used to confirm the validity of the district splits I identified.

After identifying which districts had split, I could have grouped the district boundaries in the 2013 round to reflect the old borders, aggregating back the new districts into their old borders. However, this procedure would have not taken into account the fact that the PSNP operates only within certain villages in each district, and not all newly formed districts that were originally contained

[36] Population estimates are calculated from the 2007 census.
[37] Available at `http://tinyurl.com/ocha-map13`, accessed on 09/04/2016.

in a geographically targeted district were targeted by the programme after 2006. As noted in the background section, the district officials were supposed to roll out the PSNP in the most needy villages based on the reports of the community food-security task force, which had drawn a list of food insecure households. Priority was given to villages with the highest number of food insecure households. There was no official cut-off that determined roll-out at the village-level. As such, the newly formed districts were not necessarily targeted by the programme following the boundary changes. Matching the old district to only one of the newly formed *woredas* would have incorrectly assigned treatment to certain districts, which were not in fact recipients of the PSNP. Thus, I follow the approach suggested by Imbert and Papp (2015).

Using the 2005 LFS round, for I duplicate observations in districts that split into x copies, where x is the number of newly formed districts (usually two or three). Then, I assign a 2013 district identifier to each individual in a given copy of the x newly created districts. Finally, to adjust the sample for these artificial copies, I divide the survey weights by x for the observations that were duplicated x times. I apply the same procedure to the matched observations in the 1999 LFS round, which I use for my placebo test.

C.1.1 Issues combining the 1999 LFS round

Between 1999 and 2005, certain zones changed boundaries, and so did the integers that identify them. Unfortunately, the 1999 LFS round did not have district names like the 2013 round. To match this round with the 2005 round, I have to assume that the district numeric identifiers have remained constant across the two rounds. For the most part, it is unlikely that numeric identifiers changed between the two rounds for two reasons: First, the majority of districts splits in the last two decades occurred after the 2005 elections. Further, the CSA relies on census maps to assign geographical identifiers for most of its surveys, and there was no census collected between 1994 and 2007. However, in 2000, rather than districts splitting, some zones were divided.[38] I lack the information to unambiguously match the unique district identifiers across time and rounds in the zones that changed boundaries between the 1999 round and the 2005 round. Hence, for the placebo test, I have to drop the unmatched districts from the analysis, which makes up 10% of the observations collected in 1999. This restricts my balanced panel of districts for the placebo test to 391 *woredas*.

C.2 Sources of other covariates

C.2.1 Geographic targeting data

The geographic assignment of the PSNP mostly comes from the only two publicly available lists published in the Programme Implementation Manuals (GFDRE, 2006, 2010). I also compared the list of districts names with the maps contained in the World Bank (2010b) results report, by plotting the GFDRE's lists onto administrative shapefiles. With this procedure, I ensure that I match the

[38] Zones are the intermediate administrative unit between regions and districts, usually containing 5-10 *woredas*.

geographic targeting of 290 districts by the end of 2009. The World Bank acts as the coordinator for all donor partners involved in the programme, which is why I rely on the information they publish.

C.2.2 Historical frequency of food aid

Districts were targeted based on their historical receipt of food aid prior to 2005. I collected data on the frequency of historical relief assistance at the district-level (between 1994 and 2005) from the National Disaster Risk Management Committee[39] of the GFDRE. I only observe an indicator for whether a district received aid assistance in a particular year, and not the quantity of aid received by a district in each of these years. I personally collect this data in a trip to Addis Ababa in January 2016. This information is shown in Appendix Figure 4. Its inclusion should capture some of the unobserved characteristics that are shared by targeted districts, such as the level of food insecurity, which, if omitted, could bias the estimated effect of the programme.

C.2.3 Weather controls

Weather shocks could be part of the unobserved time-varying component, and may be more frequent in PSNP *woredas*, which is why I control for climatic variables in my main specifications using gridded data sources. Gridded data, which interpolates readings from weather stations with a statistical model, are frequently used by economists.[40] However, one of the difficulties of employing these data sources in low- and middle-income countries, particularly for rainfall, is that the stations tend to be highly dispersed, increasing the potential for measurement error. For this reason, I use data from the Global Precipitation Climatology Centre (GPCC) dataset as its station coverage has been found to be better than any other publicly available source of monthly rainfall (Becker et al., 2013). The GPCC dataset is maintained by the World Meteorological Organization and contains monthly estimates of total precipitation (mm) for the global land surface at $0.5° \times 0.5°$ resolution for all years between 1900 and 2014.

For temperature, I employ the most recent version (V4.01) of the well-known Willmott and Matsuura (2015) series hosted by the University of Delaware, providing monthly temperatures at the same spatial resolution, for the period of interest. These data have been used in several other studies, such as Adhvaryu et al. (2019) and Theisen (2012), and were chosen because of their geographic scope and long time scale.[41] Since the gridded climatological data does not necessarily match the administrative district boundaries, a precipitation/temperature value is assigned to each *woreda* based on the values of the raster cells covering that *woreda*. If one single cell covers the *woreda* in question, then the *woreda* takes on the value of that cell. When two or more cells cover a single *woreda*, a weighted mean is calculated, where the weights are equal to the fraction of the polygon

[39] Formerly known as Disaster Prevention and Preparedness Committee (DPPC). I am grateful to Lemlem Abraha and Negussie Kefeni for sharing their time in assisting me during such a demanding period.

[40] See Dell et al. (2014) for a review of the recent economic literature using weather data.

[41] I use data between 1979 and 2014 to construct a sample mean and standard deviation with which I calculate standardized values of cumulative rainfall and average temperature, for each year and each cropping season.

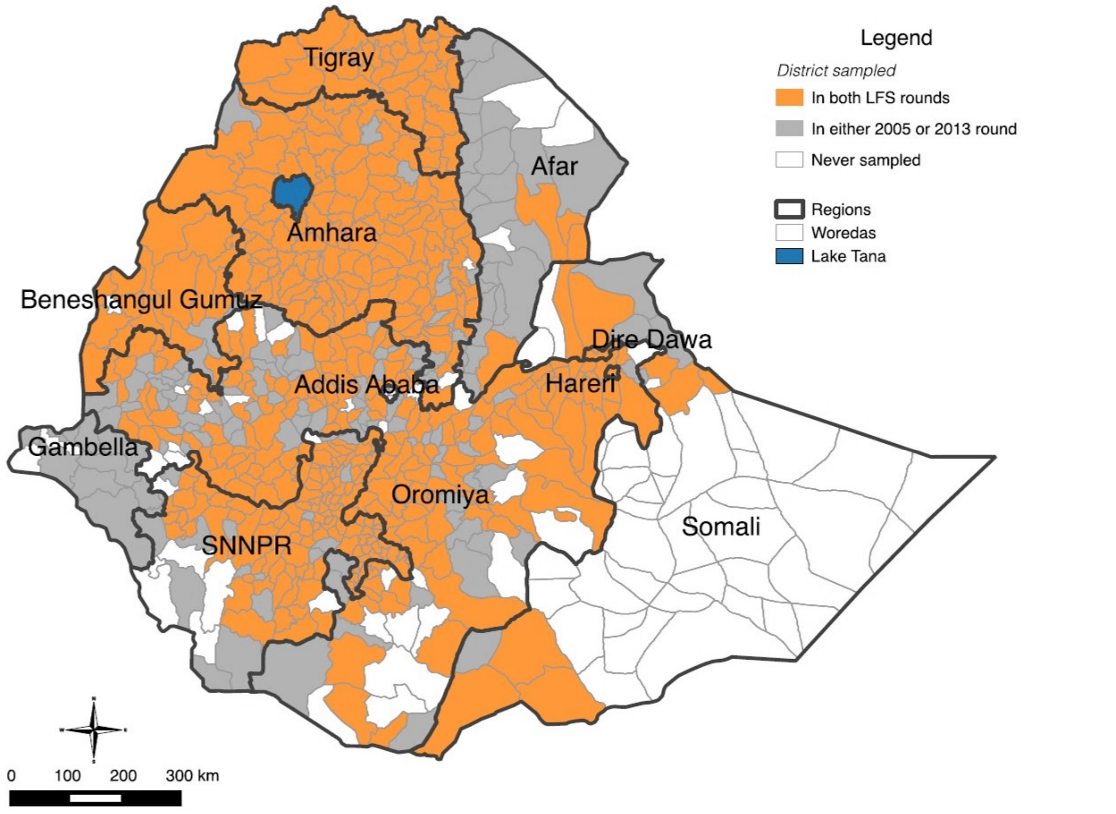

Figure 3: District balance in the Labour Force Survey

covered by each cell.[42]

Other controls, which I do not include in the main regressions (but that are shown in Appendix Table 7) come from the village-level 2007 census of Ethiopia, also carried out by the CSA. These variables could constitute a bad control, as they may have been affected by the PSNP between 2005 and 2007. Hence, I only include additional census variables controls as a robustness check, to explore whether my results could be explained by changes in the population dynamics.

[42] Temperature and rainfall data used are freely available at http://tinyurl.com/udel2014 and http://tinyurl.com/gpcc2014, respectively, accessed on 20/04/2016.

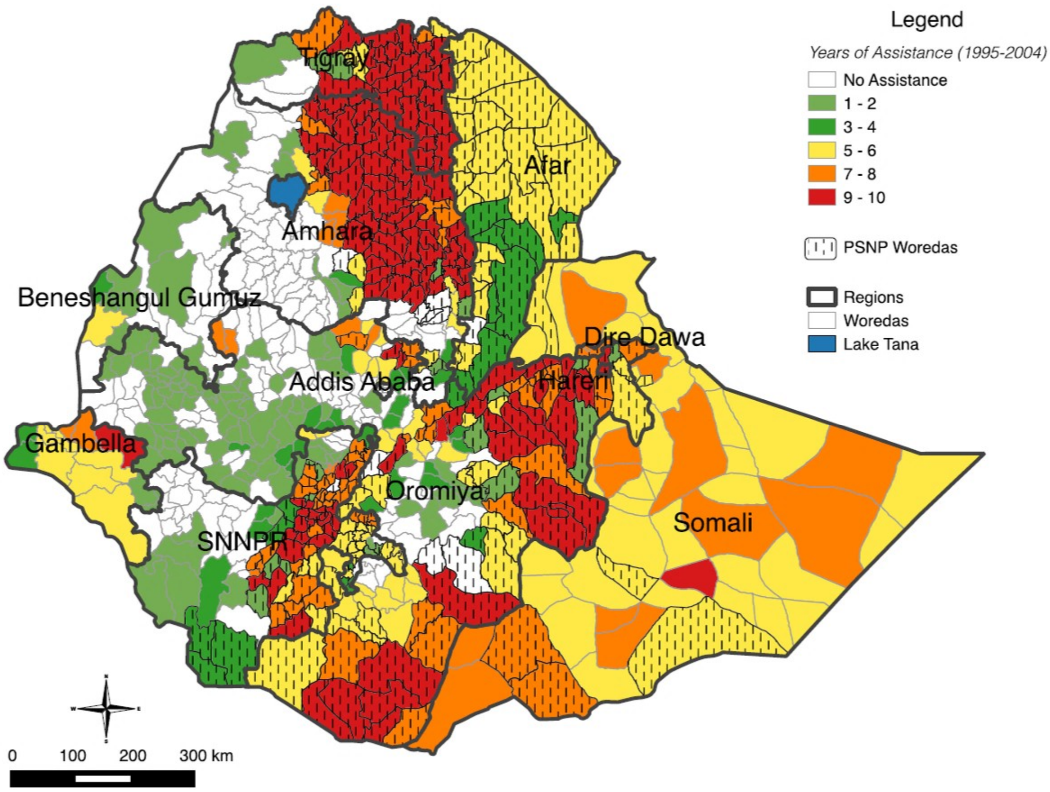

Figure 4: Cumulative years of aid receipts and PSNP targeting[a]

[a] *Notes:* PSNP assignment of 290 *woredas*, as of the end of PSNP Phase II (2007-2009). Years of assistance collected by the author from the National Disaster Risk Management Committee.

C.3 PSNP targeting in the Ethiopian Socio-Economic Survey

Table 6: PSNP targeting in the Ethiopian Socio-Economic Survey

	Mean	SD	Min	p25	p50	p75	Max
1 if PSNP-*woreda*	0.51	0.50	0.00	0.00	1.00	1.00	1.00
Years of assistance received between 1994-2005	4.94	3.78	0.00	1.00	6.00	9.00	10.00
1 if *woreda* received assistance in 2003	0.64	0.48	0.00	0.00	1.00	1.00	1.00
1 if *woreda* received assistance in 2004	0.56	0.50	0.00	0.00	1.00	1.00	1.00
1 if *woreda* received assistance in 2005	0.39	0.49	0.00	0.00	0.00	1.00	1.00
1 if PSNP-*kebele*	0.42	0.49	0.00	0.00	0.00	1.00	1.00
Number of kebeles	277						
Number of woredas	228						
Conditional on the woreda being targeted by the PSNP:							
Years of assistance received between 1994-2005	7.72	2.30	0.00	7.00	9.00	9.00	10.00
1 if *woreda* received assistance in 2003	0.94	0.23	0.00	1.00	1.00	1.00	1.00
1 if *woreda* received assistance in 2004	0.88	0.33	0.00	1.00	1.00	1.00	1.00
1 if *woreda* received assistance in 2005	0.60	0.49	0.00	0.00	1.00	1.00	1.00
1 if PSNP-*kebele*	0.77	0.42	0.00	1.00	1.00	1.00	1.00
Number of kebeles	140						
Number of woredas	112						

The first row of Appendix Table 6 shows that about 51% of the *kebeles* in this dataset are located in a *woreda* that was targeted by the PSNP. On average, these *kebeles* were in *woredas* that received about five years of aid assistance in the ten years prior to the start of the PSNP. The next three rows shows that there is some heterogeneity in the distribution of whether a *kebele* was in *woreda* that received aid assistance in the three years prior to the start of the PSNP. In the lower panel of the table, the same variables are conditioned on the *woreda* having been targeted by the PSNP. Importantly, the last row shows that 77% of the *kebeles* inside a *woreda* targeted by the PSNP the dataset had also received the programme, which leaves 23% of *kebeles* as a comparison group to described within-districts differences in the variables of interest.

Appendix Figure 5 shows *woredas* by their exposure to the PSNP and on whether they were sampled in the Ethiopian Socio-Economic Survey. Woredas with a salmon shading are those where I also observe that sampled *kebeles* were exposed to the PSNP, whereas the light blue *woredas* were not targeted by the PSNP and were sampled.

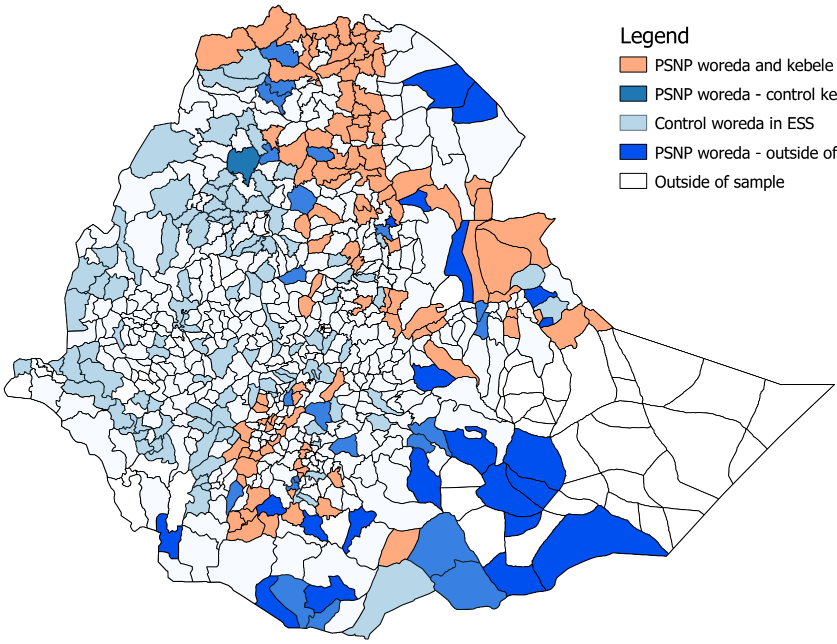

Figure 5: PSNP targeting in the Ethiopian Socio-Economic Survey.[a]

[a] Woredas with a salmon shading are those where I also observe that sampled *kebeles* were exposed to the PSNP, whereas the light blue *woredas* were not targeted by the PSNP and were sampled.

D Appendix tables

Table 7: Summary statistics on additional district covariates

Additional district-level controls and descriptive statistics	PSNP (1)	Control (2)	p-value (3)	Source (4)	Time-Varying? (5)
Fraction Orthodox	0.45	0.62	0.000	1999 LFS	No
Fraction Muslim	0.39	0.27	0.006	1999 LFS	No
Fraction Protestants	0.13	0.09	0.138	1999 LFS	No
Fraction in Other Religions	0.03	0.03	0.907	1999 LFS	No
Fraction Amhara	0.36	0.36	0.973	1999 LFS	No
Fraction Tigryina	0.15	0.01	0.000	1999 LFS	No
Fraction Somali	0.06	0.01	0.023	1999 LFS	No
Fraction Afari	0.00	0.00	0.358	1999 LFS	No
Fraction Oromo	0.28	0.43	0.002	1999 LFS	No
Fraction of other ethnicity	0.15	0.18	0.398	1999 LFS	No
Fraction of households with a death last year	0.06	0.05	0.001	2007 Census	No
Fraction of households with electrcity	0.03	0.02	0.425	2007 Census	No
Fraction of households with a private toilet	0.21	0.19	0.303	2007 Census	No
Fraction of households with a private kitchen	0.42	0.46	0.005	2007 Census	No
Population density (per sq. km)	250	167	0.000	2007 Census	No
Area (sq. km)	1097.34	1099.37	0.982	2007 Census	No
1979-2014 average cumulative Belg season rainfall (mm)	194.43	175.01	0.016	GPCC	No
1979-2014 average cumulative Meher season rainfall (mm)	581.59	816.37	0.000	GPCC	No
1979-2014 average Meher season temperature (°C)	19.44	17.85	0.000	UDel_AirT	No
1979-2014 average Belg season temperature (°C)	20.19	19.79	0.183	UDel_AirT	No
District Observations	215	238			
Individual Observations	31574	26805			

Notes: This table presents means of the district-level controls used in the additional regression models for different samples. Column 1 includes controls for districts that were targeted by the PSNP. Column 2 includes controls for districts that were not targeted by the PSNP (which form the control group). Column 3 presents the *p*-values of the student's *t*-test of equality of means. Standard errors for the student's *t*-test of equality of means are computed assuming correlation of individual observations within each district in a given year. The additional LFS controls are computed using the 1999 Labour Force Survey, with sampling weights adjusted for boundary changes. The sample is restricted to individuals of ages between 17-65, using information from the usual activity reported. Ethnicity and religion questions were not asked in the 2005 and 2013 round. Census controls are calculated aggregating the village-level 2007 census data. Cumulative rainfall is the 1979-2014 mean cumulative rainfall during the rain seasons for the *Meher* harvest (June-October) and *Belg* harvest season (February-May). Temperature is calculated as the 1979-2014 monthly averages for the respective pre-harvest rainy season.

Table 8: Effects on employment participation and sectoral composition by sex

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Employment and occupation effects on women							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
	0.465	1.046	-1.446	-8.729**	8.697**	-0.221	0.065
	(3.738)	(1.006)	(3.512)	(3.973)	(3.872)	(0.470)	(0.200)
Mean Dep. Var.	75.46	2.144	22.37	79.63	17.80	0.584	0.416
Observations	54,770	54,770	54,770	40,792	40,792	40,792	40,792
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B. Employment and occupation effects on men							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
	-1.665	0.746*	0.902	-2.618	2.433	0.227	0.381
	(1.236)	(0.450)	(1.014)	(2.033)	(1.648)	(0.589)	(0.244)
Mean Dep. Var.	82.38	1.727	15.88	83.70	11.81	1.234	0.706
Observations	50,553	50,553	50,553	45,976	45,976	45,976	45,976
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each cell reports an estimate of beta for different dependent variables; standard errors in parenthesis are clustered at the district level. Each column has a different dependent variable.
In Panel A, the sample is restricted to women. In Panel B, the sample is restricted to men. The sample consists of individuals aged 17-65, pooling data from the 2005 and 2013 LFS rounds, in the 453 districts sampled in both rounds. Columns (4)-(7) restrict the sample only to those that are currently employed. Individual observations are weighted by sampling weights that are proportional to district population. All models are estimated using ordinary least squares. The means of district-level and individual-level controls are shown in Table 1. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 9: Within-district analysis of labour supply (extensive margin)

	=1 if employed	=1 if self-employed farmer	=1 if non-farming self-employed	=1 if employee	=1 if temporary worker
Panel A: Differences across woredas					
1 if PSNP-*woreda*	-0.010	-0.024	0.014	0.004	0.011
	(0.037)	(0.038)	(0.023)	(0.010)	(0.009)
Panel B: Difference across kebeles and woredas					
1 if PSNP-*woreda*	-0.028	-0.042	0.015	0.008	0.018
	(0.039)	(0.038)	(0.025)	(0.012)	(0.013)
1 if PSNP-*kebele*	0.028	0.027	0.001	-0.007	-0.014
	(0.026)	(0.027)	(0.017)	(0.014)	(0.014)
1 if household participated in PSNP	-0.076***	-0.075***	-0.010	-0.015*	-0.011
	(0.026)	(0.026)	(0.015)	(0.009)	(0.008)
p-value : *kebele* vs. *woreda*	.274	.186	.698	.505	.201
Unit of obs.	Individual	Individual	Individual	Individual	Individual
# Clusters	228	228	228	228	228
# Obs.	37980	37980	37980	37980	37980
Dep. Var. Mean	.58	.49	.13	.04	.03
Dep. Var. St. Dev.	.49	.5	.33	.19	.17

Notes: Linear probability estimates of the difference in areas targeted by the Productive Safety Net Programme (PSNP). Each panel presents a separate regression model. Outcome variables are listed on top. The unit of observation is the individual. *Woredas* are districts and *kebeles* are wards within them. Pooled 2011,2013, and 2015 rounds of the Ethiopian Socio-Economic Surveys. All models control for survey round indicators, region indicators, the number of years of aid assistance received by the *woreda* prior to 2005, three indicators for whether the *woreda* received aid in 2004. Standard errors are in parentheses and are clustered at the *woreda*-level. Significance levels: *10%, **5%, and ***1%. "p-value : kebele vs. woreda" reports the p-value for a test of equality between the coefficients in the first and second row of panel B. The bottom panel displays the outcome mean, standard deviation, and total number of observations and clusters.

Table 10: Within-district analysis of labour supply (intensive margin)

	Hours worked as self-employed farmer	Hours worked as non-farming self-employed	Hours worked as temporary worker	Hours worked as employee
Panel A: Differences across woredas				
1 if PSNP-*woreda*	-12.449	17.432	1.651	23.816
	(71.997)	(33.394)	(5.529)	(22.837)
Panel B: Difference across kebeles and woredas				
1 if PSNP-*woreda*	-56.720	22.756	-1.458	28.546
	(72.313)	(34.497)	(5.916)	(29.226)
1 if PSNP-*kebele*	72.211**	-11.657	5.474	-7.646
	(36.584)	(17.720)	(4.264)	(12.755)
1 if household participated in PSNP	-87.386**	5.705	-9.104*	-8.154
	(40.325)	(14.009)	(5.526)	(7.500)
p-value : *kebele* vs. *woreda*	.138	.423	.404	.382
Unit of obs.	Individual	Individual	Individual	Individual
# Clusters	228	228	228	228
# Obs.	37736	37736	37736	37736
Dep. Var. Mean	455.18	120.42	23.64	34.11
Dep. Var. St. Dev.	675.22	428.2	180.42	262.63

Notes: Ordinary least squares estimates of the difference in areas targeted by the Productive Safety Net Programme (PSNP) in hours worked across different activities (annualised from a weekly recall). Each panel presents a separate regression model. Outcome variables are listed on top. The unit of observation is the individual. *Woredas* are districts and *kebeles* are wards within them. Pooled 2011,2013, and 2015 rounds of the Ethiopian Socio-Economic Surveys. All models control for survey round indicators, region indicators, the number of years of aid assistance received by the *woreda* prior to 2005, three indicators for whether the *woreda* received aid in 2004. Standard errors are in parentheses and are clustered at the *woreda*-level. Significance levels: *10%, **5%, and ***1%. "p-value : kebele vs. *kebele woreda*" reports the p-value for a test of equality between the coefficients in the first and second row of panel B. The bottom panel displays the outcome mean, standard deviation, and total number of observations and clusters.

Table 11: Within-district analysis of labour demand

	Days of hired labour post-harvest	Days of unpaid labour post-harvest	Daily wages for hired labourers post-harvest	Days of hired labour planting	Days of unpaid labour planting	Daily wages for hired labourers planting
Panel A: Differences across woredas						
1 if PSNP-*woreda*	-1.293	3.963	18.900	-20.896	-1.357	-58.234
	(5.061)	(5.060)	(65.268)	(17.486)	(2.881)	(36.112)
Panel B: Difference across kebeles and woredas						
1 if PSNP-*woreda*	-0.147	-2.371	56.651	-10.696	-3.349	-28.904
	(5.088)	(4.241)	(81.092)	(14.330)	(3.207)	(39.495)
1 if PSNP-*kebele*	-1.766	10.482**	-94.967*	-20.568	4.492	-64.398*
	(2.121)	(4.882)	(50.336)	(15.115)	(3.371)	(36.444)
1 if household participated in PSNP	-2.329*	-5.944*	-49.469**	-12.860	0.398	-15.915
	(1.347)	(3.253)	(20.412)	(11.966)	(2.984)	(52.772)
p-value : *kebele* vs. *woreda*	.773	.048	.214	.576	.168	.568
Unit of obs.	Household	Household	Household	Household	Household	Household
# Clusters	225	225	182	228	228	194
# Obs.	7903	7903	1943	8859	8859	1716
Dep. Var. Mean	11.51	14.64	122.41	27.26	11.65	116.93
Dep. Var. St. Dev.	133.53	93.58	400.12	450.71	68.12	291.6

Notes: Ordinary least squares estimates of the difference in areas targeted by the Productive Safety Net Programme (PSNP) in days of labour (hired or unpaid) and wages paid, before and after harvest. Each panel presents a separate regression model. Each panel presents a separate regression model. Outcome variables are listed on top. The unit of observation is the household. *Woredas* are districts and *kebeles* are wards within them. Pooled 2011,2013, and 2015 rounds of the Ethiopian Socio-Economic Surveys. All models control for survey round indicators, region indicators, the number of years of aid assistance received by the *woreda* prior to 2005, three indicators for whether the *woreda* received aid in 2004. Standard errors are in parentheses and are clustered at the *woreda*-level. Significance levels: *10%, **5%, and ***1%. "p-value : kebele vs. woreda" reports the p-value for a test of equality between the coefficients in the first and second row of panel B. The bottom panel displays the outcome mean, standard deviation, and total number of observations and clusters.

Table 12: Placebo test on employment participation and sectoral composition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. No individual controls							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
	2.987	-1.168	-2.124	3.250	-3.542	-0.356	0.957**
	(2.486)	(1.046)	(2.050)	(2.848)	(2.477)	(0.380)	(0.451)
Mean Dep. Var. (%)	73.73	4.150	22.12	81.16	14.18	1.765	0.702
Observations	159,902	159,902	159,902	116,321	116,321	116,321	116,321
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	No	No	No	No	No	No
Panel B. Individual controls added							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
	3.291	-1.168	-2.124	3.250	-3.542	-0.356	0.957**
	(2.486)	(1.046)	(2.050)	(2.848)	(2.477)	(0.380)	(0.451)
Mean Dep. Var. (%)	73.73	4.150	22.12	81.16	14.18	1.765	0.702
Observations	159,902	159,902	159,902	116,321	116,321	116,321	116,321
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each cell reports an estimate of β; standard errors in parenthesis are clustered at the district level. Each column reports an estimate for a different dependent variable.
In Panel A, each model includes district fixed effects and district controls. In Panel B, each model includes district fixed effects, district controls, and individual controls. Column (4)-(7) are conditional on being employed. The sample consists of individuals aged 17-65, pooling data from the 1999 and 2005 LFS rounds, sampled in 391 districts in each round. Individual observations are weighted by sampling weights that are proportional to district population. All models are estimated using ordinary least squares. The means of district-level and individual-level controls are shown in Table 1. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 13: Effects on employment participation and sectoral composition, controlling for population density

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Somali region excluded							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
	-1.996	0.904	1.119	-6.362**	5.770**	-0.029	0.272
	(2.204)	(0.685)	(1.947)	(2.660)	(2.345)	(0.473)	(0.186)
Mean Dep. Var.	83.29	1.703	15	84.26	11.50	1.340	0.500
Observations	100,731	100,731	100,731	83,319	83,319	83,319	83,319
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B. Population density included as control							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
	-1.867	0.794	1.104	-5.201*	4.011*	0.199	0.378*
	(2.297)	(0.707)	(2.026)	(2.687)	(2.364)	(0.475)	(0.212)
Mean Dep. Var.	83.17	1.701	15.12	84.25	11.54	1.328	0.494
Observations	100,731	100,731	100,731	83,319	83,319	83,319	83,319
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each cell reports an estimate of β for different dependent variables; standard errors in parenthesis are clustered at the district level. Each column has a different dependent variable. In Panel A, the sample excludes districts sampled in the Somali Region from either the 2005 or the 2013 LFS round. This region was not sampled in the 2007 census. In Panel B, district population density (000′ people/sq. km) estimated from the 2007 census, and interacted with a dummy variable equal to one if the year is 2013, is added as a control. The sample consists of individuals aged 17-65, pooling data from the 2005 and 2013 LFS rounds. Columns (4)-(7) restrict the sample only to those that are currently employed. Individual observations are weighted by sampling weights that are constant within a district across time. All models are estimated using ordinary least squares. The means of district-level and individual-level controls are shown in Table 1. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 14: Effects on employment participation and sectoral composition, controlling for pre-PSNP shocks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Interaction with belg rainfall in 2002							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
Estimate of β:	-0.989	0.504	0.495	-7.078**	5.912**	0.160	0.662**
	(2.708)	(0.807)	(2.409)	(3.152)	(2.840)	(0.497)	(0.309)
Coef. On Interaction term:	-1.650	-1.004	2.617	-3.579	1.426	0.567	1.024*
	(3.457)	(0.972)	(3.083)	(4.326)	(4.007)	(0.671)	(0.587)
Mean Dep. Var. (%)	83.17	1.701	15.12	84.25	11.54	1.328	0.494
Observations	105,323	105,323	105,323	86,768	86,768	86,768	86,768
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B. Interaction with emergency assistance received in 2005							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
Estimate of β:	0.162	0.511	-0.627	-6.317**	6.168**	-0.002	0.217
	(2.317)	(0.743)	(2.041)	(2.999)	(2.622)	(0.587)	(0.257)
Coef. On Interaction term:	0.140	0.826	-1.032	3.040	-3.500	0.108	-0.047
	(3.661)	(1.115)	(3.649)	(4.499)	(3.958)	(0.619)	(0.355)
Mean Dep. Var. (%)	83.17	1.701	15.12	84.25	11.54	1.328	0.494
Observations	105,323	105,323	105,323	86,768	86,768	86,768	86,768
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The first row in each panel reports an estimate of β for different dependent variables; standard errors in parenthesis are clustered at the district level. Each column has a different dependent variable. The second row in each panel reports the estimated coefficient of an interaction term with the standardized measure of rainfall for the 2002 *Belg* rainy season (Panel A), and a dummy variable equal to one if the district has received emergency assistance in 2005 (Panel B). The sample consists of individuals aged 17-65, pooling data from the 2005 and 2013 LFS rounds, in the 453 districts sampled in both rounds. Columns (4)-(7) restrict the sample only to those that are currently employed. Individual observations are weighted by sampling weights that are proportional to district population. All models are estimated using ordinary least squares. The means of district-level and individual-level controls are shown in Table 1. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 15: Effects on employment participation and sectoral composition, using the unbalanced sample and without weights

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Individual observations from unbalanced panel of districts							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
	-0.356	0.891	-0.510	-5.673**	5.605**	-0.064	0.262
	(2.227)	(0.640)	(2.030)	(2.447)	(2.169)	(0.429)	(0.166)
Mean Dep. Var.	83.02	1.699	15.27	84.36	11.46	1.331	0.478
Observations	111,674	111,674	111,674	91,676	91,676	91,676	91,676
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B. Individual observations are unweighted							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
	1.571	0.424	-1.991	-5.413**	5.390***	-0.358	0.716**
	(2.650)	(0.506)	(2.591)	(2.246)	(2.020)	(0.333)	(0.349)
Mean Dep. Var.	82.38	1.727	15.88	83.70	11.81	1.234	0.706
Observations	105,323	105,323	105,323	86,768	86,768	86,768	86,768
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each cell reports an estimate of beta for different dependent variables; standard errors in parenthesis are clustered at the district level. Each column has a different dependent variable.
In Panel A, the sample is restricted to women. In Panel B, the sample is restricted to men. The sample consists of individuals aged 17-65, pooling data from the 2005 and 2013 LFS rounds, in the 453 districts sampled in both rounds. Columns (4)-(7) restrict the sample only to those that are currently employed. Individual observations are weighted by sampling weights that are proportional to district population. All models are estimated using ordinary least squares. The means of district-level and individual-level controls are shown in Table 1. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 16: Effects on employment participation and sectoral composition, without controls or district fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. DID estimates: No controls and no district fixed effects							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
	-2.261	0.978	-0.403	-2.704	3.819**	-0.491	-0.754**
	(1.525)	(0.358)	(1.437)	(2.079)	(1.711)	(0.390)	(0.368)
Mean Dep. Var.	83.18	1.7	15.12	84.25	11.54	1.33	0.49
Observations	105,323	105,323	105,323	86,779	86,779	86,779	86,779
District Fixed Effects	No	No	No	No	No	No	No
District Controls	No	No	No	No	No	No	No
Individual Controls	No	No	No	No	No	No	No
Panel B. DID estimates with district fixed effects and no controls							
dependent variable:	Employed	Unemployed	Inactive	Self-employed in agriculture	Self-employed out of agriculture	Private Labourer	Public Labourer
	-2.349	0.323	2.026	-3.247	4.162**	-0.390	-0.772**
	(1.514)	(0.354)	(1.430)	(2.030)	(1.682)	(0.394)	(0.379)
Mean Dep. Var.	83.18	1.7	15.12	84.25	11.54	1.33	0.49
Observations	105,323	105,323	105,323	86,779	86,779	86,779	86,779
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	No	No	No	No	No	No	No
Individual Controls	No	No	No	No	No	No	No

Notes: Each cell reports an estimate of beta for different dependent variables; standard errors in parenthesis are clustered at the district level. Each column has a different dependent variable.

In Panel A, each model does *not* include any district fixed effects or district controls. In Panel B, each model includes only district fixed effects. The sample consists of individuals aged 17-65, pooling data from the 2005 and 2013 LFS rounds. Columns (4)-(7) restrict the sample only to those that are currently employed. Individual observations are weighted by sampling weights that are proportional to district population. All models are estimated using ordinary least squares. The means of district-level and individual-level controls are shown in Table 1. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.