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

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Abstract

This thesis assesses how a large safety-net programme in rural Ethiopia affects local labour markets. Combining repeated cross-sections of the National Labour Force Survey with information on the geographical targeting of the programme, a difference-in-differences approach is used to estimate whether employment outcomes have changed differently over time in targeted districts relative to those that did not receive the programme. The results suggest that a combination of public works employment and unconditional transfers to food-insecure households did not change the overall employment in this rural economy. I also find no evidence that the programme affected the wages of private sector labourers. However, my results show that the programme increased the share of workers self-employed in non-agricultural activities, substituting them away from self-employment in the agricultural sector. I test whether these results are due to differential trends, with a placebo test, or different population composition across districts, but do not find evidence to support these alternative explanations. I interpret the shift in employment out of agricultural activities in targeted districts to be related to the infrastructure projects created through the public works component of the programme, which is likely to have stimulated local market participation among women. Any effect of the programme beyond the targeted beneficiaries does not seem to occur as a result of a distortion in the labour market, through for example the crowding out other forms of employment or a change in the reservation wage of workers, but rather through the positive externalities of the assets created by the public works component. My conclusion is that the labour market effects of social programmes documented in similar studies from other developing countries outside Africa are less likely to be observed in the Ethiopian rural context, where wage markets are thinner.

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Contents

1 Introduction

1.1 Motivation

1.5 billion people, over half of the global working population, engage in farming or self-employment (World Bank, 2013). The majority of these individuals live in rural areas in developing countries, where natural disasters and other economic shocks frequently exert negative pressure on their livelihoods.[1] Social safety nets are crucial policy instruments that have been used to protect livelihoods in the face of these adverse shocks (Subbarao, 2003).

To properly evaluate the effects of these policies in developing countries, it is important to move beyond the effects on the treated population, and also determine the general equilibrium effects that may occur (Acemoglu, 2010).
Recent studies from India and Bangladesh have shown that policies aiming to increase the welfare of poor individuals can also affect non-participants through changes in the labour market equilibrium. For example, programmes offering skill and asset transfers, rainfall insurance, resettlement, or guaranteed employment schemes may affect labour decisions and wages of individuals not directly targeted by the interventions.[2]
However, there is still very little evidence on the potential labour market effects that similar policy interventions can have in developing countries outside South Asia. A common theme across these recent studies is the fact that landless labourers tend to bear most of the general equilibrium effects of the interventions. But how relevant are these recent findings for policy-makers in other developing countries, particularly where labourers may play a smaller role in the economy?

This paper attempts to answer this question. I investigate the effects that a large social safety net programme has had on rural labour markets in a Sub-Saharan context. The focus is on Ethiopia's Productive Safety Net Programme (PSNP).

The PSNP provides a worthwhile example to study because, unlike many other social programmes in Sub-Saharan Africa, it reaches a very large number of participants- in fact, it is the largest rural social protection system in Sub-Saharan Africa, excluding South Africa (Gilligan et al., 2009).[3] In 2005, its first year of operations, it was estimated to have reached

[1] See, for example, Dercon (2009), Fafchamps (2003), Townsend (1994), and Jayachandran (2006).
[2] For Bangladesh see Bandiera et al. (2016) and Bryan et al. (2014). For India see Mobarak and Rosenzweig (2014) and Imbert and Papp (2015).
[3] In South Africa, the Children Support's Grant reaches over 16 million beneficiaries, http://tinyurl.com/sassa160416, accessed on 16/04/2016.

4.5 million beneficiaries; in its current phase, it is expected to reach 10 million beneficiaries per year, around 10% of Ethiopia's population (World Bank, 2014). It combines various types of interventions in order to reduce food insecurity among vulnerable households. Cash or food transfers conditional on public works participation make up the largest proportion of its operations.

Its impressive scale has contributed to making it a frequently used reference in international comparisons of similar programmes in policy circles.[4] As such, rigorous evaluations of this programme can provide insights that are of significance both within and outside the Ethiopian context.

1.2 Contribution

There are at least two common stumbling blocks preventing researchers from evaluating the general equilibrium effects of social programmes and policy interventions, particularly those with potential spillover effects in the labour market. On the one hand, the policy interventions studied are implemented at a scale that is too small to be expected to generate any significant changes in the market.[5] On the other hand, even if the scale of operations can be deemed sufficiently large to affect a significant fraction of the population (as in the case of the PSNP) comprehensive data collection can be infeasible. The costs of data gathering are usually very high, especially where programmes are implemented in areas with vast geographical heterogeneity. In trying to overcome both of these hurdles, I bring the following contributions to the literature.

By combining several geo-referenced databases, I compile a dataset of pooled cross-sections observing over 400,000 individuals in all regions of Ethiopia, spanning from 1999 to 2013. This large dataset comes mostly from three rounds of the Labor Force Survey, which has not been used to evaluate the impacts of a policy like the PSNP before. 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, which I collected in person in Addis Ababa from the Government of the Federal Democratic Republic of Ethiopia (GFDRE).

The dataset I compiled overcomes the difficulty encountered by previous programme evaluations in estimating the potential effects on the local economy. My dataset contains infor-

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

[5] Two recent examples of small-scale interventions that could not measure general equilibrium labour market effects are Goldberg (2016) and Fiala et al. (2014).

mation also on districts that were not targeted by the programme. Since I observe these districts in multiple periods, I can estimate a difference-in-differences model to investigate whether the programme has affected the trends in employment participation, occupational categories, hours worked and wages in the targeted districts, relative to the rest of the country. By contrast, the previous studies on the PSNP focused (for good reasons) on estimating the impact of the programme only on individual beneficiaries, collecting information exclusively in targeted districts.[6] 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, where some of the objectives affect the whole community. For example, one of the aims of the programme is to increase resilience and agricultural productivity for the whole community residing where public works are undertaken, so as to stimulate production and local market activities of food and non-food products (World Bank, 2010a). My study provides the first assessment of the potential effects this programme has had on the local labour market as a whole. However, as will become clear in Section 3 and Section 4, the surveys are not without limitations. The results from my evaluation have to be carefully balanced in light of the potential drawbacks of using a dataset that does not contain the same amount of detail as other household questionnaires.

Specifically, my evaluation relates to the recent studies assessing how labour supply in developing countries responds to transfer programmes of different kinds. These studies can be broadly categorised into two groups: one evaluating the impact of providing communities with in-kind assets, and another one investigating the labour supply response of cash and/or food transfers. Examples of the former have shown that the provision of in-kind assets (such as roads, electrification, land-titles and better housing conditions) can stimulate the labour supply in the recipient communities.[7] The second group of studies has generally found that food and/or cash transfers do not deter beneficiaries from engaging in working activities.[8] My analysis sits in between these two groups, as I investigate how the aggregate supply of labour of both beneficiaries and non-beneficiaries responds to a programme designed to deliver both productive assets for the community as well as food and/or cash transfers.[9]

My findings suggest that the programme has not stimulated, or negatively affected,

[6] See Subbarao et al. (2013, Chapter 12) for a review of these evaluations. Evaluations are still in progress, and the latest information on the programme effects on beneficiaries can be found in Berhane et al. (2014).

[7] See Asher and Novosad (2016), Dinkelman (2011), Franklin (2015), and Field (2007).

[8] For example, Banerjee et al. (2014) find no evidence of disincentive effects among unconditional transfer recipients by combining datasets from 7 randomized control trials from different countries. Previous food aid programmes in Ethiopia have not been found to create disincentives to work either (Abdulai et al. (2005), Quisumbing and Yohannes (2005)). Also, see Barrett (2006) for a survey of the intended and unintended effects of food aid.

[9] Unfortunately, my dataset does not allow me to disentangle the effect of the different types of transfers.

the supply of labour (measured in terms of employment participation, hours worked and engagement in multiple working activities) among the recipient communities. However, I find that it may have increased the share of self-employed individuals engaging in non-agricultural activities, possibly due to the improvements in the market infrastructure brought by the creation of assets.

In a broader sense, this paper also contributes to the literature that investigates how rural labour markets operate in developing countries (Behrman, 1999). In particular, I contribute to the strand of the literature that has analysed the impacts of public works programmes on rural economies[10], which has very often focused on India.[11] My study is most closely related to the work of Imbert and Papp (2015), who also use a difference-in-differences model in order to estimate the effect of India's Mahatma Gandhi National Rural Employment Guarantee Act (NREGA) on wages and employment.[12] In contrast, with the recent 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, sample selection, or structural (compositional) differences in the labour markets analysed. Importantly, (unlike the case of 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 found to be above the private sector wage for casual labourers (Subbarao et al., 2013). This is a factor that could account for why my results differ from studies that analysed the Indian programme.

The remainder of this paper is organised as follows. Section 2 provides the reader with necessary background context on the programme analysed in this study. Section 3 outlines the empirical strategy employed to obtain the results that follow in Section 5. Section 4, together with the appendix, describes the dataset that was compiled for the analysis. Robustness checks are presented in section 6. Finally, Section 7 concludes.

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

[11] Aside from the evaluations of the PSNP, another recent exception is Beegle et al. (2016), who provide an impact evaluation of the public works programme in Malawi, highlighting how programme design features can lead to very different programme outcomes.

[12] Other recent studies estimating the labour market impacts of NREGA are Berg et al. (2015), Zimmermann (2012), Azam (2011), Imbert and Papp (2016), Fetzer (2014) and Santangelo (2016).

2 Background

This section lays out the context of my study. While it is not meant to be a complete review of the programme history and its features, I try to focus on the elements that are most relevant for the purpose of the analysis: defining the demographic characteristics of participants, the targeting procedure which selects them and reviewing the type of transfers provided by the programme. In addition, I begin with a brief account of how the programme started. This information will become useful when considering the potential channels through which local economy effects of the programme may come from, and to illustrate the objectives of the programme.

2.1 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.

Around 80% of the population lives in rural areas and mostly depends on rain-fed agriculture for a living. Due to the erratic weather patterns associated with the El Niño phenomenon, rural dwellers face frequent cyclical weather shocks which often result in food shortages.[13] Over the past six decades, Ethiopia has been particularly susceptible to droughts, with a drought occurring every three to five years.[14] In 2015, rain failure from February to May, compounded by a short and erratic primary wet season from June to September, left over 10 million people in need of emergency food support.[15] The negative impacts of droughts on households are also exacerbated by environmental degradation, adverse agro-economic conditions, reduced size of average landholdings (due to high population growth), and other institutional constraints. These factors have contributed to the erosion of the productive assets and coping mechanisms of households and communities (World Bank, 2010a).

[13] Mostly droughts, although localised floods and unseasonal rains can also lead to crop failure.

[14] See Pankhurst (1986) for an historical account, or Gill (2010) for a journalistic account, of these tragic events.

[15] National Disaster Risk Management Coordination Commission and the Ethiopia Humanitarian Country Team: `http://tinyurl.com/ndrmc071215`, accessed on 18/01/2016.

To counter such 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.[16] 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[17] 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.[18] 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.[19] Drawing from existing studies and reports, I next provide an overview of how the programme is designed.

2.2 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).

[16] 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.

[17] 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.

[18] 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.

[19] 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.

The programme is managed by the GFDRE, but remains mostly donor-funded.[20] It has grown significantly in terms of budget requirements as the number of targeted beneficiaries has expanded. The fourth and latest phase of the programme, running from 2015 until 2019, has a budget requirement of US$3.6 billion, towards which the GFDRE has committed US$500 million, with the remainder financed by its Development Partners (World Bank, 2014).

2.3 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).

2.4 Programme targeting

Targeting of beneficiaries occurs at a geographic and administrative level. Geographic targeting of districts forms the basis of my identification strategy, as not all districts (called *woredas*) in the country were targeted by the programme.[21] In attempting a comparison

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

[21] Ethiopia is a federal country divided into eleven regions. Woredas are administrative units that are roughly equivalent to a county in the United States or the United Kingdom, which lie within the regions. In my main sample, the median population in a *woreda* is 105,000 according to the 2007 census.

between targeted and non-targeted districts, it is important to note that the districts were not randomly selected by the GFDRE.

The targeted districts were officially selected on the basis of historical food aid receipts prior to 2005. Specifically, the 2006 PIM 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. In my analysis, the frequency of aid receipts (prior to 2005) is an important control, as it may be correlated with economic opportunities that could directly affect changes in employment.

While my focus is on changes at the district-level, it is still useful to summarise how beneficiaries are selected within districts. Beneficiaries do not self-target themselves into the programme, but administrative targeting identifies eligible beneficiaries within the districts selected.[22] This process involves local officials and community leaders drawing a list of eligible beneficiaries, which is later displayed for public scrutiny, and possible revisions. Within a targeted district, *woreda* officials select which wards (called *kebeles*[23]) will be targeted by the programme, with priority officially given to *kebeles* with the highest level of identified eligible beneficiaries.[24] This process takes place on an annual basis, although targeted beneficiaries are generally expected to remain in the programme for 5 years.
Coll-black et al. (2011) evaluate the administrative targeting process in detail, and find that overall the programme appropriately targets individual beneficiaries that meet the eligibility criteria. It is important to note that, once targeted, beneficiaries are entitled to participate in the programme, but not forced to. However, there is no evidence to suggest that targeted beneficiaries do not take up participation in the programme.

[22] The targeting of individuals for participation in the programme builds on the experience of distributing food aid to rural areas, prior to the PSNP. Caeyers and Dercon (2012) study how connections with officials affect the food aid allocations prior to the PSNP within Ethiopian villages.

[23] *Kebeles* are the smallest geographical unit in Ethiopia, corresponding to a ward, or neighboroghood, of about 2000-4000 individuals.

[24] Since I only observe programme assignment at the district-level, I am mindful that my analysis may suffer from attenuation bias due to measurement error in the explanatory variable.

2.5 Public works

The main feature of the PSNP operations is its public works component. The public works supported under the PSNP are small-scale, labour-intensive community projects designed to provide unskilled, temporary employment for eligible households with able-bodied members. For all sub-projects in a district, the ratio of total labour inputs to total costs should be at least 80% (GFDRE, 2010). Annually 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.[25] 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

[25] 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.

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.[26]

2.6 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.[27] By design, this level is below the usual market wage for unskilled labourers (Subbarao et al., 2013). Currently, the wage rate is on average ETB23/day of work across all regions receiving cash transfers. In 2009, the estimated value of (annual) wages earned per average household recipient was US$137 (World Bank, 2009).

The parity of cash and food transfers has eroded over the years, with food becoming more expensive and cash transfers not adjusting fast enough. This disparity was particularly accentuated during the food price spike in 2008-2009, but the share of cash transfers never went below 50%. Economic theory suggests that if the public works wage is set above the market wage, then private labour supply may be crowded-out by public employment, raising the equilibrium wage for workers in the private sectors (Ravallion, 1991). The erosion in purchasing power of the wages offered by the programme, coupled with the fact that rates were intentionally set below market wages, could potentially reduce any aggregate effect of the programme occurring through changes in the demand for labour.[28]

[26] 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.

[27] 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, [illegible] approximately 20 Ethiopian Birr (ETB).

[28] 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.

3 Empirical Strategy

My empirical strategy compares 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. The rest of this section presents the empirical strategy used, the potential threats to its validity, and how some of those will be addressed.

3.1 Treatment and controls

The PSNP was first launched in 192 rural *woredas*, in 2005. 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.[29] 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

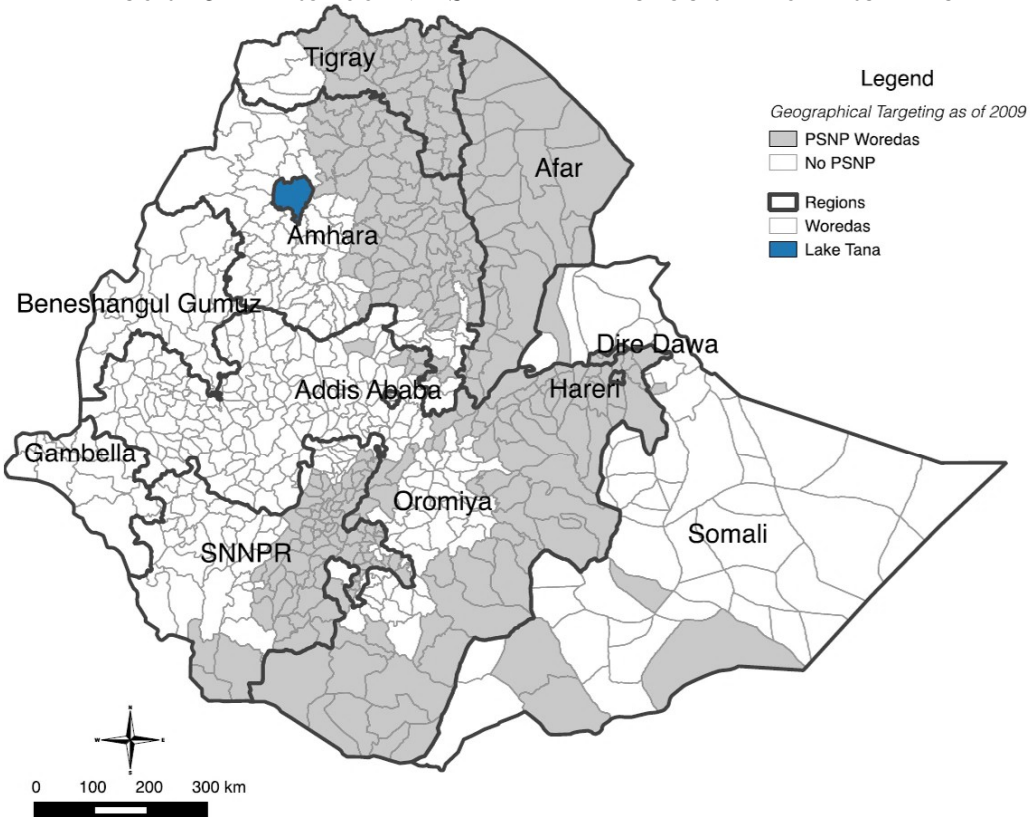
PSNP Phases	Phase I	Phase II				Phase III			
PSNP geographic expansion	PSNP launched in 192 woredas	262 PSNP woredas, as district split	290 PSNP woredas, as Afar added			319 PSNP woredas, as Somali added			
Year	**2005**	**2006**	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**	**2013**
Main data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

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 do not exploit the staggered roll-out of the programme because my sources of data on labour market

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

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.[30] Figure 3.2 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".

Figure 3.2: Productive Safety Net geographic targeting



Notes: PSNP assignment of 290 *woredas*, as of the end of PSNP Phase II (2007-2009).[31]

It is worth pointing out that my measure of "treatment" is a dummy variable equal to one if a district is targeted, and zero otherwise. As such, I am trying to measure the effect of exposure to the programme comparing districts that were targeted with those that were not, without observing individual participation. Hence, my analysis should be interpreted as estimating an Intention To Treat (ITT) effect of the programme, rather than

[30] Most socio-economic survey of Ethiopia still struggle to get information on these areas. For example the 2007 Census did not sample the Somali region at all. Figure A.1 in the appendix shows the geographical coverage of the surveys used in this study.

[31] More detail on the sources of this information is provided in the appendix.

an Average Treatment Effect (ATE) across districts. This may attenuate my estimates if the programme was not operating in some targeted districts due to unobserved institutional barriers or because not every *kebele* in the districts targeted received the programme.[32] However, *woredas* can still be seen as a meaningful unit of analysis. Being the second-smallest administrative unit, their size is not large enough to expect significant geographical and institutional heterogeneity. Further, studies estimating the labour market effects of similar programmes in other countries have also employed rural districts as the unit of analysis to estimate the general equilibrium effects of the programme.[33]

3.2 Regression model

A simple way to estimate the effect of the PSNP on labour market outcomes would be to use a cross-section of individuals and compare the outcome of interest between individuals in districts that were targeted with those living in other districts (simple differences). But then, if the selection criteria used to target districts into the programme was, say, negatively correlated with potential labour market outcomes in the absence of the programme, simple differences would be understating the effect of the programme on this outcome. For example, labour market participation may be negatively correlated with factors related to the receipt of emergency assistance, such as the propensity to face droughts.[34] For this reason, I compare 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. 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.[35] 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

[32] As noted in the previous section, assignment of *kebeles* to the programme may have varied throughout the period of analysis within a targeted district, depending on the annual number of individuals deemed eligible at the *kebele* level.

[33] See, for example, Imbert and Papp (2015) and Berg et al. (2015).

[34] An alternative naive comparison could use the difference in outcomes in a targeted district before and after the introduction of the PSNP. However, this comparison would not be able to disentangle the potential impact of the PSNP from any simultaneous change in the employment participation decision affecting all districts.

[35] In the appendix, Figure A.2 makes this point visually.

bias the estimated effect of the programme. 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.[36] I interact these time-invariant controls with a dummy equal to one if the year is 2013[37] 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, related primarily to weather shocks that could have affected labour supply differentially across PSNP and non-PSNP districts. 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).[38]

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 a dummy 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 a dummy 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

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

[37] I use this year to define postprogramme status. 2013 is the only year after programme roll-out for which I have Labour Force Survey data post-2005.

[38] Summary statistics of the district-level variables, across PSNP and other districts, are shown in Table 3.1.

unobserved characteristics of districts.[39] Finally, $\epsilon_{1,idt}$ is the unobserved idiosyncratic component in this model, 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 dummy variables for whether the individual is female, literate, has any disabilities, or is the household head. To partially mirror some of the selection criteria into 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 no children below the age of 5, or individuals aged above 70 in the household, or individuals that has 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.

Clustering and weighting

Since I estimate equation (1) and (2) using repeated cross-sectional data, standard errors require some adjustments. Difference-in-differences models using repeated cross-sections have been found to be affected by a strong degree of serial correlation in the unobserved component (Bertrand et al., 2004). To address this issue, I cluster my standard errors at the district level. There are 453 districts in my main sample, so a small number of clusters is not a concern.

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 Cen-

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

tral 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 the appendix.

4 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.[40] 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 history of relief aid receipts 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.[41] 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.

4.1 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. As discussed in the previous section, my empirical strategy relies on changes at the district level. Because the PSNP

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

[41] 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.

only targeted rural dwellers, I restrict my sample only to individuals living in rural areas and drop all households living in urban areas.[42]

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 labour supply. 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.[43] However, I only use this round as a placebo test to check for differential trends prior to the programme roll-out.

4.2 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.[44] 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.[45] In line with other research on the Ethiopian labour market (Franklin (2014), Broussar and Tekleselassie (2012)), and labour markets in Africa in general, I adopt the partially relaxed definition of unemployment, whereby 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, prior to the interview. This definition also regards as unemployed those that reported having a job that

[42] 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.

[43] 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.

[44] 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)).

[45] 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.

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 are scarce. 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 dummy 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.[46]

5 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. One exception may be occupation categories: I find that the programme may have shifted self-employed individuals from agricultural to non-agricultural occupations.

5.1 Summary statistics

Table 3.1 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.

[46] 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.

Such differences in labour market conditions are plausibly due 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*), where labour is concentrated in the second half of the year, we can expect the share of seasonally unemployed individuals in these districts to be year 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%). Since the survey in 2013 was fielded in May-June, seasonality effects may be a concern for the validity of my difference-in-differences specification, if the effects of seasonal changes were systematically different between PSNP and non-PSNP districts. By including the fraction of workers that were seasonally out of work in my vector of time-invariant controls, C_d, I attempt to control for this potential seasonality effect, due to the different timing of the surveys.

Secondly, the fielding of the surveys coincide with the major period of PSNP public works. As noted before, PSNP public works predominantly run between January and June, which means that I am at least able to observe individuals during a period of potential public works activity in both rounds of the survey. The higher share of public sector workers found in PSNP districts (3%) at baseline may be an indication that public works had already started by the time of the survey.[47] One might worry that if the programme had already started in 2005, my DID estimates of the programme may be biased downwards. 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 launch there would have been enough material difference, or 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. 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, a low share associated with the rural context) or the demographic structure of the households, in terms

[47] 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.

of age structure or gender balance. As expected, the frequency of relief assistance prior to 2005 is much higher in PSNP districts, as this information 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. In the analysis, I tackle this imbalance with the inclusion of 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.

Table 5.1 reports the baseline means of the outcomes studied for the sub-samples of the treated and control groups. 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 at baseline. In particular, we see that the employment rate is high (around 82%), indicating that most individuals undertake some form of productive activity. Out of those employed, more than 80% reports being either self-employed or unpaid family workers whose occupation is in agriculture (crop, livestock or mixed-farming) or forestry. Self-employed individuals or unpaid family workers that engage in an occupation that is not related to agriculture constitute around 10-13% of those employed. Usually, these activities involve small 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, differentiated by whether they report either the (local) government or the private sector as their employer. In particular, individuals in this category undertake unskilled tasks mostly in agriculture or construction work.[48] Public labourers in this category may potentially include PSNP participants, but it may also include labourers in other publicly funded projects. However, it is important to note that these definitions refer to the principal category of employment, which is the only one for which I have information across rounds of the survey. As such, they 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

[48] 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.

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.

Main results

5.2 Effects on labour market participation and sectoral occupation

Table 5.2 presents the main results: the effect of the PSNP on employment and occupation categories across districts. The dependent variable is a different dummy variable for each column. To improve the readability of the tables, the dummy 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% point 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, which is reasonably close to zero given the size of the programme.

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% points) in the fraction 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% 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 petty trade and selling goods at the local market. Other types

of construction-related work and crafts-work play a smaller role in the sample observed. To better understand the sectoral shifts observed in the results, the appendix table A.3 reports the estimated effects of table 5.2 for different genders. 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% 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 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. It is worth noting that the inclusion of the district controls does not drive the main results, as shown in the appendix table A.4, where I present the estimates from a simple DID model (with and without district fixed effects), similar to those in table 5.2.

Additional results

5.3 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 5.3 presents the estimated effect of the programme on different outcome variables. The first three columns report insignificant and small effects of the programme on in-migration rates and household size.

The results in the first three columns are important in supporting 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 economy. It is plausible to consider a scenario where the number of household members may have changed among beneficiaries in order to cope with the work requirements of the public works programmes. Specifically, 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 individual 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 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. Part of the interest in this measures of labour market activity is linked to any potential general equilibrium effects of the programme. For example, if the irrigation projects improved agricultural yields in the targeted districts, we might expect a reduction in hours worked across beneficiaries and non-beneficiaries alike. Moreover, it is plausible 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% potential 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.

5.4 Potential effects on the wage of private sector labourers

In Table 5.4, I restrict my focus to the wages of private sector labourers, as this is the segment of the labour market that is more likely to be affected by any general equilibrium effects of the programme. The LFS does not collect information on wages for self-employed individuals, which considerably limits the informativeness of the wage measures for the purposes of my analysis. As private sector labourer wages are only observed in 1% of my sample, in 81 of

the districts sampled in both rounds, my analysis on wages should be treated as indicative, and not be representative of rural markets in Ethiopia. A causal relationship can hardly be extrapolated from my analysis of wages. Any estimated coefficient of the impact of the PSNP on the differential trend in 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. However, I still include a very brief discussion of the changes in wages for two reasons: Firstly, I try to draw a parallel with existing studies of similar programmes in other developing countries. That was the purpose that I had originally set out to achieve. Given the limitations outlined so far, one can appreciate that such a comparison may be difficult to draw. Secondly, this discussion highlights the difficulty in using wage-level data for rural areas from national statistics in Sub-Saharan Africa. Policy-makers seemed to have neglected the extent with which national statistics may not be very effective at measuring the true extent of 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. However, interpreting them as a correlations suggest that the district wage for private sector labourers did not move together with exposure to the programme. 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.[49] 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 my dataset) tend to suggest. It is possible that workers categorised as self-employees 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 workers reporting to be 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. 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. Before turning to the robustness

[49] 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).

checks, I provide a brief discussion on the results documented in this section.

5.5 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 to justify the lack of an impact 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, as noted in the background section, days of employment are capped for individual participants so that enough time is left for other potential productive activities. Its main objectives remains 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. Its main purpose is to serve as a social support mechanisms for households that have chronic food insecurity issues, in exchange of labour requirements towards the completion of public infrastructure projects. Alternatively, a theoretical explanation for the lack of responsiveness in labour markets from exposure to the programme may be due to the fact that the labour supply may be 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 such an 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 could occur even if the PSNP has increased the reservation wages among some rural workers.

The second result indicating a shift in self-employment activities outside of agriculture observed in PSNP districts is reconcilable with the following explanation. It is possible that the community assets funded by the PSNP (e.g. new local roads) can improve market access in these districts, and increase 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 (2016), 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.

6 Robustness checks

The main concern that could undermine the validity of the results presented in table 5.2 is that other factors unrelated to the PSNP may be affecting the differential trends in employment and the fractions of workers engaged in different occupations. To check the robustness of the estimates in table 5.2, 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 in turn.

6.1 Placebo test

The results of the first robustness check are presented in table 6.1. In practice, I try to estimate the differential changes in employment and occupational categories across targeted and non-targeted districts prior to the roll-out of the programme. The specifications are the

same as in table 5.2, 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 evidence that the extensive margin of labour supply had changed differentially between 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 a differential increase of 0.9% 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. While public labourers are not necessarily those employed in the PSNP, this estimate could suggest that the PSNP public works had already started in targeted districts by the time of the 2005 LFS round. Hence, as previously noted, it seems more appropriate to refer to the estimates in table 5.2 as the additional effects of the programme after its initial adjustment phase in 2005.

A limitation of this placebo test is that it only uses data from a sub-sample of the districts that are 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.[50]

6.2 Adding population density as a control

As noted in the background section, 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 A.1 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 5.3 show that demographic composition of districts does not seem to be changing differentially in targeted districts in terms of household size or in-migration. Partly in response to this result, I add as a control to equation (2) the district-specific population density, as measured by the 2007 district-level population density, interacted with $\mathbf{1}_{(t=2013)}$.

[50] Data appendix A explains the issues I had with matching the 1999 round with the other two.

The results of this exercise are presented in table 6.2. Before adding population density as a control, I estimate equation (2) removing observations the Somali region, with the results shown in Panel A. This is because the 2007 census did not cover this region, which as pointed before, is predominantly inhabited by semi-nomadic groups. Hence, the estimates of β, after including population density as a control (Panel B), are more appropriately compared to Panel A.

The coefficients in Panel A are similar to those in table 5.2, which confirms that omitting the Somali region does not strongly affect the baseline 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 5.2, this robustness check suggests that my estimates should be interpreted with caution.

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.[51] 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

[51] The terms that $(\mathbb{1}_{(PSNP=1)} \times W_d)$,$W_d$ and $\mathbb{1}_{(PSNP=1)}$ would be collinear with η_d, so are omitted.

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 6.3 reports the estimates of β and β_2 from estimating equation (3). Including the interaction terms does not seem to alter the estimates relative to the results in table 5.2. Most estimates of β_2 are not statistically significant, while the estimates of β increase slightly relative to the baseline results. This suggests, if anything, 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 suggest a failure of the parallel trends assumptions because of pre-programme shocks.

6.4 Additional checks

Other three robustness checks are included in the appendix tables, and I briefly comment on them here. In table A.3, 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 5.2. Removing weights (Panel B) seems to decrease the standard errors of my estimates, which is curious, since their purpose was originally intended to make the estimation more precise.[52] Adding additional districts to the estimation (Panel A) has the expected effect. It does not change the DID estimates (as these are 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 A.4, 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 5.2. Notably, the potential differential increase in the share of

[52] 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.

workers engaged in non-agricultural self-employment seems to be statistically significant even without adding additional controls to the basic difference-in-differences model. Overall, it appears that the results presented in table 5.2 remain broadly robust to the checks performed in this section.

7 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 outcomes that stand out from my analysis: Firstly, the extensive and intensive margins of the labour supply seem unaffected by the expansion of the programme. Secondly, districts targeted by the PSNP appear to have featured 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 to suggest that the public works component crowds 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 infrastructure, 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, the outcome of misguiding national-level statistics (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 programme, potentially through changes in the rural-urban migration patterns. Extending the analysis presented in this study to consider urban labour markets 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 an 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. When these questions are raised, my findings can contribute to the research on Ethiopia's rural context, against which urban studies can be contrasted.

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Main Tables

TABLE 3.1: 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-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 recieved by district.
Panel B presents means of the individual-level means. Apart from age, all controls are dummy variables. The omitted category is a male individual with no schooling, unmarried, who is not a household head, and lives in a male-headed household, where the household head has no schooling, there are children aged below 5, and no member of the household is above 70 years of age, or has a disability

TABLE 5.1: MEAN BALANCE OF OUTCOMES VARIABLES 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 5.2: 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 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 includes district fixed effects, and district controls. In Panel B, each model includes district fixed effects, district controls and indiviudal contols. 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 3.1.

* denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 5.3: 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 beta; 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 a dummy 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 a dummy 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 indiviudal contols. 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 constant within a district across time. All models are estimated using ordinary least squares. The means of district-level and individual-level controls is shown in Table 3.1.
* denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 5.4: 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 beta; 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 a dummy 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 a dummy 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. All models are estimated using ordinary least squares controlling for district fixed effects, district controls and individual controls. The means of district-level and individual-level controls is shown in Table 3.1.

* denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 6.1: 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)
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
Mean Dep. Var. (%)	73.73	4.150	22.12	81.16	14.18	1.765	0.702
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)
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
Mean Dep. Var. (%)	73.73	4.150	22.12	81.16	14.18	1.765	0.702

Notes: Each cell reports an estimate of beta; 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 indiviudal contols. 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 list of district-level and individual-level controls is shown in Table 3.1.

* denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 6.2: 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 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 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 proportional to district population. All models are estimated using ordinary least squares, including district fixed effects, district controls and indiviudal contols. The means of district-level and individual-level controls are shown in Table 3.1.

* denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 6.3: 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 beta:	-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 beta:	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

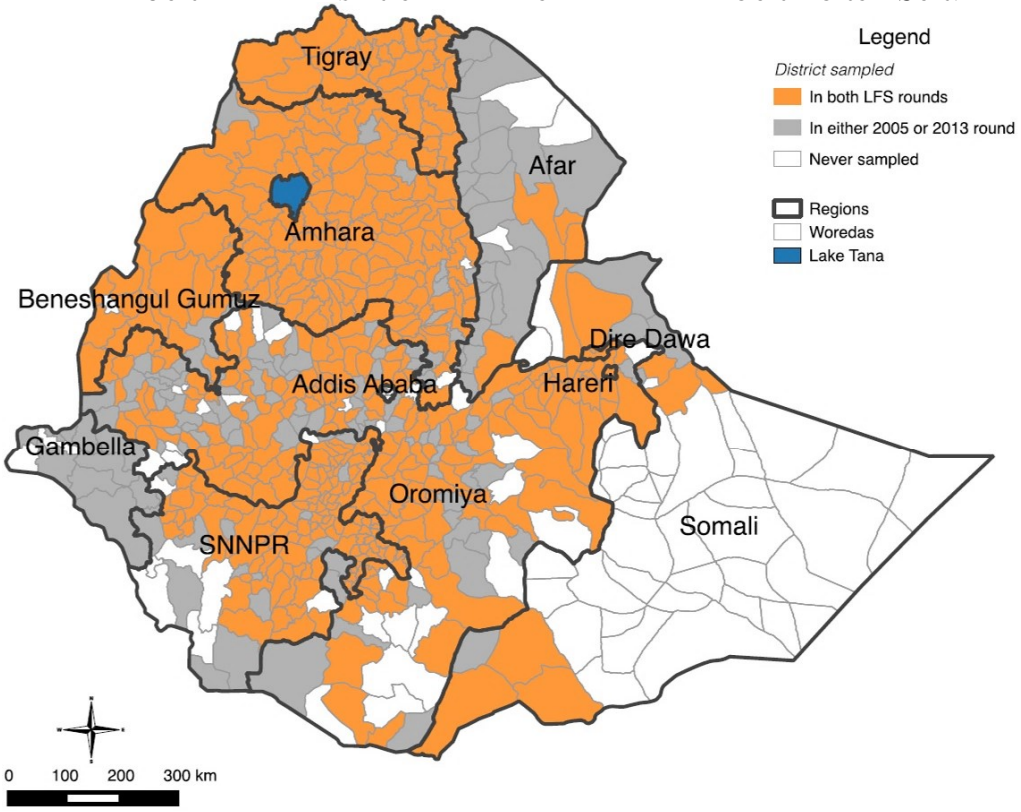
Notes: The first row in each panel reports an estimate of beta for different dependent variables; standard errors in parenthesis are clustered at the district level. Each column has a different dependent variable. The second row in each panel reports the estimated coefficient of an interaction term with 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, including district fixed effects, district controls and indiviudal contols. The means of district-level and individual-level controls are shown in Table 3.1.

* denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Appendix

Constructing a panel of districts

FIGURE A.1 - DISTRICT BALANCE IN THE LABOUR FORCE SURVEY



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 A.1 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.[53] 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

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

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[54], 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).

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

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

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.[55] 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*.

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

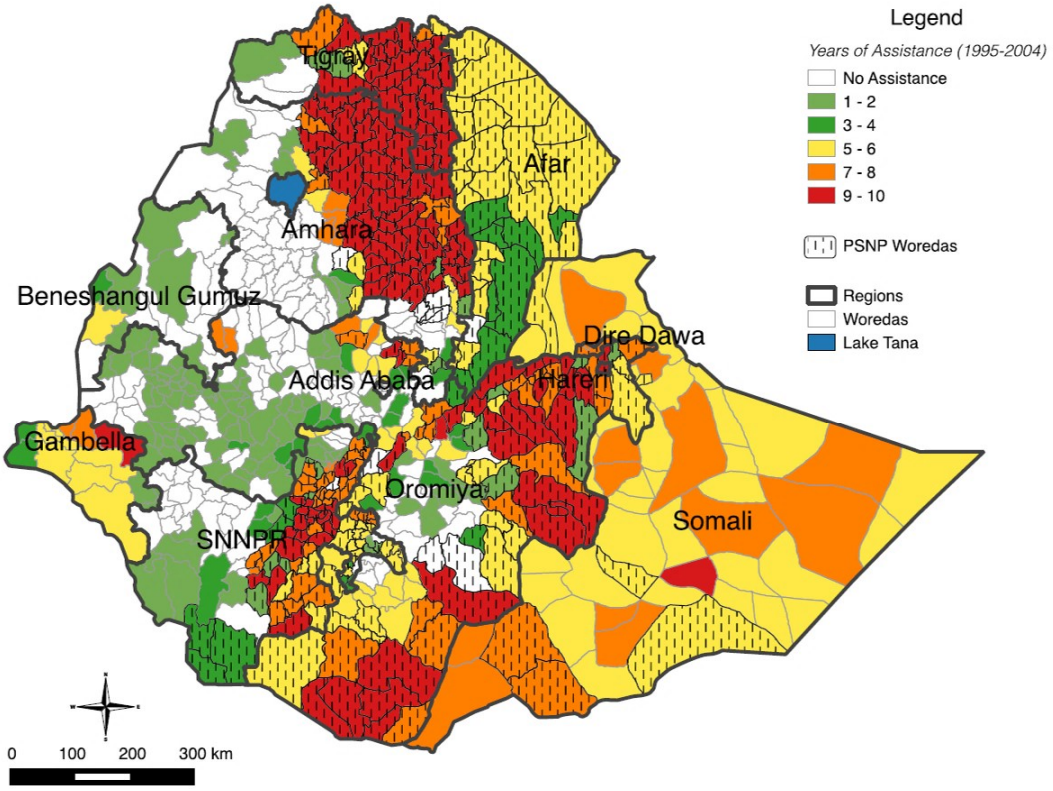
As noted in the background section, districts were targeted based on their historical receipt of food aid prior to 2005. To take this into account in my specifications, I collected data on the frequency of historical relief assistance at the district-level (between 1994 and 2005) from the National Disaster Risk Management Committee[56] of the GFDRE. I personally collect this data in a trip to Addis Ababa in January 2016. This information is shown in Figure A.2. When compared to the geographical distribution of the targeted *woredas*

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

[56] 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.

(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.

FIGURE A.2: CUMULATIVE YEARS OF AID RECEIPTS AND PRODUCTIVE SAFETY NET GEOGRAPHIC TARGETING OVERLAPPED



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.

Weather controls

Weather shocks could be part of the unobserved time-varying component, and may be more frequent in PSNP *woredas*, which is why I control for climatic variables in my main specifications using gridded data sources. Gridded data, which interpolates readings from weather stations with a statistical model, are frequently used by economists.[57] However, one of the difficulties of employing these data sources in developing 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

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

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 (e.g. Adhvaryu et al. (2014), Theisen (2012)) and were chosen because of their geographic scope and long time scale.[58] 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.[59]

Other controls, which I do not include in the main regressions (but that are shown in appendix table A.1) 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.

[58] 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.

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

Appendix Tables and Figures

Table A.1: 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)
Population density (per sq. km)	250	167	0.000	2007 Census	No
Area (sq. km)	1097.34	1099.37	0.982	2007 Census	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
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
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 A.2: 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, including district fixed effects, district controls and indiviudal contols. The means of district-level and individual-level controls are shown in Table 3.1.

* denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.3: 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 includes observations from all the 602 districts sampled in either the 2005 or the 2013 LFS round. In Panel B, the individual observations are not weighted, but is restricted only to the 453 that were sampled in both the 2005 and the 2013 LFS round. 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 only in Panel A. All models are estimated using ordinary least squares, including district fixed effects, district controls and indiviudal contols. The means of district-level and individual-level controls are shown in Table 3.1.

* denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.4: 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 district fixed effects, or 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.

* denotes significance at the 10%, ** at the 5% and, *** at the 1% level.