Labour Market Effects of Social Protection Programmes: Evidence from Ethiopia's Productive Safety Net

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Abstract

This paper assesses how a large transfer programme combining public works and unconditional transfers to food-insecure households in rural Ethiopia affects local labour markets. Using repeated cross-sections of the National Labour Force Survey, I show that the programme did not change employment rates or wages in this rural economy. Instead, I find that workers shifted from agricultural to non-agricultural self-employment. I complement this analysis using data from the Ethiopian Socio-Economic surveys and find similar results. These results are at odds with previous work due to the thinness of rural wage markets in Ethiopia.

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1 Introduction

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., forthcoming; Imbert and Papp, 2015; Muralidharan et al., 2021). 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 labourers bear most of the general equilibrium effects of the interventions. However, there is still very little evidence on the potential labour market effects of similar transfer programmes where the labour market may be structured differently. For example, in economies where most agricultural workers are small landowners, landless labourers may play a smaller role in rural labour markets, which might reduce the general equilibrium effects of similar programmes.

This paper examines how a large transfer 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 10 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, my study aims 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).

In the first part of this paper, I estimate a difference-in-differences model to 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 and spanning from 1999 to 2013. 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.

I find no impact on the extensive and intensive margins of labour supply or wages in rural districts targeted by the programme. However, I find some evidence of reallocation of the workforce towards non-agricultural self-employment (5 percent-

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

age point increase) in targeted districts.[2] The reallocation towards non-agricultural self-employment is driven by women in my sample. This result holds across several robustness checks. Using a wave of data before the start of the programme, I run a placebo test that is consistent with the outcomes of interest across targeted and untargeted districts being on parallel trends prior to the start of the programme. Adding additional demographic controls does not affect the results either.

In a second part, I employ another dataset to unpack my main results. I use the Ethiopian Socio-Economic Surveys (ESS), which contain household-level and community-level information on PSNP participation. This dataset advantegeously provides information on which communities (*kebeles*) were targeted by the programme, which I can combine with my district-level programme data. This additional source of data allows me to observe detailed information on individual-level programme participation, but has fewer districts and was not collected before the start of the programme. Using this additional data, I provide descriptive evidence comparing the labour market outcomes of non-beneficiaries living in targeted district relative to individuals living in untargeted communities, within targeted districts. I descriptively find that the reallocation of workers away from agriculture towards other forms of self-employment is larger among non-benefeciaries in untargeted communities in PSNP districts, potentially because 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.[3] 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.[4] More recently, Muralidharan et al. (2021) 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 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.,

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

[3] See Besley and Coate (1992), Ravallion (1991), and Basu (2011) for theoretical treatments of workfare programmes.

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

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, 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 studies are closely related to my analysis: Gazeaud and Stéphane (2020) 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 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 roads, electrification, land-titles and better housing conditions (Asher and Novosad, 2020; Moneke, 2020; Dinkelman, 2011; Franklin, 2020; Field, 2007). Aid or cash transfers have been found to have null or positive effects on the labour supply of recipients. Egger et al. (forthcoming) find that an unconditional cash transfer programme in Western Kenya that injected about 15% of GDP had a positive effect on labour demand. For example, Banerjee et al. (2017) find no evidence of disincentive effects among transfer recipients by combining datasets from 7 randomized control 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).

The remainder of this paper is organised as follows. Section 2 provides details on the targeting of the programme and the main empirical strategy. Section 3 describes the main dataset and outcome variables analysed. Section 4 presents my main results using data from the Labour Force Surveys, along with several robustness checks. I complement these results with an analysis of the Ethiopian Socio-Economic Surveys in section 5. Section 6 concludes.

2 Background and empirical strategy

To study how the PSNP affected labour market outcomes, I exploit the geographic targeting of the PSNP, which was assigned to districts that had received aid assistance in the years prior to the start of the programme. I compare changes in labour market outcomes in districts that were first targeted to receive the programme relative to changes in districts that were left out. I refer to the "control" group as districts that were never included in the programme, and the "treatment" group as those that were officially targeted.
In this section, I briefly describe how PSNP participants are selected. Appendix B provides more institutional details about the programme.

2.1 PSNP targeting

Targeting of PSNP occurs at a geographic and administrative level. The targeted districts were officially selected on the basis of historical food aid receipts prior to 2005.[5] In my analysis, the frequency of aid assistance received (prior to 2005) is an important control variable, proxying for economic opportunities that could directly affect changes in the local economy and that have made a district more likely to receive the programme. Within a targeted district, *woreda* officials select which communities (called *kebeles*) will be targeted by the programme, with priority officially given to *kebeles* with the highest level of identified eligible beneficiaries.[6] 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 3 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.

[5] 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 3 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.

[6] *Kebele*-level targeting takes place on an annual basis, although targeted beneficiaries are generally expected to remain in the programme for 5 years.

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).

The PSNP was first launched in 192 rural *woredas*, in 2005. By the end of 2009, 290 district were reported to be included in the programme. These districts constitute the sample that I regard as being exposed to "treatment". I cannot exploit the staggered roll-out of the programme because my sources of data on labour market outcomes were not collected between 2006 and 2012, so I only have information after the programme had expanded to new districts. In addition, all districts added after 2006 are in the Afar and Somali regions, which are incompletely covered by the surveys used in the analysis, since they are mostly populated by semi-nomadic groups.[7] Figure 1 shows the geographical distribution of the districts targeted by the PSNP by the end of 2009, and that I regard as being exposed to "treatment".

2.2 Main specification

My main identification strategy compares changes over time in targeted districts with changes over time in other districts— rather than using simple differences across district or over time. However, simple difference-in-differences (DID) estimates are likely to be biased if the outcomes in the targeted districts are trending differently relative to those not targeted by the programme. To address this concern, I include control variables that are meant to capture differential dynamics across districts.[8]

I control for the frequency of aid receipts between 1995 and 2004, as this variable was used as a selection criterion for the geographic targeting of the programme. This variable does not overlap with assignment to the PSNP, so it is not redundant.[9] 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.

[7] Several socio-economic surveys of Ethiopia have struggled to get information on these areas. For example the 2007 Census did not sample the Somali region at all. Figure 3 in appendix C shows the geographical coverage of the surveys used in this study.
[8] In appendix table 17 I show the main results without these additional controls.
[9] In the appendix, Figure 4 makes this point visually. Unfortunately, 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.

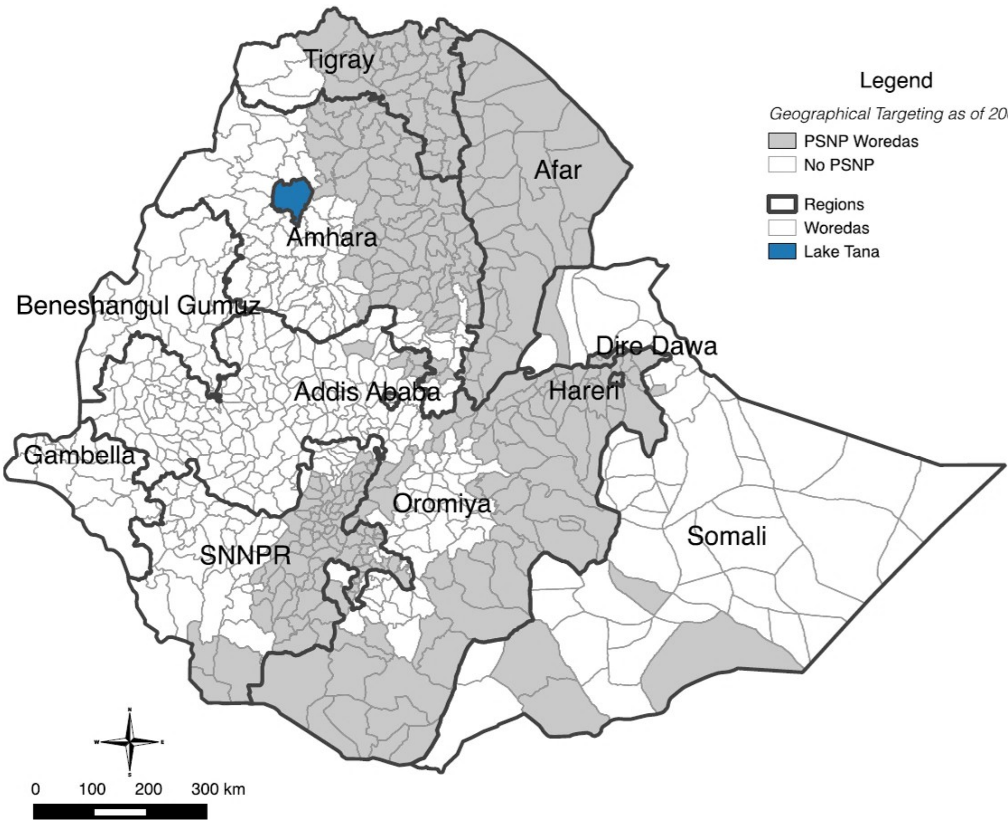

Figure 1: Productive Safety Net geographic targeting[a]

[a] *Notes:* PSNP assignment of 290 *woredas*, as of the end of PSNP Phase II (2007-2009).

I also include district-level controls that are meant to capture initial labour market conditions: the gender-specific labour participation rate, literacy and measures of labour force composition. In particular, I include the share of public sector employees to capture the degree of government involvement in a district. This variable is likely to be correlated with unobserved institutional characteristics that can lead to different programme implementation.[10] I interact these time-invariant controls with an indicator equal to one if the year is 2013[11] to pick up trends that are correlated with the controls.

I also include time-varying district controls to capture some of the unobserved idiosyncratic shock that could have affected labour supply differentially across PSNP and non-PSNP districts, related primarily to weather shocks. I include standardized measures of cumulative rainfall and average temperature for each of the two main agricultural seasons in a year. I also add the first lag of these measures to control for some of the persistence that these shocks may have. Controlling for both temperature and rainfall is important in order the avoid potential omitted variable bias, due to the correlation between these two variables (Auffhammer et al., 2013).[12]

Finally, the district fixed effects are supposed to control for any time-invariant unobserved characteristic that differs across districts, while the time-varying fixed effect should capture any aggregate change that has affect all districts simultaneously in a particular year.

Hence, I run variations of the following linear model:

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

where Y_{idt} is the outcome of interest 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; $\mathbb{1}_{(t=2013)}$ is an indicator equal to one if the year is 2013, which captures any aggregate level covariate affecting all districts in this years, whereas η_d is a district-specific fixed effect that is meant to capture time-invariant unobserved characteristics of districts.[13] Finally, $\epsilon_{1,idt}$ is the unobserved idiosyncratic component in this model,

[10] For example, districts with a higher public sector share of employment may have higher capacity to deliver the programme.

[11] I use this year to define post-programme status. 2013 is the only year after programme roll-out for which I have Labour Force Survey data post-2005. As I only observe one round after the programme started, I am estimating a classical two-period difference-in-differences model, which is not affected by potential biases in staggered difference-in-differences models with more periods (de Chaisemartin and D'Haultfoeuille, 2022).

[12] Summary statistics of the district-level variables, across PSNP and other districts, are shown in Table 1.

[13] I do not include $\mathbb{1}_{(PSNP=1)}$ as a separate regressor because it would be collinear with the district fixed effects.

while β is the main coefficient of interest to be estimated.

Since my regressor of interest (assignment to the PSNP) varies only at the district level, there should not be a need for individual controls to reduce the possibility of omitted variable bias. However, their inclusion can improve the efficiency of the estimator. I include controls for age, age squared, level of education, and indicator variables equal to one if the individual is either female, literate, has any disabilities, or is the household head. To account for selection criteria of the Direct Support component of the programme, I include household-level controls for whether the household head is a woman, the household head's level of education, whether there are any children below the age of 5, or any individuals aged above 70 in the household, or individuals that have a disability.

The model including individual-level controls is simply expressed as:

$$\begin{aligned} Y_{idt} =& \beta \times (\mathbb{1}_{(PSNP=1)} \times \mathbb{1}_{(t=2013)}) + (\mathbf{C}_d \times \mathbb{1}_{(t=2013)})'\delta + \mathbf{X}'_{dt}\theta + \\ & \mathbf{H}'_i\zeta + \eta_d + \gamma \times \mathbb{1}_{(t=2013)} + \epsilon_{2,idt} \end{aligned} \tag{2}$$

where $\mathbf{H}_i$ is a vector of individual controls, and $\epsilon_{2,idt}$ is the unobserved component in this model.

In the main results tables I show estimates of β from estimating both equation 1 and 2.

2.3 Clustering and weighting

Even if the regression model uses individuals as the units of observations, the interpretation of the estimates should be a district-level effect of the PSNP. To achieve this, I follow Imbert and Papp (2015) and adjust my individual observations so that larger districts are not under-represented by the number of observations sampled, as the numbers of observations sampled does not always reflect the population size of the district. The Central Statistical Authority (CSA), which collected the dataset I use in my analysis, provides sampling weights with which it constructs its national and regional estimates of employment statistics. These weights are equal to the inverse probability of being sampled and reflect the different population sizes of the areas where interviews took place. All statistics and estimates computed using the CSA data are adjusted using these sampling weights. I weight individual observations so 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. For robustness, I also present estimates without weights in appendix table 16. Results are not affected by the weighting strategy.

3 Data

With the empirical strategy in mind, I next describe the dataset that is used in the analysis. My primary source of data is the nationally representative Ethiopian National Labour Force Survey (LFS) fielded by the CSA of Ethiopia.[14] I match district identifiers across the latest two rounds of the LFS to construct most of my outcome and control variables. Further, I combine additional datasets containing district-level information on: (i) the geographical assignment of the programme, (ii) the frequency of relief assistance received prior to the PSNP, (iii) rainfall, and (iv) temperature. Additionally, I construct more district-level controls by using the village-level 2007 census data, which I aggregate at the district level.[15] In the appendix, I discuss the source of the covariates and explain how I adjust the LFS dataset to take into account administrative changes in the boundaries of the districts during the period considered in the analysis. The rest of this section illustrates how I selected the sample and constructed the outcome variables used in the econometric models.

3.1 Geographical selection

Since 1999, there have been three rounds of the LFS, which collects information on households living in all regions of Ethiopia. I have access to all 3 cross-sections, which gives me a sample of repeated cross-sections collected in 1999, 2005 and 2013. The main objective of this survey is to compile national employment statistics. Since the PSNP only targeted rural areas in the period I consider, I restrict my sample only to individuals living in rural areas and drop all households living in urban areas.[16]

I construct a balanced panel of 453 rural districts for the 2005 and 2013 rounds. This balanced panel constitutes the main sample on which I test the impact of the PSNP on the district-level labour market. For robustness, I also run my main specification using the unbalanced panel including all 602 rural districts that were sampled in either 2005 or 2013. As for the 1999 round, I am unable to match all the districts sampled in this round with other rounds, because of a lack of district names in the survey data.[17] However, I only use this round as a placebo test to check for

[14] I am hugely indebted to Simon Franklin for giving me access to this source of data.

[15] I only include controls from the census as an additional robustness check. This is because the PSNP, which was launched two years prior to when the census data was collected, could have affected some of the variables that I would have wanted to include as controls in my main regressions, such as population density and housing quality.

[16] The CSA defines as urban 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.

[17] For the main placebo test, I have to drop the unmatched districts, which constitute about 10% of the 1999-2005 total sample, because I cannot match district names as I explain in the data appendix.

differential trends prior to the programme roll-out, shown in table 6.

3.2 Labour market outcomes

My main outcomes are individual measures of employment and occupation categories. I categorize all individuals aged between 17 and 65 as being either currently employed, unemployed or inactive.[18] I define as currently employed those individuals that have reported engaging in a productive activity for at least one hour or more in the week prior to the interview. The hour's cutoff follows the ILO definition of employment (Hussmanns, 2007) that was used in the 2013 round.[19] In line with other research on the Ethiopian labour market, I adopt the partially relaxed definition of unemployment (Franklin, 2014; Broussar and Tekleselassie, 2012). By this definition, an individual is classified as unemployed (and economically active) if not currently employed, yet available for work, even if that person has not searched for work in the last 3 months before the interview. This definition also regards as unemployed those that reported having a job that they can return to and also expressed an intention to work. The departure from the standard definition of unemployment (which requires active job search) takes into account the seasonal nature of rural labour markets and the fact that formal employment opportunities that require job search may be scarce in rural areas. As such, an individual may be considered unemployed even without having actively searched for a job, as long as they stated their willingness to take up a job opportunity. All other individuals are defined as inactive.

In addition, for individuals that are found to be employed, I construct individual indicator variables defining the nature of employment and other variables related to the intensive margin of labour (i.e. hours worked in the last seven days in all productive activities, engagement in additional working activities, and willingness to work more hours) and construct a measure of monthly wages for manual labourers in 2011 prices.[20]

[18] 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)).

[19] The results are not sensitive to this definition, however, between the 2005 and 2013 round, the definition of employment changed slightly, which is why I construct my own measure rather than relying on the CSA's definitions. In particular, employment had been previously defined as being engaged in any productive activity for at least 4 hours of the week. Unpaid household chores such as preparing food, cleaning the house, taking care of children or collecting firewood for own consumption were not considered as economic activities that would count towards employment.

[20] I use regional deflators combining the deflator series from Headey et al. (2012) with the latest public information released by the CSA. There are no deflator series at the district-level.

4 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. These findings suggest that the PSNP did not have a significant impact on the supply of labour on districts that were targeted by the programme. However, I find that the programme shifted self-employed individuals from agricultural to non-agricultural occupations.[21]

4.1 Summary statistics

Table 1 presents the means of the controls used throughout the analysis for the PSNP districts (Column 1) and the districts that were not targeted (Column 2). Column 3 reports the p-value of a students' t-test of equality of means between the means reported in the first two columns, calculated assuming that the standard errors are correlated within a district. Panel A shows that labour market conditions differed between PSNP and non-PSNP districts in 2005, the baseline year of my comparison. PSNP districts tend to have a significantly lower fraction of workers engaged in agriculture and of seasonally unemployed individuals, but a higher fraction of manual labourers and public sector workers.

[21] A theoretical framework is sketched in Appendix A, which is taken from Imbert and Papp (2015) and minimally adapted.

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

Such differences in labour market conditions could be due to two reasons related to the timing of the surveys. Firstly, the outcomes I construct from these surveys are based on the reported activity of the seven days prior to the interview, which took place in March in 2005. Since non-PSNP districts are more likely to harvest only in the primary agricultural season (called *Meher* and concentrated in the second half of the year), we can expect the share of seasonally unemployed individuals in these districts to be higher at the time of the survey. However, the fraction of workers reporting to be seasonally out of work is small (2-3%), compared to the high participation rates (92%). I include the fraction of workers that were seasonally out of work in my vector of time-invariant controls, C_d, in order to account for this potential seasonal pattern due to the different timing of the surveys.

Secondly, the surveys coincide with the major period of PSNP public works. PSNP public works predominantly run between January and June, which means that I may be observing individuals during a period of potential public works activity in both 2005 and 2013. The higher share of public sector workers found in PSNP districts (3%) in 2005 may be an indication that public works could have already started by the time of the survey.[22] Nonetheless, 2005 provides a valid baseline 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.

Reassuringly, observable demographic characteristics tend to be balanced across the two groups of districts. There are no significant differences between the measures of human capital among individuals (e.g. 25-27% of the sample reports having gone to school) or the demographic structure of the households, in terms of age structure or sex. As expected, the frequency of relief assistance prior to 2005 is much higher in PSNP districts, as this variable informed the selection of the districts into the programme. The standardized measures of weather conditions also point out a significant difference across the two groups of the districts, with the PSNP districts having less rain on average relative to the other districts. To account for these unbalanced variables in the analysis, I include the vectors of district controls, X_{dt} and C_d, where the latter is interacted with a dummy variable equal to one if the year is 2013, $\mathbb{1}_{(t=2013)}$. These controls are meant to account for the unobserved trends that could bias the estimates of β, and which may have occurred between 2005 and 2013.

[22] A large 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.

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.

Table 2 reports the baseline means of the outcomes studied for the treated and control districts. Despite the differences highlighted among the control variables, the outcomes of interest are balanced between districts that were targeted by the PSNP and other districts in 2005. In particular, we see that the employment rate is high (around 82%), indicating that most individuals engage in productive activities. Out of those employed, more than 80% reports being either self-employed (or an unpaid family worker) engaged in agriculture (crop, livestock, mixed-farming, or forestry). Self-employed individuals (or unpaid family workers) not engaged in agriculture represent around 10-13% of those employed. Usually, these non-agricultural occupations involve trade or crafts work, and are more commonly undertaken by women. In my main sample, 22% of working women are classified as being self-employed in non-agricultural activities, while the proportion of men is only 6%. Public and private sector labourers constitute a relatively small category of manual labourers earning a wage. Labourers undertake relatively low-skill tasks usually in agriculture or construction work.[23] Public labourers may include PSNP participants, as well as labourers in other publicly funded projects. I can only observe the occupational categories for the main employment activity. As such, these categories can be useful for identifying, for example, transitions to wage employment as the main source of livelihood, but they do not capture the diversified portfolio of activities that individuals in rural Ethiopia engage in (Dercon and Krishnan, 1996).

Even the additional outcome variables related to the intensive margin of labour supply are balanced across the two groups of districts. Importantly, even if the first phase of the project had started, there is no evidence to suggest that it had affected the working patterns of those living in the targeted districts within its first few months of implementation.

4.2 Effects on labour market participation and sectoral occupation

Table 3 presents the main results: the effect of the PSNP on employment and occupation categories across districts. The dependent variable is a different indicator variable for each column. To improve the readability of the tables, the indicator variables are multiplied by 100. Since I am using a weighted linear probability model, the estimated coefficients can be interpreted as a percentage change in the fraction of workers in a particular employment/occupation category in a district. For example, in panel A, column 1 shows a 0.5 percentage points decrease in the fraction of employed individuals, which is not statistically significant from zero at the 10% significance level. Columns 4 to 7 restrict the sample only to workers that are currently employed. Panel A shows estimates of β from the specification outlined in equation (1). Panel B also adds individual controls, as illustrated in equation (2). Focusing on columns 1 to 3: the estimates of β are not significant and mostly small in magnitude, across both specifications. These results imply that the PSNP did not change participation in the labour market in targeted districts between 2005 and 2013. The estimated standard errors appear to be smaller once individual-level controls are included (Panel B), but the coefficients' magnitude is roughly similar. For such a large programme, the lack of an effect on the extensive margin of the labour supply is unlikely to be spurious. The 90% confidence interval of my estimate of the PSNP on employment rate (Panel A) is between -4.3 and 3.2 percentage points, which is reasonably close to zero given the size of the programme.

[23] Activities are defined using the International Standard Classification of Occupation codes in the survey. The codes can be found on `http://tinyurl.com/csa-isco08`, accessed on 09/05/2016.

Table 3: Estimates using the main balanced sample (2005-2013): Effects on employment participation and sectoral composition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. No individual controls							
Dependant 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							
Dependant 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.

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When turning to the last four columns, the results show changes in the composition of the labour force in districts that were targeted by the PSNP. The coefficients in column 5 point to a moderate increase (around 5 percentage points) in the share of workers engaged in non-agricultural self-employment, an effect that is statistically different from zero at the 5% significance level and of considerable magnitude, given that the baseline percentage of workers in this category was 13 percentage points. Workers in districts targeted by the PSNP have shifted towards self-employment activities that are not directly related to agriculture. The jobs defined as self-employment outside of agricultural are predominantly undertaken by women, and usually involve small-scale trade and selling goods at the local market. Other types of construction-related work and crafts-related work play a smaller role in the sample observed.

To better understand the sectoral shifts observed in the results, the appendix table 15 reports the estimated effects of table 3 for different men and women, seperately. This table confirms that the estimated increase in the share of workers engaging in non-agricultural activities is driven by women. All estimated coefficients for men appear not statistically significant, except for a potential increase in unemployment rates, which is only significant at the 10% level (Panel B).

Furthermore, I observe a significant increase in the share of public sector labourers, the category of workers that would be most likely participating in PSNP-funded public works (Column 7). The estimated differential change in the proportion of public sector labourers is 0.29 percentage points in targeted districts, statistically significant at the 10% significance level. This additional finding is consistent with the expected increase in PSNP participants within targeted districts. Even if these employment categories refer only to the primary activity, it is reassuring to observe an increase, albeit small, in public sector labourers in targeted district. It is plausible that PSNP beneficiaries may not report public works as their primary employment activity, which is why I next turn to analyse whether the programme has affected other measures of labour market activity along the intensive margin of the labour supply. The inclusion of the district controls does not drive the main results, as shown in the appendix table 17, where I present the estimates from a simple DID model (with and without district fixed effects), similar to those in table 3.

4.3 Effects on demographic composition and other measures of labour supply

Here, I examine whether the programme has affected the demographic composition of the households or other measures of labour supply (e.g. intensive margin) across districts. Table 4 presents the estimated effect of the programme on different outcome variables. The first three columns report small effects of the programme on in-migration rates and household size that are not statistically different from zero.

The results in the first three columns are support the validity of the estimates presented in the previous sub-section. Had the demographic composition of the districts targeted changed significantly as a result of the programme, any potential increase in the overall level of employment may have been confounded by a simultaneous change in the overall supply of workers in the targeted district. Alternatively, the number of household members may have changed among beneficiaries in order to cope with the work requirements of the public works programmes. For example, if public works participation crowded out other productive activities, young women may be more likely to remain in the household longer (by delaying marriage or reduce early marriages) as a way of increasing the labour supply of the household. However, I find no evidence to confirm such changes of household sizes in targeted districts (Column 1). Further, I do not find evidence of in-migration rates changing differentially, for example due to households moving to targeted districts in the hope of becoming beneficiaries of the programme (Column 2 and 3).

As in the previous table, columns 4 to 7 restrict the sample to those that are currently employed. The results in these columns do not show any significant effect of the programme on the additional measures of labour supply and unemployment. I analyse the intensive margin of labour supply as an additional labour market effects of the programme. Moreover, it is possible that PSNP could have decreased underemployment by providing additional working hours to beneficiaries during the lean agricultural season. However, I do not find evidence of any change in underemployment across targeted districts as a whole, or in hours worked. The estimated coefficients in column 4 are not statistically significant, and are modest in magnitude (indicating a 3-4 percentage point decrease in underemployment). The estimated effects on hours worked in the previous seven days, and on engagement in multiple working activities are small in magnitude and not statistically significant. These results suggest that even if sectoral reallocation may have occurred, this shift does not seem to have been accompanied by changes to the intensive margin of the labour supply across targeted districts. Crucially, they also do not provide evidence that alternative forms of employment have been crowded out by the programme.

Table 4: Estimates using the main balanced sample (2005-2013): Effects on demographic composition and other measures of labour supply and unemployment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. No individual controls							
Dependant 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							
Dependant 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 Table 1.* denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

4.4 Potential effects on the wage of private sector labourers

In Table 5, I focus on the wages of private sector labourers, as this category of workers could be most affected by any general equilibrium effects of the programme. The LFS does not collect information on wages for self-employed individuals, which limits the informativeness of the wage measures in my analysis. I only observe private sector labourers' wages in 1% of my sample, in 81 of the districts sampled in both rounds. Thus, my analysis on wages should be treated as indicative and not representative of rural markets in Ethiopia. However, I still include a brief discussion of the changes in wages for two reasons: first, my original goal was to compare my results with existing studies of similar programmes in other low- and middle-income countries that focus on wage dynamics. Given the limitations outlined so far, such a comparison may be difficult to draw. Second, this discussion highlights the difficulty in using wage-level data for rural areas from national statistics in low-income countries. Policy-makers often neglect that national statistics may not be very accurately measure wage employment in rural areas (Rizzo, 2011).

Due to the potential of selection bias, alongside a steep decrease in the sample size, the results in this table remain illustrative, and no causal relationship is claimed.[24] However, interpreting them as a correlations suggest that the district wage for private sector labourers did not change once the programme was rolled-out. The coefficient in column 1 can be interpreted as a 31% reduction in the level of private labourers' wages in PSNP districts relative to control districts, which seems large, but this effect is not statistically significant at the 10% significance level.[25]

It is hard to assess whether the private labourers' wages I observe can be representative of rural wages in Ethiopia. Rizzo (2011) points out that wage employment in Ethiopia may be more important than what the official statistics (which are compiled using the same surveys I employ in my analysis) tend to suggest. It is possible that workers categorised as self-employed may engage in causal wage work, but since this does not occur close to the interview, it may go under-reported. As an informal check to assess the extent of selection bias within the sub-sample of private sector labourers, columns 2 to 8 report estimates of the effect of the PSNP on the outcomes of interest discussed in the previous subsection for this sub-sample. At least, the sub-sample of labourers does not appear to be affected by the programme, as all coefficients are not statistically significant, although the magnitudes of the coefficients differ relative to the rest of the sample.

[24] Any estimated coefficient of the impact of the PSNP on wages is potentially biased due to the selection in the sample, which is unlikely to be missing at random. For example, if wages were more likely to be observed in districts with better local institutions, which also happened to be districts in our treatment group, and local institutions had evolved over time to raise wages, the impact of the PSNP would be potentially biased upwards.

[25] 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 $100 \times [\exp(\hat{\beta} - 0.5 \times \hat{V}(\hat{\beta})) - 1]$ (even if this technically requires normality of the error terms).

Table 5: Estimates restricted to private sector wage labourers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependant 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.

4.5 Discussion of results

There are two key results that can be inferred from the analysis. First, I find no effect of the programme on the local labour supply, both in terms of intensive and extensive margin. Second, the results show that a significant share of workers, particularly women, tends to shift out of agriculture and into non-agricultural self-employment activities. While I remain agnostic on the potential mechanism that could generate these outcomes, I provide a few plausible explanations.

There are two possible interpretation for the lack effects on the labour supply of targeted districts. One explanation is related to the goals of the programme and how it was designed; another explanation is linked to the structural features of the rural labour markets in Ethiopia. By design, the PSNP is not meant to replace other sources of livelihood among its participants. On the contrary, days of employment are capped for individual participants so that enough time is left for other potential productive activities. Its main objective is ensuring that food insecure household can become self-sufficient, rather than guaranteeing a source of alternative employment. While the programme might stimulate the local economy, it is not meant to boost the creation of new jobs. Alternatively, a theoretical explanation for the lack of responsiveness of labour markets to the programme may be because the labour supply is quite inelastic. Recent experimental evidence from Malawi, by randomly offering daily wages to potential public works participants, estimate a very inelastic labour supply among casual labourers Goldberg (2016). This is in stark contrast to the anecdotal assumption that this elasticity was assumed to be infinite (Lewis, 1954). An inelastic labour supply among rural workers would contribute to an explanation of why I find muted response on extensive margin of work, which would occur even if the PSNP increased the reservation wages among some rural workers.

The second result points to a shift in self-employment activities outside of agriculture observed in PSNP districts that may be consistent with the following explanation. The community assets funded by the PSNP (e.g. new local roads) may have improved market access in these districts and increased the share of the labour force employed in non-agricultural activities that trade in those markets. This explanation runs parallel to the logic of Asher and Novosad (2020), who use a regression discontinuity approach to identify the sectoral changes that can be accounted for by the construction of publicly-funded rural roads in India. Further, Dercon et al. (2009) find that improvements in infrastructure are strongly correlated with increased market participation in rural villages in Ethiopia. The important take-away from my interpretation is that any effect of the programme beyond the targeted beneficiaries does not seem to occur as a result of a distortion in the labour market, like for example crowding out other forms of employment or affecting the reservation wage of workers, but rather through the positive externalities of the assets created by the public works component. Spillovers 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.

4.6 Robustness checks

The main concern that could undermine the validity of the results presented in table 3 is that other factors unrelated to the PSNP may be affecting employment and the fractions of workers engaged in different occupations differently over time. To check the robustness of the estimates in table 3, I use three different strategies. First, I conduct a placebo test replacing the dummy variable $\mathbf{1}_{(t=2013)}$ with $\mathbf{1}_{(t=2005)}$ and estimate equation (1) and (2), pooling data from the 1999 and 2005 LFS rounds. Second, I re-estimate equation (2) by including population density from the 2007 census interacted with a dummy equal to one if the year is 2013. Population density could constitute an important control, but since the census does not pre-date the implementation of the PSNP, it may also be seen a "bad control", as defined by Angrist and Pischke (2008). As a third check, I augment the baseline specification to assess whether pre-programme shocks appear to be biasing the estimates. I do so by including additional covariates that proxy for the potential shocks that occurred prior to 2005 and interacting these covariates with assignment to the programme. I discuss each of these three checks in the rest of this subsection.

4.6.1 Placebo test

The results of the first robustness check are presented in table 6. In practice, I test for changes in employment and occupational categories across targeted and non-targeted districts before the start of the programme. The specifications are the same as in table 3, except that the sample is composed of the 1999 and 2005 rounds and $\mathbf{1}_{(t=2013)}$ is replaced by $\mathbf{1}_{(t=2005)}$.

I do not find statistically significant changes in the extensive margin of labour supply comparing targeted and non-targeted districts between 1999 and 2005. Moreover, there is no indication of any shift of self-employed individuals out of agricultural activities. However, I do observe an increase of 0.9 percentage points in the fraction of public sector labourers, which is large (relative to the overall sample mean of 0.7%) and statistically significant at the 5% level. This effect suggests that the PSNP public works had already started in targeted districts by the time of the 2005 LFS round. Hence, as previously noted, the estimates in table 3 should be interpreted as the additional effects of the programme after its initial adjustment phase in 2005. A caveat of this placebo test is that it is estimated using a sub-sample of the data used in the main analysis. In particular, I have to restrict observations contained in 391 districts, rather than 453, as not all districts were sampled in all 3 rounds of the LFS, or could be matched unambiguously between 1999 and 2005.[26]

[26] Data appendix C explains the issues I had with matching the 1999 round with the other two rounds.

Table 6: Estimates using the placebo balanced sample (1999-2005): Effects on employment participation and sectoral composition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. No individual controls							
Dependant 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							
Dependant 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.

4.6.2 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). Moreover, as table 14 shows, population density appears to be much higher in districts that are targeted by the PSNP. Hence, population pressure is likely to be a potential omitted variable that could bias my results, by differentially affecting targeted districts over time.

The population data available comes from the 2007 village-level census of Ethiopia, meaning that this variable may have responded to district targeting, thus being a bad control. However, the results in table 4 show that demographic composition of districts did not change in targeted districts in terms of household size or in-migration. As such, I add as a control to equation (2) the district-specific population density, as measured by the 2007 district-level population density, interacted with $\mathbf{1}_{(t=2013)}$.

The results of this exercise are presented in table 7. Before adding population density as a control, I estimate equation (2) removing observations from the Somali region, with the results shown 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.

The coefficients in Panel A are similar to those in table 3, which confirms that omitting the Somali region does not strongly affect the main results. Interestingly, after population density is added as a control, both the magnitude and significance of the coefficients drops, particularly for columns 4 and 5. This result suggests that the baseline controls were omitting some relevant explanatory variable correlated with population density. Alternatively, it may be indicative of the fact that population was differentially affected by the PSNP between 2005 and 2007. While the results remain consistent with those presented in table 3, this robustness check suggests that the magnitude of my results may be about 1 percentage point smaller once we account for changes in population dynamics.

Table 7: Estimates on employment participation and sectoral composition, adding population density as a control

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Somali region excluded							
Dependant 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							
Dependant 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.

4.6.3 Testing for heterogenous effects due to pre-programme shocks

As a third robustness check, I try to assess the potential failure of the parallel trends assumption by augmenting the baseline model with a district-specific variable that is likely to be correlated with pre-programme shocks. I do so since it is possible that the pre-PSNP shock may have affected labour market outcomes in 2005 differentially in PSNP districts. In other words, I explore whether districts that were targeted by the programme, and were more likely to have suffered from a pre-programme shock, appear to respond differently to PSNP assignment. Specifically, I estimate the following model on the main sample:

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

where W_d is a district-specific time-invariant characteristic that is likely to be correlated with a shock prior 2005.[27] I use two measures of W_d: the standardized cumulative rainfall for the 2002 *Belg* season, and a dummy variable equal to one if the district continued to receive relief assistance in 2005. The first measure reflects the fact that districts that suffered from the 2003 drought were generally those that had received little rain in the 2002 *Belg* season (Gill, 2010). The second measure is meant to capture the fact that since some of the districts that were targeted by the PSNP may have still required emergency assistance to deal with food insecurity, those districts may also have been more likely to suffer from a pre-programme negative shock.

Table 8 reports the estimates of β and β_2 from estimating equation (3). Including the interaction terms does not alter the estimates relative to the results in table 3. Most estimates of β_2 are not statistically significant, while 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. Assuming that my measures of W_d successfully capture pre-programme shocks, the estimated results do not point to a failure of the parallel trends assumptions because of pre-programme shocks.

[27] I omit the terms $(\mathbb{1}_{(PSNP=1)} \times W_d)$,$W_d$ and $\mathbb{1}_{(PSNP=1)}$ since they would be collinear with η_d.

Table 8: Estimates using the main balanced sample (2005-2013) on employment participation and sectoral composition, adding interaction terms with pre-PSNP shocks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Interaction with belg rainfall in 2002							
Dependant 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							
Dependant 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.

4.6.4 Additional checks

The appendix tables present three additional robustness checks, and I briefly comment on them here. In table 16, I check that my results are not driven by the choice of weights or by restricting the sample only to districts that were sampled in both the 2005 and 2013 rounds. Practically, I re-estimate equation (2) by including all individual observations in the 601 districts sampled in the either of the the 2005 or the 2013 LFS rounds (Panel A); and do not weight individual observations (Panel B). The sign and magnitude of the estimates remain similar to the ones reported in table 3. Removing weights (Panel B) decreases the standard errors of my estimates relative to the main results, even if weighting was intended to increase the precision of my estimation procedure.[28] Adding additional districts to the estimation (Panel A) has the expected effect. It does not change the main estimates (that were only estimated from observations in the balanced sample of districts), but it reduces the magnitude of the standard errors, thanks to the higher sample size used to estimate the control variables of the model more efficiently.

As a final check, in table 17, I present the effect of the PSNP on the main outcomes of interest without any controls (Panel A) and including only district fixed effects (Panel B). Including district-level controls appears to remove a potential bias due to the selection criteria, as the signs of the coefficients, which are not statistically significant, are different relative to those in table 3. Notably, the increase in the share of workers engaged in non-agricultural self-employment remains statistically significant even without additional controls in the basic difference-in-differences model. Overall, the results presented in table 3 remain broadly robust to the checks performed in this section.

5 Unpacking within-district heterogeneity[29]

In this final empirical section, I employ a panel dataset from the Ethiopian Socio-Economic Surveys (ESS), to complement my difference-in-differences analysis.[30] In particular, I exploit the fact that this dataset allows me to observe *within*-district variation in PSNP recipients at both the *kebele*-level and individual-level.

Not all *kebeles* in targeted districts were targeted by the programme. The *kebeles* were not randomly selected by the government, but were selected based on the list of food insecure households compiled by the local administrators. As such, controlling for the geographic targeting rule of districts *cannot* fully account for the selection of PSNP below the district-level. Table 9 summarises the main geographic targeting variables at the district-level that were merged with the ESS data, which

[28] Solon et al. (2013) note that weighting may harm precision if the intra-group (district) correlation makes up a large proportion of the variance of the error term.

[29] As noted in my thesis outline and progress, I plan to further develop the empirical strategy using the data described in this subsection for the final version of this chapter.

[30] The panel consists of three waves collected in 2011, 2013 and 2015.

are further described in appendix C. In this table, the unit of observation is the *kebele*.

Despite this limitation, this section provides descriptive analysis that can complement the results in section 4. In particular, it can illustrate whether the *woreda*-level effects of the programme are driven by changes within-targeted communities (*kebeles*) or whether these effect may due to changes in untargeted *kebeles* within targeted districts. In this subsection, I employ the same *woreda*-level targeting indicator obtained from the project implementation manuals of the earlier phases of the programme, rather than relying solely on the ESS data to measure district-level assignment to the PSNP. Whereas, the *kebele*-level targeting comes from the Community-level database of the Ethiopian Socio-Economic Surveys. As in the main analysis, the sample focuses on individuals living in rural areas, excluding *kebeles* located in medium and large-sized town.

Table 9: PSNP Geographic Targeting Variables in the sample of the Ethiopian Socio-Economic Surveys

	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 the table 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

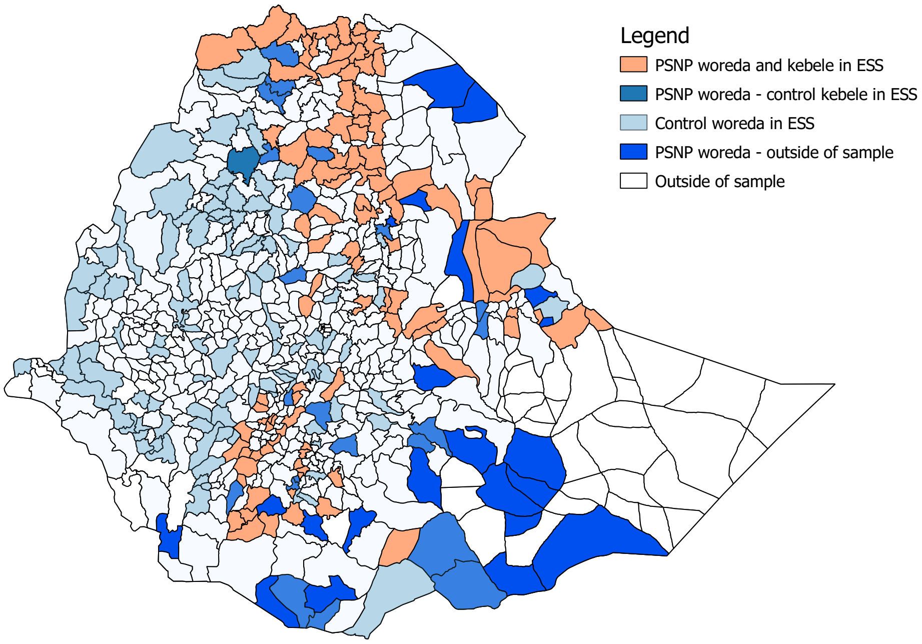

Figure 2: *Woredas* (districts) and *kebeles* (communities) by exposure to the Productive Safety Net Programme (PSNP) sampled in the Ethiopian Socio-Economic Survey (ESS).

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.

Figure 2 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.

I estimate the relationship between PSNP-assignment at the woreda-level and labour market and agricultural investment decisions, using the first three rounds of the Ethiopian Socio-Economic Surveys. Because this data was collected after the PSNP programme was launched, I cannot rely on my earlier difference-in-differences strategy. However, I can still control for the targeting rule variables, to account for the selection at the *woreda*-level.

I first estimate variations of the following linear model:

$$Y_{irwkt} = \beta \times \mathbb{1}_{\{PSNP=1\},w} + \mathbf{X}'_w\theta + \eta_r + \gamma_t + \epsilon_{1,irwkt} \tag{4}$$

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 in this model, while β is the main coefficient of interest to be estimated.

Second, to study how the programme may have influenced the labour market decisions in untargeted communities inside targeted *woredas*, I extend the model in equation 4 to include an indicator for whether the *kebele* participated in the PSNP according to the Community-level respondent. This model would illustrate whether non-beneficiaries in targeted district appear different from non-beneficiaries in untargeted districts.

$$\begin{aligned} Y_{irwkt} =& b_1 \times \mathbb{1}_{\{PSNP=1\},w} + b_2 \times \mathbb{1}_{\{PSNP=1\},k} + b_3 \times \mathbb{1}_{\{PSNP=1\},i} \\ &+ \mathbf{X}'_w \theta + \eta_r + \gamma_t + \epsilon_{2,irwkt} \end{aligned} \tag{5}$$

Throughout the next four tables, panel A reports estimates of β from model 4 across different outcomes using the ESS dataset. Each column presents this partial correlation for different outcome. Whereas panel B reports estimates of b_1, b_2, b_3 from model 5. 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 p-value testing the equality of these two coefficients is reported below panel B.

5.1 Descriptive results

I analyse four sets of outcomes. First, I describe the relationship between labour supply and targeting of the programme. I use the individual-level dataset to categorise individuals into different four main occupations. Next, I turn to the intensive margin of labour supply, looking at the relationship of hours worked in different occupations and programme targeting. These outcomes are similar to the ones I previously analysed using the National Labour Force Survey data. On the labour demand-side, I describe patterns on the different labour requirements across planting and post-planting seasons (both paid and unpaid), as well as the wages paid to these labourers by households.[31] The sample of wage-paying households becomes more selected, but it is still an important sub-group to study, if equilibrium effects of the programme exist. Finally, I turn to potentially complementary mechanisms that may have occurred alongside labour market changes. In particular, I focus

[31] Wages are reported in nominal Ethiopian Birr.

on the households' decision to invest and use fertiliser, and also engage in watershed activities, which is one of the type of activities that public works programme stimulate.

Table 10: Results - 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.

I do not find large or statistically significant difference in the extensive margin of labour supply between PSNP-targeted and untargeted *woredas*. In panel A of table 10, the overall change in the probability of working at least an hour a week is 1 percentage point smaller in PSNP woredas, but the difference is not statistically significant. Across different occupation, I find a small (2 percentage points) negative difference in the proportion of workers self-employed in agriculture in PSNP-woredas, alongside a 1 percentage point increase in the share of workers self-employed outside of agricultural and private temporary employment. While none of these differences are statistically significant, or causal, they are descriptively consistent with the results from my previous analysis, which found a 5 percentage point (decrease) increase in the share of workers self-employed (in) outside of agriculture. The smaller magnitude of the results presented in table 10 compared to my earlier analysis suggest a potential downward selection bias among *woredas* targeted by the PSNP.

Table 11: Results - Labour Supply (Intensive Margin)

Panel A: Differences across woredas	Hours worked as self-employed farmer	Hours worked as non-farming self-employed	Hours worked as temporary worker	Hours worked as employee
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.

The small positive differences described in panel A in the share of individuals employed outside of agriculture seem to be mostly present among individuals in untargeted *kebeles* within targeted *woredas*, as shown in the first row of panel B. PSNP-beneficiaries are less likely to work than non-PSNP beneficiaries, which in turn reduces the probability of them reporting to be in agricultural self-employment. Turning to panel B of table 10, I find a statistically significant difference in the extensive margin of labour supply among PSNP-beneficiaries. I do not find large or statistically significant indirect differences among non-beneficiaries, both within PSNP-targeted and untargeted *woredas*.

Table 12: Results - 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.

The differences in the intensive margin, in terms of time spent working, are consistent with results on the extensive margin. As shown in table 11, we see that individuals in *woredas* targeted by the PSNP are overall spending marginally fewer hours in agricultural self-employment and marginally more hours in other occupations. Though, none of these differences are statistically significant. In panel B, the differences become more accentuated and remain consistent with the patterns of the extensive margin. I find a negative difference in the time spent working in agricultural self-employment among individuals in untargeted *kebeles* but within PSNP-*woredas* as well as individuals from households that directly benefited from the programme. The former difference is not statistically and corresponds to about 8% of the mean, whereas the latter corresponds to about 13% of the mean annualised hours spent working in self-employment. On the other hand, non-beneficiaries in targeted *kebeles* spend 72 more hours annually working in self-employment. Non-beneficiaries in *kebeles* not targeted by the programme, but in targeted *woredas*, report working 18% more of the average amount of time, but this difference is not statistically significant.

To better understand these changes in the intensive margin of labour, I turn to the analysis of agricultural labour demand, given the agricultural sector is the most important economic sector in rural economies. Labour demand is split into unpaid and hired labour across the planting and post-harvest seasons. The public works of the PSNP are most frequently planned outside of these seasons, to avoid putting pressure on the labour market.

Table 13: Results - Agricultural Investments

	Fertiliser purchased	Days spent in watershed activities	Fertiliser used
Panel A: Differences across woredas			
1 if PSNP-*woreda*	-21.167	1.161	2.949
	(75.601)	(2.412)	(48.019)
Panel B: Difference across kebeles and woredas			
1 if PSNP-*woreda*	-19.688	-0.655	-7.223
	(77.537)	(2.766)	(43.838)
1 if PSNP-*kebele*	-9.679	2.661	15.986
	(59.284)	(1.838)	(21.771)
1 if household participated in PSNP	-43.130	-0.158	-25.037**
	(40.180)	(1.253)	(12.419)
p-value : *kebele* vs. *woreda*	.926	.401	.56
Unit of obs.	Household	Household	Household
# Clusters	228	214	228
# Obs.	7510	3909	6114
Dep. Var. Mean	197.4	16.92	88.88
Dep. Var. St. Dev.	891.21	13.71	635.27

Notes: Ordinary least squares estimates of the difference in areas targeted by the Productive Safety Net Programme (PSNP) in fertiliser purchases, use, and days spent on watershed activities. 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. *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.

I do not find significant differences between targeted and untargeted *woredas* in outcomes related to labour demand. Panel A of table 12 shows that differences in the amount of days hired of labourers is lower in *woredas* targeted by the PSNP. This difference is larger (almost 70% of the sample mean) during the planting season, but not statistically significant. Wages during the post-harvest season, though relative rarer in the sample, are 14% higher than the sample mean in targeted *woredas* relative to untargated ones, but again this difference is not statistically significant. The difference in planting wages is negative and corresponding to about 50% of the sample mean, but not statistically significant.

Differences in labour demand are larger within targeted *kebeles*, among the non-beneficiaries of the PSNP programme. Households without PSNP-beneficiaries in targeted *kebeles* demand more time of unpaid labourers in the post-harvest season relative to non-beneficiaries in other *kebeles*, both outside and within the same *woreda*. This pattern is consistent with the earlier finding of an increase among individuals working in agricultural self-employment in this category. This pattern is accompanied by a decrease in the demand for hired labourers and a subsequent lower wage paid to them. The difference is large (almost 77% of the sample mean) but it should not be interpreted causally. These households may have very few hired labourers for reasons that are not related to the PSNP. In the last row of panel B, we also see that households with PSNP-beneficiaries demand less labour relative to non-beneficiaries, which could potentially be explained by the smaller size of their plots.

Aside from labour markets, other inputs and decisions may have been locally affected by the income generated from the programme or through a substitution in the factors of production. However, I do not find evidence of large changes in some of these inputs and decisions. Table 13 shows that households in PSNP-*woredas* purchase a smaller amount of fertiliser relative to households in non-targeted *woredas*, with the smallest amount being purchased by PSNP-beneficiaries, who might be most credit-constrained. These differences remain not statistically significant. Non-beneficiaries in targeted *kebeles* spend more days investing in watershed activities, which may be related to the public works created by the PSNP. However, this difference (about 15% of the sample mean) is not statistically significant. Finally, in terms of actual fertiliser use, I report (consistent with lower labour demand) a lower amount of fertiliser used among PSNP-beneficiaires relative to non-beneficiaries in untargeted *woredas*. Unlike for the fertiliser purchased, non-beneficiaries in targeted *kebeles* report a marginally higher, though not statistically significant, amount of fertiliser used.

6 Conclusion

Using a difference-in-differences approach, this paper has analysed the impacts of the Productive Safety Net Programme (PSNP) on rural labour markets in Ethiopia. My contribution has been empirical in nature. After compiling a large dataset with information on rural labour markets, I estimate the effects that the programme may have had on the labour market conditions of this rural economy. There are two results that stand out from my analysis: Firstly, the extensive and intensive margins of the labour supply were unaffected by the expansion of the programme. Secondly, districts targeted by the PSNP had a higher share of self-employed individuals engaging in non-agricultural activities.

The results suggest that labour supply is mostly unaffected across targeted districts. One way of interpreting this result is to conclude that the PSNP is not a substitute for policies that are meant to stimulate job creation and growth. Its primary objective remains that of ensuring the food security of its beneficiaries. As such, my results are complementary to earlier evaluations of the programme, which focussed on assessing its ability to protect participants from shocks to their livelihoods.

I also find little evidence consistent with the public works having crowded out other private sector activities at the district-level. I do, however, find that one of the secondary programme objectives may have been achieved. Namely, it is plausible to conclude that the productive assets created by the PSNP projects may have improved market access, shifting workers into non-agricultural self-employment activities like petty trading.

The results presented on the potential impact on wages remain mostly illustrative in nature. While I set out to mimic previous studies of similar programmes from other countries (primarily India), the context of my analysis proved to be significantly different due to the nature of the Ethiopian rural labour market. This difference is either due to the structural thinness and lack of wage opportunities in rural areas, or, as some have argued, due to the coarse nature of the labour force survey data (Rizzo, 2011). As such, any analysis of rural wage dynamics relying on these datasets is limited.

This limitation need not discourage future researchers from attempting to investigate other potential general equilibrium effects of a programme like the PSNP. In particular, this paper has left open the potential to investigate whether urban labour markets may have been affected by the rural programme, potentially through changes in the rural-urban migration patterns. Extending the analysis presented in this study to consider urban-rural linkages would also have the advantage of generally being more likely to observe a larger sample of wage workers (Franklin, 2014). The potential and demand for future research may also arise from contemporary developments. At the end of 2015, for example, the Ethiopian government launched the Urban Productive Safety Net. The expansion of safety net programmes in an urban context raises policy-relevant questions that may diverge significantly from policies based on rural dynamics and data, as found by Abebe et al. (2021). My findings can contribute to the research on Ethiopia's rural context as a comparison to the findings from the Urban Productive Safety Net.

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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 Basics

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

$$F_i(D_i) = A_i G(D_i) \tag{6}$$

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:

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

$$\begin{aligned} c_i &= y_i + \tilde{W}_i L_i^s \\ y_i &= \pi_i \\ &= A_i G(D_i) - \tilde{W}_i D_i \end{aligned} \tag{7}$$

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 , given separability of consumption and production decisions, households will set the marginal product of labour equal to the shadow wage:

$$A_i G'(D^*) = \tilde{W}_i \tag{8}$$

A.2 Competitive labour markets

Suppose that labour markets are competitive, such that: $\tilde{W}_i = W$, the shadow wage that measure 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 Labour markets 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:

$$y_i = \pi_i + (W_g - \tilde{W}_i)L_i^g \tag{9}$$

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:

$$\underbrace{p\int_{\underline{A}}^{\phi(pW)} [L_i^s(pW) - D_i(pW) - L_i^g]dA_i}_{\text{low-productivity suppliers}} = \underbrace{\int_{\phi(W)}^{\overline{A}} [D_i(W) - L_i^s(W) + L_i^g]dA_i}_{\text{high-productivity buyers}} \tag{10}$$

To understand the equilibrium effects of public works, we would want to totally differentiate equation (10) 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:

$$\frac{dW}{dL^g} = \frac{E_1 - E_2}{-E_3 + E_4} \tag{11}$$

where:

$$E_1 = p\int_{\underline{A}}^{\phi(pW)} \frac{dL_i^g}{dL^g}dA_i + \int_{\phi(W)}^{\overline{A}} \frac{dL_i^g}{dL^g}dA_i > 0 \tag{12}$$

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.

$$E_2 = p\int_{\underline{A}}^{\phi(pW)} \frac{dL_i^s}{dy_i}(W_g - pW)\frac{dL_i^g}{dL^g}dA_i + \int_{\phi(W)}^{\overline{A}} \frac{dL_i^s}{dy_i}(W_g - W)\frac{dL_i^g}{dL^g}dA_i \tag{13}$$

E_2 is the effect on aggregate labour supply through non-labour income. (I think) 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 (11) 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 (11) 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.

$$E_3 = p^2\int_{\underline{A}}^{\phi(pW)} D'(pW)dA_i + \int_{\phi(W)}^{\overline{A}} D'(W)dA_i \tag{14}$$

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

$$E_4 = p^2 \int_{\underline{A}}^{\phi(pW)} [\frac{dL_i^s}{dW}|_u + \frac{dL_i^s}{dy_i}(L_i^s - D_i - L_i^g)]dA_i + \int_{\phi(W)}^{\overline{A}} [\frac{dL_i^s}{dW}|_u + \frac{dL_i^s}{dy_i}(L_i^s - D_i - L_i^g)]dA_i \qquad (15)$$

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 (11) 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, aid 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.[32] 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[33] 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.[34] 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.[35] 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 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.[36] It has grown

[32] 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.

[33] 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.

[34] 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.

[35] 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.

[36] The World Food Programme covers implementation in the Somali region.

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.[37] Hence, the actual number of targeted districts, relative to the 2005 administrative boundaries, had not actually increased by 2006. Figure 3.1 provides a timeline of the programme phases during the years I analyse.

FIGURE 3.1: TIMELINE OF THE PSNP AND DATA SOURCES

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 and major shocks	Labour Force Survey 2005 Round		2007 Census	Food Price Spike			Drought in East Africa		Labour Force Survey 2013 Round

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 3 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,

[37] I refer to the conversation I had with World Bank Officials in January 2016.

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 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.[38] 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.[39]

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 the local market.[40] By design, this level is below the usual market wage for unskilled labourers

[38] 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.

[39] 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.

[40] 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).

(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.[41]

[41] 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. 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.[42] 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[43], 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 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).

[42] Population estimates are calculated from the 2007 census.

[43] Available at `http://tinyurl.com/ocha-map13`, accessed on 09/04/2016.

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.

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.[44] 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

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), GFDRE (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 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.

Historical frequency of food aid

As noted in the background section, districts were targeted based on their historical receipt of food aid prior to 2005. To take this into account in my specifications, 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[45] of the GFDRE. I personally collect this data in a trip to Addis Ababa in January 2016. This information is shown in Figure 4. When compared to the geographical distribution of the targeted *woredas* (Figure 3.2), there is a broad overlap, but this is not perfect. There are several districts in targeted regions, particularly in Oromiya and SNNPR, that had received relief assistance prior to 2005, but were not targeted by the programme.

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

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

[45] 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.

gridded data sources. Gridded data, which interpolates readings from weather stations with a statistical model, are frequently used by economists.[46] 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); Theisen (2012), and were chosen because of their geographic scope and long time scale.[47] 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 covered by each cell.[48]

Other controls, which I do not include in the main regressions (but that are shown in appendix table 14) 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.

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

[47] 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.

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

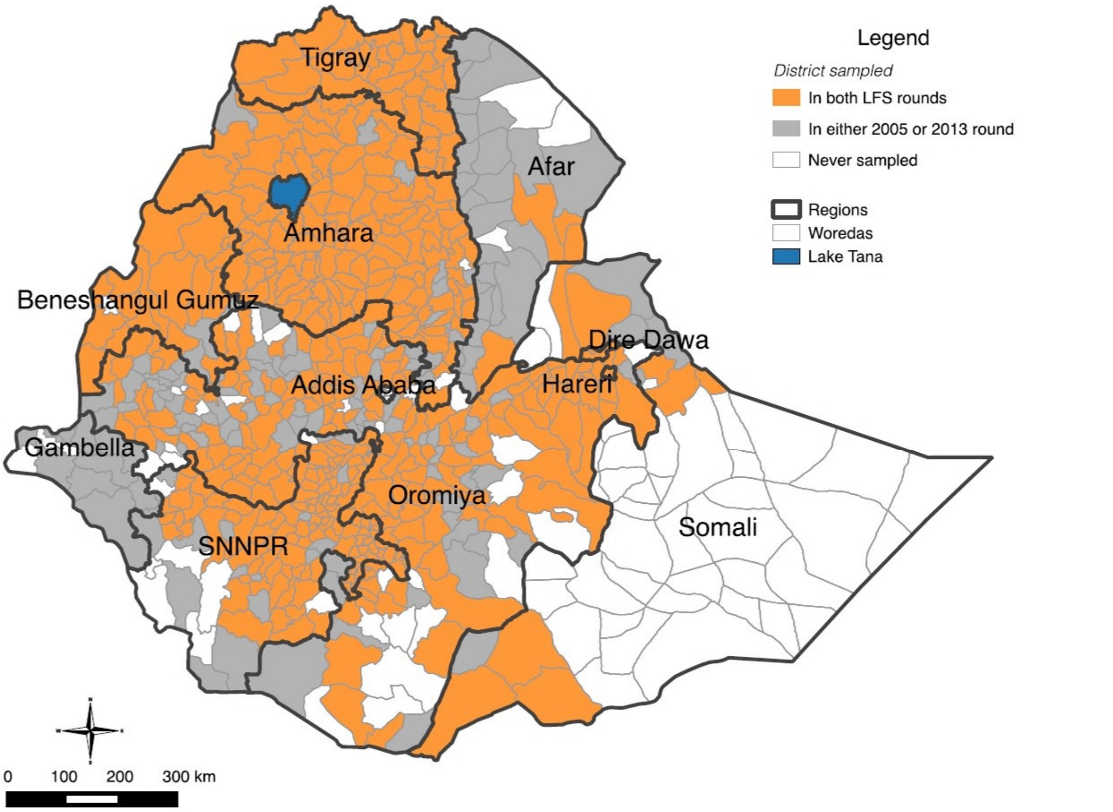

Figure 3: District Balance in the Labour Force Survey

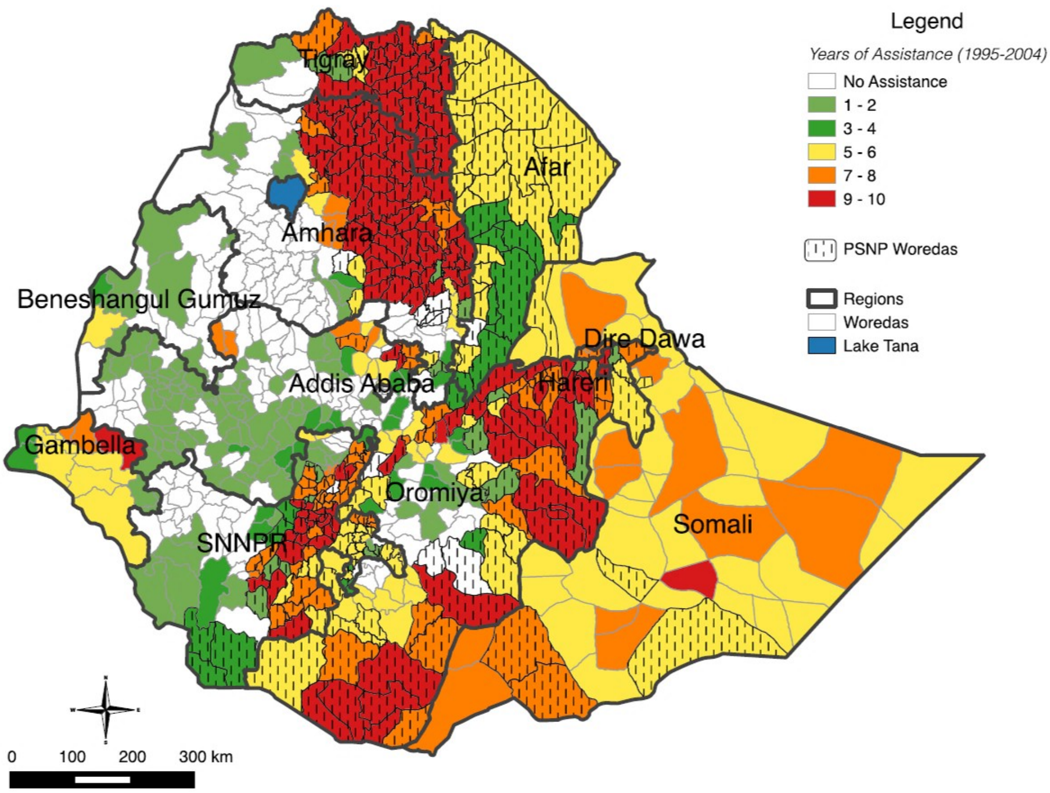

Figure 4: Cumulative years of aid receipts and Productive Safety Net geographic targeting overlapped[a]

[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.

D Appendix tables

Table 14: Additional District Controls and Descriptive Statistics

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 15: Estimates of the DID estimator on employment participation and sectoral composition by gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Employment and occupation effects on women							
Dependant 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							
Dependant 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 16: Estimates of the DID estimator
unweighted and using all districs from the 2005 and 2013 rounds

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Individual observations from unbalanced panel of districts							
Dependant 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							
Dependant 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 17: Estimates of the DID estimator with no controls, and only with district fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. DID estimates: No controls and no district fixed effects							
Dependant 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							
Dependant 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.