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Essays on Social Protection

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Summary

This thesis analyses issues related to aid in Ethiopia and provision of elder care in the US.

The first essay assesses the targeting of two major aid interventions, public works and food aid. Both types of aid are primarily allocated through community-based targeting. The few studies that have analysed the accuracy of aid targeting in Ethiopia have shown biases along demography, geography and political affiliations lines. With the introduction in 2005 of the Productive Safety Net Programme (PSNP), a major social protection programme, several administrative guidelines were introduced aimed at improving targeting. This paper uses the last two rounds of the Ethiopian Rural Household Survey to investigate whether there were changes in both targeting determinants, and amount received for PSNP public works and food aid components from 2004 to 2009. Overall, the PSNP appears to be allocated on the basis of observable-poverty-related characteristics, and food aid on household demographics. In addition, results suggest for both PSNP and food aid beneficiaries, political connections are significant in determining receipt of the program in 2004, but no longer in 2009, indicating an improvement towards means-based targeting.

The second essay investigates the long-term effectiveness of emergency aid in Ethiopia in protecting child health from the negative effects of a severe drought that hit the country in 2011. Child malnutrition remains a critical issue in Ethiopia and the literature has shown that shocks can have long lasting effects on physical and cognitive development. Using the two rounds of the Ethiopian Rural Socioeconomic Survey (ERSS) collected in 2011 and 2013, I explore the impact of emergency aid on child height-for-age aged 0-36 months two years after the drought had occurred. Because aid was not randomly allocated, I use a matching estimator to account for selection into the programme. The results show that emergency aid was effective in protecting children that experienced the drought.

In the last essay, I research the effects of kindergarten eligibility on the provision of elder care in the US. I am able to identify the trade-off between child care and elder care by exploiting age eligibility criterion for public kindergarten, in combination with state-level variation, in the provision of full-time kindergarten. Through a reduced form approach, I estimate the Intention to Treat (ITT) for the effect of eligibility to kindergarten on provision of elder care. The results show that having the youngest child aged 5 in states that offer full-time kindergarten increases the probability of providing elder care by around 9 percentage points, which corresponds to 63 percentage increase to the baseline. The effect is higher for females (9.2) than for males (8.1).

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Chapter 1

Introduction

This thesis investigates various aspects related to social protection policies, both in developing and developed countries. In the context of Sub-Saharan Africa, in particular, they are one of the main tools used to protect and sustain the livelihood of the poor. Social protection programmes in the continent have been on the rise since the beginning of the 2000s, with an increasing number of countries implementing them and reaching larger shares of the population (World Bank, 2015). The vulnerable groups in developed countries differ from those in developing countries, as they are mostly found within specific groups, identified by more narrow characteristics, i.e. disability or age. The elderly are one of these groups, who are not only in need of financial support, typically through pensions, but they also require medical assistance and care, which may constitute a substantial burden in terms of resources needed.

This thesis seeks to fill the gaps in the literature around what works in relation to social protection tools that target different vulnerable groups, in Ethiopia and the United States. In particular, the first chapter provides an assessment of changes in targeting performance in Ethiopia, after the introduction of a major social protection programme, the Productive Safety Net Programme (PSNP). While much of the literature has focused on comparing different targeting methodologies, it has failed to assess the changes in performance over

time, driven for instance by reforms introduced in the system. The second chapter provides an analysis of the role that aid plays in protecting individual well-being from shocks. In particular, I focus on the effect of droughts on child health, and I investigate whether emergency aid successfully protected children from the negative impact of these type of shocks in Ethiopia. Notwithstanding the high policy relevance of this topic, particularly in relation to the current debate on climate change and the interventions that can mitigate the negative effects attached to it, the empirical evidence is still very scarce. Chapter 4 studies the trade-off between child-rearing and elder care. The literature has mostly concentrated on the opportunity costs of caregiving in terms of lost income, although it has neglected the effect that policies such as the provision of universal pre-school may have on the supply of elder care.

A contribution common to the three essays is the focus on social protection. Programmes aimed at supporting the livelihood of vulnerable groups are often constrained by limited resources. Exploring what policy instruments are more effective in reaching their intended goals and exploring unintended effects of these policies therefore is of primary interest, in particular for the policy implications. My findings contribute to the understanding of what works in relation to social protection programmes in specific contexts.

The interest in assessing the targeting performance of a safety net programme over time lies in the crucial role of identifying the target group to successfully reach the goal of an anti-poverty programme. The debate around what targeting method results in higher accuracy has for long captured the interest of researchers and policy makers. However, most of the empirical evidence in the past has focused on cash transfer programmes in Latin America, where proxy-means-test and means-test were the most common targeting methodologies. In Sub-Saharan Africa, with the relatively recent spread of cash transfers in the region, administrative and logistical constraints impose to opt for other targeting strategies. These often entail the combination of different methods (i.e. geographical,

categorical, community-based). Community-based-targeting (CBT), in particular, is one potential solution that has been suggested to cost-effectively and accurately identify and select beneficiaries. The evidence on CBT is mixed, mostly pointing to the importance of minimizing the risks of capture, rent seeking behaviour and patronage from local leaders to outbalance the benefits associated with better knowledge that local agents have of the community, higher ownership and satisfaction from the community, and flexibility stemmed by the possibility of customising the criteria to identify the poor (Alatas et al., 2012; Conning and Kevane, 2002). The literature that has focused on targeting performance of past aid programmes in Ethiopia pointed to political connections playing an important role in allocation of aid (Broussard et al., 2014; Caeyers and Dercon, 2012) and errors of inclusion and exclusion (Jayne et al., 2002). Since the introduction of the PSNP in 2005, the Government has put a lot of effort to increase transparency and to reduce targeting errors, by implementing a number of reforms. Assessing whether these efforts translated into a more accurate targeting may provide guidance on which measures can improve targeting implementation.

The analysis of the impact of shocks on children's health is of high relevance, both for the intrinsic value of the well-being of individuals affected by the shock, particularly when they are as vulnerable as children, and for the economic costs and the consequences in terms of development prevention that derive from disasters and unexpected negative events. A large body of literature has in fact shown how detrimental the consequences that shocks experienced at an early age can be, even in the long run, on health (Portner, 2010; Dercon and Porter, 2014), and education and cognitive abilities (Alderman et al., 2006, 2009). Identifying what type of interventions can mitigate the negative effects of shocks therefore becomes crucial to foster economic development and protect the vulnerable groups of the population.

Current demographic trends in developed countries, characterised by population ageing

and growing rates of chronic conditions, have the potential to increase the demand for long-term care. The study of possible mechanisms or policies that can effectively relieve the increased need for resources associated to elder care are pressing. The academic debate surrounding this issue has mostly focused on the opportunity costs of caregiving in terms of lost income, particularly on the trade-off between work and caregiving, providing mixed results. Central to this literature has been the role of women, who constitute the majority of caregivers (Feinberg et al., 2011). On the extensive margin, some authors have provided evidence of a lack of trade-off between these two uses of time, suggesting that time dedicated to non-work activities is accommodated in favour of caregiving (Wolf and Soldo, 1994; Wolf, 2001; Lilly et al., 2007). On the intensive margin, instead, caregivers are found to reduce their working hours and to face a reduced wage (Ettner, 1995; Graves, 2010; Van Houtven et al., 2013). However, the trade-off between child-rearing and elder care has been so far unresearched. To the extent that there is a substitution between child care and elder care, this relationship would provide insightful information to policy makers that try to address issues related to caregiving.

Ethiopia and the United States are particularly suitable and relevant countries to address these areas of investigation. Ethiopia is an appropriate country for the study of changes in targeting performance over time. It is among the largest aid recipients of aid worldwide (OECD, 2016b) and its safety net programme, reaching nearly 8 million of Ethiopians (11% of the total population) (World Bank, 2009), is one of the largest among low income countries (World Bank, 2015), and the largest in Sub-Saharan Africa, excluding South Africa. In addition to the PSNP, emergency food aid is still channelled to the country in times of crises, such as droughts. The study of the impact of drought on children's health and the mitigating role of aid is also of particular relevance in this context for a number of reasons. First, Ethiopia is a country prone to severe droughts, which happen with alarming frequency. The well-known 1984 famine alone caused the

death of an estimated 1 million people. Since the beginning of the new millennium, major droughts happened in 2003, 2005, 2008, 2011 and 2015, affecting 12.6 million of people in 2003 and 6.4 million in 2008 (the worst in this period). Weather-shocks are one of the main causes that contribute to the food insecurity, which, despite the importance of agriculture to its economy, remains pervasive. This issue directly relates to the second reason for which Ethiopia is a particularly relevant case study. Chronic malnourishment, especially among children, remains at worrying levels. Even though the past two decades experienced a significant decrease in malnutrition, current levels remain high, even when compared to other Sub-Saharan countries. Prevalence of stunting among children under 5 years of age has decreased by 20 percentage points, from 58% in 2000 to 38% in 2016 (CSA and ICF, 2012, 2016). Hence, investigating the role that interventions aimed at protecting individuals affected by severe shocks play on the vulnerable category of children, can have valuable policy implications.

The United States is a particularly interesting context to investigate caregiving. The fact that the principal source of care for elderly and disabled is informal stems primarily from the lack of universal access to a publicly funded long-term care system in the United States. Informal caregiving is therefore the sole source of care for the majority of people in need, around 70% (Ettner, 1995), and it is a major source of care for 90% of disabled elderly (Spillman and Pezzin, 2000). In addition, the reason to focus on elder care lies in the fact that the Unites States provide access to publicly funded education only starting from kindergarten for children aged five (with the exception of three states that also provide pre-kindergarten), much later compared to other Western countries, and with high degree of variability in the provision. While in some states the access is guaranteed for full-time publicly funded kindergarten, in others only part-time or no kindergarten at all is offered.

The choice of these two countries is motivated also by practical reasons. All three chapters tackle hypothesis grounded on microeconomic theory, which required the use of

household survey data. For the two chapters on Ethiopia I use two different sources of data. The Ethiopian Rural Household Survey, a long panel data set tracking households for 7 rounds covering fifteen years, from 1994 to 2009, in 15 rural villages across different agro-ecological regions. While the data are not representative of the country population, it has the advantage of eliciting information on a broad set of topics, particularly relevant for the study of targeting of aid in the country. The second chapter makes use of a different source of data, namely the Ethiopian Rural Socioeconomic Survey, a nationally representative panel data set, collected in 2011 and 2013. The severe shock that hit the country in 2011 proved this data set very relevant to study the impact of the drought. In addition, data on children's anthropometrics, not available in may data sets, made the analysis possible. Lastly, the American Time Use Survey, collected by the Bureau of Labor Statistics, was used to research on elder care. Since 2011, the survey has introduced a module to assess the time spent by informal caregivers helping the elderly. I use four rounds of the survey for the purpose of this study.

This thesis comprises of three substantive chapters, from 2 to 4, each with a specific objective explained below. Chapter 5 draws some broad conclusions from them. Chapter 2 assesses the changes in targeting performance over time. Since the focus is on intra-village allocation of aid, the sample is restricted only to villages that were targeted by either one or both programmes. The analysis is performed separately for the two main types of interventions, emergency food aid and public works, and the comparison over time is between programmes implemented before and during the implementation of the PSNP. The aim is to assess whether the weaknesses associated to targeting of pre-PSNP programmes are still present or if the efforts of the government aimed at improving targeting has resulted in higher accuracy. Chapter 3 investigates the effect of the 2011 drought on children's health two years after the occurrence of the shock and whether emergency aid distributed with the intent of protecting from this shock has proved effective. As emergency aid is typically

less well targeted than other safety net programmes and is often distributed with delays, whether aid successfully protected children's health is ultimately an empirical question. Chapter 4 explores the effect of kindergarten eligibility on the provision of elder care. By exploiting age eligibility criterion for public kindergarten, in combination with state-level variation in the provision of full-time kindergarten, the trade-off between child care and elder care is identified. The analysis is here conducted separately for men and women as different effects are expected by gender.

Chapter 2

Targeting of food aid in rural Ethiopia

2.1 Introduction

One of the core challenges in operationalising effective poverty reduction programming is ensuring that investments reach intended populations. The credibility and effectiveness of development efforts are undermined when resources fail to reach those most in need. To address this, programmes are increasingly required to engage in monitoring and evaluation efforts, including showing whether or not poor communities and households actually benefit (Van Domelen, 2007). However, an important obstacle in reaching the people most in need is the high costs that can be involved in obtaining accurate information on their comparative deprivations. Targeting is the mechanism that is used to "identify households or individuals who are defined as eligible for resource transfers and simultaneously screening out those who are defined as ineligible" (Sabates-Wheeler et al., 2015). The choice of the targeting mechanism, which in practice is often a combination of different methods (i. e. geographical, categorical, proxy means tests, community-based), is selected in accordance

with programme objectives, while constrained by the trade-off between the accuracy[1] and costs associated with each method.

In contrast to Latin America, the methods used to identify beneficiaries for cash transfers in Sub-Saharan Africa often have some degree of community involvement (Handa et al., 2012). Administrative and logistical constraints coupled with high costs dictate that methods applied in Latin America (i.e. means tests) are challenging to implement in the African context (Robertson et al., 2014). In resource poor settings, decentralisation through community-based targeting (CBT) is one potential solution that has been suggested to cost-effectively and accurately identify and select beneficiaries. The advantages associated with CBT are a result of the better knowledge that local agents have of the community, which allows them to more accurately identify poor households, and in stem from the lower probability that community members are able to hide information about their welfare situation from local agents. In addition, CBT fosters ownership and empowerment from the community. Finally, since poverty and vulnerability are likely to differ across different contexts, allowing local agents to customise the criteria to identify the poor can additionally improve the targeting (Alderman, 2002; Yamauchi, 2010). However, a number of studies highlight potential disadvantages of this approach, mostly linked to the risks of capture, rent-seeking behaviour and patronage from local leaders (Conning and Kevane, 2002; Alatas et al., 2012; Handa et al., 2012). As there is no consensus in the literature on standardized metrics to assess the superiority of any given targeting method, approaches continue to be debated across diverse poverty programming in different settings (Coady et al., 2004; Ravallion, 2009).

Over the past decades, Ethiopia has become one the largest aid recipient countries in the world, driven by severe recurring droughts and famines, which threaten the physical

[1] Watkins (2008) defines accuracy as "the extent to which a targeting scheme correctly selects participants in the scheme and deselects non-participants."

survival of millions of people (Venton et al., 2013)[2]. For the approximately 80% of national labour force that depend on agriculture and animal husbandry, crop and animal failures deriving from droughts can be devastating (The World Factbook, 2017; Van Domelen and Coll-Black, 2012). However, emergency aid appeals have major drawbacks, as they are often unpredictable and experience delays, which translates into loss of livelihoods, physical or human-capital, household assets and an inability to address chronic poverty (World Bank, 2009). In 2003 the Government of Ethiopia started a consultation process with its main development partners aimed at creating a more effective solution able to address both the immediate needs of food insecure households, as well as evoking a sustainable and long-term vision to enable households to graduate from emergency systems (Van Domelen and Coll-Black, 2012).

The government flagship social protection instrument established in response to these concerns is the Productive Safety Net Programme (PSNP). The PSNP is the largest safety net programme in Sub-Saharan Africa, after South Africa, and the second-largest in a low-income country, after Bangladesh (World Bank, 2015). At its peak in 2010, the PSNP reached over 7.9 million[3] people, corresponding to about 10% of the national population, in 290 chronically food insecure rural *woredas* (over 40% of countrys *woredas*) (World Bank, 2016). The stated programme objective is to reduce poverty by assuring food consumption and preventing asset depletion for chronically food insecure households, while stimulating markets, improving services and rehabilitating natural resources (PSNP PIM, 2006). The PSNP builds on previous aid interventions, both in terms of structure, distribution modalities, and intervention typologies. The main intervention of PSNP is public works,

[2]Since the beginning of the new millennium, major droughts happened on a frequent basis, 2003, 2005, 2008, 2011, 2013, and 2015/16, affecting 12.6 million of people in 2003, 6.4 million in 2008, and 10.2 million in 2016 (the worst ones in this period) (Venton et al., 2013; World Food Programme (WFP), 2016). According to OECD figures (OECD, 2016b,a), Ethiopia was the fourth, second and first recipient of aid in Africa in the nineties, in the first decade of the new millennium, and in the current decade, respectively. Compared to worldwide figures, in the past three decades Ethiopia was tenth, fifth and third, respectively

[3]The number of beneficiaries has decreased over the course of the programme life, and in 2015 was reaching 5.2 million people due to graduation of beneficiaries (World Bank, 2016).

which replaces the previous food-for-work and employment generation schemes.[4] Public works maintains the principle of supporting vulnerable households through the provision of work on community projects during the slack season. The overall system relies heavily on local community agents, including for the targeting, which is community-based. Relief aid[5] is still in place, but only as an intervention during times of crisis, such as droughts or floods. Aid eligibility in the PSNP is determined in different steps, combining two targeting methods, geographical and CBT. Following its federal administrative structure, the central government first selects the *woredas* (districts) and assigns each an amount to be disbursed. Then, *kebele* (representing several villages, the lowest administrative level in Ethiopia) committees, comprised of local administrators and community leaders select beneficiary households.

Political power in Ethiopia is in principle derived from "free and fair" elections, although there is evidence of manipulation and threat (Aalen and Pausewang, 2002). The leaderships of different levels (sub-regional, regional, and federal) are typically linked and the current local elite has been in charge for more than two decades. In order to avoid repression and exclusion from accessing benefits and local services, households tend to vote for the ruling party. Therefore, households that receive food aid are likely to be politically connected to the local leadership (Caeyers and Dercon, 2012).

A number of empirical studies have compared two common targeting methods applied in developing countries, proxy means tests (PMTs) and CBT. Some find that local agents in communities have additional information to refine targeting, beyond a PMT (Alderman, 2002; Galasso and Ravallion, 2005), while others find no better accuracy in identifying the

[4]Food-for-work (FFW) and employment generation schemes (EGSs) were essentially very similar, namely providing aid in exchange of work on projects to build public assets. The main difference between the two is that FFW were implemented on a relatively small scale with development objectives, whilst employment generation schemes had the same logic as food-for-work but were implemented in emergency contexts (Sharp, 1998).

[5]Food aid, relief aid, emergency aid and free food distributions are used here interchangeably, as is common in the literature.

poor of one method over the other (Alatas et al., 2012).[6] However, targeting based on community knowledge typically results in higher satisfaction and greater legitimacy of the process by recipients. Relatively few studies have investigated the relationship between political economy of transfers and targeting performance in general (Alatas et al., 2012; Conning and Kevane, 2002), or in Ethiopia specifically (Broussard et al., 2014; Caeyers and Dercon, 2012), and none have looked at the changes in targeting performance over time. This paper fills a gap by providing evidence on the relationship between targeting performance and a change in aid implementation guidelines. In particular, I exploit the panel nature of the Ethiopian Rural Household Survey (ERHS) by using the last two rounds (2004 and 2009) to compare if targeting performance changed as a consequence of the effort by the government to improve monitoring and transparency within the context of the PSNP. My findings suggest there was an improvement in targeting for public works, both in the selection process and in the amount of aid received. By 2009, aid was more means-based and no longer dependent on political connections. Although the data does not allow the investigation of the channels behind such improvement, the implementation of the PSNP and the related efforts to improve transparency of the selection process brings one to speculate that this was a key factor in determining improvements. My results are broadly in line with the limited related literature. For instance, (Ravallion, 2000) reports within-province improvement after a set of rules on implementation and targeting was provided by the central Argentinian government, together with a larger budget. Targeting of food aid, on the contrary, does not seem to show as clear-cut improvements in 2009 compared to 2004. Whilst political connections no longer determine food aid allocation, vulnerability is only a significant contributor in 2009 when the analysis is restricted to villages which receive both food aid and public works.

[6] In Indonesia, authors find the PMT to produce a lower error rate overall, although when focusing on the very poorest households within the poor category, community-based targeting slightly outperforms PMT.

The chapter proceeds as follows: Section 2.2 offers an overview of aid in Ethiopia and delineates the system used to distribute aid and the criteria used for targeting aid beneficiaries in Ethiopia; Section 2.3 summarises the literature on targeting, focusing on aid in Ethiopia; Section 2.4 provides a description of the data used for this study and presents summary statistics; Section 2.5 explains the models used to identify the differences in targeting across the two survey waves and Section 2.6 presents and interprets the main results. Section 2.7 conducts robustness checks and Section 2.8 summarises the conclusions of this chapter.

2.2 Targeting of aid in Ethiopia

For over 20 years, emergency food aid was the main response to ensure survival of hundreds of thousands of poor households in Ethiopia in the face of widespread food insecurity and famine. The Ethiopian government had to launch annual international emergency appeals for assistance. This aid was channelled to meet the consumption needs of all food insecure households, independently of the type of insecurity they were facing, whether or not it was temporary and driven by a specific shock (i.e. drought), or the household faced chronic extreme poverty conditions. These emergency appeals asked for international support particularly in times of severe droughts, which manifest in the country with alarming frequency and affect millions of people. Despite the consistent amount of food aid that Ethiopia had received (estimated at 700,000 metric tons per year between mid-1980s to early 2000s, corresponding to roughly 20% of all food aid deliveries to Sub-Saharan Africa, Van Domelen and Coll-Black (2012)), operations were unpredictable, and deemed ineffective in reducing chronic poverty.

By the end of the 1980s, the rationale for the food assistance was gradually expanded from famine relief to 'rehabilitation'. In 1993, a new strategy was outlined in the National Policy on Disaster Prevention Management (NPDPM) which mandated the move

from free food distributions towards providing relief food to able-bodied in exchange for labour on public or community development works (called Employment Generation Schemes (EGSs)), with only those unable to work entitled to free food (Sharp, 1998). Initially it was foreseen that only 20% of food aid was to be disbursed as free food, while the remaining 80% was supposed to be delivered through EGSs. In practice, however, in most locations EGSs degenerated into free food distributions, due mainly to lack of non-food budgetary support to the local administrations to implement workfare programmes (World Bank, 2005). Due in part to these operational issues, emergency aid remained the principal component of food aid through the early 2000s.

By 2000, it was increasingly clear that the emergency food aid system was unsustainable and of limited effectiveness.[7] The crises of 1999-2000 and of 2002-2003, brought about by severe and widespread droughts in which 10 and 14 million individuals were in need of aid, proved the need for reforming the food aid system (Van Domelen and Coll-Black, 2012). In 2003, the Government of Ethiopia started a consultation process with its main development partners aimed at creating a more effective solution able to address both the immediate needs of food insecure households, as well as a long-term vision to enable households to graduate from emergency systems. As a result, the Food Security Programme (FSP) was outlined which consisted of three main pillars, namely: 1) developing a safety net for chronically food insecure households, 2) supplying agricultural and financial services to food insecure households to promote their graduation out of food insecurity, and 3) resettling households from unsustainable and environmentally degraded

[7] Few studies have investigated the impact of aid programmes in Ethiopia. Gilligan and Hoddinott (2007) analyse whether aid distributed after the 2002/2003 drought had a persistent effect on consumption, food security and asset holdings. For food-for-work beneficiaries, positive impacts are found in consumption growth and in the reduction of perceived famine risk. Free food distribution recipients also positively benefited in terms of consumption, although their perception of famine risk remains negative. In addition, by differentiating the analysis by household welfare distribution, food-for-work appeared to have a stronger impact on households belonging to higher levels of the welfare distribution, whereas free food distributions revealed to have a stronger impact on the poorer. Amare and Asfaw (2012) estimate the impact of the two aid programmes on poverty and inequality over the same period. They find that food-for-work has had a positive and significant effect only on head count index, whereas the gap and severity indexes are not significantly affected. Free food distributions, instead, had a positive and significant impact on all three poverty measures.

lands. This combination of programmes was launched in 2005 as the Productive Safety Net Programme (PSNP).

As previously stated, the PSNP has two main components, public works and direct support, both paid either in cash (method of payment preferred by Government and donors), in-kind, or a mix, depending on the location and the timing of the year. The main component, public works, aims at covering 80% of total beneficiaries and originally paid 6 Birr per day (increased to 8 Birr in 2008 and to 10 Birr in 2010 due to inflation) for work on community infrastructure, labour-intensive projects during the months of non-farming activities.[8] Direct support targets labour-constrained households with payments based on public works daily wage for public works. Each PSNP household member is entitled to receive a transfer based on 5 days of work, up to a maximum of 15 days per month, at the above-reported wage over a period of six months (Van Domelen and Coll-Black, 2012). If a household is identified as being eligible for the PSNP, all household members are listed as clients of the programme, as per the full-family family targeting principle (PSNP PIM, 2010). However, transfers are only provided for up to five household members and the public works requirement is based on the number of transfers the household is receiving. For households of five and under, all family members, regardless of their age, are listed and eligible for a transfer (PSNP PIM, 2014). A household is entitled to be a PSNP beneficiary until reaching graduation, when they are food secure.[9]

Despite the focus on chronic food insecurity, transitory needs are also taken into account within the PSNP. First, an annual retargeting was designed to correct for inclusion and exclusion errors in order to respond to changes in the relative positions of house-

[8] The original government rate for public works was set at 3 kg of grain per day although it was never calculated if that corresponded to a below-market wage, mainly due to the difficulty in attributing a realistic value of a local wage rate in food insecure areas where work is not or scarcely available at the relevant time of the year. The PSNP daily payment is the same as pre-PSNP public works (Van Domelen and Coll-Black, 2012).

[9] "A household has graduated when, in the absence of receiving PSNP transfers, it can meet its food needs for all 12 months and is able to withstand modest shocks" (Food Security Coordination Bureau (FSCB) , 2007).

holds. Secondly, a contingency budget of 20% is added to the programme budget to cover additional households that might become chronically food insecure during the course of the programme and to respond to transitory needs deriving from shocks. Lastly, the emergency response system continues to cover food-insecurity in non-PSNP *woredas*.

Aid eligibility for PSNP, is determined in three steps, following the federal administrative structure of the Ethiopian government. First, the government selects the chronically food insecure *woredas* for programme receipt using historical data (a *woreda* that has received food aid for the preceding three years or longer is classified as food insecure (PSNP PIM, 2006)). Second, *woredas*, in turn, allocate funds to *kebeles*, assigning funds to 'PSNP quotas' defined on the basis of the number of eligible households indicated by the *kebele*. And finally, households are selected through CBT.

At each step, the main responsibility for targeting falls under specially constituted Food Security Task Forces (FSTFs). The Woreda FSTF is responsible for adapting the general national guidelines in terms of beneficiary selection criteria to make them relevant to the local context and for training the Kebele FSTF. In turn, the Kebele FSTF is responsible for establishing a Community FSTF in each village and training the Community FSTF on targeting procedures. The Community FSTF is the ultimate body responsible for the actual screening of households for eligibility and for developing the list of beneficiaries. The Community FSTF is composed of representatives from the Kebele FSTF, a Development Agent, two or three elected female representatives, two or three elected male representatives, an elected youth representative, and an elected representative of the elderly.

As stated in the Programme Implementation Manual (PIM) (PSNP PIM, 2006), the eligibility for participating into the PSNP is restricted to the chronically food insecure households residing in PSNP *kebeles*. Chronically food insecure households are defined as: (a) households that have faced continuous food shortages (usually 3 months or more) in the

last 3 years and received food assistance prior to PSNP implementation; (b) households that suddenly become more vulnerable as a result of a severe loss of assets and are unable to support themselves (last 1-2 years); (c) any household without family support and other means of social protection and support. Households that meet these preliminary criteria are further examined by the Community FSTF to refine the selection of beneficiaries on the basis of additional characteristics: household assets (landholdings, quality of land, food stocks on hand, etc.), income from non-agricultural activities, and support or remittances from relatives or other actors. Starting with a list of past aid beneficiaries, the Community FSTF updates the list based on the refined beneficiary selection criteria. The selection process is carried out on a yearly basis to update the list of most in need households. Depending on the presence of able-bodied members, households are assigned to either public works or to direct support.

Several aspects of the PSNP targeting are very similar to the system of targeting for the emergency relief, including the institutional structure, the key role of community representatives, the division between public works and direct support according to their ability to work, and criteria for households' selection. In the PSNP PIM it is recommended to build the FSTFs on the existing Disaster Prevention Committees, and the *woredas* included in the PSNP are by definition those which have previously received food aid. Notwithstanding the number of advantages deriving from relying on a pre-existing set up for the operationalisation of the PSNP, some weaknesses were found in the first years of PSNP implementation. For example, there was a tendency to spread or dilute transfers among more households than those officially targeted; there was variation in interpretation of guidelines; there was a tendency in some locations to favour those with connections with the local administration (Sharp et al., 2006).

Efforts were made over the years of implementation of the PSNP in order to improve transparency and ownership by the community. For example, since 2007, grievance proced-

ures have been separated from the targeting process and addressed by newly established Kebele Appeal Committees (World Bank, 2009). In addition, during annual retargeting, the proposed list of beneficiaries is displayed in public for at least a week; upon its endorsement by the general meeting of the village residents the list is finalised and passed it onto the Kebele FSTF for verification (Van Domelen and Coll-Black, 2012). Training of local agents and *kebele* officials were also aimed at increasing knowledge of the programmes' objectives and procedures. Similarly, the introduction of Client Cards aimed at "reducing the risk of rotation and improving the security of the transfer mechanism by ensuring client attendance during payments" (PSNP PIM, 2010). Assessment studies, on this and other operational issues, are periodically implemented to monitor the level of implementation of all procedures (i.e. World Bank 2009). These reforms were among the main differences included in the PSNP guidelines as a measure to improve fairness and transparency of targeting.

2.3 Literature on targeting effectiveness

The main objective of social protection programmes is to provide financial support and linkage to services and systems to the most impoverished and vulnerable individuals in society. Targeting is the tool that is used to identify eligible individuals and screening out the ineligible from a population (Devereux et al., 2015). Means testing, proxy means tests, categorical, geographic, and community based are among the most common methods of targeting, which are often used in combination and chosen in accordance to programmes objectives. For instance, the Kenya Cash Transfer for Orphans and Vulnerable Children (CT-OVC) implemented first a geographic targeting, by selecting the poorest districts. It then implemented a categorical targeting, by selecting as eligible those households caring for orphans, and a community-based component by which the community identifies the poorest households with orphans. The PSNP itself applies a mix of geographic and CBT.

Targeting can be assessed against different dimensions, for example its inclusion and exclusion errors, whether by design or by implementation (Sabates-Wheeler et al., 2015). There is no consensus in the literature in terms of metrics to be used in assessing the superiority of one method over another. Coady et al. (2004) suggest the ratio of the value of transfers going to the poor to the (relative) size of the poor in the population as a potential metric to compare across targeting methods. However, Ravallion (2009) casts doubt on the external validity of cross-programmes comparisons based on these measures. Other authors (Hoddinott, 1999; Besley and Kanbur, 1990) use the poverty gap or poverty headcount to assess errors of inclusion and exclusion.

There are acknowledged advantages and disadvantages related to CBT. On one hand, CBT has more and locally adapted information taking into account local conditions and culture. Furthermore, CBT is typically associated with lower costs of administration, and higher levels of satisfaction of beneficiaries and accountability. All these elements are weighed against the potential risks of elite capture and rent-seeking behaviours (Alatas et al., 2012; Conning and Kevane, 2002). The CBT used in conjunction with other targeting methods are examined in three African countries implementing cash transfers, Malawi, Kenya and Mozambique (Handa et al., 2012). The authors find that in these contexts targeting was effective at reaching the poorest households, which is attributed to the hybrid methodology (CBT alongside demographic criteria in Malawi and Mozambique and PMT in Kenya). These programmes are ultimately found to perform well also when assessed using international indicators. Sabates-Wheeler et al. (2015) exploits the randomisation of targeting methods in a cash transfer in Kenya to directly compare three methods of targeting, CBT, and two categorical methods based on demographics (households with high dependency ratios and households with members older than 55 years of age) and find that CBT performs better in identifying the poorest households and, at the same time, was more likely to be perceived by the communities as fair. However, some weaknesses

were also identified, especially related to local elite capture.

In Ethiopia, interest of researchers and policy makers around targeting began in the end of the 1990s due to concerns about food aid dependency, coupled with a demand for greater accountability over its use. A first stream of literature around 2000 concentrated on assessing targeting effectiveness in terms of errors of inclusion and exclusion, both at the household and community level (Clay et al., 1999; Jayne et al., 2001, 2002). All these studies found large differences in food aid allocations across regions that cannot be explained by observable characteristics such as mean per capita income or rainfall (Clay et al., 1999). The Tigray region in particular was found to be the mostly favoured, which is notable as the ruling party in Ethiopia is from the Tigray region. Despite some evidence of means-based targeting at woreda and at household level, the main determinant of geographic food aid allocations was past aid allocation. Such rigidity in food aid distribution does not seem to be accounted for by chronic needs or weather shocks (Jayne et al., 2002). These findings seem to support the speculation that food aid was being used by the Ethiopian government to transfer resources to favoured regions. In other words, allocation at the woreda level followed negotiations between the government and the local administrative staff on grounds other than effective needs (Jayne et al., 2001; Ferriere and Suwa-Eisenmann, 2014). Within woredas, households were found to be targeted partly according to their levels of income, although more effective targeting was generally found for food aid compared to food-for-work. Both types of aid were significantly explained by past allocation of aid, and in addition disproportionate number of female and elderly heads were targeted for food aid (Clay et al., 1999).

Rules determining allocation of either type of aid programme varied widely geographically. In relation to food-for-work specifically, no consistency in the selection methodology of beneficiaries was found. In some cases, there were no eligibility rules set by local authorities based on the assumption that this programme would automatically select the poor

via self-selecting those most in need (Jayne et al., 2001; Sharp, 1998). In other cases, local leaders selected households satisfying certain criteria, at times rotating beneficiaries lists. Due to lack of employment opportunities, households working on public works projects were found to be those at the two extremes of the income distribution.[10]

More recently, Coll-Black et al. (2012) assessed targeting effectiveness within the context of the first phase of the PSNP (2005-2008) through results of a survey directed at local officials in PSNP woredas. While large differences across regions were found, with some regions prioritising poverty and others focusing mainly on labour supply endowments and demographic characteristics, overall there is evidence of means-based targeting within public works. Poverty was the most important criterion reported by officials, while food insecurity was less important, although this may be due to the difficulty in ascertaining degrees of household food insecurity. Other characteristics that were reported as important in targeting households for public works were low asset holdings (livestock and land), large households, households with many elderly or orphaned members, or those affected by drought. In targeting households for direct support, higher priority is given to households with limited labour endowments, especially households with elderly, disabled or sick members as household head or primary income earners. Fewer differences across regions are found in the context of direct support as compared to public works. In order to compare the PSNP targeting effectiveness with other programmes, they compile the Coady-Grosh-Hoddinott (CGH) indicator for different deciles of the income distribution. The CGH for the poorest deciles scores 1.69, for the bottom two deciles 1.46, and 1.26 for the bottom four deciles. Thus, compared to the international average reported in Coady et al. (2004) of 1.25, the PSNP performs better and shows a more progressive targeting.[11]

[10] Due to the lack of employment opportunities, better-off households, often endowed with labour abundant supply, sought to participate in public works for additional income. Furthermore, anecdotal evidence reports that public works were generally regarded as part-time work, which could be combined with farming and other local income strategies (Sharp, 1998).

[11] A CGH index equal to 1 refers to a neutral targeting where everyone would receive the transfer, higher than 1 indicates a progressive targeting.

In the Ethiopian context, there is also empirical evidence pointing to political connections playing an important role in allocation of aid more generally. Broussard et al. (2014) focus on food aid allocation (excluding food-for-work) to explore the linkages between political connections, self-reported measure of power[12] and the probability of receiving food aid. They find strong and positive relations, especially for the richer households. They also investigate whether households received more aid in years in which they needed the most. Some of the findings are in line with targeting criteria, although it also emerges that on average households seem to receive more aid in less needy years. Caeyers and Dercon (2012) further explore the allocation of both food aid and public works by extending the analysis to horizontal and vertical connections after the severe drought that hit Ethiopia in 2002/2003. They also find that political connections play an important role in allocating food aid, while they do not for public works. They divide the analysis into two periods, right after the drought, at the peak of the crises, and the following year and they find that especially during the crises targeting was only slightly based on economics needs and mostly on political connections, while it seems to improve in the following period. The amount of food aid does not seem to be based on any observable characteristics, while for food for work, the most important determinant is political connections.

2.4 Data and descriptive statistics

The dataset used for this study is the Ethiopian Rural Household Survey (ERHS), a longitudinal household data set collected by the International Food Policy Research Institute (IFPRI), in collaboration with Addis Ababa University, and the Centre for the Study of African Economies, Oxford. The data was collected in seven rounds from 1994-2009 in 15 rural Ethiopian villages across different agro-ecological regions and from different

[12]They use the first 6 rounds of the Ethiopian Rural Household Survey (ERHS). From round 3 (1995), they use questions on involvement of the household in the local administration and their membership in other local organisations. From round 6 (2004), they use self-reported perception of power in the village.

woredas giving a sample of 1,477 households. For this analysis I use the last two rounds (2004 and 2009) as the aim is to compare targeting effectiveness before and during PSNP implementation (which started in 2005). Previous rounds of the survey are not used for comparability reasons. In rounds 1 - 4 questions on aid are asked with reference only to the previous four months whereas the last three rounds collect information on aid over the previous twelve months making the calculation of received aid comparable. In addition, in the last two rounds households were asked the number of months during which they suffered food insecurity. This variable is preferred as a proxy for vulnerability instead of traditional consumption as explained further in subsequent sections.

The measure of food aid utilized in this analysis is from the questionnaire section on off-farm income and is constructed by summing all gifts from the government or non-government organisations received by the households in the form of food aid or cash in the previous 12 months of the interview.[13] The public works variable is constructed by combining information from two sections of the questionnaire. One is a section entirely dedicated to PSNP and public works and the second is the off-farm income from which public works provided by government or NGOs are considered.

As previously stated, this analysis considers only the villages that received public works or food aid, which following Broussard et al. (2014) is defined as those with at least 10 households reporting receipt of aid. Over the years under analysis 9 villages received public works and 10 received food aid. However, since I look at intra-village aid allocation, the sample is further restricted to include villages only in rounds in which they actually received aid. The public works sample includes 740 households and 1,121 household-round observations across 9 villages in 2004 and 6 villages in 2009; the food aid sample includes 880 households giving a total of 1,363 household-round observations across 8 villages in

[13] Among those households that reported receipt of food aid 12% claim having received direct support as well. This figure does not seem to be reliable, as according to official data, only 3% of PSNP beneficiaries received direct support. It might well be that households confused food aid with the direct support component of the PSNP. This should not be a major issue for this analysis as targeting criteria for food aid and direct support should be similar, especially along vulnerability lines.

2004 and 9 villages in 2009.

Table 2.1: Poverty headcount ratio (HCR) by village and survey year

	Total		Public Works only		Food Aid only		Both types of aid		None	
	N	HCR	N	HCR	N	HCR	N	HCR	N	HCR
2004	15	0.39	1	0.45	0	-	8	0.42	6	0.34
2009	15	0.58	2	0.52	5	0.72	4	0.74	4	0.25

Notes: Poverty rates are calculated at household level using net-of-aid consumption in adult equivalent units.

As discussed in Section 2.2, there are two layers of targeting, the first from the federal government to *woredas*, and the second within selected villages to households. Despite the small number of villages making it difficult to fully assess the effectiveness of targeting at the *woreda* level, it is still possible to see if there are average differences across villages that received aid and those that did not. Table 2.1 reports the poverty head count ratio across villages type by survey year. Overall, average village poverty rates are 39% in 2004. In 2009, the average poverty rate dramatically increased to 58%.[14] In both periods, it seems that villages receiving aid were worse-off than those not receiving aid. In 2004, 34% of households were living below the poverty line in villages that were not targeted for aid, while in villages that received aid of any type (public works only or both types of aid), the proportion was much higher (45% and 42% respectively). The difference in poverty rates is even more striking in 2009. Villages that received both types of aid and villages that received food aid only registered average poverty head count ratios as high as 74% and 72% respectively. In villages that received public works only, 52% of households were living below the poverty line, a figure more than double the 25% in villages that did not

[14]The poverty rate in 2004 is in line with national figures for rural areas reported by the government (MOFED, 2012). For 2009, there are no official statistics. The closest available refers to 2010/2011 and reports 30% of households under the poverty line (Ministry of Finance and Economic Development (MOFED), 2012). Despite no comparable rates for 2009, it is highly unlikely that the figures for the ERHS reflect those at national level. As Dercon et al. (2012) show, the mean consumption growth in the ERHS villages does not track the real GDP per capita growth which is positive over this period. The authors attribute this divergence from national trends to two main reasons. First, several villages in two regions (Tigray and SNNPR) experienced severe localised droughts that caused considerable income losses. Secondly, the collection of data in the 2009 round was carried out approximately six months after the 2008 harvest and in the aftermath of the rapid rise in food prices in 2008. The 2009 round may have taken place just at the point where food stocks had run low. Since most ERHS households are net food purchasers, during this period of high prices, households may have been reducing food consumption.

receive any type of aid. These figures seem to suggest sound targeting from the federal government, and an improvement compared to findings carried out using national data for a decade earlier (Clay et al., 1999). However, given the limited number of villages these findings cannot be generalised to the overall country. As a point of comparison to understand variation in village-level poverty rates and aid receipt, Table A.1 of the Appendix reports similar figures as Table 2.1, disaggregated for each village averaged across the two rounds.

Table 2.2: Percentage of beneficiaries in sample villages by aid status and survey year

	Public Works only	Food Aid only	Both types of aid	None
2004	0.29	0.20	0.25	0.26
2009	0.20	0.37	0.06	0.37

Notes: Figures includes only villages that received either Public Works or Food Aid (n=9 in 2004 and n=6 in 2009 for Public Works. n=8 in 2004 and n=9 in 2009 for Food Aid).

The vulnerability of the villages under analysis contributes to explain the high proportion of aid beneficiaries in this sample, with 74% households receiving at least one type of aid in 2004 and 63% in 2009 (Table 2.2). The high proportion of aid beneficiaries in both years is explained by the fact that both 2003 and 2008 were years of severe droughts, with large emergency aid responses.[15] The main difference between the two years is that the proportion of households receiving both types of aid dropped dramatically (from 25% in 2004 to 6% in 2009), driven by decrease in public works participation, which decreased from 29% in 2004 to 20% in 2009.

I next present descriptive information on beneficiary targeting at the household level. The criteria used by village leaders to select beneficiary households in 2004 is available from the community survey. These criteria are specific to food aid, and ranked from the most to less frequently reported, are: people unable to work, old people, poor people, landless families, large families, people with limited livestock, and female-headed households (not

[15] US$804 and US$886 were received by Ethiopia to cope with the severe droughts (Development Initiative, 2010).

shown). The 2009 questionnaire did not include a similar section on criteria used by village leaders, however according to general guidelines and studies on targeting of PSNP (Sharp et al., 2006), criteria seem in be similar across the two rounds.

Table 2.3 reports beneficiary status by survey wave and poverty. In the villages targeted by either public works or food aid, poverty rates dramatically increased from a headcount ratio of 43% in 2004 to 70% in 2009. Two main remarks come out from these figures. First, the fraction of non-poor households receiving aid is quite high, pointing at targeting errors. For instance, among non-poor households 55% and 36% received public works in 2004 and 2009 respectively. In terms of amount, on average poor households do receive higher quantities of aid, in both years and for both types of aid. The second point regards the difference in targeting between 2004 and 2009. In 2004, the proportion of poor and non-poor households were very close among beneficiaries for both public works and food aid, while in 2009, the proportion of beneficiaries among non-poor households is much lower suggesting an improvement in targeting. A caveat however must be considered in looking at these figures. Poverty rates here are calculated based on net-of-aid consumption, which is not an accurate counterfactual for household consumption in the absence of aid as it ignores the behavioural response to aid. Therefore these descriptive statistics (similar to Table 2.2), should be only considered as indicative differences between beneficiary and non-beneficiary households.

A similar trend can be seen in Figure 2.1 where the left-side panels show the probability of receiving public works (top) and the per capita amount of aid received (among aid beneficiaries, bottom) by percentiles of pre-aid consumption per capita; the right-side panels show the same for food aid. In terms of accessing the programmes, both top graphs show an improvement for public works and food aid. In 2004, targeting seems to be independent of need as the line for both types of aid is almost horizontal.

However, as earlier alluded, consumption is not an ideal measure of need. Not only it

Table 2.3: Poverty and aid targeting by survey year and type of aid

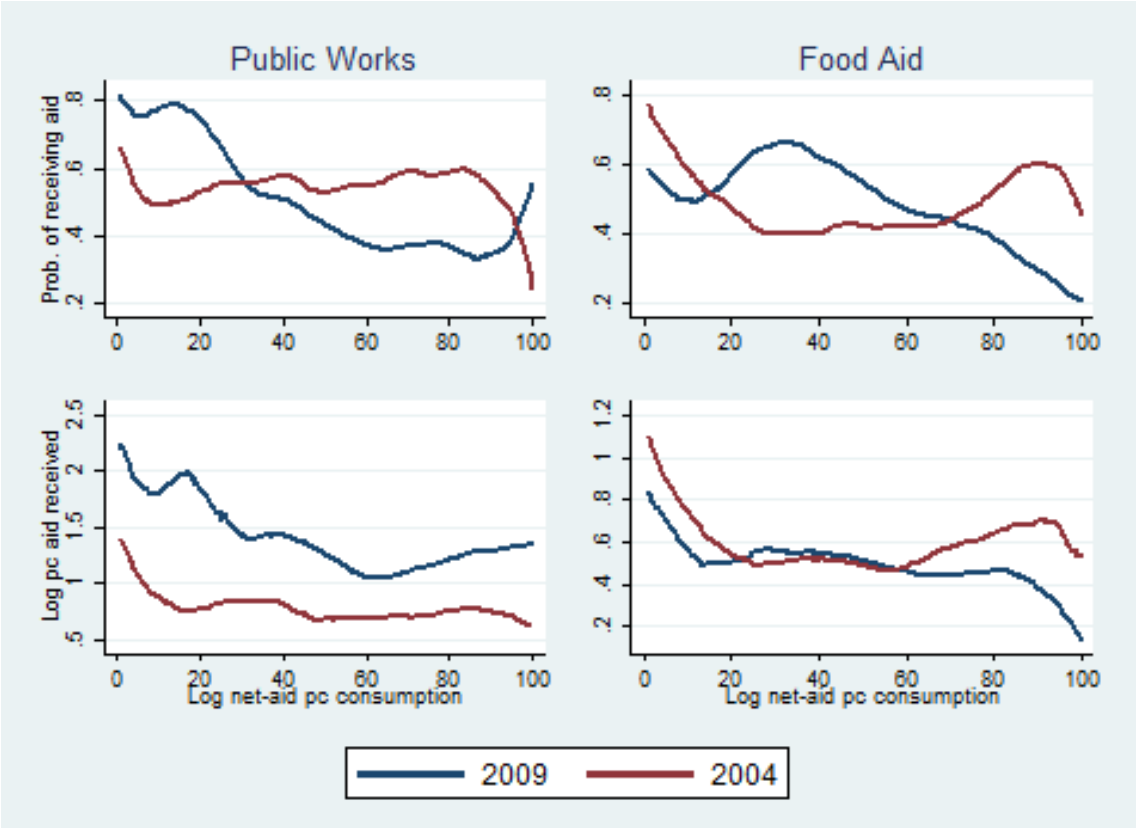
	Total households		Poverty rate			
	Public Works	Food Aid	Full sample			
2004	686	626	0.43			
2009	418	720	0.70			
Total	1,104	1,346	0.58			
	Public Works			Food Aid		
	All	Poor	Non-poor	All	Poor	Non-poor
2004	0.54	0.54	0.55	0.49	0.50	0.48
2009	0.51	0.57	0.36	0.49	0.54	0.34
Total	0.53	0.56	0.50	0.49	0.53	0.43
	Public Works			Food Aid		
	All	Poor	Non-poor	All	Poor	Non-poor
2004	2.23	2.78	1.83	1.23	1.38	1.12
2009	4.99	5.58	4.01	1.04	1.21	0.67
Total	3.36	4.19	2.51	1.13	1.27	0.94

Notes: Poverty rates are calculated at household level and include public works and food aid villages only. Villages are included only for the years in which they received aid (n=9 in 2004 and n=6 in 2009 for Public Works. n=8 in 2004 and n=9 in 2009 for Food Aid). Poverty rates are calculated by using net-of-aid consumption in adult equivalent units. Amount is calculated in real terms and per capita and includes only households that received aid.

is based on a very limited period of recall time (over the last 7-days) but in cases of aid given to better-off households (as shown to be the case in the previous studies), pre-aid consumption might provide a distorted picture of initial welfare conditions. A preferred measure would be asset stores, represented by per capita livestock holdings 12 months prior to the interview, reflecting wealth holdings before receiving aid. In addition, the use of livestock as a proxy for assets is justified in this context also by its importance in contributing to livelihood of Ethiopian households and to the overall economy. Ethiopia, in fact, has the largest livestock population in all Africa, whose contribution amounts to 12% of the GDP (Endalew and Ayalw, 2016) and to over 45% of agricultural GDP (Roy Behnke and Fitaweke Metaferia, 2011). Figure 2.2 shows the probability of receiving aid (top panels) and the amount of aid received (among aid beneficiaries, bottom panels) by per capita livestock asset percentiles. For both public works and food aid there seems to be an improvement in targeting from 2004 to 2009.

Table 2.4 shows selected statistics of the households' characteristics in the sample

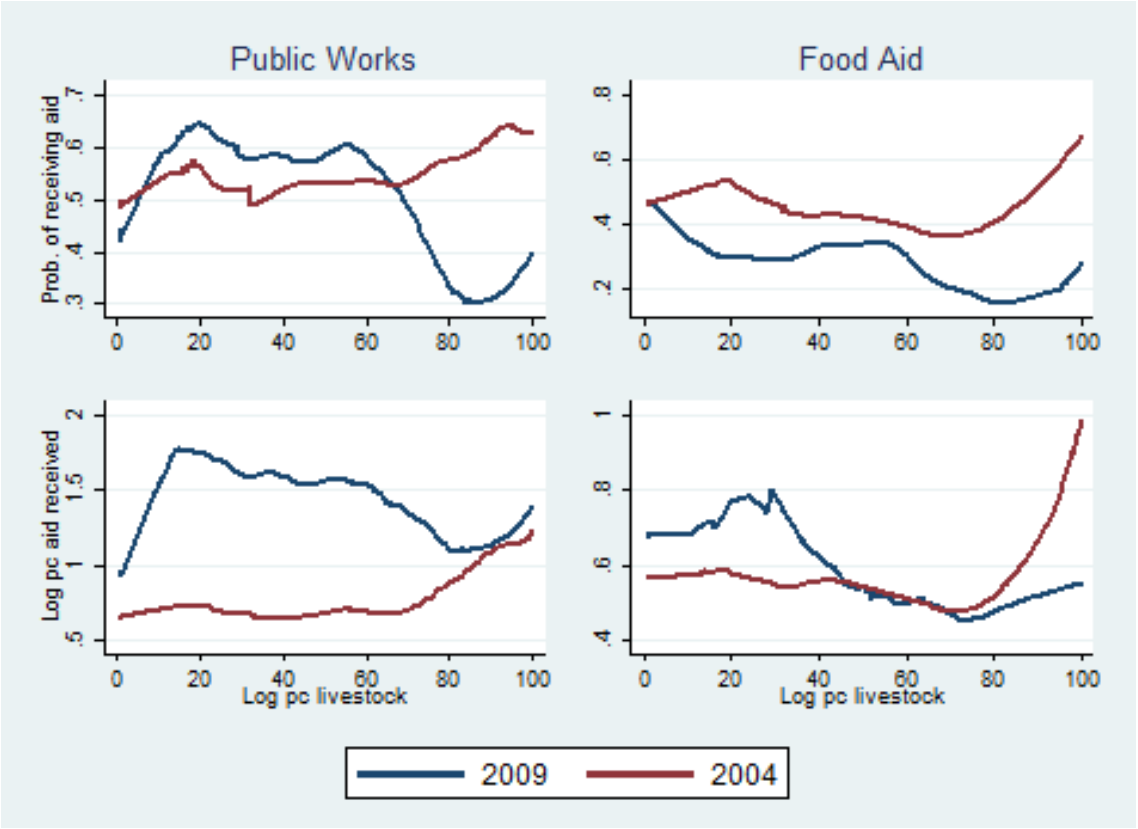
Figure 2.1: Public Works and Food Aid targeting by year and by consumption net of aid



Notes: Consumption is real consumption per capita in logs net of aid. Reported lines represent locally weighted scatter-plot smoothing, obtained with a 0.3 bandwidth. Graphs for public works include only public works recipients' villages (n=9 in 2004; n=6 in 2009); graphs for food aid include only food aid recipient villages (n=8 in 2004; n=9 in 2009). Top panels include all households (n=686 in 2004 and n=425 in 2009 for public works; n=626 in 2004 and n=737 in 2009 for food aid), while bottom panels include only aid beneficiaries (n=373 in 2004 and n=215 for public works; n=307 in 2004 and 360 in 2009 for food aid).

receiving public works and food aid for beneficiaries and non-beneficiaries separately for the two rounds. Demographic variables are household size (log), proportion of elderly (defined as members $<$ 64 years old), a dummy variable for whether the household head is female or not. A variable to capture the human capital of the household head, proxied by a dummy variable which takes value 1 if the household head has completed primary school is included. In this context this variable might proxy for wealth or income earning potential, but also for the ability to enforce access to aid. Assets are proxied by the lagged livestock value per capita (log) 12 months before the interview. I include the lagged value of livestock as it better reflects the economic situation of households before aid distribution therefore providing a better picture of the wealth situation before targeting

Figure 2.2: Public Works and Food Aid targeting by year and by livestock holdings



Notes: Livestock is the value of lagged per capita livestock expressed in real terms and in logs. Reported lines represent locally weighted scatterplot smoothing, obtained with a bandwidth of 0.4. Graphs for public works include only public works recipients villages (n=9 in 2004; n=6 in 2009); graphs for food aid include only food aid recipient villages (n=8 in 2004; n=9 in 2009). Top panels include all households (n=686 in 2004 and n=425 in 2009 for public works; n=626 in 2004 and n=737 in 2009 for food aid), while bottom panels include only aid beneficiaries (n=373 in 2004 and n=215 for public works; n=307 in 2004 and 360 in 2009 for food aid).

had taken place. Although lagged livestock values may be determined in part by past aid, this should not affect the results as selection of beneficiaries happens on a yearly basis.

As a measure of 'need' and vulnerability, unlike other studies on targeting in Ethiopia (Broussard et al., 2014; Jayne et al., 2001, 2002), I use a direct indicator of food insecurity instead of consumption (or income) net of aid. As previously mentioned, consumption net of aid is not a valid counterfactual of what the household would have consumed in the absence of aid. This measure ignores the behavioural response of the household. Additionally, in case of errors in targeting, it would provide a distort representation of the initial level of consumption. This could be the case if households that are not particularly in need receive high amounts of aid. The consumption net of aid in this case may

Table 2.4: Characteristics of households by beneficiary status and survey year

	2004			2009		
	Non beneficiary (1a) Mean	Beneficiary (1b) Mean	Diff. (1a) - (1b)	Non beneficiary (1a) Mean	Beneficiary (1b) Mean	Diff. (1a) - (1b)
Panel A: Public Works						
Head primary education	0.14	0.14	-0.00	0.16	0.13	-0.03
Female head	0.36	0.28	-0.08**	0.37	0.46	0.09**
Age head	51.30	48.01	-3.29***	54.29	50.23	-4.06***
Ability score	1.46	1.24	-0.22***	1.62	1.38	-0.24***
Household size	5.41	5.94	0.53**	5.50	5.75	0.25
Share of elders	0.10	0.04	-0.06***	0.12	0.05	-0.08***
Livestock pc	307.25	362.27	55.03*	350.67	217.47	-133.20***
Food insecurity	2.89	3.60	0.72***	3.45	4.58	1.13***
Political connections	0.32	0.46	0.13***	0.32	0.33	0.01
N	313	373		207	211	
Panel B: Food Aid						
Head primary education	0.13	0.12	-0.01	0.24	0.21	-0.03
Female head	0.33	0.33	-0.00	0.34	0.33	-0.00
Age head	49.08	51.55	2.48**	50.15	55.02	4.87***
Ability score	1.27	1.47	0.20***	1.38	1.53	0.15**
Household size	5.98	5.50	-0.47**	5.97	5.66	-0.31
Share of elders	0.05	0.09	0.05***	0.06	0.13	0.08***
Livestock pc	311.34	362.29	50.95*	324.74	172.06	-152.67***
Food insecurity	3.11	3.49	0.38**	3.36	4.14	0.79***
Political connections	0.38	0.45	0.08**	0.40	0.43	0.02
N	319	307		370	350	

Notes: Figures include only villages that received either Public Works (n=9 in 2004 and n=6 in 2009) or Food Aid (n=8 in 2004 and n=9 in 2009).

incorrectly picture these households as particularly in need. In addition, since I am interested in the amount of aid distributed to households, this indicator might conceal the real pattern of distribution of aid, especially if more aid is given to relatively better off households, as it had been shown in the literature (Broussard et al., 2014; Clay et al., 1999). The indicator that I use instead to proxy for vulnerability is the number of months during which a household has had problems in satisfying the food needs in the previous 12 months. Political connections are proxied by a dummy variable which equals to 1 if households have relatives or friends holding an official position in the *kebele* or elsewhere. All positions in the *kebele* are 'political' as appointed by political leadership (Caeyers and Dercon, 2012). This indicator was only collected in 2004, therefore, as other papers have done (Broussard et al., 2014), I assume that political elections and connections do not change over time. The work inability score is an average score based on five different

questions concerning the household head's ability to do daily activities.[16] Each question has four possible answers from performing the task easily (value 1) to not able at all to perform the task (value 4). The average score across the five questions provide an index of inability to work, ranging from one to four, one being able and four not at all.

In 2004 the main differences between public works beneficiaries and non-beneficiaries show some alignment of targeting with guidelines. While labour supply characteristics (lower share of elderly, with younger and with a lower work inability score household heads) and food insecurity are statistically higher among beneficiary households, at the same time, higher assets and political connections are also statistically higher among beneficiaries as compared to non-beneficiary households.

The differences among food aid beneficiary and non-beneficiary demographic households' characteristics move in the opposite direction as compared to public works, particularly for labour supply variables. Household heads are significantly older in targeted households than in non-targeted households, and have higher work inability scores. Households are smaller and with higher share of elderly among food aid beneficiary households than in non-beneficiary households. In relation to well-being and wealth, similarly to public works, beneficiary households have on average faced more months of food insecurity but, have also a statistically lower level of livestock holdings and a statistically higher share of beneficiary households reporting political connections.

The last three columns of Table 2.4 report the characteristics of beneficiary and non-beneficiary households and their difference in 2009. There are striking differences between 2004 and 2009 significance levels of the beneficiary and non-beneficiary groups, particularly in livestock and political connections. The difference in livestock holdings between beneficiary and non-beneficiary households in 2004 is statistically significant and positive while in 2009 the difference is negative, meaning that on average in 2004 targeted

[16] The questions were: "Can this person: (1) Stand up after sitting down? (2) Sweep the floor? (3) Walk for 5 kilometers? (4) Carry 20 liters of water for 20 meters? (5) Hoe a field for a morning?".

households held higher levels of livestock than non-targeted households while in 2009 the opposite is found. Political connections instead are statistically different in 2004 but not in 2009. These differences in livestock and political connections have a similar pattern for public works and for food aid targeting. These first figures suggest an improvement in targeting of the two aid programmes along some lines. However, these are only descriptive statistics that can only provide first hints into targeting patterns over time.

Political connections are not merely a proxy for the economic status of households. In Table A.2 in the Appendix, I report the estimates of the correlates to political affiliations based on a linear probability model. Political affiliations are positively correlated with heads with a minimum of primary education and to livestock holdings, and negatively with female household heads.

2.5 Empirical strategy and model specification

The same modelling approach, including choice of covariates are used for estimating targeting of public works and food aid. For each, I estimate two sets of outcomes, the probability of benefiting from the programme and the amount of aid received, as a function of observable household background characteristics. These characteristics are selected following inclusion in the official PSNP guidelines (which are later interpreted by village officials), as well as other variables that are expected to play a role in the aid distribution process. I estimate the following equation for household i in village j at time t:

$$Y_{ijt} = f(X_{ijt}, Z_{ijt}, v_{jt}) \tag{2.1}$$

where X_{ijt} is a vector of household characteristics (e.g. households' assets and demographics), Z_{ijt} includes variables that represents household's political connections, and v_{jt} controls for unobservable time-varying village fixed effects.

Although the official guidelines directly mention households that experienced a serious loss of assets as a consequence of shocks as households to be targeted, in my main model I do not include shock indicators. I run several specifications in which I include a number of shocks (death of household head, illness of household head or other members, drought, shocks related to livestock or crops and harvest, among others), as well as additional covariates including the size of the *iddir*[17] the household belongs to, the number of people the household could rely on in times of need. However, none of them appear to be correlated with aid receipt and thus I omit them in the main analyses (Results reported in Table A.3 in the Appendix).

As the interest of this study is to assess targeting effectiveness and to compare 2004 with 2009 targeting, and because the political connections indicator is time invariant, I first run a pooled model by appending the two rounds including only villages for years that received aid. I then run a fully interacted model between all the covariates and the 2009 time dummy to see if critical differences in implementation of targeting are actually found. I therefore estimate the latter model as the following specification:

$$Y_{ijt} = \beta_0 + \beta_1 X_{ijt} + \beta_2 Z_{ijt} + \beta_3 t_{2009} + \beta_4 (X_{ijt} * t_{2009}) + \beta_5 (Z_{ijt} * t_{2009}) + v_{jt} + \varepsilon_{ijt} \quad (2.2)$$

OLS estimation of parameters of this model is straightforward, but inference needs to control for likely correlation of the error ε_{ijt} over time for a given individual. For short panels, it is possible to obtain cluster-robust standard errors that cluster on the individual under the assumptions that errors are independent across individuals and that N $\to \infty$. I estimate equation 2.2 using probit modelling separately for participation in public works and for receiving food aid.

I further investigate aid by focusing on the determinants of amounts of aid received.

[17] The *iddir* is a funeral society. Members pay a regular contribution, and its benefits are that the group pays for the costs of a funeral of a member and any of its close relatives.

The specifications are the same as equation 2, however the dependent variable represents the amount of aid. For public works I consider three different dimensions: 1) the monthly average value of the quantity of food and cash received over the previous 12 months in real value of logged Birr, 2) the number of days any household member worked on public works projects (in the past 12 months), and 3) the daily wage over that same period. For food aid only total payments (cash and food) received in the previous 12 months in Birr are available. This second set of equations is estimated with a Tobit model to account for participation in or receipt of any aid. A number of critiques are made of Tobit models, mostly for the restrictions imposed on how relationship are modelled (constraining the signs of the covariates to align with the probit model) and for the normality assumption. However, various tests indicate that Tobit models can be used[18]. To satisfy the normality assumption, amounts in Birr are transformed into natural logarithms. In later sections, the robustness of this approach is further discussed.

2.6 Results

2.6.1 Public works

Table 2.5 reports the estimation results for public works. Column 1 gives the marginal effects of covariates of interest on the probability of receiving aid for public works in 2004 and 2009 (pooled model). Column 2 reports the marginal effects for the fully interacted model. The coefficients from the basic model show the probability of accessing public works in 2004, while once interacted with the 2009 time dummy, the coefficients reflect any comparative differences over time. To understand the net effect of targeting in 2009 the coefficients of the variables and their interaction with 2009 year indicator must be

[18]First I run a specification test, which is rejected, like in most applications as it is particularly strict. I then run an alternative test. Under the null hypothesis of a correct specification, the scaled maximum likelihood estimates from the censored Tobit model ($\hat{\beta}_{Tobit}/\hat{\sigma}$) should be reasonably close to the estimated Probit maximum likelihood coefficients $\hat{\beta}_{Probit}$. This test suggests that the Tobit model can be used.

summed. The net effect for 2009 is reported in Column 3. Overall the results from the pooled model show that public works participation is determined by labour supply characteristics (share of elderly and work inability score of household head), wealth, vulnerability and political connections. The coefficient of wealth, proxied by livestock, is negative and statistically significant, suggesting that increases in wealth are negatively correlated with the probability of accessing public works. Food insecurity is positively correlated with the probability of accessing public works. All results are in line with official guidelines promoting means-based targeting. However, the political connection variable is also statistically significant and positive, showing that having relatives or friends holding official positions in the local administration or other official positions increases the probability of participating in public works.

Columns 2 and 3 indicate there is no clear pattern of improvement in terms of means-based targeting in 2009 as compared to 2004. If on one hand there is an improvement related to the ownership of livestock (now showing a negative and statistically significant relationship), on the other hand food insecurity is no longer a statistically significant predictor in 2009. For livestock assets specifically, when evaluated at the mean value of all other characteristics, households at the 25th, 75th, and 95th percentile of lagged livestock value in 2009 have, respectively, 58%, 52%, and 50% of probability of being targeted for public works. Notwithstanding the improvement compared to 2004, it is worth noting that the richest households still have 50% of probability of accessing public works while a significant proportion of worse-off households were left out of the programme. On the other hand, food insecurity had a significant effect on the probability of accessing public works in 2004 but not in 2009. Keeping all other variables constant, in 2004 an additional month of food insecurity on average increases the probability of accessing public works by 3.2 percentage points.

The results show that political affiliations, while playing an important role in allocation

Table 2.5: Determinants of participation and amount of Public Works (2004-2009)

	Participation			Amount		
	(Pooled)	(Interacted)	(Net)	(Pooled)	(Interacted)	(Net)
Log household size	0.082	0.103*		0.332**	0.302**	
	(0.050)	(0.055)		(0.141)	(0.142)	
Share of elderly	-0.762***	-0.618***		-2.389***	-1.794***	
	(0.133)	(0.150)		(0.500)	(0.440)	
Female head (d)	-0.030	-0.081*		-0.080	-0.246***	
	(0.039)	(0.044)		(0.097)	(0.095)	
Head primary education (d)	-0.065	-0.076		-0.113	-0.152	
	(0.048)	(0.055)		(0.124)	(0.119)	
Work inability score of head	-0.070**	-0.064*		-0.180*	-0.132	
	(0.031)	(0.034)		(0.095)	(0.092)	
Log lagged pc livestock value	-0.024***	-0.008		-0.060**	-0.008	
	(0.009)	(0.009)		(0.024)	(0.023)	
Food insecurity	0.020***	0.032***		0.056***	0.075***	
	(0.007)	(0.010)		(0.020)	(0.021)	
Political connections (d)	0.061*	0.081**		0.156	0.173*	
	(0.037)	(0.039)		(0.098)	(0.099)	
Year 2009 (d)	-0.169*	0.242		-0.281	0.542	
	(0.089)	(0.215)		(0.298)	(0.652)	
t_1 * Log household size		-0.065	0.037		0.027	0.327
		(0.079)	(0.097)		(0.351)	(0.343)
t_1 * Share of elderly		-0.119	-0.739***		-1.032	-2.711***
		(0.194)	(0.248)		(0.951)	(0.959)
t_1 * Female head (d)		0.099*	0.019		0.520*	0.217
		(0.058)	(0.075)		(0.287)	(0.242)
t_1 * Head education (d)		0.049	-0.026		0.218	0.042
		(0.076)	(0.096)		(0.374)	(0.335)
t_1 * Inability score		0.001	-0.062		-0.120	-0.239
		(0.043)	(0.056)		(0.216)	(0.213)
t_1 * Log pc livestock		-0.032**	-0.040**		-0.183***	-0.170***
		(0.014)	(0.017)		(0.062)	(0.061)
t_1 * Food insecurity		-0.024**	0.007		-0.068	0.015
		(0.011)	(0.015)		(0.048)	(0.049)
t_1 * Political connections (d)		-0.063	0.017		-0.114	0.072
		(0.054)	(0.067)		(0.242)	(0.241)
Village - year FE	Yes	Yes		Yes	Yes	
Pseudo R^2	0.181	0.194		0.111	0.117	
Observations	1104	1104		1104	1104	
LL	-624.383	-615.648		-1660.587	-1650.312	

Notes: Amount is in real terms. Work inability score is an index based on 5 questions: "Can this person: (1) Stand up after sitting down? (2) Sweep the floor? (3) Walk for 5 kilometers? (4) Carry 20 liters of water for 20 meters? (5) Hoe a field for a morning?". Food insecurity refers to the number of months during which the household had problems satisfying its food needs. Political connections is a dummy equal to 1 if households have relatives or friends holding an official position in the *kebele*. Participation is estimated through probit model; amount is estimated through Tobit model. Reported coefficients refer to marginal effects for both models. Standard errors are clustered at the household level. Significance levels * 10% ** 5% *** 1%.

of public works in 2004, do not matter anymore in 2009. The interacted coefficient is not statistically significant although it is negative (showing an improvement with 2004 and not very small). In 2004, households that have friends or relatives holding an official position, including within the local administration, had an 8.1 percentage point higher probability of being selected for public works compared to households that do not have such affiliations. The magnitude of this effect might seem not particularly large, however when compared to other significant variables in determining targeting, the importance

becomes clearer. For instance, an increase of 1 month in the period of food insecurity faced by a household increases the probability of accessing public works of 3.2 percentage points. This implies that, *ceteris paribus*, a household with political connections has the same probability of accessing public works as one with 2.5 additional months of food insecurity (and without political connections). The coefficient on the interacted variable is not statistically significant, nor is the net effect of political connections in 2009.

Variables that capture labour supply characteristics also show a different pattern between 2004 and 2009 in a direction of fairer targeting in the latter period. While in 2004 female headed households were negatively correlated with the probability of accessing public works, in 2009 the coefficient is no longer significant. The coefficient of the interacted variable is, instead, significant and positive, showing that in 2009, compared to 2004, female headed households were more likely to be targeted. This, again, is in line with the guidelines of the PSNP, which foresee a number of gender-specific arrangements in an effort to make the PSNP as inclusive as possible. In 2004 female headed households were 8.1 percentage points less likely to access public works compared to non-female headed households. In 2009 the net effect of female-headed households is no longer significant. Work inability of the household head is negatively correlated with accessing public works in 2004, and no difference is found in 2009 (the net effect is also not statistically significant).

Household size is positively correlated with the probability of accessing public works in 2004 but no longer in 2009. This seems also an improvement since the past as previous studies showed public works were assigned on the basis of labour supply surplus, independently of the need of the household (Clay et al., 1999). Share of elderly is significant in both periods. An increase in the proportion of elderly in the household is associated with a decrease in the probability of accessing public works, in line with the guidelines which foresee an age limit to work on public works.

In addition to assessing the probability of accessing public works, I also investigate the amount of aid received from public works by looking at the monthly aid receipts (on average 2.43 in 2004 and 3.9 in 2009), the number of days assigned on public works (1.57 in 2004 and 1.72 in 2009), and the rate of daily wage (3.47 in 2004 and 4.89 in 2009, all expressed as logged values and reported in Table A.4 of the Appendix). The results are reported in columns 4-6 of Table 2.5 (monthly payment) and in Table A.5 in the Appendix (number of days and daily wage rate). The amount of aid received is expected to be subject to even more susceptible to political influence as compared to participation, as quantities are difficult to monitor. However, the results for all three outcomes reflect broad conclusions similar to those for participation.

2.6.2 Food aid

Table 2.6 reports the results of the probability of receiving food aid and the determinants of amounts of aid received. The pooled model shows that targeting of food aid is fairly means-based, although political connections also play a role. Households with fewer work-abled members, namely households with higher proportions of elderly and with higher work inability of the head, are more likely to receive food aid. However, levels of livestock are associated with a lower probability of accessing food aid.

The interacted model (Column 2) gives similar results to the pooled model, with significant determinants of food aid mostly consisting of labour-constrained households in 2004, as well as household political connections. A one-point increase in the work inability score (that ranges from 1 to 4) increases the probability of receiving food aid by 6 percentage points. Similarly to what was found for public works, having political connections increases the probability of receiving food aid by 7 percentage points. None of the coefficients of the interacted variables with the 2009 time dummy are significant, suggesting that no major difference in targeting procedures were applied in 2009 compared

to 2004, but also that targeting in 2009 is not based on any of the variables included in the model.

Table 2.6: Determinants of participation and amount of Food Aid (2004-2009)

	Participation			**Amount**		
	(Pooled)	(Interacted)	(Net)	(Pooled)	(Interacted)	(Net)
Log household size	-0.021	-0.029		0.018	0.016	
	(0.043)	(0.061)		(0.107)	(0.140)	
Share of elderly	0.373***	0.325**		0.684***	0.566*	
	(0.106)	(0.161)		(0.207)	(0.304)	
Female head (d)	-0.013	-0.033		-0.030	-0.056	
	(0.036)	(0.047)		(0.087)	(0.107)	
Head primary education (d)	0.010	0.011		0.009	-0.017	
	(0.042)	(0.060)		(0.102)	(0.142)	
Work inability score of head	0.078***	0.060*		0.221***	0.158**	
	(0.024)	(0.035)		(0.055)	(0.072)	
Log lagged pc livestock value	-0.013*	-0.012		-0.035*	-0.021	
	(0.008)	(0.010)		(0.018)	(0.023)	
Food insecurity	0.002	0.005		0.003	0.013	
	(0.007)	(0.010)		(0.017)	(0.023)	
Political connections (d)	0.078**	0.069*		0.149*	0.161	
	(0.030)	(0.042)		(0.078)	(0.101)	
Year 2009 (d)	0.108	0.043		0.444**	0.131	
	(0.075)	(0.208)		(0.225)	(0.707)	
t_1 * Log household size		0.014	-0.015		-0.068	-0.052
		(0.068)	(0.091)		(0.155)	(0.201)
t_1 * Share of elderly		0.042	0.366		0.367	0.933**
		(0.184)	(0.244)		(0.286)	(0.417)
t_1 * Female head (d)		0.032	-0.001		0.095	0.039
		(0.056)	(0.074)		(0.120)	(0.161)
t_1 * Head education (d)		0.003	0.015		-0.124	-0.142
		(0.061)	(0.086)		(0.144)	(0.202)
t_1 * Inability score		0.023	0.083		0.168**	0.326***
		(0.041)	(0.054)		(0.078)	(0.106)
t_1 * Log pc livestock		-0.002	-0.014		0.018	-0.003
		(0.012)	(0.016)		(0.024)	(0.033)
t_1 * Food insecurity		-0.003	0.001		0.001	0.014
		(0.010)	(0.014)		(0.020)	(0.030)
t_1 * Political connections (d)		0.009	0.078		-0.053	0.109
		(0.047)	(0.063)		(0.098)	(0.141)
Village - year FE	Yes	Yes		Yes	Yes	
Pseudo R^2	0.176	0.177		0.087	0.132	
Observations	1346	1346		1346	1346	
LL	-768.215	-767.595		-1763.278	-1239.227	

Notes: Amount is in real terms. Work inability score is an index based on 5 questions: "Can this person: (1) Stand up after sitting down? (2) Sweep the floor? (3) Walk for 5 kilometers? (4) Carry 20 liters of water for 20 meters? (5) Hoe a field for a morning?" Food insecurity refers to the number of months during which the household had problems satisfying its food needs. Political connections is a dummy equal to 1 if households have relatives or friends holding an official position in the *kebele*. Participation is estimated through probit model; amount is estimated through Tobit model. Reported coefficients refer to marginal effects for both models. Standard errors are clustered at the household level. Significance levels * 10% ** 5% *** 1%.

The results on the amount of aid are fairly encouraging as it seems that work endowments are the main determinants of amounts. Both in 2004 and in 2009, higher levels of aid are given to households with higher proportions of elderly and to households with higher work inability scores, with 2009 showing an increment in the role played by these variables. Political connections do not seem to play a role in the amount of aid distributed

(although based on their size, the coefficients are likely to be imprecisely estimated).

2.7 Robustness tests

One concern regarding the main results could be the argument that political connections themselves are influenced by the food aid distribution process. In particular, it might be that people that had received aid in the past had become to know local administrators, as part of the programme process. If this was the case, political connections and aid might be simultaneously determined or even reflect reverse causality. Table A.6 of the Appendix shows the results of selection into public works and food aid where I added a variable that captures the receipt of aid in the past.[19] Past aid is generally a significant determinant in the receipt of aid, with higher levels of significance in 2009 as compared to 2004. The net coefficients (not reported) for past aid are 0.169 and 0.143 for public works and food aid respectively, both significant at 5% level. For public works, in particular, there is a large difference from 2004 (where the coefficient on past aid is insignificant). The role of past aid is not surprising as official targeting rules that past aid should be used to determine eligibility. On the other hand, past aid might be related to unobserved omitted variables, in which case it may improve the power of the model. Importantly, the inclusion of past aid does not modify the coefficients in any of the models. The only variable that slightly changes is the political connection in the full interacted model for food aid where the coefficient is no longer significant, although the difference with the model without the inclusion of the past aid variable is quite small. These results overall reassure the validity of the main model.

As shown in Section 2.4, in both years there are households that receive both types of aid. This might raise concerns about the reported estimates if the participation (amount received) in one programme influence the beneficiary status (amount received) in the other programme. To explore this possibility, I run a seemingly unrelated regression (SUR)

[19] The past aid variables are dummy variables constructed as whether households participated in public works in the past (using the previous rounds of the survey, since 1995 (round 3) and as whether households received food aid in the past (since 1995). I also try with a broader definition of past aid by considering the receipt of any kind of aid in the past and the results are consistent.

linear probability model and a bivariate probit model, for participation equations and amount of aid received, where appropriate for functional form. When errors are correlated across equations for a given individual but uncorrelated across individuals, a system of linear equations exploits the cross-correlation of the errors to improve estimator efficiency. Appendix Tables A.7 and A.8 report the determinants of the probability of participating in public works and receiving food aid respectively. The two aid programmes are negatively correlated (-0.062) although only at 10% of significance, a first indication that bivariate probit is an appropriate model choice. When testing whether ρ is equal to zero, the null hypothesis is rejected,[20] suggesting that a bivariate probit may be a superior model.

The results for public works do not substantially differ from those obtained through the main modelling presented in Table 2.5. However, for food aid, estimates from the bivariate probit and the SUR linear probability model present some differences. These are found in the interacted coefficients and in the 2009 net results. In particular, the magnitude and the sign of most coefficients change, with two variables now being highly statistically significant (In 2009, the share of elderly and the work inability score are positively associated with the probability of accessing food aid).[21]

2.8 Conclusions

Ethiopia continues to rely on aid transfers for the subsistence of a large part of its population. Targeting of aid, whether in form of relief food aid or public works, is community-based with guidelines provided by the federal government on how to select beneficiaries, focusing on poverty and food insecurity. The lack of clear indication on which criteria

[20] The likelihood-ratio test gives a χ^2 equal to 14.935 and 15.603 respectively for equation without interactions and for equation fully interacted, with a corresponding p-value of 0.000 in both cases.

[21] A further concern in relation to the comparability of this robustness check to the main analysis could be that both the SUR linear probability model and the bivariate probit include only villages that received both public works and food aid in a given survey wave. This improvement in targeting of food aid in 2009 therefore only emerges when the analysis is restricted to villages that receive PSNP too. This could be explained by the PSNP setting, from the system in place for managing targeting to the monitoring system in place. Unfortunately, these are only speculations. There is no data that could help us to further investigate if this was the case or the channels that led the improvement in targeting.

to use in the selection and the poor monitoring on how targeting was carried out has often raised concerns about the efficiency of this strategy to reach the most in need. In particular, anecdotal evidence suggested the possibility of elite capture and rent-seeking behaviours as *kebele* leaders and local officials have power over beneficiary selection (Alatas et al., 2012; Conning and Kevane, 2002). Few empirical studies have confirmed this anecdotal evidence, corroborating the evidence of political connections in aid distribution in the Ethiopian context (Broussard et al., 2014; Caeyers and Dercon, 2012).

In this paper, I investigate whether targeting of the two main aid interventions has improved using the last two rounds of the ERHS, one just before the implementation of the PSNP (2004) and the other after a few years of its implementation (2009). I directly compare the differences in targeting with a focus on three main variables that capture food insecurity, poverty and political connections. The results for 2004 are fairly consistent with previous studies (Gilligan and Hoddinott, 2007; Broussard et al., 2014; Caeyers and Dercon, 2012) which found political connections to be particularly important in selection of beneficiaries for both public works and food aid. However, my results suggest an overall improvement in targeting in 2009, especially for public works. In particular, wealth in 2009 is a strong predictor of targeting while political connections no longer appear to play a role. For food aid, while there are similar encouraging trends for political connections, additional indicators related to household demographics (share of elderly, work inability of the household head) are also no longer significant. However, these factors become significant when the sample is constrained to villages receiving both public works and food aid, and thus should be interpreted with caution. These findings might indicate that public works have had some externalities on targeting of food aid. It might be that the improvement in the selection of beneficiaries brought by the PSNP affected the overall aid distribution system in a given village.

There are a number of limitations this paper was unable to address which are worth

mentioning. First, the results are not nationally representative. Although the EHRS was sampled to be representative of agro-ecological zones, this does not necessarily represent population or national demographics in Ethiopia. Furthermore, as other studies (qualitative as well as quantitative) have shown, there is a great heterogeneity in the processes of selection of beneficiaries. Although Table A1 in the Appendix also supports this finding of across village heterogeneity, the small size of the sample does not allow to further analyse these heterogeneities, for instance by regional patterns. In addition, the main findings can only pertain to the situation up to 2009 and more recent data could shed light on progress made since 2009.

Targeting continues to be a heavily debated and critical component of any aid or poverty related programme, particularly in settings where poverty levels are high and large segments of the population struggle to meet daily subsistence needs (Devereux et al., 2015; Ellis, 2012; Brown et al., 2016; del Ninno and Mills, 2015). Because of its political nature, and linkages to public acceptance of programming, as well as the role it plays in the ultimate impact and success in meeting poverty objectives, it is essential that these decisions are evidence informed. It is likely that the 'best' targeting formula for each programme will vary based on the level of administrative and monitoring capacity in each setting, the programme objectives and the time frame of the programme, among others. Notwithstanding the improvements in targeting with the introduction of the PSNP, as other research on other African countries has shown (Handa et al., 2012), targeting using a combination of CBT with other targeting methods, such as categorical based on demographics proved to be successful. The creation of more specific and clear eligible groups or criteria would in fact reduce the room for elite capture, and it would help with the transparency of the process and its perceived fairness by the community. In addition, small changes in targeting operations, including for example mechanisms to improve transparency, information sharing and monitoring and to address grievances, can be im-

plemented to decrease potential drawbacks associated with CBT. As an increasing number of programmes implement short-term shock-responsive schemes and as programmes are scaled up in developing settings, innovations in technology and more complex programme designs are needed to ensure that the most in need are reached with the objectives of protecting the most vulnerable populations and breaking the inter-generational transmission of poverty.

Appendix A

Tables

Table A.1: Poverty rate, shares of participants and amount of aid received by village

	Poverty rate	Public Works			Food Aid		
		All	Poor	Non-poor	All	Poor	Non-poor
Panel A: Participation							
Haresaw	0.69	0.55	0.55	0.54	0.45	0.52	0.27
Geblen	0.73	0.67	0.70	0.60	0.58	0.61	0.47
Dinki	0.59	0.26	0.22	0.32	0.61	0.63	0.58
Yetemen	0.19	0.00	0.00	0.00	0.00	0.00	0.00
Shumsha	0.38	0.48	0.51	0.46	0.43	0.32	0.50
Sirbana Godeti	0.08	0.00	0.00	0.00	0.01	0.00	0.01
Adele Keke	0.44	0.35	0.37	0.34	0.22	0.21	0.23
Korodegaga	0.56	0.83	0.87	0.77	0.46	0.47	0.44
Trirufe Ketchema	0.39	0.00	0.00	0.00	0.15	0.25	0.09
Imdibir	0.61	0.00	0.00	0.00	0.01	0.00	0.02
Aze Deboa	0.53	0.36	0.24	0.49	0.40	0.43	0.37
Adado	0.72	0.00	0.00	0.00	0.45	0.48	0.37
Gara Godo	0.64	0.13	0.08	0.22	0.43	0.49	0.32
Doma	0.61	0.34	0.33	0.36	0.06	0.07	0.04
D.B. Milki	0.17	0.00	0.00	0.00	0.02	0.02	0.02
Total	0.48	0.25	0.29	0.21	0.49	0.53	0.43
Panel B: Amount							
Haresaw		7.15	8.19	4.77	3.20	3.35	2.52
Geblen		8.88	10.02	5.19	4.85	4.66	5.53
Dinki		0.36	0.36	0.37	1.26	0.85	1.92
Yetemen							
Shumsha		5.75	7.95	4.27	1.66	1.70	1.65
Sirbana Godeti							
Adele Keke		5.15	4.90	5.36	2.44	2.44	2.43
Korodegaga		10.89	10.87	10.92	3.55	3.54	3.58
Trirufe Ketchema					0.97	0.96	1.00
Imdibir							
Aze Deboa		0.15	0.09	0.18	1.16	1.33	0.94
Adado					1.75	1.61	2.25
Gara Godo		2.28	4.31	1.26	1.22	1.26	1.12
Doma		6.04	6.54	5.33			
D.B. Milki							
Total		6.65	7.94	5.07	2.23	2.26	2.18

Notes: Amount of aid is averaged among those that received aid. Figures are averaged across the two survey rounds.

Table A.2: Correlates of political connections

	Political connections (0,1)	
	2004	2009
Log consumtpion pc net of aid	0.040*	0.016
	(0.022)	(0.026)
Head primary education (d)	0.107**	0.099**
	(0.042)	(0.045)
Female head (d)	-0.129***	-0.088***
	(0.024)	(0.024)
Age head	0.000	0.001
	(0.001)	(0.001)
Log household size	0.040	0.052
	(0.055)	(0.058)
Share of elderly	-0.045	0.082
	(0.095)	(0.088)
Share of children	0.093	0.218**
	(0.096)	(0.083)
Log lagged pc livestock value	0.017**	0.019**
	(0.007)	(0.009)
Log land per capita	0.074	0.051
	(0.069)	(0.079)
Constant	0.080	-0.005
	(0.119)	(0.125)
Village FE	Yes	Yes
Adjusted R^2	0.092	0.085
Observations	1262	1263

Notes: Dependent variable is a dummy equal to 1 if the household has relatives or friends holding an official position in the *kebele* in 2004. Linear Probability Model. Standard errors are clustered at the village level. Significance levels * 10% ** 5% *** 1%.

Table A.3: Determinants of aid participation with additional covariates

	Public Works		**Food Aid**	
	(Pooled)	(Inter)	(Pooled)	(Inter)
Log household size	0.062	-0.021	0.094*	-0.026
	(0.045)	(0.037)	(0.056)	(0.063)
Share of elders	-0.583***	0.308***	-0.542***	0.336**
	(0.083)	(0.080)	(0.110)	(0.143)
Female head (d)	-0.022	-0.001	-0.072*	-0.024
	(0.033)	(0.031)	(0.042)	(0.047)
Head primary education (d)	-0.056	0.024	-0.057	0.026
	(0.043)	(0.035)	(0.054)	(0.061)
Work inability score of head	-0.061**	0.066***	-0.058*	0.051
	(0.027)	(0.021)	(0.034)	(0.034)
Log lagged pc livestock value	-0.023***	-0.011*	-0.01	-0.011
	(0.008)	(0.007)	(0.009)	(0.011)
Food insecurity	0.017***	0.003	0.028***	0.005
	(0.006)	(0.006)	(0.009)	(0.010)
Political connections (d)	0.052*	0.065**	0.077**	0.077*
	(0.031)	(0.026)	(0.038)	(0.042)
Year 2009 (d)	-0.169*	0.138*	0.175	0.01
	(0.090)	(0.078)	(0.225)	(0.200)
No. people to rely	0.002	-0.001	0.002	-0.005**
	(0.002)	(0.001)	(0.002)	(0.002)
Iddir size	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Drought (d)	-0.038	-0.034	-0.027	-0.042
	(0.035)	(0.034)	(0.040)	(0.045)
Loss of crop (d)	-0.050	0.033	-0.033	0.007
	(0.042)	(0.033)	(0.049)	(0.054)
Livestock shocks (d)	-0.019	0.009	-0.004	0.024
	(0.043)	(0.047)	(0.055)	(0.067)

. . . continued

	Public Works		**Food Aid**	
	(Pooled)	(Inter)	(Pooled)	(Inter)
Household member died (d)	0.000	0.000	-0.011	0.011
	(0.035)	(0.033)	(0.035)	(0.036)
Household member ill (d)	-0.051	0.075**	-0.069	0.058
	(0.037)	(0.033)	(0.047)	(0.056)
Year 2009 * Log household size		-0.071		0.015
		(0.088)		(0.081)
Year 2009 * Share of elders		-0.115		-0.053
		(0.165)		(0.192)
Year 2009 * Female head (d)		0.103		0.056
		(0.065)		(0.066)
Year 2009 * Head education (d)		0.009		-0.001
		(0.086)		(0.071)
Year 2009 * Inability score		-0.006		0.027
		(0.048)		(0.048)
Year 2009 * Log pc livestock		-0.033**		-0.001
		(0.016)		(0.013)
Year 2009 * Food insecurity		-0.025**		-0.004
		(0.013)		(0.012)
Year 2009 * Political connections (d)		-0.068		-0.018
		(0.059)		(0.053)
Year 2009 * No. people to rely		0.001		0.005**
		(0.003)		(0.003)
Year 2009 * Iddir size		0.000		0.000
		(0.000)		(0.000)
Year 2009 * Drought (d)		0.006		0.033
		(0.080)		(0.071)
Year 2009 * Loss of crop (d)		-0.055		0.046
		(0.094)		(0.069)
Year 2009 * Livestock shock (d)		-0.03		-0.026

. . . continued

	Public Works		Food Aid	
	(Pooled)	(Inter)	(Pooled)	(Inter)
		(0.084)		(0.089)
Year 2009 * Hh member died (d)		0.221**		-0.092
		(0.110)		(0.088)
Year 2009 *Hh member ill (d)		0.058		0.035
		(0.074)		(0.073)
Constant	0.492***	0.279***	0.341**	0.348**
	(0.121)	(0.105)	(0.140)	(0.154)
Adjusted R^2	0.209	0.204	0.215	0.199
Observations	1082	1321	1082	1321

Notes: Work inability score is an index based on 5 questions: "Can this person: (1) Stand up after sitting down? (2) Sweep the floor? (3) Walk for 5 kilometres? (4) Carry 20 litres of water for 20 meters? (5) Hoe a field for a morning?". Food insecurity refers to the number of months during which the household had problems satisfying its food needs. Political connections is a dummy equal to 1 if households have relatives or friends holding an official position in the *kebele*. The *iddir* is a funeral society to which members pay a regular contribution, used here as a proxy for social connections. Participation is estimated through Linear Probability Models. Standard errors are clustered at the household level. Significance levels * 10% ** 5% *** 1%.

Table A.4: Aid payments summary statistics

	Monthly Payments	Daily Wage	No. Days
Public Works			
2004	2.43	1.57	3.47
2009	3.90	1.72	4.89
Food Aid			
2004	2.20		
2009	1.93		

Note: Figures restricted to households that received aid.

Table A.5: Determinants of days worked and daily wage of Public Works (2004-2009)

	No. of Days			**Daily Wage Rate**		
	(Pooled)	(Interacted)	(Net)	(Pooled)	(Interacted)	(Net)
Log household size	0.344*	0.374*		0.083	0.072	
	(0.181)	(0.205)		(0.063)	(0.083)	
Share of elderly	-3.108***	-2.389***		-1.045***	-1.004***	
	(0.671)	(0.603)		(0.238)	(0.268)	
Female head (d)	-0.130	-0.356***		-0.014	-0.110*	
	(0.126)	(0.136)		(0.046)	(0.059)	
Head primary education (d)	-0.191	-0.262		-0.073	-0.099	
	(0.158)	(0.169)		(0.056)	(0.071)	
Work inability score of head	-0.240*	-0.225*		-0.095**	-0.104*	
	(0.126)	(0.132)		(0.044)	(0.054)	
Log lagged pc livestock value	-0.083**	-0.024		-0.029**	-0.005	
	(0.033)	(0.033)		(0.011)	(0.013)	
Food insecurity	0.069***	0.099***		0.021**	0.039***	
	(0.026)	(0.031)		(0.009)	(0.012)	
Political connections (d)	0.162	0.239*		0.096**	0.146**	
	(0.124)	(0.141)		(0.047)	(0.061)	
Year 2009 (d)	-0.417	0.707		-0.245**	0.066	
	(0.365)	(0.910)		(0.116)	(0.349)	
t_1 * Log household size		-0.078	0.295		0.083	0.156
		(0.449)	(0.494)		(0.162)	(0.182)
t_1 * Share of elderly		-1.920	-4.309***		-0.319	-1.323***
		(1.212)	(1.355)		(0.434)	(0.510)
t_1 * Female head (d)		0.631*	0.275		0.236*	0.126
		(0.366)	(0.390)		(0.134)	(0.147)
t_1 * Head education (d)		0.288	0.260		0.071	-0.028
		(0.466)	(0.496)		(0.167)	(0.182)
t_1 * Inability score		-0.057	-0.282		-0.005	-0.109
		(0.265)	(0.297)		(0.101)	(0.115)
t_1 * Log pc livestock		-0.210***	-0.234***		-0.086***	-0.091***
		(0.078)	(0.085)		(0.028)	(0.031)
t_1 * Food insecurity		-0.082	0.017		-0.036*	0.003
		(0.061)	(0.068)		(0.021)	(0.025)
t_1 * Political connections (d)		-0.275	-0.035		-0.137	0.008
		(0.293)	(0.325)		(0.109)	(0.125)
Village - year FE	Yes	Yes		Yes	Yes	
Pseudo R^2	0.075	0.081		0.143	0.150	
Observations	1086	1086		1086	1086	
LL	-1804.074	-1793.559		-1243.550	-1233.247	

Notes: Work inability score is an index based on 5 questions: "Can this person: (1) Stand up after sitting down? (2) Sweep the floor? (3) Walk for 5 kilometres? (4) Carry 20 litres of water for 20 meters? (5) Hoe a field for a morning?". Food insecurity refers to the number of months during which the household had problems satisfying its food needs. Political connections is a dummy equal to 1 if households have relatives or friends holding an official position in the *kebele*. The models are estimated through Tobit model. Reported coefficients refer to marginal effects. Standard errors are clustered at the household level. Significance levels * 10% ** 5% *** 1%.

Table A.6: Determinants of participation Public Works and Food Aid (2004-2009) including controls for past aid

	Public Works		**Food Aid**	
	(Pooled)	(Interacted)	(Pooled)	(Interacted)
Log household size	0.076	0.100*	-0.017	-0.029
	(0.049)	(0.055)	(0.043)	(0.061)
Share of elderly	-0.748***	-0.614***	0.363***	0.319**
	(0.131)	(0.151)	(0.107)	(0.160)
Female head (d)	-0.025	-0.079*	-0.015	-0.033
	(0.038)	(0.045)	(0.036)	(0.048)
Head primary education (d)	-0.060	-0.073	0.013	0.013
	(0.047)	(0.056)	(0.042)	(0.060)
Work inability score of head	-0.068**	-0.064*	0.080***	0.056
	(0.030)	(0.034)	(0.024)	(0.035)
Log lagged pc livestock value	-0.024***	-0.007	-0.013	-0.011
	(0.009)	(0.009)	(0.008)	(0.010)
Food insecurity	0.021***	0.032***	-0.000	0.004
	(0.007)	(0.010)	(0.007)	(0.010)
Political connections (d)	0.058	0.080**	0.070**	0.064
	(0.036)	(0.039)	(0.031)	(0.042)
Year 2009 (d)	-0.184**	0.111	0.077	0.006
	(0.089)	(0.226)	(0.081)	(0.216)
Past Aid	0.105**	0.046	0.149***	0.139**
	(0.045)	(0.051)	(0.045)	(0.056)
t_1 * Log household size		-0.062		0.018
		(0.083)		(0.067)
t_1 * Share of elderly		-0.106		0.029
		(0.203)		(0.178)
t_1 * Female head (d)		0.107*		0.028
		(0.063)		(0.055)
t_1 * Head education (d)		0.046		0.005
		(0.080)		(0.061)
t_1 * Inability score		0.012		0.029
		(0.046)		(0.039)
t_1 * Log pc livestock		-0.035**		-0.002
		(0.015)		(0.011)
t_1 * Food insecurity		-0.025**		-0.006
		(0.011)		(0.010)
t_1 * Political connections (d)		-0.071		0.003
		(0.057)		(0.046)
t_1 * Past aid		0.119*		0.007
		(0.071)		(0.064)
Village - year FE	Yes	Yes	Yes	Yes
Pseudo R^2	0.186	0.199	0.183	0.184
Observations	1104	1104	1346	1346
LL	-621.535	-611.269	-762.169	-761.388

Notes: Work inability score is an index based on 5 questions: "Can this person: (1) Stand up after sitting down? (2) Sweep the floor? (3) Walk for 5 kilometres? (4) Carry 20 litres of water for 20 meters? (5) Hoe a field for a morning?". Food insecurity refers to the number of months during which the household had problems satisfying its food needs. Political connections is a dummy equal to 1 if households have relatives or friends holding an official position in the *kebele*. Participation is estimated through probit model. Reported coefficients refer to marginal effects. Standard errors are clustered at the household level. Significance levels * 10% ** 5% *** 1%.

Table A.7: Determinants of participation in Public Works (2009-2004)

	LPM			SUR LPM			Bivariate Probit		
	(Pooled)	(Inter)	(Net)	(Pooled)	(Inter)	(Net)	(Pooled)	(Inter)	(Net)
Log household size	0.066	0.094*		0.073	0.076		0.096*	0.086	
	(0.043)	(0.054)		(0.046)	(0.054)		(0.058)	(0.059)	
Share of elderly	-0.569***	-0.533***		-0.581***	-0.560***		-0.789***	-0.640***	
	(0.083)	(0.109)		(0.093)	(0.117)		(0.140)	(0.149)	
Female head (d)	-0.023	-0.074*		-0.012	-0.075*		-0.015	-0.079*	
	(0.033)	(0.041)		(0.035)	(0.042)		(0.044)	(0.046)	
Head primary education (d)	-0.052	-0.068		-0.043	-0.082		-0.061	-0.097*	
	(0.042)	(0.054)		(0.047)	(0.057)		(0.057)	(0.059)	
Work inability score of head	-0.065**	-0.065**		-0.063**	-0.063*		-0.069**	-0.062*	
	(0.026)	(0.033)		(0.029)	(0.033)		(0.035)	(0.034)	
Log lagged pc livestock value	-0.021***	-0.007		-0.025***	-0.014		-0.031***	-0.015	
	(0.008)	(0.009)		(0.008)	(0.009)		(0.010)	(0.010)	
Food insecurity	0.017***	0.028***		0.017***	0.027***		0.022**	0.030***	
	(0.006)	(0.008)		(0.007)	(0.009)		(0.009)	(0.010)	
Political connections (d)	0.049	0.070*		0.081**	0.092**		0.105***	0.107***	
	(0.030)	(0.037)		(0.033)	(0.037)		(0.041)	(0.040)	
Year 2009 (d)	-0.152*	0.222		-0.098	0.105		-0.200*	0.023	
	(0.087)	(0.215)		(0.069)	(0.253)		(0.103)	(0.263)	
t_1 * Log household size		-0.072	0.023		-0.008	0.068		-0.004	0.082
		(0.088)	(0.103)		(0.104)	(0.117)		(0.098)	(0.101)
t_1 * Share of elderly		-0.082	-0.615***		-0.041	-0.601***		-0.093	-0.718***
		(0.166)	(0.199)		(0.196)	(0.228)		(0.222)	(0.238)
t_1 * Female head (d)		0.111*	0.036		0.161**	0.086		0.157**	0.052
		(0.063)	(0.075)		(0.067)	(0.079)		(0.066)	(0.073)
t_1 * Head education (d)		0.051	-0.017		0.134	0.052		0.132	0.012
		(0.085)	(0.100)		(0.109)	(0.124)		(0.092)	(0.098)
t_1 * Inability score		-0.006	-0.070		-0.008	-0.071		0.004	-0.059
		(0.047)	(0.057)		(0.057)	(0.066)		(0.051)	(0.054)
t_1 * Log pc livestock		-0.036**	-0.043**		-0.030*	-0.044**		-0.028	-0.039**
		(0.015)	(0.017)		(0.017)	(0.019)		(0.018)	(0.018)
t_1 * Food insecurity		-0.023*	0.006		-0.021	0.006		-0.026*	0.009
		(0.012)	(0.015)		(0.014)	(0.017)		(0.015)	(0.016)
t_1 * Political connections (d)		-0.059	0.011		-0.039	0.053		-0.051	0.064
		(0.059)	(0.069)		(0.064)	(0.074)		(0.067)	(0.068)
Village - year FE	Yes	Yes		Yes	Yes		Yes	Yes	
Adjusted/Pseudo R^2	0.208	0.215		0.239	0.251				
Observations	1104	1104		923	923		923	923	
LL							-1033.418	-1011.993	

Notes: Work inability score is an index based on 5 questions: "Can this person: (1) Stand up after sitting down? (2) Sweep the floor? (3) Walk for 5 kilometres? (4) Carry 20 litres of water for 20 meters? (5) Hoe a field for a morning?". Food insecurity refers to the number of months during which the household had problems satisfying its food needs. Political connections is a dummy equal to 1 if households have relatives or friends holding an official position in the *kebele*. LPM: Linear Probability Model. SUR LPM: Seemingly Unrelated Regression Linear Probability Model. LPM includes villages that received Public Works. SUR LPM and Bivariate Probit are restricted to villages that received both Public Works and Food Aid. Standard errors are clustered at household level.Significance levels * 10% ** 5% *** 1%.

Table A.8: Determinants of participation in Food Aid (2004-2009)

	LPM			SUR LPM			Bivariate Probit		
	(Pooled)	(Inter)	(Net)	(Pooled)	(Inter)	(Net)	(Pooled)	(Inter)	(Net)
Log household size	-0.017	-0.030		-0.041	-0.030		-0.015	-0.029	
	(0.037)	(0.061)		(0.048)	(0.064)		(0.020)	(0.061)	
Share of elderly	0.288***	0.309**		0.413***	0.309**		0.199**	0.328**	
	(0.080)	(0.142)		(0.102)	(0.144)		(0.092)	(0.163)	
Female head (d)	-0.011	-0.031		-0.011	-0.031		-0.004	-0.034	
	(0.030)	(0.046)		(0.034)	(0.046)		(0.015)	(0.047)	
Head primary education (d)	0.016	0.010		-0.017	0.010		-0.009	0.011	
	(0.035)	(0.060)		(0.045)	(0.059)		(0.018)	(0.060)	
Work inability score of head	0.070***	0.058*		0.096***	0.058*		0.042**	0.060*	
	(0.020)	(0.033)		(0.025)	(0.031)		(0.018)	(0.035)	
Log lagged pc livestock value	-0.012*	-0.012		-0.010	-0.012		-0.004	-0.012	
	(0.007)	(0.010)		(0.009)	(0.011)		(0.004)	(0.010)	
Food insecurity	0.002	0.004		-0.001	0.004		-0.001	0.005	
	(0.006)	(0.009)		(0.007)	(0.009)		(0.003)	(0.010)	
Political connections (d)	0.066**	0.070*		0.068**	0.070*		0.028	0.070*	
	(0.026)	(0.041)		(0.032)	(0.042)		(0.018)	(0.042)	
Year 2009 (d)	0.110	0.047		-0.633***	-0.772***		-0.282***	-1.048***	
	(0.075)	(0.192)		(0.074)	(0.246)		(0.072)	(0.325)	
t_1 * Log household size		0.024	-0.006		-0.033	-0.063		-0.028	-0.056
		(0.078)	(0.098)		(0.104)	(0.122)		(0.091)	(0.107)
t_1 * Share of elderly		-0.035	-0.274		0.209	0.518**		0.497*	0.813**
		(0.189)	(0.237)		(0.202)	(0.248)		(0.258)	(0.323)
t_1 * Female head (d)		0.038	0.007		0.062	0.031		0.083	0.047
		(0.063)	(0.078)		(0.076)	(0.089)		(0.073)	(0.085)
t_1 * Head education (d)		0.012	0.023		-0.083	-0.073		-0.102	-0.089
		(0.070)	(0.092)		(0.078)	(0.098)		(0.081)	(0.100)
t_1 * Inability score		0.022	0.080		0.114**	0.172***		0.123**	0.180***
		(0.047)	(0.057)		(0.049)	(0.058)		(0.048)	(0.059)
t_1 * Log pc livestock		-0.001	-0.012		0.014	0.002		0.014	0.002
		(0.013)	(0.017)		(0.016)	(0.019)		(0.015)	(0.018)
t_1 * Food insecurity		-0.004	0.001		-0.012	-0.007		-0.008	-0.004
		(0.012)	(0.015)		(0.014)	(0.017)		(0.012)	(0.014)
t_1 * Political connections (d)		-0.008	0.062		-0.017	0.052		-0.006	0.064
		(0.052)	(0.067)		(0.065)	(0.077)		(0.060)	(0.072)
Village - year FE	Yes	Yes		Yes	Yes		Yes	Yes	
Adjusted/Pseudo R^2	0.202	0.198		0.201	0.214				
Observations	1346	1346		923	923		923	923	
LL							-1033.418	-1011.993	

Notes: Work inability score is an index based on 5 questions: "Can this person: (1) Stand up after sitting down? (2) Sweep the floor? (3) Walk for 5 kilometres? (4) Carry 20 litres of water for 20 meters? (5) Hoe a field for a morning?". Food insecurity refers to the number of months during which the household had problems satisfying its food needs. Political connections is a dummy equal to 1 if households have relatives or friends holding an official position in the *kebele*. LPM: Linear Probability Model. SUR LPM: Seemingly Unrelated Regression Linear Probability Model. LPM includes villages that received Food Aid. SUR LPM and Bivariate Probit are restricted to villages that received both Public Works and Food Aid. Standard errors are clustered at the household level. Significance levels * 10% ** 5% *** 1%.

Chapter 3

Long term effect of drought and emergency food aid on child health

3.1 Introduction

The impacts of climate change and weather variability have been gaining increasing attention in the past decade. The Sub-Saharan continent is particularly vulnerable to natural disasters. Half of risk-prone countries in the world are in fact in Africa, where the number of natural hazards, particularly droughts and floods, have been increasing in the past 30 years (UNISDR, 2016). Ethiopia is one of the most vulnerable countries to these events. Low and irregular rains, often resulting in droughts, have detrimental effects on the livelihood of people who are predominantly dependent on subsistence rain-fed agriculture (Dercon et al., 2005; Porter, 2012). Children often bear a large share of the burden caused by the negative shocks, first because households tend to divert resources towards most productive members, and second because they are biologically more vulnerable (Jensen, 2000). A large body of literature has shown the consequences that shocks experienced at an early age can have (Hoddinott and Kinsey, 2001), even in the long term, on health (Portner, 2010; Dercon and Porter, 2014) and education and cognitive abilities (Alderman et al., 2006, 2009). More generally, health of children is found to be good a predictor of later achievements in life (Behrman et al., 2004).

An increasing number of cash transfer and social protection programmes are being

utilised as a tool for tackling poverty and for protecting the most vulnerable. 131 countries worldwide provide in-kind transfers, especially in the form of school feeding. In Africa, cash transfers are present in 40 countries, a figure that has doubled in the past decade (World Bank, 2015). One of the key role of social protection schemes is the protection against shocks, whether idiosyncratic or covariate. However, the literature on the contribution of aid and cash transfers in helping vulnerable households to cope with shocks is still very limited. This paper aims at filling this gap by providing evidence of the role that food aid played in the aftermath of a severe drought on children's health. The focus is on Ethiopia's 2011 drought, one of the worst in the past decade, which caused an estimated 4.5 million people to be in acute food shortage (Cabot Venton et al., 2013). Using two rounds of the Ethiopian Rural Socioeconomic Survey (ERSS), I investigate the effect that the drought had two years later (2013) on the height-for-age of 0-36 months old children and I examine the impact that food aid had in protecting their nutritional status from the negative consequences of the drought.

The analysis of the role that aid plays in protecting children's health is particularly relevant in the context of Ethiopia, which has been historically characterised by severe problems of chronic malnourishment, especially among children. Even though in the past two decades malnutrition has significantly decreased, current levels remain high, even when compared to other Sub-Saharan countries. Prevalence of stunting in children under 5 years of age has decreased by 20 percentage points, from 58% in 2000 to 38% in 2016. The prevalence of wasting in Ethiopia has instead remained constant over the last fifteen years, averaging 11% (CSA and ICF, 2012, 2016).[1]

The only two studies that have looked into the effectiveness of aid in protecting children's health from negative weather-related shocks are Yamano et al. (2005) and Dercon and Porter (2014), both also in Ethiopia. Dercon and Porter (2014) examine the long-term effects of the extremely severe famine that hit Ethiopia in 1984 on the height of in-utero and up to 36 months old children. Twenty years after the disaster, young adults, who were 12-36 months old at the time of the famine, were found significantly shorter than individuals of the same cohort who were not exposed to the shock. No effect was instead found for other cohorts. The authors then assess the extent to which food aid was able

[1] A child is considered stunted, or chronically malnourished, when its height-for-age is below minus two standard deviations from the median of the reference population. A child is considered wasted when its weight-for-height (which captures current nutritional status) is below minus two standard deviations from the median of the reference population.

to reduce the negative effect of the famine and find that relief aid did not reduce the long-term effect of the drought. The lack of a proper counterfactual, though, does not allow them to claim whether the null effect of aid is due to the imprecise targeting, to the amount of aid received or other factors. Yamano et al. (2005) explore the short-term effects of extensive crop damages caused by weather-related shocks in 1995, on 6 to 24 months old children. After six months, they find children's height loss to amount to 0.9 centimetres. In those communities that received food aid, however, these negative effects were completely offset by food aid.

Ethiopia has historically been one of the largest recipients of aid (Van Domelen and Coll-Black, 2012). The relief system went through several changes and reforms, the main one being the implementation of the Productive Safety Net Programme (PSNP), the largest social protection program in the continent (excluding South Africa), which aimed at providing reliable support to chronically food insecure households. A relief system is still operational to address occurrences of severe shocks. The relief system went through a number of reforms too, with increasing efforts been put into setting up a system of early warning and improving the coordination of the response. Notwithstanding the institutional and operational reforms, there are still many challenges linked to the emergency response, particularly to the timing and reactivity to appeals (Cabot Venton et al., 2013). In light of these factors, it is important to assesses the effect of emergency aid in protecting the well-being of households exposed to shocks.

The contributions of this chapter are threefold. First, I provide evidence on the effectiveness of emergency aid in protecting individuals' well-being since the introduction of the PSNP and the reforms in the relief system. Second, this is the first study that quantifies the impact of the 2011 drought, one of the most severe ones in the last decades. Lastly, I fill the gap in the literature that aims at assessing which interventions are effective in protecting children's health from weather-related shocks.

From a methodological perspective, I apply a doubly-robust matching estimator, the Inverse Probability Weighting regression-adjustment (IPWRA), to address the issue of selection into the relief programme. I estimate the effect that the 2011 drought had on children's height-for-age and I assess whether emergency aid protected them from the drought. The results show that aid was able to protect those children that experienced the drought. Two years after the occurrence of the drought, children from households

affected by the drought, but not receiving aid, were 1.76 standard deviations shorter than those that did not experience the drought. This negative effect was successfully avoided for those children that instead received aid, who were 0.37 standard deviations taller than those children that did not receive aid. These results prove robust to a number of robustness checks, including additional analysis aimed at addressing concerns related to the self-reported nature of the drought variable.

The chapter is structured as follows: the next section reviews the literature on the impact of negative shocks, and of cash transfers and social protection programmes on child health. Section 3.3 introduces the context; Section 3.4 provides an analytical framework for the analysis of child health; Section 3.5 describes the data used for the analysis and reports descriptive statistics; Section 3.6 outlines the empirical strategy and the econometric model; Section 3.7 presents the main results, followed by Section 3.8 with robustness checks. Finally, Section 3.9 summarises the results and highlights the policy recommendations.

3.2 Literature review

There is a growing interest in the impact of weather shocks on welfare and human development. A first strand of the literature examines the impact of weather-related shocks on households' welfare, with several studies focusing on African countries. Christiaensen et al. (2007) investigate the effect of economic, idiosyncratic and weather-related shocks on household welfare. In particular, they focus on shocks from different sources in two rural regions in Tanzania and estimate that the largest negative effect derives from droughts, which have a negative impact on per capita consumption. Also in Tanzania, Hirvonen (2016) shows how increases in temperature cause a decrease in household per capita consumption, while controlling for precipitation, household fixed effects and other time-varying factors. In Zimbabwe Hoddinott (2006) also analyses losses deriving from a drought, finding a negative impact on households' income. This occurs because a fall in gross crop income is only partially offset by sales of livestock and income from other sources. Household welfare effects of a broader set of weather events are at the core of Wineman et al. (2017). Droughts in rural Kenya are found to be the most damaging adverse events across various regions, with reductions in income, from both on- and off-farm sources. The calorie intake,

instead, is less affected as households compensate for the lower crop production with food purchases. Dercon et al. (2005) assess the impact of a range of shocks on household welfare in Ethiopia and find that droughts and illnesses are the shocks with the highest negative effect. Households exposed to at least one drought in the previous five years see their consumption levels decreasing by 20%. Further, they investigate heterogeneous effects and show that female-headed households, households where the head has no schooling and households in the bottom three quintiles of landholdings within their villages report a much bigger impact of drought shocks on consumption levels. Using the same dataset from Ethiopia, and similarly investigating the effect of covariate as well as idiosyncratic shocks, while extending the analysis over a longer time period, Porter (2012) shows that droughts are the events with the strongest impact on household's consumption (between 10 and 20%). Negative impacts of adverse weather shocks are found also in studies focusing on other geographical areas. In rural Indonesia, Skoufias et al. (2012) estimates the effects of two rain-related shocks, delayed monsoons and low rainfall on household welfare. The negative impact is particularly severe for rice farmers that experienced low rainfall shocks on their levels of non-food consumption while delayed monsoons have a statistically detrimental effect on food consumption of non-rice farmers.

Children are vulnerable, especially up to the age of five, even more in cases of natural disasters (UNISDR, 2011). A number of studies have quantified the impact of these events on children's health, both in the short and in the long term. While the evidence on the short term is quite concordant on the negative effects of shocks on children's health, results on long-term are mixed, depending on contexts, time span considered and methodology applied, with some studies showing persistent effects while others finding evidence of catch-up growth.

Among the studies that provide evidence on weather-related shocks on children's health in the short term, Hoddinott and Kinsey (2001) find that in rural Zimbabwe children aged 12-24 months experienced a slower growth by 1.5 - 2 cm in the aftermath of a drought. In Ethiopia, Yamano et al. (2005) estimate that the crop damage caused by a drought was associated with a 0.9 cm lower child growth for children aged 6-24 months, after a six-month period, compared to villages where the crop damaged area was 50% lower. Rabassa et al. (2014) investigate the short-term effects of positive and negative rainfall shocks, distinguishing between current and lagged rain, and their differential impact through different

channels (income effects of lagged rainfall on crop yields and contemporaneous effect on disease environment) in rural Nigeria. Children's health status is affected by both, with the income effect prevailing on the disease environment effect.[2] Similarly, Tiwari et al. (2013) distinguish between income and disease effects and find that a 10 percent increase in rainfall from the historic trend increases weight-for-height by 0.15 standard deviations among children aged 0-36 months in rural Nepal.[3] The impact of negative weather shocks on children's health is instead identified by Jensen (2000) to be a direct consequence of changes in investment behaviours, with parents decreasing health expenditures in Ivory Coast. In this context, drought exposure is associated with an increase of 3.5 percentage points in the proportion of wasted children aged 0-10 years. In rural Tanzania, Bengtsson (2010) finds body weight of children 0-9 years old to be responsive to transitory income changes due to weather variations, with marked differences by gender. While female children result the most vulnerable, losing 0.4 kg as a consequence of a ten-percent decrease in household income, boys' weight decreases by about 0.2 kg. Older members, adolescents and adults, are found less vulnerable. As shown by Portner (2010), children's health is affected by a larger set of natural hazards, not only rain-related. Using multiple rounds of DHS data for rural Guatemala, the author shows that each shock decreases height-for-age by between 0.1 and 0.2 standard deviations, with indigenous children more affected than non-indigenous. Among the cited studies, some provide further evidence on the long-term effects of shocks on children's height-for-age, with some finding lack of persistence of the shock (Rabassa et al., 2014; Tiwari et al., 2013), Hoddinott and Kinsey (2001) do find catch-up growth. A long-term effect of a drought is found instead by Dercon and Porter (2014). By focusing on children that were exposed to the Ethiopian 1984 famine, the authors find that 20 years later they were significantly shorter by at least 5 cm.

In addition to its intrinsic importance for the direct impact on child mortality and morbidity, early childhood health has been found to predict adult health as well as cognitive abilities, educational achievements, and employment and productivity. Alderman et al. (2006), for instance, use civil war and drought shocks in Zimbabwe to identify differences

[2]For instance, among children aged 0-35 months, a 10 percent increase in contemporaneous rainfall from historic mean shock reduces weight-for-height by 0.034 standard deviations, while a 10 percent increase in rainfall in the last completed rainy season before the survey increases in the weight-for-height by 0.087 standard deviations, suggesting that the positive lagged income effect is larger than the negative disease environment effect.

[3]They disentangle the total impact between the negative disease environment effect of around 0.02 standard deviations and a positive income effect of proximately 0.17 standard deviations.

in preschool nutritional status across siblings. Using maternal fixed effects in combination with instrumental variables, they are able to associate the improvements in height-for-age in children under age 5 with increased height as young adults and the number of grades of schooling completed. Hoddinott et al. (2011) show that the differential impacts in later life events pertain to a number of dimensions. Using data from rural Guatemala, they find that individuals who did not suffer growth failure in the first three years of life complete more schooling, score higher on tests of cognitive skill in adulthood, have better outcomes in the marriage market, earn higher wages and are more likely to be employed in higher-paying skilled labour and white-collar jobs, and are less likely to live in poor households. In addition, women have fewer pregnancies and smaller risk of miscarriages and stillbirths. Using a set of shocks to instrument for the childhood nutritional status applied to a rural region in Tanzania, Alderman et al. (2009) show that malnourished children delay their school entry and achieve lower schooling grade.

Another strand of literature focuses on the impact that aid and social programmes had on child health, either in the short or long term. Findings generally point to positive impacts of programmes on child health outcomes, although results change depending on settings, interventions' characteristics, and age of the children. Several studies focus on Latin America, where Conditional Cash Transfers (CCTs) were designed to break inter-generational poverty traps by promoting also child development, providing mixed results.[4] I report here the summary of the literature on Sub-Saharan Africa, as the institutional setting is quite dissimilar to the one in other geographical areas.

Few studies have focused on Ethiopia and the effect that aid had on a number of outcomes, including child health. The large attention that Ethiopia has gained in this sense is due to the fact that the country has received large amounts of aid over the past decades, significantly higher than in other countries. The literature on pre-PSNP aid

[4]Studies on Oportunidades/Progresa in Mexico find positive and significant impact on height or change in height, for children under 2 years at baseline. For instance, Gertler (2004) and Behrman and Hoddinott (2005) find a positive impact on children 12-36 months old, who were younger than 2 years at the baseline, whereas Rivera et al. (2004) find an impact only on those children who were younger than 6 months at the baseline and who lived in households with below-median wealth. Positive impact is also found by Maluccio and Flores (2005) of RPS in Nicaragua on height-for-age for children younger than 5 years of age. Instead, investigating the impact of another programme in the same country, Macours et al. (2012) find no effect on child height among children of any group. No impact on height is also found by Hoddinott (2010) in Honduras by PRAF programme, probably due to the small size of the transfer. Familias en Accin in Colombia had a positive impact on z-scores on children younger than 2 years, but no impact on height of older children (Attanasio et al., 2005). A negative impact on weight-for-age and height-for-age is instead found by Morris et al. (2004) on the Brazilian Bolsa Alimentao. Stifel and Alderman (2006) find that notwithstanding being a well-targeted programme, the Peruvian Vaso de leche does not have any significant nutritional impact on children.

programmes on child nutrition is mixed, depending on shocks considered, data used and aid modality. While some find no long-term effect of these programmes on children hit by famine (Dercon and Porter, 2014), others find a positive impact of both food-for-work and free distributions on the short term health of children (Quisumbing, 2003), successful also in offsetting negative shocks (Yamano et al., 2005). More recently, the PSNP has been assessed on children's health outcomes too, with positive impacts found both in the short term (Debela et al., 2015) and in the long-term (Porter and Goyal, 2016). The impact of aid has been assessed also on schooling and child labour, both before and during PSNP implementation. Hoddinott et al. (2011) find that participation into PSNP led to a moderate decrease in agricultural labour hours for boys and large increases in school attendance in households receiving more regular transfers. On the contrary, girls (the younger only, 6-10 years) appear to be negatively affected by the participation of a household in PSNP with decrease in school attendance and increase in child labour. Focusing on pre-PSNP aid, Broussard et al. (2016) find that food-for-work had a negative impact on schooling. Younger children were found to attend fewer years of schooling and older ones were less likely to attend school because of work responsibilities. Free distribution had instead a positive impact on the number of years of schooling for younger boys.

In other African countries the impact of aid shows mixed patterns. In South Africa the Child Support Grant and the Old Pension scheme were both found to have a positive impact on height-for-age (Aguero et al., 2007; Duflo, 2003), although the Old Pension scheme had an impact only on girls and in households where the recipient of the grant was a female[5]. The Zambian Child Grant Programme had a positive impact on the Infant and Young Child Feeding (IYCF) indicator but not on anthropometric outcomes (Seidenfeld and Handa, 2015). In Malawi, the Social Cash Transfer Programme was found to have a statistically significant impact only in the reduction of wasting incidence, but not on other nutritional outcomes (Caroline Population Center, 2016). In Kenya the CT-OVC and the Kenya Hunger Safety Net Programmes were also not found to have an impact on anthropometric indicators (Oxford Policy Management, 2010, 2013).

Much more limited is the literature that addresses the role of aid in protecting from shocks, particularly on child health. This study contributes to assess whether aid can

[5]The positive impact is found both on weight-for-height and height-for-age.

protect children from negative shocks. Two papers are found that address this issue, both in Ethiopia. Dercon and Porter (2014) conclude that aid was not effective in protecting children that were exposed to the 1984 famine. The response system at that time was, however, not particularly efficient. A decade later, the system did seem to have improved as shown by the results reported in Yamano et al. (2005), who find that aid was able to offset the shocks and protect children's nutritional growth. In this chapter, I assess the emergency aid system almost two decades later, after several changes were introduced to the relief system in Ethiopia.

3.3 Context

Ethiopia is a country vulnerable to natural hazards. Drought is one of the most significant and recurrent. Among different climate-related shocks, droughts impact the largest number of people, regularly affecting food production, livestock production and livelihoods of the poor (Van Domelen and Coll-Black, 2012). 2011 was one of those years. Two consecutive rains failed and resulted in a severe drought. With an estimated 4.5 million people in acute food shortage and $823 million of food aid delivered, the 2011 drought was one of the most severe in the last two decades.[6] The crisis hit the southern, eastern and north-eastern parts of the country, with Oromia and Somali regions particularly affected (1.8 and 1.4 million people, respectively), but also Tigray (400,000 people). This drought spread across the entire Horn of Africa. With 13 million people affected across Ethiopia, Somalia and Kenya, the 2011 drought was considered to be the worst in the past 60 years (Mack Smith, 2012).

Notwithstanding the high growth rates that the economy has experienced in the past decade (10.8% per year in 2003/04 - 2014/15, compared to a regional average of 5.4%[7]), droughts that frequently affect the country have a significant effect on the national economy. It was estimated that on average droughts cost the country $1.1 billion per year, roughly 1% of the GDP in 2011 (GDP was $95 billion) (Cabot Venton et al., 2013), which almost balances out the $1.3 billion per year that is received from international assistance to reduce poverty and foster development (Hillier, 2011).

Ethiopia has relied on international aid for decades now. The relief system went

[6] Other drought affected years were 2003, 2005, 2008, 2013, 2016.
[7] http://www.worldbank.org/en/country/ethiopia/overview

through several reforms. The main one was the introduction in 2005 of a social protection programme, the largest in terms of coverage in Sub-Saharan Africa. The main intent of the Productive Safety Net Programme (PSNP) is to provide a reliable support to chronically poor households. Previously, the main tool was emergency aid, which was unpredictable as the Government had to call for annual appeals to the international community. This system proved neither sustainable nor effective as it was used, not only to cope with shocks or exceptional negative events, but also to tackle chronic poverty. In addition, it was mostly a late response humanitarian system, proven to be much more expensive and less effective at preventing losses compared to one based on early response.[8] The Ethiopian Government and its development partners' ongoing efforts are concentrated into strengthening the system of early warning and disaster risk management.[9] In case of exceptional negative events, PSNP *woredas* have 20% of programme budget that can be used for emergency situations such as droughts. The *woredas* that are not included in the PSNP continue to be covered by the emergency response system. Despite the information of poor rainfall was already available in May 2010 and February 2011, it was only in July that, with the declaration of a famine, funding started to increase.

3.4 Analytical framework

Child nutrition is in the literature analysed through econometric models derived from a household utility maximisation problem subject to income and biological health production constraints. Several authors have modelled this maximisation problem which I build on (Behrman and Hoddinott, 2005; Hoddinott and Kinsey, 2001; Strauss and Thomas, 2008; Yamano et al., 2005) to outline the reduced form health demand function.

As one of the household welfare function arguments, child nutritional status depends on parents' preferences regarding the use of family resources. However driven, these decisions are constrained by several factors. There are resource constraints, mainly in the form of income, time available and prices faced, but also constraints from the production process for health outcomes, including nutritional status. The latter depends directly from

[8] Cabot Venton et al. (2013) for instance estimated that early response could economise between $662 million and $1.3 billion in a single event.

[9] One of the more recent results of these efforts is the 2013 new National Policy and Strategy on Disaster and Risk Management which aimed at improving the overall risk management response. The new document replaced the previous national policy which dated back in 1993.

nutrient intakes, both from consumption of macro (calories and protein) and micronutrients (minerals and vitamins) as well as time dedicated to health and nutrition, locality characteristics such as the presence of health facilities and prevalence of infection diseases, the individuals genetic endowment, and health and nutrition knowledge.

Based on Beckers (Becker, 1981) household models for human capital analysis, maximising the household welfare function subject to the total labour constraint, any unearned income, including food aid, and behavioural health and nutrition production function, a set of first order conditions can be solved to derive reduced-form commodity and child health demand functions. The reduced-form for child health demand function takes the following form:

$$H_{ikt+1} = h_{ikt}(C_{ikt}, M_{kt}, W_{kt}, A_{kt}, P_{vt}, Z_{vt}, u_t) \tag{3.1}$$

where H_{ikt+1} is child i health indicator in household k at period $t+1$ and is a function of a set of vectors: C_{ikt} is a vector of child characteristics such as age, sex, innate healthiness, growth potential, and inherited immunities; M_{kt} is a vector of the principal caregiver characteristics such as age, education, relationship to household head, and health knowledge; W_{kt} is a vector reflecting household wealth; A_{kt} is a vector of other household characteristic that may affect child health such as child life-cycle position; P_{vt} is a vector of all relevant prices and Z_{vt} is a vector of health, sanitation and environmental characteristics of the locality where the household lives and that are expected to affect child health (Behrman and Hoddinott, 2005).

It is assumed that H_t can be a sufficient statistic for health endowments which reduces the empirical problem to explaining flows in health and not the evolution of the stock of health over the entire life course. However, the estimation needs to account for the fact that prior health and contemporaneous inputs are likely to be correlated with time invariant innate healthiness and time-varying unobserved variation in healthiness. The literature (Strauss and Thomas, 2008) suggests to address these issues by using individual fixed effect, in order to absorb the impact of time-invariant characteristics and to instrument the lagged health variable with prices and earlier period characteristics.

Therefore, child health can be modelled as a dynamic production function in which current health depends on initial health endowments, health related inputs, and other demographic and background characteristics (Strauss and Thomas, 2008). Child nutrition

is a function of past nutritional status, which proxies for past nutritional inputs, a vector of observed child, household and community characteristics, as well as unobserved child, household, and community characteristics.

3.5 Data

The data used for this study come from the two rounds of the Ethiopian Rural Socioeconomic Survey (ERSS), a joint project between the Central Statistical Authority (CSA) and the World Bank Living Standards Measurement Study-Integrated Surveys of Agriculture (LSMS-ISA). Wave 1 was designed to be representative of rural and small town areas in Ethiopia. The sample was drawn from a population frame that included all areas of Ethiopia, with the exception of a few zones in the Afar and Somali regions. The sample had a two-stage stratified design, where the regions of Ethiopia were taken as strata. The data is representative at the regional level for the major regions, namely Amhara, Oromia, SNNP, and Tigray. Wave 2 was enlarged to include also urban areas, making this wave representative of the entire Ethiopian population.

Households were interviewed three different times, between September and October 2011 (2013) on post-planting agricultural activities, between November and December 2011 (2013) on livestock and related activities, and, finally, between February and April 2012 (2014) to collect information on post-harvest agriculture, household and community questionnaires. In the restricted sample used here the information pertains to the last round of data collection and all households were interviewed in February 2012 (March 2014). The information on food aid and drought therefore refers to 2011, as questions on these topics cover the previous 12 months from the data of the interview. The information obtained is thus to be considered on the main drought that hit vast parts of the country in 2011.

For this study the sample is restricted to the rural population only. This is because the interest is on the effect of drought is most relevant in rural areas, where most of the Ethiopian population live (85%[10]), and where livelihoods mostly depend on (rain-based) agriculture.

The ERSS collects information on height and weight of children 0-60 months old.

[10] http://data.worldbank.org/indicator/SP.RUR.TOTL.ZS

The sample is restricted to children that were between 0 and 36 months old in 2011, therefore 24-60 months old in 2013. The outcome used for this analysis is height-for-age, which provides an indicator of linear growth retardation and cumulative growth deficits in children, and it is calculated using the recent growth standards published by the World Health Organization (WHO) in 2006. These new growth standards were generated using data collected in the WHO Multicentre Growth Reference Study (WHO, 2006). The study draws on a sample of 8,440 children in six countries (Brazil, Ghana, India, Norway, Oman, and the United States) and describes how children should grow under optimal conditions. Therefore, the WHO Child Growth Standards can be used to assess children all over the world, regardless of ethnicity, social and economic influences, and feeding practices.[11]

The measure is expressed in standard deviation units from the median of the reference group. Children whose height-for-age Z-score is below minus two standard deviations (-2 SD) from the median of the WHO reference population are considered short for their age (stunted), or chronically malnourished. Children who are below minus three standard deviations (-3 SD) are considered severely stunted. Stunting reflects failure to receive adequate nutrition over a long period of time and is affected by recurrent and chronic illness. Height-for-age, therefore, represents the long-term effects of malnutrition in a population and is not sensitive to recent, short term changes in dietary intake. Weight-for-height instead measures body mass in relation to body height and it describes current nutritional status. Children with Z-scores below minus two standard deviations (-2 SD) are considered thin (wasted) or acutely malnourished. Wasting represents the failure to receive adequate nutrition in the period immediately preceding the survey and may be the result of inadequate food intake or a recent episode of illness causing loss of weight and the onset of malnutrition. Children with a weight-for-height index below minus three standard deviations (-3 SD) are considered severely wasted.

The sample is further restricted to those villages that received emergency food aid. Emergency aid is in fact directed to villages that face severe shocks and the aim is to compare households from similar environments that received food aid to those that did not. In addition, it has been largely documented that emergency aid in Ethiopia is directed not only towards areas that face shocks but also to villages that have historically been

[11] The new child growth standards replace the previously used reference standards of the U.S. National Centre for Health Statistics, accepted by the U.S. Centers for Disease Control and Prevention (NCHS/CDC/WHO).

targeted, independently of needs. The literature refers to this phenomenon as *inertia* of food aid (Jayne et al., 2002). If these villages have been receiving aid for long periods it is plausible to assume that they are structurally different from villages that have never been targeted by emergency aid making them not a valid control group.

The ERSS is a panel data set that tracks households but not individuals. To create a child-specific panel, I merged children by households, sex and age. After excluding children that were not possible to match because of age or gender measurement errors and for whom anthropometric measures were taken in none or only in one of the two rounds or whose values were defined as not realistic by WHO standards[12], the sample reduces to 168 children. Since the number of observations reduces considerably from the original nationally representative sample, in Table 3.1 I report the differences in characteristics means between the two samples, restricting to only villages that received emergency aid. The only statistical difference is found in the age of the household head, who is two years younger in the restricted sample.

The ERSS includes a module on shocks, from which the information on whether the household faced any drought in the previous 12 months is retrieved. In Section 3.7 the implications related to the use of this self-reported measure of drought are discussed. Information on aid received is obtained from the module on non-earned income, where the information is asked separately for different types of aid, allowing to separate emergency aid from PSNP. Since the sample of PSNP beneficiaries with available anthropometric data on children was too small, this analysis focuses on emergency aid only.

Table 3.2 reports trends in nutritional outcomes across the two waves on the restricted sample. Height-for-age worsens between 2011 and 2013, in absolute value and in the incidence of stunting, which slightly increases, from 35% to 37%. Wasting rates are much lower compared to stunting, and decreases between 2011 and 2013 (from 15% to 12%). However, weight-for-height is on average lower in 2013 compared to 2011, suggesting worse-off condition among those that are wasted. These figures do not match the national statistics, both in the levels and in the trend. Among rural households, in 2011 the national prevalence of stunted children below age of 5 was 44% and decrease in 2013. Figures on wasting are more similar although higher than in national statistics (11%).

In Table 3.1 means of children and households characteristics are reported. In 2011,

[12]WHO reports indicators to be not biologically implausible if above or below ±6 for height-for-age and above or below ±5 for height-for-weight.

Table 3.1: Child and household characteristics

	Full sample	Restricted sample	Diff.
Children characteristics			
Age in months	19.289	21.327	-2.038
Female	0.466	0.452	0.014
First child	0.557	0.470	0.087
Months of breastfeeding	18.202	19.155	-0.952
Household characteristics			
Emergency aid	0.438	0.369	0.069
Drought	0.361	0.286	0.075
Father education	2.482	2.923	-0.440
Mother education	0.941	0.940	0.001
Female head	0.133	0.095	0.038
Age head	39.990	37.631	2.359***
Age mother at birth	27.704	27.182	0.522
Household size	6.182	6.113	0.069
Toilet improved	0.413	0.405	0.008
Land (ln)	9.229	9.406	-0.178
Asset index	-0.048	-0.009	-0.038
Livestock TLU (ln)	1.735	1.596	0.139
Number of rooms	1.537	1.482	0.055
Distance population centre	47.758	46.250	1.508
Crop damage	0.062	0.089	-0.027
Increase in prices	0.405	0.423	-0.017
Illness	0.110	0.125	-0.015

Notes: The unrestricted sample includes 0-36 children from households living in emergency aid villages (881 observations for the children sample and 264 for the household sample). The restricted sample includes observations with same universe but for which there is information on anthropometrics (168 observations for the children sample and 153 for the households sample).

Table 3.2: Nutritional indicators by survey round

	2011	2013
Height-for-age	-1.17	-1.37
	(2.17)	(1.72)
Stunted	0.35	0.37
	(0.48)	(0.49)
Weight-for-height	-0.54	-0.62
	(1.51)	(1.19)
Wasted	0.15	0.12
	(0.36)	(0.32)

Notes: Wasting is defined as a weight-for-height z-score less than -2; stunting is defined as a height-for-age z-score of less than -2. Stunting and wasting refer to proportion of children. Height-for-age and weight-for-height refer to means in values. Standard deviations are reported in parenthesis. Sample size: 168 observations.

children were on average 21 months old, and 45% were female. In this sample, 47% of children were first child and were breastfed for 19 months on average. Within the sample restricted to villages that were targeted for emergency aid, 37% of households report having received emergency aid and 28% suffered a drought in the previous 12 months. On average fathers have higher levels of education compared to mothers. Fathers have on average completed at least the third grade while mothers less than the first grade. 10% of the sample is composed by female headed households. The average age of household heads is 37, quite high considering that these are 0-3 years old children. The mother's age at child birth is 27. Household size is 6 members per household, quite small compared to other African countries, which reflects Ethiopian nuclear organisation of families. 40% of households use improved toilet facilities, defined here as any facility superior to bucket or field, and live in premises with 1.5 rooms on average. As for other shocks faced, 42% reported increase in prices, 12% severe illnesses of households members, and 9% crop damages.

In Table 3.3 I finally report the difference in height-for-age between children that did receive emergency aid and those that did not. There is no statistically significant difference in the means of height-for-age between children that received aid and those that did not. No difference is found either when I split the sample between those that experienced a drought and those that did not.

Table 3.3: Height-for-age differences by aid and drought

	No Emergency Aid (a)	Emergency Aid (b)	Diff. (a) - (b)	t statistics
Height-for-age	-1.349	-1.573	0.224	0.784
Observations	106	62		
	No Drought (a)	Drought (b)	Diff. (a) - (b)	t statistics
Height-for-age	-1.506	-1.246	-0.260	-0.851
Observations	120	48		

Notes: Sample is restricted to children aged 0-36 months in 2011 who are living in villages that received emergency aid. Observations n=168.

3.6 Empirical strategy and Econometric model

The aim of this paper is to investigate the role that food aid had in protecting long-term children's health from the negative effect of a drought. Since food aid is not randomly

allocated, I estimate the average treatment on the treated (ATT or ToT) by using a matching estimator to address the issue of selection bias. Beneficiaries of emergency aid may be more likely to be poor or without means to cope with shocks. As a consequence, estimates on the impact of drought that do not take into account the differences between beneficiary and non-beneficiary households are likely to be upward biased.

Matching is implemented by several methods, the best known being Propensity Score matching (PSM). PSM constructs a statistical comparison group that is based on a model of the probability of participating in the treatment (T) conditional on observed characteristics X, or the propensity score: $P(X) = Pr(T = 1|X)$. Rosenbaum and Rubin (1983) show that, provided that some conditions are met, matching on $P(X)$ is as good as matching on X. The first assumption is the conditional independence assumption (CIA) or unconfoundedness, which requires the treated and the untreated groups to differ only on observable characteristics. In case the selection on observables condition is not met, the estimates will be biased (Dehejia and Wahba, 1999; Heckman et al., 1997). The second condition that must be satisfied is the common support or overlap condition, which states that the observed characteristics of treated and untreated units have to be similar. In the case of the ATT, this condition is relaxed to a weak overlap assumption ($P(T = 1|X) < 1$), where units outside the common support are dropped. One last condition requires treated and untreated units to have access to the same markets (Heckman et al., 1997). I ensure this condition is met by introducing village dummies in the estimation of the propensity score.

PSM is implemented in two stages. First, treatment is modelled, typically with a logit or probit regression. Then the propensity score, obtained from these estimates, is used to match treated with the nearest untreated observations. For each pair the difference in the outcome is calculated and the impact is the non-parametric mean of these differences. There a number of different matching methods that can be used to construct the control group (e.g. nearest neighbour, kernel).

The estimator used is the doubly-robust inverse-probability-weighted regression-adjustment (IPWRA). The choice for this estimator is driven by a number of reasons. First, with standard (non parametric, i.e. nearest neighbour, kernel) matching estimators it is not possible to interact treatment variable with other covariates. The main interest of this study is to assess the differential effect of food aid for those that experienced a drought.

The IPWRA instead is more flexible as in the second step the outcome model is fully specified and it can be modelled with the functional form that is most suitable. In addition to being able to include interaction terms, this estimator allows to explicitly control for variables which are important in modelling the outcome. In the case of child nutritional status, for instance, characteristics of the child, such as gender and age, are crucial in the estimation of the outcome and reduce the potential biases from heterogeneity in the error term (Outes and Porter, 2013). In the case of this study, the treatment is at household level whereas I assess the impact on an individual outcome. It is plausible to assume that some characteristics are more relevant to be included in the model of nutritional status than in the participation equation. Matching does not control for these differences, thus their inclusion improves efficiency of the final outcome estimates. An additional feature of the IPWRA refers to the balancing condition. Balance across baseline characteristics included in participation equation is no longer needed as these also appear in the IPWRA.

By using weights, IPWRA is superior in terms of statistical precision to PSM. PSM in fact compares each treatment observation to only one (or few) control observations with a similar probability of being treated. IPWRA instead uses weights, thus compares every treated observation to every control observation by placing higher weights to those observations with closer likelihood of being treated and lower to those more dissimilar. This implies that more observations are used and therefore precision is improved. Another appealing feature of the IPWRA is that it is doubly-robust to misspecification of either the propensity score model or the conditional means (OLS) (Wooldridge, 2010). If either the treatment or the outcome model is misspecified, the estimates will still be consistent.

The estimation of the IPWRA is implemented in three main steps. First, the probability of being treated is estimated through a logit (or probit) model including the variables that are thought to be important in program selection but that are not influenced by it. The predicted outcome represents the estimated probability of participation or propensity score, which is defined as $e = P(T = 1|X)$.

In the second step the propensity score is used to derive the weights, which in the case of the ATT are derived as:

$$w_{ATT} = T + \frac{e(1-T)}{1-e} \tag{3.2}$$

These weights are obtained by multiplying the ATE weights[13] by e, so that treated subjects receive a weight of one. Each unit weight is equal to the inverse of the probability of receiving the treatment that the unit received. The inverse of the estimated propensity score is used to weight the units in order to eliminate biases associated with differences in observed covariates. The weights are then normalised to improve the mean-squared-error properties of the estimator (Imbens and Rubin, 2015). The treated observations with small propensity score (or those untreated with close probability to one) could in fact result in very large weights (Austin and Stuart, 2015). In addition, to ensure common support, as suggested in the literature (Smith and Todd, 2005), I eliminate the outer 5% of the observations from the tails and I make sure that there is no observation within this interval that lacks common support.

The average treatment effect on the treated is defined as:

$$\hat{ATT} = \frac{1}{N_T} \sum_{i \in T} \hat{w}_i y_i - \frac{1}{N_C} \sum_{i \in T} \hat{w}_i y_i \tag{3.3}$$

where N_T is the number of treated observations and N_C is the number of control observations. The ATT is obtained by comparing the treatment mean to the reweighted comparison group mean.

The IPWRA is finally obtained via estimating the following regression through weighted least squares:

$$\begin{aligned} H_{ikt+1} =& \alpha_0 + \beta_1 Drought_{kt} + \beta_2 Aid_{kt} + \beta_3 Drought_{kt} * Aid_{kt} + \beta_4 C_{ikt} \\ &+ \beta_5 X_{kt} + v_j + \varepsilon_{it} \end{aligned} \tag{3.4}$$

where H_{ikt+1} is the height-for-age z-score of child i in time $t+1$, *Drought* is a dummy variable equal to 1 if the household reported having experienced a drought in period t and 0 otherwise, and *Aid* is s dummy indicating the treatment status for emergency aid of household k in time t. The regression is weighted by the inverse propensity score as defined in Equation 3.2. The main coefficients of interest are β_1 and β_3. The former captures the long term impact the drought had on nutritional status of children living in households that did not receive emergency aid. β_3 identifies the differential impact that emergency

[13] Defined as $W_{ATE} = \frac{T}{e} + \frac{1-T}{1-e}$

aid had on nutritional status of children from beneficiary households hit by a drought in comparison to non-beneficiary ones. The standard errors are clustered at village level.[14] Since emergency aid is targeted first at village level, and drought is expected to have an effect on the village economy, I add village fixed-effects (v_j).

The choice of covariates to include in the child health regression are driven by the economic and medical literature. Child-level covariates (C_{ikt}) that are included in the model are child age in months, gender, birth order, and other health-related information. In East Africa, in particular, females are found to be better nourished than males (Charmarbagwala et al., 2004). Birth order has been found to matter in several studies, with nutritional outcomes deteriorating as child order increases (Jayachandran and Pande, 2015). To capture this effect a dummy variable that indicates whether the child is the first born is included. The number of months the child was breastfed for is an important source of information on child health and past nutrition, as well mother feeding knowledge (UNICEF, 2006). Past child illnesses are likely to have influenced child health but also the resources given to the child. I include a dummy to identify whether the child has been sick for more than three months in the previous year. H_{it-1} is used to proxy for health endowments that are not captured by the variables included in the model.

At the household level, several variables reflecting household income, parental education and household composition are included (X_{kt}). Wealth is proxied by the size of land under cultivation, the number of rooms the house has, the livestock owned (expressed in tropical livestock units), and a wealth index[15] which summarises the assets owned by the household. Several studies (Yoong et al., 2012) have shown that income earned by women has a higher probability of benefiting children status than men's income. Empirically, given the lack of detailed information on intra-household resource allocation, the gender of the household head is used as a proxy. Female household heads may be associated with better child nutritional status as they could have more decision making power; at the same time, however, they tend to have lower levels of income and thus lower nutritional status. Wealth levels, however, are controlled for by other variables.

Household size and composition are other important factors for income generation as well as for resource allocation within the household. Larger households may have a carer

[14] Imbens and Wooldridge (2009) suggests that standard errors from the naive variance estimator based on weighted least squares can be used. Another option is to bootstrap the entire procedure.

[15] The wealth index is calculated through factor analysis.

for the children, or may have less need to involve children in heavy works. In terms of composition, households with more able-bodied members in working age are generally found to be better off. Instead, in households with high dependency ratios, children may be facing competition over resources, with younger ones particularly vulnerable. Age of the household head and the age of the mother when given birth are also included. Ethiopia is a country where early marriage is still widespread and birth at early ages has been found to be negatively correlated with child health (Gibbs et al., 2012).

Parental education is expected to play an important role in child nutrition. On the one hand, higher education is related to higher income and, on the other, higher educated parents tend to have better access to information about child nutrition and health and make use of facilities. The former channel is more likely to be represented by men's education while the latter by the mothers'. In addition, the empirical evidence finds maternal level of education to be a significant determinant of child nutrition in Eastern African countries (Charmarbagwala et al., 2004).[16] The variables used for education represent the highest grade achieved.

In addition to food availability, sanitation and water supply are essential factors in determining child nutritional status. By reducing the risk of bacterial infections and diarrhoeal diseases, sanitation and clean water will indirectly contribute to child nutrition (Charmarbagwala et al., 2004). In the model this is proxied by a dummy for the use of improved toilet facilities. The distance to the nearest population centre is included as a proxy of access to facilities and markets. Additional dummies for idiosyncratic and covariate shocks are controlled for (severe illness of a household member, crop damage and price increase).

Propensity scores for emergency aid participation are estimated through a logit model. To satisfy the CIA, the set of variables included in the model should capture factors that affect both participation and outcome. Economic theory, previous research on the topic, and knowledge on the institutional setting should all contribute to the choice of variables to be included. Empirical evidence also underlines that the set of variables that satisfy matching conditions is not necessarily the most inclusive one. Adding too many variables might lead to violations of those same conditions (Smith and Todd, 2005).

As suggested by Imbens and Rubin (2015), in addition to basic strong determinants

[16] Western African countries, instead, are characterised by different household patterns as women tend to have greater power in terms of resource allocation.

of outcome and project participation, additional covariates are added through a stepwise procedure provided they achieve a p-value below 15%. Additionally, a similar stepwise procedure is used to add interactions and squared terms (p-value below 5%) to capture non linearities. The first set of basic variables that are thought to be important determinants of the outcome and project participation is composed of demographic and wealth variables: household size, father education, a dummy variable for whether the household is female headed, the age of the head, and the size of the land under cultivation. The additional variables that are added through the stepwise procedure are the distance to population centre and its square, whether a household member had been ill in the previous year, the number of rooms the household house has, and the interaction between the number of rooms and the land size. Table B.2 of the Appendix reports the marginal effects of the logit for selection into emergency aid. Higher levels of household head education are negatively correlated with the probability of accessing emergency aid. Female headed households are less likely to receive aid. Land size and number of rooms, two proxies for wealth, are both negatively related to the probability of getting aid. Overall targeting in 2011 in the villages considered for this analysis are based on wealth, with decreasing probabilities of accessing aid as wealth increases. On the other hand, though, female headed households are less likely to receive aid, which can be interpreted as discriminatory against these more vulnerable households.

Table B.1 of the Appendix reports the means of selected variables between treatment and control group before being adjusted (first two columns), and the differences between the two groups before and after adjusting with the weights obtained from matching. The means of covariates across the treated and the control groups are quite similar. Only the interaction term between number of rooms and land size is statistically different in the unadjusted sample, which is no longer significant after applying the weights. Even though almost no statistical differences are found in the unadjusted sample covariates, the matching provides an improvement to OLS as the differences reduce once weights are applied. Matching in this case is still preferred to OLS as it helps producing a more similar control group and helps removing those observations that are off the common support. The importance of accounting for the selection in the programme is also proven by the logit estimates which show that there is a statistical difference in accessing aid based on a number of observables (Blundell and Dias, 2000).

Figure B.1 in the Appendix shows that the condition of common support is satisfied.

3.7 Results

Before proceeding with the analysis of the long-term impact of food aid in response to the drought on child health , I investigate the short-term impact that the drought had on households food security and children nutritional status. Table 3.4 shows the results of the impact of drought on consumption in adult equivalent units (Column 1), on number of months during which the household faced shortages in food (Column 2), on the probability of a child being wasted (Column 3), on weight-for-height (Column 4), and on height-for-age. The first two columns refer to household level analysis, while the last three to child level. The 2011 drought has a statistically significant negative effect on households' food security and welfare: it is correlated with decreases in consumption and with increases in the number of months during which the households faced food shortages. The impact of the drought on children's health outcomes is statistically significant only for the probability of being wasted. The drought has no statistical impact instead on weight-for-height, although it has a negative effect. No statistically significant impact is found on height-for-age either, although in this case the lack of impact is expected. Overall these results indicate that the 2011 drought had a negative impact on household welfare and on children's health.

Table 3.4: Short term impact of drought on welfare

	Consumption (1)	Food Insecurity (2)	Wasted (3)	Weight-for-height (4)	Height-for-age (5)
Drought 2011	-1.092**	1.310**	0.327*	-0.875	-0.546
	(0.404)	(0.538)	(0.174)	(0.634)	(0.706)
Adjusted R^2	0.287	0.614	0.112	0.226	0.361
Observations	153	153	168	168	168

Notes: Sample is restricted to children aged 0-36 months in 2011 living in villages that received emergency aid. Regression for consumption (expressed in logarithm and in adult equivalent units) and food insecurity (number of months the household experienced food insecurity) are run at household level; last 3 columns (child health) at child level. Estimates are OLS. Standard errors are clustered at village level. Additional covariates not reported include household and head characteristics, wealth proxies, village fixed effects, and additional shocks faced by the households. In the child level regressions, child characteristics are added.

The use of self-reported drought might raise some concerns in relation to its exogeneity. While this variable has the advantage of measuring the drought at the households level, at the same time the fact that not all households within the same village report exposure to drought might reflect differences in households characteristics and behaviours, in which case this measure would be endogenous, thus biasing the coefficients of its effect upwards.

On the other hand, conditions within a village may substantially vary, in terms of micro-climate, geography, soil conditions, and farm practices. As a consequence, the use of rainfall and climatic data data may produce underestimated estimates of drought impacts (del Ninno and Mills, 2015; del Ninno et al., 2001). In Section 3.8 I provide some evidence in support of the use of self-reported drought.

Table 3.5 reports the results of the long-term effects of drought and emergency aid on children's health. Specifically, it shows the effect of 2011 emergency aid and drought on height-for-age two years later. The first three columns are estimates for model 3.4 without lagged height-for-age, the last three columns with its inclusion. Since the interest of this analysis is not on catch-up growth, which is assessed through the coefficient of lagged height-for-age, the problems related to the endogeneity of the variable that are usually raised and addressed in the literature are not of major concern here. I only present the results with and without it to test for robustness.[17]

When not interacted (Columns 1 and 2, and 4 and 5), drought and emergency aid do not have an effect on height-for-age. Once interacted (columns 3 and 6), instead, the differential impact becomes significant. The drought in 2011 has a negative and significant impact on long-term health of children at the 5 percent significance level. Experiencing a drought without receiving emergency aid decreases height-for-age two years later by 1.76 standard deviations. This effect can be translated into centimetres by taking the standard deviations of height from the WHO Child Growth Standard for children aged 44 months by gender (the average age in this sample in 2013). Drought exposure decreases the height of a boy by 5.26 centimetres and the height of a girl by 5.57 centimetres. The interaction term between aid and drought, instead, is positive and statistically significant at the 5 percent level. The coefficient is larger than the one of drought, showing that emergency aid protected children from the negative effect of the drought. The net effect is 0.371 standard deviations, which in centimetres can be translated into 1 centimetre, both for boys and girls, of higher height for children that experienced the drought and received emergency aid compared to children that did not experienced the drought and did not receive aid.[18]

[17] Following the literature, I tried several instruments that unfortunately did not work with these data. The results, as mentioned, should not be affected.

[18] The impact calculated is obtained by multiplying the estimated coefficient by the standard deviation of height in the international reference data set, which is age- and gender-specific (Groppo and Schindler, 2014). The age group that is used here is the one corresponding to the average age in 2013 in this sample, 44 months.

The size of the results are in line with those found in the literature. Yamano et al. (2005) find a detrimental effect of crop damage of 0.9 centimetres after six months, and a positive effect of food aid on height growth by 2 centimetres. Also in this case aid is found to offset the negative impact from the shock.

Table 3.5: Matching: Long term impact of shocks and emergency aid on child height-for-age

	(1)	(2)	(3)	(4)	(5)	(6)
Drought 2011	-0.534		-2.262**	-0.303		-1.757**
	(0.755)		(1.062)	(0.599)		(0.802)
Emergency aid 2011		0.135	-0.213		-0.083	-0.418
		(0.327)	(0.337)		(0.307)	(0.309)
Emergency aid 2011 × Drought 2011			2.383**			2.128**
			(1.126)			(1.013)
HAZ 2011				0.445***	0.448***	0.439***
				(0.066)	(0.067)	(0.067)
Female	0.374	0.389	0.416	0.283	0.284	0.314
	(0.532)	(0.532)	(0.533)	(0.404)	(0.406)	(0.410)
Age in months	0.025	0.026	0.027	0.023	0.023	0.024*
	(0.018)	(0.017)	(0.018)	(0.014)	(0.014)	(0.014)
Father's education	0.088	0.091	0.062	0.053	0.059	0.039
	(0.073)	(0.070)	(0.070)	(0.051)	(0.045)	(0.047)
Mother's education	0.150	0.149	0.196	0.184	0.178	0.215*
	(0.126)	(0.123)	(0.126)	(0.129)	(0.121)	(0.116)
Land size (ln)	0.360*	0.393*	0.281	0.484**	0.481***	0.382**
	(0.211)	(0.207)	(0.186)	(0.187)	(0.177)	(0.154)
Asset index	0.069	0.070	0.117	0.156**	0.151**	0.189***
	(0.077)	(0.080)	(0.083)	(0.060)	(0.061)	(0.069)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	168	168	168	168	168	168
Adjusted R^2	0.328	0.326	0.354	0.549	0.548	0.569

Notes: Estimates obtained through IPWRA. Variables used for matching are taken at baseline and include: household characteristics and composition, idiosyncratic shocks, land, village dummies. Covariates included in the regression: age in months and gender of child, duration in months of breastfeeding, father and mother education level, age household head, household size (log), improved toilet, land size (log), asset index, livestock (expressed in tropical livestock units and log), number of rooms, distance to population centre, and its square, shocks (child illness, crop damage, increase in prices). Standard errors are clustered at village level (44 villages). Significance levels * 10% ** 5% *** 1%.

Other statistically significant control variables have the expected sign. In Table B.3 of the Appendix the full set of controls are reported. Wealth proxies are positively correlated with height-for-age. Increases in land size under cultivation, asset index, and number of rooms of the house have a positive effect on height of the child. Livestock, on the other hand, is not statistically significant. Separately I run a regression omitting variables that could be affected by drought and emergency aid, i.e. livestock and wealth index, and results do not change. Among other variables correlated with child health, mother education and access to improved toilet are positively associated with taller children. A counter-intuitive coefficient is the one of rice in prices, which is positive. The reason could be that households in the sample that reported experiencing this shock are also net-sellers.

As an alternative measure of child health, I use the dichotomous stunting variable instead of the continues heigh-for-age. Results are reported in Table B.4 of the Appendix and show no statistically significant effect of drought or aid on the probability of being stunted. The lack of significant results could depend on the small size of the sample, which might prevent showing the positive effect of aid on the nutritional status of children. The covariates statistically associated with higher probability of being stunted are the asset index and the gender of the child. Higher values of assets are associated with a lower probability of being stunted while girls are 14.7 percentage points less likely to be stunted compared to boys.

Next I report the results for OLS estimates, shown in Table B.5 of the Appendix. The coefficients of interest (drought and interaction between drought and emergency aid) are no longer significant. This is not surprising as OLS does not address the selection of receipt of emergency aid. The significance of other variables also disappears, with only assets and lagged height-for-age now being statistically correlated with child health.

To show that the 2011 drought coefficient is not picking up the impact of more recent drought or aid, in Panel A of Table 3.6 I report the results of the same model as equation 3.4, with the addition of 2013 drought, aid and their interaction. The results are in line with those reported in Table 3.5, with almost identical coefficients and level of significance. The coefficients of 2013 variables, are, instead, not significant, confirming that the 2011 variables are not picking more recent events.

In Panel B, in addition, I report the results for the same model but for weight-for-height. This is done to check whether the 2011 drought had an impact also on short term health. The results show that there is no long term impact of 2011 drought on weight-for-height, which is expected as this is an indicator of short term health, thus it should be affected mostly by recent events. This is confirmed by the coefficients of 2013 drought and its interaction with emergency aid. Both are highly significant and with the expected signs. Similarly to the results for height-for-age (long term effects), the coefficient of drought is negative, while the interaction with aid is positive, and larger than the drought one, showing that aid has an important role in protecting children health.

Table 3.6: Matching: Impact of past and present shocks and emergency aid on HAZ and WHZ

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Height-for-age						
Drought 2011	-0.533	-2.251**	-2.222**	-0.303	-1.759**	-1.776**
	(0.751)	(1.050)	(1.048)	(0.601)	(0.811)	(0.827)
Emergency aid 2011		-0.175	-0.122		-0.432	-0.449
		(0.329)	(0.354)		(0.333)	(0.368)
Emergency aid 2011 × Drought 2011		2.350**	2.313**		2.138**	2.158**
		(1.110)	(1.098)		(1.034)	(1.053)
Drought 2013	-0.367	-0.338	-0.910	0.023	0.115	0.224
	(0.564)	(0.543)	(1.218)	(0.438)	(0.490)	(1.090)
Emergency aid 2013			0.003			0.293
			(0.803)			(0.515)
Emergency aid 2013 × Drought 2013			0.977			-0.158
			(1.355)			(1.009)
Observations	168	168	168	168	168	168
Adjusted R^2	0.324	0.349	0.342	0.544	0.565	0.557
Panel B: Weight-for-height						
Drought 2011	-0.798	-0.927	-0.838	-0.788	-0.900	-0.816
	(0.606)	(0.589)	(0.613)	(0.596)	(0.587)	(0.614)
Emergency aid 2011		0.147	0.285		0.120	0.260
		(0.308)	(0.281)		(0.332)	(0.308)
Emergency aid 2011 × Drought 2011		0.091	-0.021		0.077	-0.040
		(0.578)	(0.585)		(0.587)	(0.594)
Drought 2013	0.065	0.026	-1.296***	0.113	0.083	-1.196***
	(0.429)	(0.448)	(0.453)	(0.424)	(0.440)	(0.416)
Emergency aid 2013			-0.562			-0.550
			(0.388)			(0.377)
Emergency aid 2013 × Drought 2013			2.205***			2.129***
			(0.605)			(0.520)
Observations	168	168	168	168	168	168
Adjusted R^2	0.723	0.719	0.732	0.737	0.732	0.744
Lagged HAZ	No	No	No	Yes	Yes	Yes
Village FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates obtained through IPWRA. Variables used for matching are taken at baseline and include: household characteristics and composition, idiosyncratic shocks, land, village dummies. Covariates included in the regression: age in months and gender of child, duration in months of breastfeeding, father and mother education level, age household head, household size (log), improved toilet, land size (log), asset index, livestock (expressed in tropical livestock units and log), number of rooms, distance to population centre, and its square, shocks (child illness, crop damage, increase in prices). Standard errors are clustered at village level (44 villages). Significance levels * 10% ** 5% *** 1%.

3.8 Robustness checks and sensitivity analysis

In this section I present additional analyses undertaken to address potential concerns related to the robustness of the results. First, I run a sensitivity test on the proportion of observations included to ensure common support. While in the main results I use a sample trimming the outer 5 percent observations of the propensity distribution, Table B.6 of the Appendix reports the results when I trim the outer 2 (first three columns) and 10 (last three) percent observations respectively. The results are very close, especially the first three columns. This is not surprising since there is only one additional observation than the sample used for the main analysis. The results still hold when the ten percent

of the tails of the distribution are trimmed. The coefficients of interest only marginally shrink and the coefficient of the interaction is now significant only at 10 percent.

As discussed in section 3.5, the use of drought as self-reported variable may raise concerns. Notwithstanding rainfall data was available, I preferred to use the self-reported drought and include village fixed-effects in the analysis. The need to control for unobservables linked to the village economy and aid receipt made me opt for the self-reported drought. The concern is that, since not all households reported drought within the same village, it could be that there are unobservable differences correlated with households reporting the drought and those that did not. Looking at the trend in the incidence of drought across the two years, it is clear that there is only a minor proportion of households that report the drought in both years (5% of them). This could depend, however, also on the low proportion of households experiencing a drought in 2013, which overall amounts to 10%. A remaining 20% reported being exposed to the drought in 2011 and 70% did not experience the drought in neither of the years.

A more formal test to control that results from drought and its interaction with aid are not picking up other factors can be run by first estimating the determinants of drought to identify which covariates are mostly correlated with those who report drought. These variables can then be interacted with emergency aid in the main regression to make sure that at last the interaction between aid and drought is reflecting these two variables, net of other factors. Table B.7 shows the correlates of the probability of a household reporting having experienced a drought in 2011. The variables associated with drought are the level of livestock holdings, number of rooms that compose the house, households female headed, and experiencing two shocks, illness of a households member and increase in prices.

In Table 3.7 I report the results with these interactions, without (Column 1) and with (Column 2) lagged height-for-age. The results confirm the positive effect that emergency aid had in protecting the long-term health of children in households that reported a drought. The coefficients are very close to those presented in Table 3.5 in the previous Section, reassuring on the findings presented on the role that aid played in protecting children health.

As a further robustness check, I estimate the main model with a different matching method. In Table B.8 of the Appendix I report the estimates using Inverse Probability Weighting (IPW), without adding covariates in the second step. The results are in line with

Table 3.7: Matching: Interaction of aid with variables correlated with drought

	(1)	(2)
Emergency aid 2011	0.777	1.059
	(0.997)	(0.911)
Drought 2011	-2.844**	-1.753**
	(1.291)	(0.845)
Emergency aid 2011 × Drought 2011	2.935**	2.153**
	(1.335)	(1.042)
Emergency aid 2011 × Price rise 2011	0.673	0.199
	(0.666)	(0.409)
Emergency aid 2011 × 11 Livestock TLU (ln)	-0.200	-0.464**
	(0.260)	(0.210)
Emergency aid 2011 × Head female	-1.526	-0.175
	(1.494)	(0.834)
Emergency aid 2011 × Illness	-2.366	-2.101*
	(1.572)	(1.087)
Emergency aid 2011 × No. rooms	-0.384	-0.328
	(0.350)	(0.364)
HAZ 2011		0.447***
		(0.068)
Village FE	Yes	Yes
Observations	168	168
Adjusted R^2	0.359	0.577

Notes: Variables used for matching are taken at baseline and are: household characteristics and composition, idiosyncratic shocks, land, zone dummies. All regressions are estimated with village fixed effects. Covariates included in the regression: age in months and gender of child, father and mother education level. Standard errors are clustered at village level. Significance levels * 10% ** 5% *** 1%.

the ones presented in Section 3.7. The main difference lies in the higher coefficient of the interaction term. The estimates presented as main results, though, should be considered as more reliable since they additionally control for a number of important factors related to height and aid receipt.

3.9 Conclusions

The increasing weather variability, especially in developing countries, is raising interest among researchers and policy makers around what kind of interventions could help vulnerable individuals in coping with negative shocks. In this paper I explore the impact of drought on children's health and the role that food aid had in mitigating the negative effect of the shock. I explore this issue in Ethiopia, one of the country most prone to weather shocks that has historically, and even more frequently in the past two decades, been subject to droughts and other weather-related hazards.

The analysis focuses on children aged 0-36 months living in villages that received emergency aid, and investigates the impact that the drought had two years later on their

health, as captured by their height-for-age. Further, I examine whether food aid mitigated the adverse effect of the shock. To address the issue of selection into the aid programme, I use a doubly-robust matching estimator, the inverse probability weighting adjusted-regression (IPWRA). I find that drought had a detrimental effect on children's height-for-age by 1.76 standard deviations compared to children that did not experience the drought. On the other hand, this effect was offset by food aid.

These results are robust to a number of further analyses. First, even when I use a different matching estimator, results hold. One source of concern in this context derives from the use of a self-reported definition of drought. Additional analyses that control for covariates that are correlated with drought reassure on the validity of the main results. However, some caveats are to be considered. One limitation of this study relates to the impossibility of using household or mother fixed effects to deal with the issue of correlation of errors, particularly with the unobserved child-specific health endowment, but also with the reporting of drought and the receipt of aid. The use of fixed effects was hampered in particular by the small size of the sample, which prevented to find a substantial number of households with more than one child within the age range used for this analysis. Furthermore, it was not possible to instrument the lagged height-for-age as no valid instrument was available. This, however, should not be a major issue as the main aim here is not to assess catch-up growth, although results should still be interpreted with caution.[19]

Additional research on the topic would greatly benefit from improvements in the collection of the anthropometric variables and in the age variable of the children as these were the main issues that constrained the analysis to a small sample. The policy implications from the results of this type of analysis are in fact of high relevance. Assessing if aid is indeed producing its intended aim is of great importance. Further analysis of the role of aid programmes in protecting from weather-related shocks is needed as the evidence is still quite limited.

[19] A potential remaining issue is the one related to measurement error of height-for-age, which could cause attenuation bias towards zero.

Appendix B

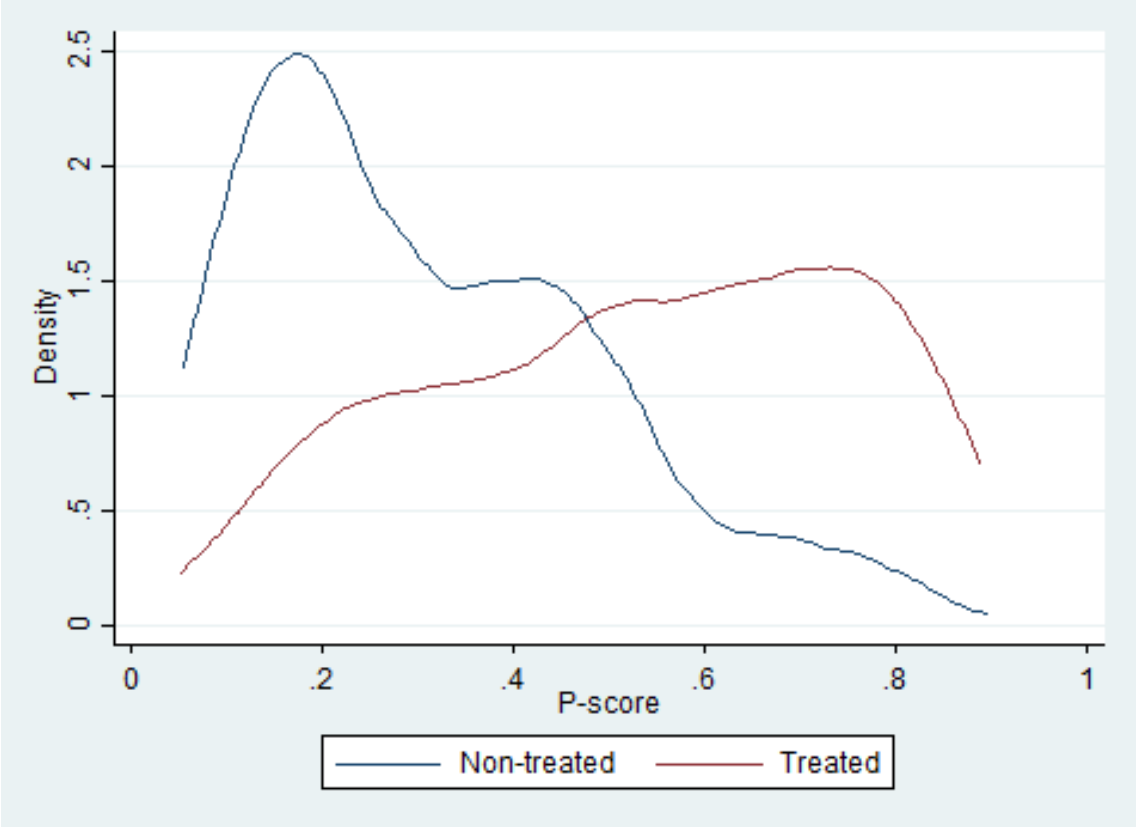
Tables

Table B.1: Characteristics of sampled households: Selection variables for emergency aid

	Control	Treatment	Diff. Unadjusted	Diff. Adjusted
Hh size (ln)	2.422	2.411	0.011	0.013
Education father	2.180	1.769	0.411	0.146
Female head	0.135	0.164	-0.029	0.027
Age head	39.751	40.723	-0.972	0.584
Land (ln)	9.373	9.124	0.249	-0.025
Hh members problems selfcare	0.010	0.031	-0.020	-0.028
Distance to population centre	43.183	44.553	-1.369	-0.613
Illness	0.104	0.123	-0.019	-0.005
No rooms	1.547	1.400	0.147	0.005
No rooms X Land	14.619	13.114	1.505**	0.035
Distance pop. centre sq.	3,126.461	3,384.115	-257.654	32.129

Notes: Unadjusted and adjusted differences using inverse probability score weighting. Means for control and treatment are weighted. Sample restricted to 153 households.

Figure B.1: Common support for emergency aid



Notes: Common support trimming the outer 5% of the observations. After the trimming 153 households remain.

Table B.2: Logit estimates for emergency aid receipt

Household size (ln)	-0.069
	(0.140)
Father's education	-0.032*
	(0.018)
Head female	-0.246**
	(0.097)
Head age	0.004
	(0.005)
Land size (ln)	-0.169***
	(0.063)
Distance to population centre	-0.018
	(0.019)
Illness 2011	-0.085
	(0.160)
No. rooms	-1.425**
	(0.598)
Distance to population centre sq.	0.0004*
	(0.0002)
No. rooms X Land	0.130**
	(0.053)
Village fixed-Effect	Yes
Observations	153
Pseudo R^2	0.284
LL	-72.619

Notes: Reported coefficients are average marginal effects from logit model. Dependent variable is a dummy equal to 1 if household received emergency aid in 2011. Sample restricted to households living in villages that received emergency aid. Standard errors are clustered at village level (44 villages). Significance levels * 10% ** 5% *** 1%.

Table B.3: Matching: Long term impact of shocks and emergency aid on child height-for-age

	(1)	(2)	(3)	(4)	(5)	(6)
Drought 2011	-0.534		-2.262**	-0.303		-1.757**
	(0.755)		(1.062)	(0.599)		(0.802)
Emergency aid 2011		0.135	-0.213		-0.083	-0.418
		(0.327)	(0.337)		(0.307)	(0.309)
Emergency aid 2011 × Drought 2011			2.383**			2.128**
			(1.126)			(1.013)
HAZ 2011				0.445***	0.448***	0.439***
				(0.066)	(0.067)	(0.067)
Female	0.374	0.389	0.416	0.283	0.284	0.314
	(0.532)	(0.532)	(0.533)	(0.404)	(0.406)	(0.410)
Age in months	0.025	0.026	0.027	0.023	0.023	0.024*
	(0.018)	(0.017)	(0.018)	(0.014)	(0.014)	(0.014)
Months of BF	0.018	0.018	0.016	0.014	0.013	0.011
	(0.023)	(0.022)	(0.022)	(0.015)	(0.014)	(0.014)
Improved toilet	2.423***	2.304***	2.245***	1.516***	1.473***	1.397***
	(0.511)	(0.486)	(0.592)	(0.371)	(0.339)	(0.382)
Crop damage	-0.954*	-0.975*	-1.085*	0.650	0.628	0.474
	(0.562)	(0.499)	(0.562)	(0.492)	(0.481)	(0.504)
Price rise	1.709***	1.647***	1.886***	1.261**	1.201**	1.380**
	(0.569)	(0.573)	(0.628)	(0.597)	(0.576)	(0.603)
Livestock Tlu (ln)	0.306	0.285	0.319	0.174	0.140	0.151
	(0.329)	(0.319)	(0.328)	(0.291)	(0.269)	(0.286)
Household size (ln)	0.443	0.413	0.519	0.103	0.104	0.203
	(0.727)	(0.729)	(0.707)	(0.589)	(0.599)	(0.563)
Father's education	0.088	0.091	0.062	0.053	0.059	0.039
	(0.073)	(0.070)	(0.070)	(0.051)	(0.045)	(0.047)
Mother's education	0.150	0.149	0.196	0.184	0.178	0.215*
	(0.126)	(0.123)	(0.126)	(0.129)	(0.121)	(0.116)
Head female	-0.766	-0.852	-0.187	-0.615	-0.719	-0.204
	(0.978)	(0.907)	(0.939)	(0.596)	(0.494)	(0.508)
Head age	0.003	0.001	0.005	0.009	0.008	0.011
	(0.020)	(0.020)	(0.020)	(0.016)	(0.016)	(0.016)
Land size (ln)	0.360*	0.393*	0.281	0.484**	0.481***	0.382**
	(0.211)	(0.207)	(0.186)	(0.187)	(0.177)	(0.154)
Asset index	0.069	0.070	0.117	0.156**	0.151**	0.189***
	(0.077)	(0.080)	(0.083)	(0.060)	(0.061)	(0.069)
Distance to population centre	0.062	0.118	0.039	0.234	0.237	0.173
	(0.442)	(0.441)	(0.407)	(0.327)	(0.338)	(0.313)
Illness	-0.410	-0.454	-0.245	-0.798	-0.831	-0.661
	(0.797)	(0.819)	(0.795)	(0.567)	(0.558)	(0.542)
No. rooms	2.941	3.262	2.021	3.668**	3.635**	2.546*
	(2.199)	(2.126)	(1.973)	(1.785)	(1.669)	(1.395)
Distance to population centre sq.	0.001	0.001	0.001	-0.000	-0.000	-0.000
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
No. rooms X Land	-0.312	-0.338*	-0.234	-0.385**	-0.380**	-0.287**
	(0.197)	(0.188)	(0.177)	(0.158)	(0.145)	(0.123)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	168	168	168	168	168	168
Adjusted R^2	0.328	0.326	0.354	0.549	0.548	0.569

Notes: Estimates obtained through IPWRA. Dependent variable: height-for-age in 2013. Variables used for matching are taken at baseline and include: household characteristics and composition, idiosyncratic shocks, land, village dummies. Covariates included in the regression: age in months and gender of child, duration in months of breastfeeding, father and mother education level, age household head, household size (log), improved toilet, land size (log), asset index, livestock (expressed in tropical livestock units and log), number of rooms, distance to population centre, and its square, shocks (child illness, crop damage, increase in prices). Sample restricted to households living in villages that received emergency aid. Standard errors are clustered at village level (44 villages). Significance levels * 10% ** 5% *** 1%.

Table B.4: Matching: Long term impact of shocks and emergency aid on probability of being stunted

	(1)	(2)	(3)	(4)	(5)	(6)
Drought 2011	0.175		0.399	0.071		0.273
	(0.180)		(0.256)	(0.155)		(0.231)
Emergency aid 2011		-0.080	-0.049		-0.038	0.022
		(0.091)	(0.104)		(0.108)	(0.132)
Emergency aid 2011 × Drought 2011			-0.289			-0.303
			(0.311)			(0.314)
Stunted 2011				0.315***	0.314**	0.319***
				(0.114)	(0.117)	(0.116)
Female	-0.103	-0.107	-0.109	-0.144*	-0.148*	-0.147*
	(0.092)	(0.091)	(0.090)	(0.085)	(0.085)	(0.084)
Age in months	-0.008	-0.008	-0.008	-0.009	-0.009	-0.009
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Father's education	-0.012	-0.012	-0.006	-0.010	-0.011	-0.006
	(0.017)	(0.017)	(0.017)	(0.018)	(0.017)	(0.018)
Mother's education	-0.045	-0.047*	-0.054*	-0.023	-0.023	-0.031
	(0.028)	(0.027)	(0.029)	(0.025)	(0.024)	(0.026)
Land size (ln)	-0.009	-0.023	-0.009	-0.043	-0.049	-0.029
	(0.052)	(0.049)	(0.049)	(0.046)	(0.051)	(0.053)
Asset index	-0.043*	-0.044*	-0.051**	-0.044**	-0.045**	-0.051**
	(0.023)	(0.023)	(0.025)	(0.020)	(0.021)	(0.023)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	168	168	168	168	168	168
Adjusted R^2	0.337	0.337	0.337	0.429	0.429	0.426

Notes: Estimates obtained through IPWRA. Dependent variable is a dummy variable equal to 1 if the child is stunted, and 0 otherwise. Variables used for matching are taken at baseline and include: household characteristics and composition, idiosyncratic shocks, land, village dummies. Covariates included in the regression: age in months and gender of child, duration in months of breastfeeding, father and mother education level, age household head, household size (log), improved toilet, land size (log), asset index, livestock (expressed in tropical livestock units and log), number of rooms, distance to population centre, and its square, shocks (child illness, crop damage, increase in prices). Standard errors are clustered at village level (44 villages). Significance levels * 10% ** 5% *** 1%.

Table B.5: OLS: Long term impact of shocks and emergency aid on child height-for-age

	(1)	(2)	(3)	(4)	(5)	(6)
Drought 2011	-0.156		-0.858	0.081		-0.376
	(0.615)		(0.855)	(0.498)		(0.697)
Emergency aid 2011		-0.012	-0.248		-0.132	-0.330
		(0.349)	(0.408)		(0.323)	(0.351)
Emergency aid 2011 × Drought 2011			1.255			0.907
			(1.137)			(1.112)
HAZ 2011				0.435***	0.436***	0.433***
				(0.068)	(0.068)	(0.069)
Female	0.303	0.303	0.329	0.329	0.324	0.342
	(0.413)	(0.415)	(0.418)	(0.295)	(0.296)	(0.303)
Age in months	0.021	0.021	0.020	0.021*	0.020*	0.021
	(0.016)	(0.015)	(0.016)	(0.012)	(0.012)	(0.012)
Father's education	0.063	0.065	0.057	0.049	0.051	0.049
	(0.066)	(0.062)	(0.064)	(0.046)	(0.041)	(0.042)
Mother's education	0.086	0.085	0.101	0.103	0.100	0.109
	(0.117)	(0.114)	(0.116)	(0.115)	(0.111)	(0.110)
Land size (ln)	0.268	0.271	0.223	0.352**	0.332**	0.299**
	(0.170)	(0.166)	(0.161)	(0.143)	(0.138)	(0.132)
Asset index	0.060	0.059	0.074	0.114*	0.112*	0.121*
	(0.074)	(0.076)	(0.079)	(0.062)	(0.065)	(0.068)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	168	168	168	168	168	168
Adjusted R^2	0.249	0.249	0.248	0.488	0.489	0.486

Notes: Estimates obtained through IPWRA. Dependent variable: height-for-age in 2013. Variables used for matching are taken at baseline and include: household characteristics and composition, idiosyncratic shocks, land, village dummies. Covariates included in the regression: age in months and gender of child, duration in months of breastfeeding, father and mother education level, age household head, household size (log), improved toilet, land size (log), asset index, livestock (expressed in tropical livestock units and log), number of rooms, distance to population centre, and its square, shocks (child illness, crop damage, increase in prices). Standard errors are clustered at village level (44 villages). Significance levels * 10% ** 5% *** 1%.

Table B.6: Matching: Long term impact of shocks and emergency aid on child height-for-age. Sensitivity analysis to trimmed sample

	0.2 - 0.98			0.10 - 0.90		
	(1)	(2)	(3)	(4)	(5)	(6)
Drought 2011	-0.272		-1.739**	-0.411		-1.695**
	(0.605)		(0.788)	(0.586)		(0.805)
Emergency aid 2011		-0.069	-0.406		-0.151	-0.421
		(0.306)	(0.307)		(0.301)	(0.303)
Emergency aid 2011 × Drought 2011			2.112**			1.938*
			(1.002)			(1.076)
HAZ 2011	0.442***	0.444***	0.436***	0.439***	0.445***	0.436***
	(0.066)	(0.067)	(0.067)	(0.064)	(0.065)	(0.066)
Female	0.284	0.285	0.316	0.350	0.349	0.355
	(0.403)	(0.405)	(0.409)	(0.407)	(0.411)	(0.414)
Age in months	0.023	0.023	0.024*	0.024*	0.024	0.024*
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Father's education	0.053	0.059	0.039	0.048	0.058	0.038
	(0.051)	(0.044)	(0.046)	(0.052)	(0.046)	(0.047)
Mother's education	0.183	0.177	0.214*	0.196	0.185	0.218*
	(0.128)	(0.121)	(0.116)	(0.131)	(0.123)	(0.117)
Land size (ln)	0.485**	0.482***	0.382**	0.422**	0.412**	0.341**
	(0.188)	(0.178)	(0.155)	(0.175)	(0.163)	(0.149)
Asset index	0.152**	0.148**	0.187**	0.158***	0.150**	0.187**
	(0.061)	(0.063)	(0.070)	(0.058)	(0.060)	(0.070)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	169	169	169	162	162	162
Adjusted R^2	0.549	0.548	0.569	0.549	0.548	0.564

Notes: Estimates obtained through IPWRA. Dependent variable: height-for-age in 2013. Variables used for matching are taken at baseline and include: household characteristics and composition, idiosyncratic shocks, land, village dummies. Covariates included in the regression: age in months and gender of child, duration in months of breastfeeding, father and mother education level, age household head, household size (log), improved toilet, land size (log), asset index, livestock (expressed in tropical livestock units and log), number of rooms, distance to population centre, and its square, shocks (child illness, crop damage, increase in prices). Standard errors are clustered at village level (44 villages). Significance levels * 10% ** 5% *** 1%.

Table B.7: Correlates of drought

	(1)
Improved toilet	0.196
	(0.118)
Crop damage	-0.044
	(0.123)
Price rise	0.182*
	(0.091)
Livestock TLU (ln)	0.081**
	(0.033)
Household size (ln)	-0.021
	(0.091)
Father's education	-0.012
	(0.008)
Mother's education	0.010
	(0.010)
Head female	0.253**
	(0.095)
Head age	0.002
	(0.002)
Land size (ln)	0.002
	(0.016)
Asset index	0.007
	(0.009)
Distance to population centre	-0.039
	(0.040)
Illness	0.088*
	(0.047)
No. rooms	-0.098**
	(0.041)
Village FE	Yes
Observations	153
Adjusted R^2	0.739

Notes: Estimates obtained through linear probability model. Dependent variable equal to 1 if household reported experiencing a drought in 2011. Standard errors are clustered at village level. Significance levels * 10% ** 5% *** 1%.

Table B.8: Matching: IPW

	(1)	(2)	(3)
Drought 2011	0.047		-1.746*
	(0.561)		(0.881)
Emergency aid 2011		-0.070	-0.656
		(0.500)	(0.474)
Emergency aid 2011 × Drought 2011			3.059***
			(1.110)
Village FE	Yes	Yes	Yes
Observations	168	168	168
Adjusted R^2	0.133	0.134	0.190

Notes: Estimates obtained through IPWRA. Dependent variable: height-for-age in 2013. Variables used for matching are taken at baseline and include: household characteristics and composition, idiosyncratic shocks, land, village dummies. Standard errors are clustered at village level. Significance levels * 10% ** 5% *** 1%.

Chapter 4

Kindergarten and caregiving in the US

4.1 Introduction

Informal caregiving[1] is the principal source of care for older and disabled people in the United States (and in many other countries).[2] Indeed, informal caregiving by family and friends has in the majority of cases (about 70%) become the sole source of long-term care (Ettner, 1995), while more than 90% of disabled elderly receive at least some informal care (Spillman and Pezzin, 2000). The supply of informal care for the elderly has therefore traditionally been the subject of considerable academic and policy interest. In recent years, this interest has intensified for a number of reasons: First, the repeal of the long-term care provisions contained in the Affordable Care Act has signaled that informal caregiving will continue to be the principal source of long-term care for the foreseeable future. Second, demographic trends appear to have worrying implications for the robustness of the informal care system. Population aging, increasing racial / ethnic diversity, as well as increasing rates of chronic conditions, have the potential to increase the demand for long-term care (Yang et al., 2003; Wolf, 2001). Whether this increased demand can be adequately accommodated by informal caregivers is an important question, given

[1] This chapter has been co-authored with Amalavoyal Chari, one of my supervisors.

[2] Formal long-term care (LTC) services are expensive (the average annual cost of nursing home care was estimated to be $75,000 (Iglehart, 2010)) and less than 15% of the elderly population in the United States is covered by LTC insurance (Brown et al., 2012).

that caregivers face multiple competing demands on their time. Spillman and Pezzin (2000) particularly highlight the trilemma of the "sandwich generation" that is caught between the demands of caregiving, child-rearing and labour-force participation.

Whereas the trade-off between work and caregiving has been extensively studied (the evidence is surveyed by Lilly et al. (2007)), the extent to which child care "crowds out" elder care remains largely unknown, and we are not aware of any systematic analysis bearing on this question. We utilise time-use data from the nationally-representative American Time Use Survey to shed light on the trade-off between the two kinds of care by examining the relationship between caregiving and child age (of the youngest child) for households with young children. We find that the supply of elder care increases with child age, and peaks by the time the children are of school-going age. At this point, the amount of elder care supplied (in terms of time) is more than twice the amount supplied when the child is not yet of school age, indicating the sharp trade-off between the two kinds of care.

There is a strong indication that the relationship between caregiving and child age is mediated by access to schooling: The two largest increases in caregiving time occur between the ages of 4 and 5 (when children become eligible for kindergarten) and between the ages of 5 and 6 (when they become eligible for school). This naturally raises the question of whether the supply of elder care is elastic with respect to publicly subsidised child care/schooling. In this chapter we focus attention on the effect of kindergarten provision. A number of studies, beginning with Gelbach (1999), have examined the effect of public kindergarten on labour force participation (usually of women). To our knowledge, ours is the first study to examine the effects on the provision of elder care. Gelbach (1999) utilises variation in quarter of birth to instrument for kindergarten enrolment - because we do not observe the child's quarter of birth, we instead take advantage of inter-state variation in the provision of subsidised kindergarten. Our empirical design exploits this state-level policy variation, in combination with the age-eligibility criterion for kindergarten.

We find that the increase in informal caregiving when the youngest child in the household becomes eligible for kindergarten is largely concentrated in states that provide full-time kindergarten access. Not surprisingly, we find little effect of kindergarten eligibility of the oldest child. Disaggregating the results by gender, we find that kindergarten eligibility not only increases care supply among women, but also among men. This is a surprising

result that challenges the notion that the trade-off between child-rearing and elder care is only experienced by women.

The significance of these findings is not merely academic. In the absence of a well-functioning formal chronic care system, public policies that can support informal caregiving are critical. The academic debate surrounding this issue has emphasized the opportunity costs of caregiving in terms of lost income (i.e. the trade-off between work and caregiving), and this is also reflected in the policy debate which centres around flexible work arrangements, mandated (paid or unpaid) leave, direct compensation of caregivers, etc.[3] Relatively little attention, however, has been paid to the trade-off between child-rearing and caregiving. Our results suggest that this is an important omission, and that targeting the trade-off between child care and caregiving may be an effective complement to policies that target work arrangements. The findings are also significant in that the positive benefits of child care subsidies are typically considered to include greater workforce participation (particularly on the part of women), whereas the potential effect of such policies on the provision of elder care has gone unexamined. Our results therefore have the potential to alter the cost-benefit assessment of child care subsidies.

4.2 Data

1. Caregiving data

To examine the trade-off between caregiving and child care, we utilise time-use data from the American Time Use Survey (ATUS). The ATUS, conducted by the Bureau of Labor Statistics, records the time allocated to various activities by non-institutionalised civilians 15 years old and older in the United States. The focus is on collecting a time diary in which survey respondents are asked to report their main activities sequentially for the 24-hour period that began at 4 a.m. on the previous day and ended at 4 a.m. on the day of the interview. Interviewees are randomly selected from households participating in their final (eighth) round of the Current Population Survey (CPS) sample. The CPS

[3] Studies that examine the trade-off between work and caregiving on the part of women (who constitute the majority of caregivers (Feinberg et al., 2011)) have found mixed results (McLanahan and Monson, 1990; Spitze and Logan, 1991; Boaz and Muller, 1992; Moen et al., 1994; Pavalko and Artis, 1997; Moen et al., 1995). Studies on sub-populations of caregivers found that co-residence with the care recipient is associated with reduced participation into the labour force (Ettner, 1995). Similarly, caregivers engaged with heavy activities are less likely to be in the labour force. On the intensive margin, evidence from the US suggests that caregivers (especially females) tend to reduce their working hours and face a reduced wage (Ettner, 1995; Graves, 2010; Johnson and Sasso, 2000; Van Houtven et al., 2013).

is the nation's monthly labour survey, eliciting detailed labour market information, such as labour force participation, hours of work and earnings. At the time of the ATUS interview the CPS questions are updated. We make use of the following demographic variables pertaining to the respondent: (i) Age in years, (ii) Gender, (iii) Race, which is coded into 4 categories, namely White, Black, Hispanic, and an "Other race" category, (iv) Education, which we code into a dummy, College, and (v) Marital Status.

This study uses pooled data from the 2011, 2012, 2013 and 2014 rounds of the survey. Starting in 2011, the ATUS introduced a special module to assess the time spent by informal (i.e. unpaid) caregivers helping elderly friends and relatives. This module asks respondents if they have provided (non-financial) unpaid care to any elderly relatives/friends in the last three months. Conditional on respondents having provided care in the last three months, they are then asked how much time they spent doing so during the last 24 hours. The responses are cross-checked with the time diary to verify their accuracy.

In the ATUS elder care is defined as the "provision of care or assistance to an individual because of a medical condition related to ageing" (Denton, 2012). This definition focuses on the need for care related to a particular condition, physical or emotional, that typically affects older people, while at the same time stresses the importance of ongoing care, implying an expectation of long-term care. In the analysis, we consider two broad measures of elder caregiving. The first is an indicator for whether the individual self-identifies as a long-term caregiver (provision of care in the previous 3 months). The second measure is the amount of care (in minutes) that the caregiver provided in the 24-hour recall period (this amount is zero for those who did not provide care on the preceding day).

The elder care survey module also reports the age of the care recipient (as reported by the caregiver). In line with the definition set out in the ATUS and in the literature about elder care, we define elder care to be care provided to individuals 65 years old and older.

The time diaries elicited by the ATUS also allow us to measure the amount of time spent by the respondent with children (including children not residing in the household) in the 24-hour recall period. Because it is difficult to identify what part of this time should be properly counted as "child care", we do not make use of this information in the main analysis.

2. Child enrolment and eligibility

In addition to elder care and time use, the ATUS elicits a household roster from which

we obtain the ages of all children residing in the household. The sample used for this study includes all respondents living in households with children who are twelve years old or younger.

The use of the ATUS imposes two major constraints on the analysis. First, it does not collect the quarter of child birth nor the exact date. As a consequence it is not possible to include in the sample only those respondents whose children would have likely been eligible to enter kindergarten in the previous fall. Instead, our treatment group comprises all respondents whose youngest child is aged five, thus it includes respondents potentially treated by the provision of publicly funded schooling. The treated group includes respondents whose children would not have been eligible for kindergarten as well as respondents whose children should be enrolled in primary school. However, the enrolment of these children would not have changed with the introduction of state policy funding, therefore our results should be biased downwards (Cascio, 2009).

In addition, the ATUS does not collect information on school enrolment of children, which could be ideally used within an instrumental variable approach to estimate the first stage equation. The CPS instead does collect information on enrolment, although it does so only for one month per year (typically October). Unfortunately the ATUS and the CPS cannot be merged. We use the information on enrolment for descriptive purposes (commented in the next section (4.2.1)). However, it is worth noting that while the reported descriptive statistics on enrolment rates from the CPS refer to interviews performed in the sole month of October, the ATUS is carried out throughout the entire year.

3. State-level variation in kindergarten provision

Compulsory education in the United States on average starts at the age of six[4]. However most states offer universal, non-compulsory, free-of-charge public kindergarten. Eligibility criteria to enrol in kindergarten are mostly defined at state level, with the general rule that only children who turned five within a state-specific cut-off are eligible for enrolment. With the exception of a few states that leave it to Local Education Agencies to define the cut-off date[5], most states set the cut-off between August and September, with only three states (California, Kentucky and Maine) setting in October, one (Michigan) in

[4] 25 states at age of 6, 9 states at the age of 5, 7 states at the age of 7 and 2 states at the age of 8.

[5] Massachusetts, New Hampshire, New Jersey, New York, Ohio, Pennsylvania and Vermont. (https://nces.ed.gov/programs/statereform/tab5_3.asp, State Education Reforms, 2014)

November and two (Connecticut and Vermont) at the end/beginning of the solar year.

Kindergarten provision has spread rapidly in recent years as part of a national effort to increase the rigour of elementary school, raise tests scores and increase learning in higher grades. There remains, however, some inter-state variation in the extent of kindergarten provision. Eleven states[6] and District of Columbia require school districts to provide publicly funded full-day kindergarten,[7] thirty-four states require to provide at least half-day kindergarten and five states do not require districts to provide kindergarten at all[8]. However, in some states (i.e. Illinois and Indiana) it is foreseen that districts could offer free full-time kindergarten.[9] In our analysis, we distinguish between the states that require full-day provision and all other states. In order to cleanly focus on the effects of kindergarten eligibility, we exclude from our analysis the three states that offer publicly funded pre-kindergarten (Florida, Georgia, and Oklahoma).

4.2.1 Summary Statistics

Tables 4.1 reports the summary statistics from the ATUS for different outcomes investigated, overall and by states' policy respectively. Of the 15,026 individuals in the restricted sample[10], 1,713 (11.4%) identified themselves as caregivers to elderly individuals within the past three months. A slightly higher proportion of females (12%) than males (11%) provide elder care. Those reporting having provided care during the previous 24 hours are, as expected, much fewer (only 288 or 1.9%). The average time spent providing care amounts to 3.2 minutes, while among those that did actually provide elder care is 167 minutes (2 hours and 47 minutes). When averaged across the entire sample, it seems that females devote greater time to elder care (3.9 minutes) than males (2.3). Within the

[6] Alabama, Arkansas, Delaware, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee and West Virginia.

[7] With reference to part-time and full-time kindergarten policies, most states lack a clear definition of what half-day and full-day kindergarten constitute. The U.S. Census Bureau considers a child to attend a full-day kindergarten program if he/she usually attends both in the morning and afternoon of each day, for at least five days per week. Half-day kindergarten typically lasts two or three hours, while full-day can range from four to seven hours (Kauerz, 2005).

[8] Alaska, Idaho, New Jersey, New York and Pennsylvania. (https://nces.ed.gov/programs/statereform/tab5_3.asp, State Education Reforms, 2014)

[9] Although overall the provision of publicly funded kindergarten is increasing, with more states upgrading into full-time (i.e. Washington has been phasing full-day kindergarten since 2012/13, beginning with the highest poverty schools, with state-wide implementation to be achieved by 2017/18), it is worth mentioning that some states have cut funds and went back to part-time kindergarten as a reaction to the crises (i.e. Arizona in 2010).

[10] We keep households with at least one children younger than 13 years old and drop the 3 states that offer pre-kindergarten.

sample of care givers, females provide around 10 minutes more of care than males (171 versus 161). The provision of elder care is greater in full-time kindergarten states than in other states, both in terms of proportion and time.

With respect to respondent characteristics, 58% of the sample is white, 24% is Hispanic, 11% is black and a remaining 7% is composed by other races. In terms of education, 71% of respondents in our sample do not hold a collage degree. 64% of respondents in the sample are married, and the average respondent is 35 years old.

Table 4.1: Sample characteristics

	All states			**No full-time states**			**Full-time states**		
	All	Female	Male	All	Female	Male	All	Female	Male
Outcomes									
Caregiver (0,1)	0.11	0.12	0.11	0.11	0.11	0.10	0.14	0.15	0.12
	(0.32)	(0.33)	(0.31)	(0.31)	(0.32)	(0.30)	(0.34)	(0.35)	(0.33)
Caregiving time (min.)	3.20	3.94	2.34	2.69	3.46	1.82	5.35	5.82	4.74
	(38.20)	(41.44)	(34.07)	(33.78)	(39.21)	(26.35)	(52.91)	(49.23)	(57.37)
Working (0,1)	0.76	0.67	0.87	0.76	0.67	0.87	0.77	0.69	0.86
	(0.43)	(0.47)	(0.34)	(0.43)	(0.47)	(0.34)	(0.42)	(0.46)	(0.35)
Hours worked	25.99	19.90	33.02	26.03	19.66	33.19	25.82	20.88	32.26
	(21.64)	(19.77)	(21.57)	(21.61)	(19.63)	(21.51)	(21.74)	(20.33)	(21.85)
Controls									
Race									
White	0.58	0.56	0.59	0.57	0.56	0.59	0.59	0.58	0.61
	(0.49)	(0.50)	(0.49)	(0.49)	(0.50)	(0.49)	(0.49)	(0.49)	(0.49)
Black	0.11	0.12	0.09	0.08	0.09	0.07	0.22	0.26	0.18
	(0.31)	(0.33)	(0.29)	(0.27)	(0.28)	(0.26)	(0.42)	(0.44)	(0.38)
Hispanic	0.24	0.24	0.24	0.27	0.28	0.27	0.13	0.11	0.14
	(0.43)	(0.43)	(0.43)	(0.44)	(0.45)	(0.44)	(0.33)	(0.32)	(0.35)
Other	0.07	0.07	0.07	0.08	0.08	0.07	0.06	0.05	0.07
	(0.26)	(0.26)	(0.26)	(0.27)	(0.27)	(0.26)	(0.23)	(0.21)	(0.25)
Education									
No college	0.71	0.70	0.71	0.71	0.70	0.71	0.70	0.68	0.71
	(0.46)	(0.46)	(0.45)	(0.45)	(0.46)	(0.45)	(0.46)	(0.47)	(0.45)
College	0.29	0.30	0.29	0.29	0.30	0.29	0.30	0.32	0.29
	(0.46)	(0.46)	(0.45)	(0.45)	(0.46)	(0.45)	(0.46)	(0.47)	(0.45)
Married	0.64	0.60	0.69	0.65	0.61	0.68	0.62	0.57	0.69
	(0.48)	(0.49)	(0.46)	(0.48)	(0.49)	(0.46)	(0.49)	(0.50)	(0.46)
Age	35.48	34.81	36.26	35.43	34.72	36.23	35.70	35.15	36.41
	(11.83)	(11.53)	(12.12)	(11.78)	(11.39)	(12.16)	(12.02)	(12.06)	(11.94)

Notes: N = 15,026. Sample restricted to those respondents that have the youngest child younger than 12 years old and to states that do not offer pre-kindergarten. Weighted observations. Standard deviations reported in brackets.

Table 4.2 shows the sample averages and the t-test[11] of equal means by elder caregiving status for all, and females and males separately. Caregivers and non-caregivers differ along a number of dimensions. Caregivers are more likely to be white (73%) compared to non-caregivers (63%), and less likely to be Hispanic (12% versus 19%). Caregivers are less likely to have a college degree, but this difference is driven by the female sub-sample. No difference is found across caregivers and non-caregivers by marital status. Lastly, caregivers appear to be on average two years and a half older than non-caregivers, with a higher difference coming from the female sub-sample.

[11] For categorical variables a chi-square test is instead applied.

Table 4.2: Comparing caregivers to non-caregivers

	Non caregiver	Caregiver	Diff.
Panel A: All			
Race			
White	0.63	0.73	0.10***
Black	0.10	0.10	-0.00
Hispanic	0.19	0.12	-0.08***
Other	0.08	0.06	-0.02***
Education			
No college	0.64	0.59	-0.05***
College	0.36	0.41	0.05***
Married	0.67	0.68	0.01
Age respondent	36.95	39.60	2.66***
Panel B: Females			
Race			
White	0.61	0.71	0.10***
Black	0.12	0.12	0.01
Hispanic	0.20	0.12	-0.08***
Other	0.08	0.05	-0.03***
Education			
No college	0.63	0.58	-0.06***
College	0.37	0.42	0.06***
Married	0.60	0.62	0.02
Age respondent	36.11	39.57	3.46***
Panel C: Males			
Race			
White	0.66	0.75	0.10***
Black	0.08	0.07	-0.01
Hispanic	0.18	0.11	-0.07***
Other	0.08	0.07	-0.01
Education			
No college	0.64	0.61	-0.03
College	0.36	0.39	0.03
Married	0.73	0.72	-0.01
Age respondent	38.07	39.66	1.60***

Notes: N = 15,026. Sample restricted to those respondents that have the youngest child younger than 12 years old and to states that do not offer pre-kindergarten.

Next, we examine how school enrolment varies with child age. Since we do not have information on enrolment in the ATUS, we use information from the October CPS School Enrolment Supplements. Table 4.3 reports enrolment rates for each school level by age, with pre-kindergarten and kindergarten further disaggregated into part-time or full-time attendance. At age 4, 67% of children are enrolled, mostly (60%) in pre-kindergarten, with 7% already enrolled in kindergarten. At age 5, instead, almost all children (92%) are enrolled in some level of schooling, mostly (72%) in kindergarten. Disaggregating by part-time and full-time attendance, at age 4 children are equally split between part-time and full-time, while at age 5 the majority (56%) attends full-time kindergarten and only 16% part-time.

Table 4.3 further disaggregates by type of state kindergarten policy, respectively for

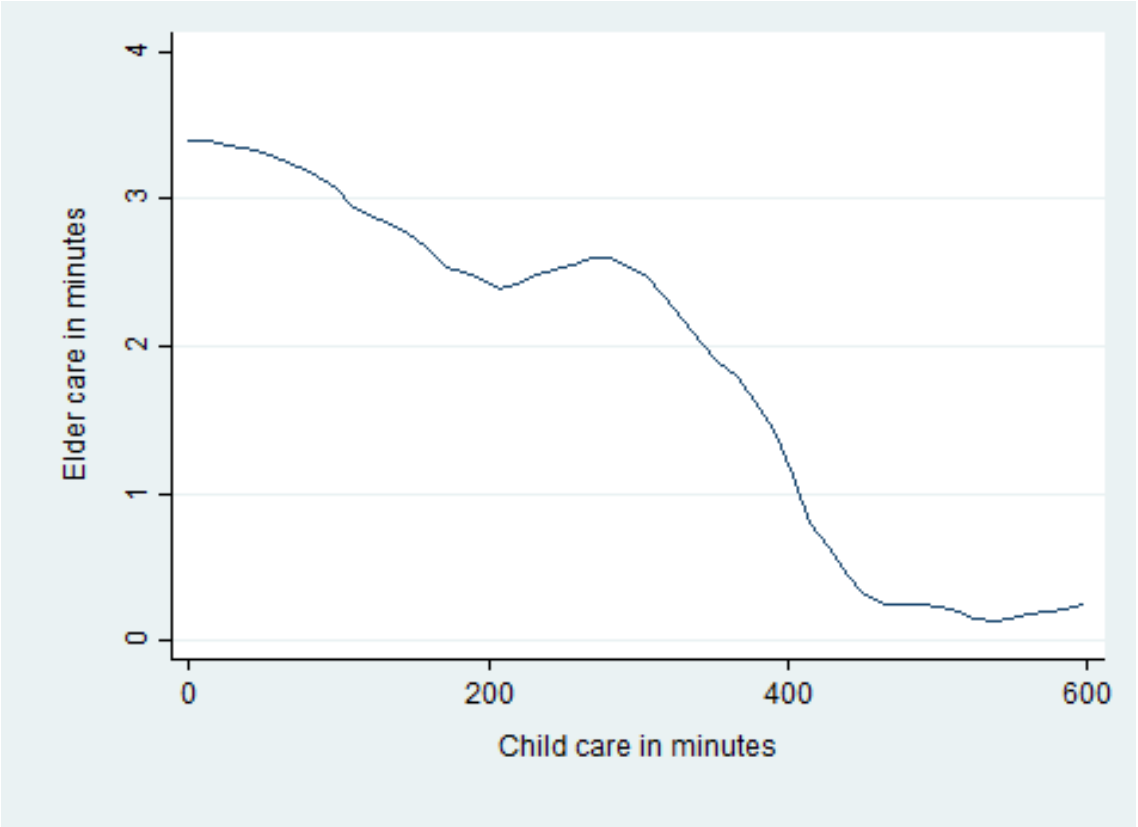
children age 4, 5 and 6. For children aged 4 there is no difference in the proportion of those that attend full-time kindergarten by states' policy, both for public and private schools. For aged 5, instead, among children enrolled in public schools, 76% are enrolled in full-time kindergarten in states that offer full-time kindergarten versus a lower 61% in states that do not offer full-time kindergarten. The opposite is true for part-time enrolment. Only a minority of children (9%) attend part-time kindergarten in states that fund full-time kindergarten, while in other states the proportion is higher (21%). The differences among children attending private school are less marked but still present (52% in full-time states attend full-time kindergarten while 41% attend full-time kindergarten in the other states). Comparing the proportion of children attending full-time kindergarten in full-time states, we see that the majority attend full-time kindergarten, both among children enrolled in public schools (76%) and in private schools (52%). When looking at these figures though, it is important to remember that at the age of 5 only 15% of children are enrolled in private schools. Among children aged 6, overall a slightly higher proportion attend primary school in states non-funding full-time kindergarten, which might be explained by the fact that households in those states tend to send their children to primary school as soon as they are entitled in order to access full-time education.

Table 4.3: School enrolment by state kindergarten policy and age

	Nursery Full-time	Nursery Part-time	Kindergarten Full-time	Kindergarten Part-time	Primary
Age 4					
All states	0.28	0.31	0.05	0.02	0.00
No Full-time states	0.26	0.33	0.04	0.02	0.00
Full-time states	0.38	0.24	0.05	0.02	0.00
Age 5					
All states	0.06	0.09	0.56	0.16	0.05
No Full-time states	0.06	0.09	0.53	0.18	0.06
Full-time states	0.07	0.06	0.66	0.08	0.05
Age 6					
All states	0.01	0.00	0.15	0.04	0.77
No Full-time states	0.00	0.00	0.14	0.05	0.78
Full-time states	0.01	0.01	0.18	0.03	0.74

Notes: Source: Current Population Survey (CPS).

Figure 4.1: Amount of elder and child care provision



Notes: The figure graphs a local polynomial regression line fitted to the relationship between the amount of elder care (in minutes) and the amount of time spent with children (in minutes) during the 24-hour recall period.

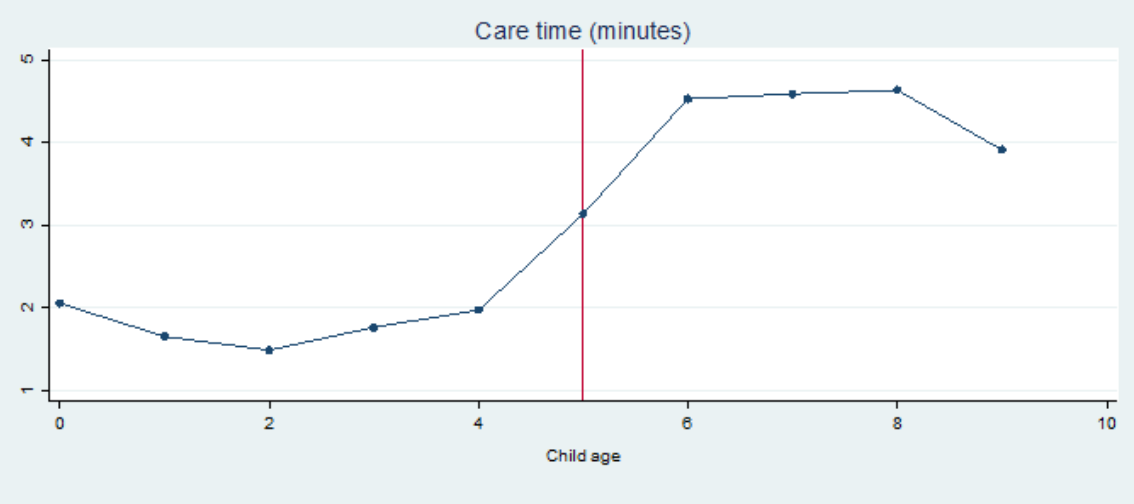
4.3 Analysis

Our aim is to study how caregivers trade off child care versus elder care. We begin by examining the association between time spent on the two activities, restricting the sample to only include households that have a child under the age of 13. Figure 4.1 graphs a smooth local polynomial regression line, with elder care time on the y-axis and child care time on the x-axis. The scale of the y-axis is noticeably different from that of the x-axis, reflecting the fact that only a subset of the households with children are "at-risk" of being elder caregivers. In general, we find a negative association between the two types of care, which is not surprising. The overall elasticity of elder care with respect to child care is approximately -0.2.

The association shown in Figure 4.1 is admittedly descriptive: Because time allocated to these two activities are jointly determined, the association between them is characterised by the classic simultaneity issue in econometrics. Ideally, one would like to observe the effect of an exogenous change in child care time, but such an experiment is difficult to

obtain within the confines of the data. However, a natural approach is to examine how the provision of elder care changes with child age, as one would expect that (a) older children need less intensive care, and (b) the burden of child care will likely diminish when children reach school-going age.

Figure 4.2: Amount of elder and child care provision



Notes: The figure graphs a local polynomial regression line fitted to the relationship between the amount of elder care (in minutes) and the age of youngest child.

Figure 4.2 plots the average amount of elder care provided at each level of child age, where the latter refers to the age of the youngest child in the household. The supply of elder care does not change significantly up until the child reaches the age of 4, after which there is a sharp increase between the ages of 4 and 6, and a levelling out thereafter. This accords well with our intuition, since wide-spread access to full-time child care in the United States begins with kindergarten (at age 5), and by age 6, most children are in school. The amount of elder care provided more than doubles in going from age 4 to age 6, underlining both the extent to which child care crowds-out elder care, as well as the importance of kindergarten/school access in alleviating the time constraint of the caregiver.

Next we turn to an analysis of whether the increase in the supply of elder care between the ages of 4 and 5 is related to the between-state variation in kindergarten provision. This analysis is interesting in its own right, in terms of assessing the effect of the child care subsidy embodied in kindergarten provision on the supply of elder care, but is also a useful check on our intuition that the increase in elder care supply at this age is indeed due to a change in child care burden as a result of kindergarten eligibility rather than due to a change in other factors.

Because of the difficulties in measuring child care burden accurately, we adopt a reduced-form methodology that examines the impact of kindergarten eligibility on the supply of elder care, rather than attempting an instrumental variable analysis that uses kindergarten eligibility as a source of exogenous variation in time devoted to child care. The reduced-form estimation is also preferred because of the possibility that public school eligibility may affect elder care in other ways (i.e. not only by easing the child care constraint). For instance, the implicit child care subsidy inherent in public school may also increase the disposable income of some caregivers, and this income may be transferred to the elderly relative, in lieu of care time. Alternatively, the increased income could be used to purchase paid substitutes for non-care activities such as cooking and cleaning, which would tend to increase the overall time allocated to care-related activities.

The estimates from the reduced-form have an Intent To Treat (ITT) interpretation in two distinct senses: First, actual enrolment in kindergarten may not be full (i.e. compliance with eligibility is not perfect), and second, not all households are "at-risk" of supplying elder care.

Our empirical strategy is to estimate the change in the supply of elder care at the point of kindergarten eligibility (i.e. age 5). We estimate this change by fitting separate age trends on either side of the eligibility threshold and measuring the discontinuous change at the point of eligibility. This is thus a form of regression discontinuity design. The basic empirical specification is as follows:

$$y_i = \pi_0 + \pi_1 Eligible_i + \pi_2 Age_i + \pi_3 Age_i * Eligible_i + \epsilon_i \tag{4.1}$$

where i indexes the individual respondent, y denotes a measure of elder care provision, *Eligible* is a dummy variable that takes value 1 if the youngest child in the household is above the age of 5. *Age* is the age of the youngest child recoded so that it takes the value 0 if the child is 5 years of age, and ϵ_i is an unobserved error term. In this specification, the coefficient of interest, π_1, captures the discontinuous change in the outcome at the point of school eligibility, π_2 captures the pre-eligibility trend in y, and π_3 (the coefficient on the interaction between Age and Eligible) captures the change in the trend after the youngest child becomes eligible. We also examine the robustness of the results by refining the specification to allow for higher order pre- and post-eligibility trends, and by including

socio-economic controls and state-year fixed effects. As our assignment variable is discrete, we follow the suggestion in Lee and Card (2008) to correct for the group structure in the standard errors by clustering them. All estimates are clustered at state-year level.

We then further expand the basic specification in order to examine how the effect of eligibility varies with the level of kindergarten provision in the respondent's state. The empirical specification is now given by:

$$y_i = \beta_0 + \beta_1 Eligible_i + \beta_2 Age_i + \beta_3 Age_i * Eligible_i + \beta_4 KF_i + \\ \beta_5 KF_i * Eligible_i + \beta_6 KF_i * Age_i + \beta_7 KF_i * Age_i * Eligible_i + \epsilon_i \tag{4.2}$$

where the coefficient of interest β_5 captures the difference in the discontinuous change in the outcome at the point of school eligibility in states that offer full-time kindergarten compared to states that do not, while β_1 captures the discontinuous change in states that do not offer full-time kindergarten. The net effect for the states that do offer full-time kindergarten is therefore given by the sum of β_1 plus β_5. β_2 represents the pre-eligibility trend in y among non full-kindergarten states, β_3 the change in trend after the point of discontinuity in non -full-time kindergarten states. β_6 represents the pre-eligibility trend in y in full-time kindergarten states, while β_7 the change in the trend after the point of eligibility in full-time kindergarten states.

Table 4.4 presents the regression estimates: In Columns 1-3, the regression in (4.1) is estimated for the entire sample and then separately for men and women; in Columns 4-6, we present the corresponding results from the estimation of (4.2). We also report the net effect of being eligible in a state that offers full-time kindergarten ($\beta_1 + \beta_5$ calculated as a linear combination of the two coefficients) in addition to the individual coefficients. We present results for a number of measures of caregiving: (i) A binary indicator for whether the respondent identifies as having provided care in the last three months (Panel A), (ii) An indicator for whether the respondent provided care to more than one individual (Panel B), (iii) An indicator for whether the respondent provided care on the day prior to the interview (Panel C), and (iv) The amount of care (in minutes) provided on the day prior to the interview Panel D).

The results are consistent across the various measures of elder care provision (although not always statistically significant). Looking at the overall effect of eligibility (Columns 1-

Table 4.4: Kindergarten eligibility (youngest child) and caregiving

		All (1)	Female (2)	Male (3)	All (4)	Female (5)	Male (6)
Panel A: Respondent provided eldercare past 3 months (0,1)							
Eligible	β_1	0.027**	0.014	0.046**	0.012	-0.008	0.038*
		(0.012)	(0.018)	(0.019)	(0.013)	(0.020)	(0.022)
Kindergarten FT × Eligible	β_5				0.076**	0.100**	0.043
					(0.031)	(0.041)	(0.045)
$\beta_1 + \beta_5$					0.088***	0.092**	0.081**
					(0.028)	(0.035)	(0.040)
Adjusted R^2		0.019	0.026	0.013	0.020	0.027	0.013
Panel B: Respondent provides care to more than 1 elder (0,1)							
Eligible	β_1	0.011	-0.001	0.030***	0.002	-0.009	0.019
		(0.008)	(0.010)	(0.011)	(0.009)	(0.012)	(0.013)
Kindergarten FT × Eligible	β_5				0.044***	0.037*	0.062**
					(0.015)	(0.022)	(0.025)
$\beta_1 + \beta_5$					0.047***	0.028	0.080***
					(0.012)	(0.018)	(0.022)
Adjusted R^2		0.008	0.012	0.005	0.008	0.012	0.005
Panel C: Respondent provided eldercare yesterday (0,1)							
Eligible	β_1	0.006	-0.003	0.018**	0.002	-0.012	0.021**
		(0.006)	(0.007)	(0.008)	(0.006)	(0.008)	(0.010)
Kindergarten FT × Eligible	β_5				0.016	0.039**	-0.014
					(0.014)	(0.019)	(0.016)
$\beta_1 + \beta_5$					0.018	0.027	0.006
					(0.013)	(0.017)	(0.012)
Adjusted R^2		0.007	0.009	0.003	0.007	0.009	0.004
Panel D: Minutes of eldercare provided yesterday							
Eligible	β_1	2.998*	2.461	3.707**	2.240	1.344	3.291*
		(1.546)	(2.366)	(1.616)	(1.762)	(2.755)	(1.822)
Kindergarten FT × Eligible	β_5				3.704	4.977	2.273
					(3.714)	(5.324)	(3.859)
$\beta_1 + \beta_5$					5.944*	6.320	5.564
					(3.261)	(4.565)	(3.407)
Adjusted R^2		0.005	0.005	0.001	0.005	0.005	0.001
Controls		Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE		Yes	Yes	Yes	Yes	Yes	Yes
Observations		13880	7958	5922	13880	7958	5922

Notes: Panels A, B and C estimated through Linear Probability Model; Panel D via OLS. Eligible is a dummy indicating if the youngest child is older than 5 years. Kindergarten FT is a dummy indicating if the state provides publicly funded full-time kindergarten. Sample excludes states that provide pre-kindergarten (Florida, Georgia and Oklahoma) and children aged 5. Linear functional form with age trends. Control variables are: dummy variables for race, age and gender of respondent. Standard errors are clustered at state-year level. Significance levels * 10% ** 5% *** 1%.

3), having the youngest child eligible for kindergarten increases the probability of providing elder care by 2.7 percentage points, which corresponds to a 24.5 percent increase over the baseline provision of elder care (the overall mean elder care provision is 11%); we also find a significant increase in care supply at the intensive margin - kindergarten eligibility increases the amount dedicated to elder care by 3 minutes, which corresponds to a 94 percent increase from the baseline (3.2).

These aggregate effects appear to be driven mainly by responses among men, rather than women. Among males, having an eligible child increases the probability of providing care in the last 3 months by 4.6 percentage points (corresponding to a 41.8 percent increase in the baseline value), while for females the coefficient is not statistically significant and is lower (0.014); men are 3 percentage points more likely to take care of more than one elder, whereas the effect for women is negative and statistically insignificant; men are 1.8 percentage points more likely to have provided care in the 24-hour recall period and provide 3.7 extra minutes of elder care, a considerable increase from the baseline value of 2.3 minutes (corresponding to a 158% increase).

Turning to the effect of differential kindergarten access (Columns 4-6), we find that the increases in elder care at the extensive margin are largely concentrated among full-time kindergarten states. There is an 8.8 percentage point increase in the likelihood of being a caregiver in states that offer full-time kindergarten, which corresponds to a 62.9 percentage increase in the baseline (14%). The effect is highly significant for the full sample and also separately for females and males, with the impact being 9.2 percentage points for females and 8.1 for males. By contrast, in states that do not offer full-time kindergarten the effect of having the youngest child eligible for public school vanishes. A statistically significant effect is found only among the males sub-sample, although smaller and significant only at 10%. The impact of eligibility on the probability of taking care of more than one elder is also higher in states offering full-time kindergarten, where the effect mostly comes from males, who are 8 percentage points more likely to take care of more than one elder.

In terms of care provided during the recall period, the results are less precise and not consistent. Having a child eligible in full-time kindergarten states increases the amount of elder care provided by 5.9 minutes, an increase of 111 percent from baseline (5.35), which is marginally significant at the 10% level. The increase in care time for the female sub-sample is quite substantial although the estimates are imprecise.

Table C.1 in the Appendix reports the coefficients of all regressors included in the model. The effect of the covariates are as expected. Males are on average less likely to provide elder care (by 2.2 percentage points) and the probability of being an elder care provider increases with the age of the respondent, as the age of their parents (or people they are close to) is likely to be higher too. Among the dummy variables that control for race, all categories (black, Hispanic and others) are less likely to provide elder care

compared to white (the baseline category).

Robustness

A set of robustness checks and sensitivity analyses are performed to ensure that our main results hold throughout different changes in sample used or covariates included. We first repeat the estimations on a trimmed sample by dropping the outermost points, namely the respondents whose youngest child is aged 0 or 12, which account for 17% of the entire sample. As shown in the top panel of Table C.2 in the Appendix, the results are robust to this sample restriction, and the coefficients are only slightly smaller than those obtained on the entire sample. This result provide further support of the functional form and the reliability of the results.

We also estimate our main models by including a set of additional covariates (Panel B). We include controls for the month and day of the week the interview was carried out. We also include other covariates that are being controlled for in similar contexts that might capture differences in respondents which, in turn, may have an effect on time use, including level of education, marital status, and household composition (number of household members younger than 18 years and number of older then 18). The results are robust to the inclusion of these controls.

Panel C reports the estimation of the models with quadratic trends. The estimates obtained are similar to those obtained under linear trends, although the standard errors are higher and some coefficients lose statistical significance.

In Table C.3, we examine the effect of kindergarten eligibility of the oldest child. The coefficients of interest are smaller and not statistically significant. This does not comes as a surprise since respondents with younger children are most likely still dedicating a considerable amount of time to the care of younger children.

Heterogeneous effects

We further provide results for sub-groups of the population. In particular we investigate whether there are any heterogeneous effects by marital status and race of the respondent. Columns 1 and 2 of Table 4.5 show the results by marital status. The impact is positive and significant only among states that offer full-time kindergarten, with the effect being larger for non-married respondents than for married (9.6 and 8.2 percentage points respectively). Within the non-married, it is the women that benefit the most (9.6

percentage points) while for men the impact is not statistically significant[12]. Among the married sub-group, the impact is similar for women and men (8.9 and 8.5 percentage points, respectively, significant only at 10%).

Table 4.5: Kindergarten eligibility (youngest child) and heterogeneous effects

		Marital status		*Race*	
		Not Married	Married	Not White	White
		(1)	(2)	(3)	(4)
Eligible	β_1	0.037	-0.002	0.004	0.017
		(0.030)	(0.017)	(0.023)	(0.018)
Kindergarten FT × Eligible	β_5	0.059	0.083**	0.050	0.089**
		(0.048)	(0.042)	(0.054)	(0.040)
$\beta_1 + \beta_5$		0.096**	0.082**	0.054	0.106***
		(0.037)	(0.039)	(0.048)	(0.036)
Adjusted R^2		0.013	0.026	0.012	0.019
Controls		Yes	Yes	Yes	Yes
State-year FE		Yes	Yes	Yes	Yes
Observations		4575	9305	4970	8910

Notes: All models estimated through Linear Probability Model. Dependent variable equals to 1 if respondent provided elder care in the previous 3 months. Eligible is a dummy indicating if the youngest child is older than 5 years. Kindergarten FT is a dummy indicating if the state provides publicly funded full-time kindergarten. Sample excludes states that provide pre-kindergarten (Florida, Georgia and Oklahoma) and children aged 5. Linear functional form with age trends. Control variables are: dummy variables for race, age and gender of respondent. Standard errors are clustered at state-year level. Significance levels * 10% ** 5% *** 1%.

Disaggregating the analysis by race (Columns 3 and 4), we find that eligibility has a statistically significant impact for white people and in full-time states only. Having the youngest child eligible for public school increases the probability of providing elder care among white people by 10.6 percentage points. For other races there is no significant effect on the provision of elder care.

Who receives the extra care?

The increase in the extensive margin of care supply as a result of kindergarten eligibility suggests that some elders who were not previously receiving care may now be receiving care. It is not possible to directly identify these individuals, but we can draw some inferences based on changes in the average characteristics of care recipients. In Table 4.6 we examine the effect of kindergarten eligibility on age of care-recipient (panel A), at whether he/she is a household member (Panel B), and whether the he/she is the parent of the caregiver (Panel C). Among elderly receiving care, eligibility is not associated with age or with the probability of being a household member. Elder care recipients are, instead,

[12]The sample size for the male non-married sub-group shrinks to 1,544 while the female one is 3,031. This could contribute to the lack of a statistical findings for men, though not necessarily. Results of these regressions are not reported.

more likely to be parents of female caregivers by 15 percentage points in all states. When we disaggregate by state policy, we find that caregivers are 18.8 percentage points more likely to provide care to parents in full-time states.

Table 4.6: Kindergarten eligibility (youngest child) and elder care recipients

		All (1)	Female (2)	Male (3)	All (4)	Female (5)	Male (6)
Panel A: Age of eldercare recipient							
Eligible	β_1	-0.413	-0.758	-0.147	-0.519	-0.954	0.010
		(0.797)	(1.048)	(1.140)	(0.914)	(1.206)	(1.276)
Kindergarten FT × Eligible	β_5				0.615	1.138	-1.390
					(1.813)	(2.262)	(2.964)
$\beta_1 + \beta_5$					0.096	0.183	-1.380
					(1.565)	(1.921)	(2.666)
Adjusted R^2		0.030	0.028	0.007	0.029	0.028	0.006
Panel B: Eldercare recipient is a household member (0,1)							
Eligible	β_1	-0.031	-0.067	0.028	-0.038	-0.086	0.038
		(0.035)	(0.046)	(0.042)	(0.041)	(0.057)	(0.046)
Kindergarten FT × Eligible	β_5				0.044	0.091	-0.038
					(0.066)	(0.086)	(0.090)
$\beta_1 + \beta_5$					0.006	0.005	0.000
					(0.052)	(0.063)	(0.079)
Adjusted R^2		0.042	0.027	0.070	0.042	0.025	0.076
Panel C: Respondent provides care to his/her parent (0,1)							
Eligible	β_1	0.077	0.151**	-0.005	0.046	0.143*	-0.063
		(0.049)	(0.062)	(0.080)	(0.056)	(0.073)	(0.087)
Kindergarten FT × Eligible	β_5				0.141	0.009	0.338*
					(0.111)	(0.137)	(0.187)
$\beta_1 + \beta_5$					0.188**	0.153	0.275
					(0.095)	(0.115)	(0.167)
Adjusted R^2		0.129	0.131	0.137	0.129	0.130	0.139
Controls		Yes	Yes	Yes	Yes	Yes	Yes
State-year FE		Yes	Yes	Yes	Yes	Yes	Yes
Observations		1729	1049	675	1729	1049	675

Notes: Panel A estimated through OLS; B-D via Linear Probability Model. Eligible is a dummy indicating if the youngest child is older than 5 years. Kindergarten FT is a dummy indicating if the state provides publicly funded full-time kindergarten. Sample excludes states that provide pre-kindergarten (Florida, Georgia and Oklahoma) and children aged 5, and is restricted to respondents that are elder care providers (in cases care is provided to more than one elder, the variables are averaged at the carer level). Linear functional form with age trends. Control variables are: dummy variables for race, age and gender of respondent. Standard errors are clustered at state-year level. Significance levels * 10% ** 5% *** 1%.

Compositional effects

One concern that arises in our context is that the conditioning on the youngest child in the household potentially induces compositional effects, since household fertility decisions may depend on the age of the current children. Thus, the sample of households whose youngest child is 4 years old (say) may be systematically different from the sample of households whose youngest child is 5 years old. To examine whether there are any

discontinuous changes in composition at the point of eligibility, we estimate the basic specification using observable characteristics as outcome variables. Table 4.7 reports the results for each of the following characteristics: White, respondent gender, and number of children in the household under the age of 18 (this variable will pick up fertility changes).

Overall, we find little evidence of significant discontinuities in any of the observable characteristics, indicating that the estimated effects of eligibility on the supply of care are not an artefact of compositional changes.

Table 4.7: Kindergarten eligibility (youngest child) and respondents' characteristics

	White (1)	Male (2)	No. children < 18 (3)
Eligible	0.016	0.026	0.082
	(0.017)	(0.025)	(0.052)
Kindergarten FT × Eligible	0.073	0.029	0.008
	(0.045)	(0.045)	(0.106)
$\beta_1 + \beta_5$	0.089	0.055	0.090
	(0.042)	(0.038)	(0.092)
Adjusted R^2	0.134	0.002	0.029
State-year FE	Yes	Yes	Yes
Observations	13879	13879	13879

Notes: All models estimated through Linear Probability Model. Eligible is a dummy indicating if the youngest child is older than 5 years. Kindergarten FT is a dummy indicating if the state provides publicly funded full-time kindergarten. Sample excludes states that provide pre-kindergarten (Florida, Georgia and Oklahoma) and children aged 5. Linear functional form with age trends. Standard errors are clustered at state-year level. Significance levels * 10% ** 5% *** 1%.

Labour supply

Lastly, we present the results for the effect of eligibility on labour supply. Although other studies estimate a different parameter from ours (ATE versus ITT), the estimates are in line with those found in the literature. Gelbach (2002), for instance, finds an impact of 4 percentage points for single mothers and 4.8 for married mothers. Overall we find kindergarten eligibility increases the probability of entering the labour force by 4 percentage points for women in all states[13].

Table 4.8 shows the results for participation into labour force and for hours worked. The effect of eligibility on labour force participation (Columns 1 - 3) strongly differs by gender. Males living in states that offer full-time kindergarten that have an eligible child are 8.7 percentage points more likely to participate in the labour force, which corresponds to a substantial 10 percent increase relative to the baseline (0.86). Females, on the other

[13]Estimates for all states not reported.

Table 4.8: Kindergarten eligibility (youngest child) and labour supply

		Worked previous week (0,1)			*Hours worked previous week*		
		All	Female	Male	All	Female	Male
		(1)	(2)	(3)	(4)	(5)	(6)
Eligible	β_1	0.027	0.053**	-0.014	0.586	1.084	-0.233
		(0.020)	(0.027)	(0.023)	(0.916)	(1.071)	(1.437)
Kindergarten FT × Eligible	β_5	0.008	-0.055	0.101**	3.902*	-0.337	10.409***
		(0.041)	(0.058)	(0.050)	(2.184)	(2.475)	(3.354)
$\beta 1 + \beta 5$		0.035	-0.002	0.087**	4.488**	0.747	10.176***
		(0.036)	(0.052)	(0.044)	(1.989)	(2.232)	(3.022)
Adjusted R^2		0.065	0.031	0.038	0.127	0.030	0.067
Controls		Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE		Yes	Yes	Yes	Yes	Yes	Yes
Observations		13880	7958	5922	13880	7958	5922

Notes: Columns 1-3 estimated through Linear Probability Model; Columns 4-6 via OLS. Eligible is a dummy indicating if the youngest child is older than 5 years. Kindergarten FT is a dummy indicating if the state provides publicly funded full-time kindergarten. Sample excludes states that provide pre-kindergarten (Florida, Georgia and Oklahoma) and children aged 5. Linear functional form with age trends. Control variables are: dummy variables for race, age and gender of respondent. Standard errors are clustered at state-year level. Significance levels * 10% ** 5% *** 1%.

hand, are not more likely to participate into the labour force in full-time kindergarten states, but only in states that do not offer full-time kindergarten. The increase in probability is around 5.3 percentage points (an equivalent of 8 percent increase from the baseline - 0.67). The differential effect of eligibility for men and women is even more evident from estimates on the hours worked (Columns 4 - 6 of Table 4.8). Men work an extra 10 hours on average as a result of kindergarten eligibility, and this effect is again concentrated in full-time kindergarten states.

These results are interesting when viewed in conjunction with those on elder care provision. Overall it appears that women devote the extra time from full-time kindergarten to the provision of elder care, whereas men increase both their labour supply as their supply of care.

4.4 Conclusions

Understanding how to facilitate and/or increase the supply of informal care has become imperative, with a number of policy options being under active consideration, including flexible work arrangements, mandated (paid or unpaid) leave, and monetary compensation for informal caregivers. Common to these policies is the assumption that the essential trade-off confronted by potential caregivers is that between employment and caregiving; a trade-off that has been extensively studied in the empirical literature (the evidence is

surveyed in Lilly et al. (2007)).

Relatively little attention, however, has been paid to the fact that individuals and households engage in yet another competing activity that is highly time-intensive: child care. An exception is Spillman and Pezzin (2000), who have drawn attention to the trilemma of the "sandwich generation" that is caught between the demands of caregiving, child-rearing and labour-force participation, noting in particular that the trend towards delayed fertility implies that potential caregivers (especially women) are increasingly likely to face simultaneous child care and elder care responsibilities. From a policy perspective, it is important to determine the extent to which individuals are trading off child care and informal care, and to assess whether child care subsidies, especially in the form of public pre-K and kindergarten access, will have traction in terms of inducing a greater supply of caregiving. The answer to the latter question, in particular, is a priori uncertain. In theory, a child care subsidy could result in an increase in work as well as caregiving. In practice, though, both activities require large time commitments and an individual may end up choosing one or the other, unless the subsidy is very large. Quasi-experimental evidence indicates that child care subsidies in the form of access to public pre-school have a sizeable impact on female labour force participation (Gelbach, 2002), but whether this population average effect implies a low elasticity of caregiving is difficult to infer because the sub-population of individuals "at-risk" of being caregivers is not easily identified.

The contribution of this paper is to present the first estimates of the elasticity of informal care with respect to child care subsidies. We focus attention on the effect of access to public preschool (kindergarten). Our empirical design exploits the age-eligibility criterion for public kindergarten, in combination with state-level variation in the provision of full-time kindergarten. We find that kindergarten eligibility of the youngest child in states that offer full-time kindergarten increases the probability of providing elder care by around 9 percentage points, which correspond to 63 percent increase to the baseline. Kindergarten eligibility also increases care provision at the intensive margin, but we do not obtain well-estimated effects on the differential effect in full-time kindergarten states.

Exploring heterogeneous effects, it appears that not married and white respondents benefit the most from the full-time policy as they are more likely to provide elder care. Our results are robust to a number of validation and sensitivity tests. Finally, no effect is found when the oldest, as opposed to the youngest, child becomes eligible for kindergarten.

Appendix C

Tables

Table C.1: Kindergarten eligibility (youngest child) and caregiving

	All (1)	Female (2)	Male (3)	All (4)	Female (5)	Male (6)
Age child	0.008***	0.010***	0.004	0.006**	0.007**	0.004
	(0.003)	(0.003)	(0.005)	(0.003)	(0.003)	(0.005)
Eligible	0.014	-0.004	0.038*	0.012	-0.008	0.038*
	(0.014)	(0.020)	(0.021)	(0.013)	(0.020)	(0.022)
Kindergarten FT	-0.029	-0.020	-0.049			
	(0.021)	(0.027)	(0.036)			
Eligible × Age child	-0.005	-0.004	-0.006	-0.005	-0.003	-0.007
	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)	(0.006)
Kindergarten FT × Eligible	0.078**	0.099**	0.049	0.076**	0.100**	0.043
	(0.032)	(0.042)	(0.046)	(0.031)	(0.041)	(0.045)
Kindergarten FT × Age child	-0.012*	-0.009	-0.017	-0.012*	-0.009	-0.018*
	(0.006)	(0.007)	(0.011)	(0.006)	(0.007)	(0.010)
Kindergarten FT × Eligible × Age child	0.009	0.003	0.020	0.010	0.002	0.022
	(0.007)	(0.008)	(0.014)	(0.007)	(0.008)	(0.014)
Other races (d)				-0.041***	-0.052***	-0.024*
				(0.009)	(0.013)	(0.014)
Black (d)				-0.018*	-0.008	-0.039***
				(0.010)	(0.013)	(0.015)
Hispanic (d)				-0.047***	-0.055***	-0.038***
				(0.007)	(0.010)	(0.011)
Age respondent				0.002***	0.003***	0.001***
				(0.000)	(0.000)	(0.000)
Male (d)				-0.022***		
				(0.005)		
Constant	0.121***	0.132***	0.105***			
	(0.009)	(0.011)	(0.017)			
Net effect	0.091***	0.094**	0.087**	0.088***	0.092**	0.081**
	(0.029)	(0.037)	(0.040)	(0.028)	(0.035)	(0.040)
State-year FE	No	No	No	Yes	Yes	Yes
Adjusted R^2	0.008	0.011	0.004	0.020	0.027	0.013
Observations	13880	7958	5922	13880	7958	5922
AIC	8533.046	5267.655	3235.734	8317.775	5086.937	3135.529

Notes: Model estimated through Linear Probability Model. Dependent variable equals to 1 if respondent provided elder care in the previous 3 months. Eligible is a dummy indicating if the youngest child is older than 5 years. Sample excludes states that provide pre-kindergarten (Florida, Georgia and Oklahoma) and children aged 5. Linear functional form with age trends. Control variables are: dummy variables for race, age and gender of respondent. Standard errors are clustered at state-year level. Significance levels * 10% ** 5% *** 1%.

Table C.2: Robustness checks. Linear functional form

		All (1)	Female (2)	Male (3)	All (4)	Female (5)	Male (6)
Panel A: Trimmed sample (excluding children 0 and 13 years old)							
Eligible	β_1	0.025*	0.014	0.044**	0.013	-0.004	0.038
		(0.014)	(0.019)	(0.022)	(0.016)	(0.022)	(0.024)
Kindergarten FT × Eligible	β_5				0.060*	0.077**	0.035
					(0.031)	(0.039)	(0.052)
$\beta_1 + \beta_5$					0.073***	0.073**	0.073
					(0.026)	(0.032)	(0.045)
Adjusted R^2		0.018	0.024	0.014	0.019	0.025	0.013
Observations		11457	6582	4875	11457	6582	4875
Panel B: Additional covariates							
Eligible	β_1	0.027**	0.014	0.045**	0.012	-0.008	0.037*
		(0.012)	(0.018)	(0.019)	(0.013)	(0.019)	(0.022)
Kindergarten FT × Eligible	β_5				0.076**	0.100**	0.045
					(0.031)	(0.041)	(0.045)
$\beta_1 + \beta_5$					0.087***	0.092**	0.081**
					(0.028)	(0.036)	(0.039)
Adjusted R^2		0.021	0.028	0.014	0.022	0.029	0.014
Observations		13880	7958	5922	13880	7958	5922
Panel C: Quadratic functional form							
Eligible	β_1	0.037	0.050	0.023	0.020	0.031	0.009
		(0.026)	(0.033)	(0.038)	(0.029)	(0.037)	(0.042)
Kindergarten FT × Eligible	β_5				0.082	0.078	0.078
					(0.054)	(0.080)	(0.095)
$\beta_1 + \beta_5$					0.101**	0.109	0.088
					(0.046)	(0.071)	(0.085)
Adjusted R^2		0.019	0.026	0.013	0.020	0.028	0.012
Observations		13880	7958	5922	13880	7958	5922
AIC		8322.680	5092.494	3135.278	8318.713	5084.280	3141.966
Controls		Yes	Yes	Yes	Yes	Yes	Yes
State-year FE		Yes	Yes	Yes	Yes	Yes	Yes

Notes: Models estimated through Linear Probability Model. Eligible is a dummy indicating if the youngest child is older than 5 years. Kindergarten FT is a dummy indicating if the state provides publicly funded full-time kindergarten. Sample excludes states that provide pre-kindergarten (Florida, Georgia and Oklahoma) and children aged 5. Control variables are: dummy variables for race, respondent's age and gender. Additional covariates (Panel B): no. of households members younger than 18, no. of members older than 19 years, a dummy variable for whether the respondent went to college or more, a dummy variable for respondent marital status (married), squared respondent's age, dummy variables for month and day of interview. Significance levels * 10% ** 5% *** 1%. Standard errors are clustered at state-year level.

Table C.3: Kindergarten eligibility (oldest child) and caregiving

		All	Female	Male	All	Female	Male
		(1)	(2)	(3)	(4)	(5)	(6)
Eligible	β_1	-0.010	0.000	-0.026	-0.016	-0.010	-0.029
		(0.024)	(0.028)	(0.036)	(0.025)	(0.031)	(0.037)
Kindergarten FT × Eligible	β_5				0.038	0.065	0.032
					(0.066)	(0.067)	(0.106)
$\beta_1 + \beta_5$					0.022	0.055	0.002
					(0.062)	(0.060)	(0.099)
Adjusted R^2		0.021	0.024	0.019	0.021	0.024	0.020
AIC		3301.806	1990.978	1251.146	3303.639	1994.911	1250.140
State-year FE		Yes	Yes	Yes	Yes	Yes	Yes
Observations		6262	3557	2705	6262	3557	2705

Notes: Model estimated through Linear Probability Model. Dependent variable equals to 1 if respondent provided elder care in the previous 3 months. Eligible is a dummy indicating if the youngest child is older than 5 years. Kindergarten FT is a dummy indicating if the state provides publicly funded full-time kindergarten. Sample excludes states that provide pre-kindergarten (Florida, Georgia and Oklahoma), children aged 5 and single child observations. Linear functional form with age trends. Control variables are: dummy variables for race, age and gender of respondent. Standard errors are clustered at state-year level. Significance levels * 10% ** 5% *** 1%.

Table C.4: Youngest child public school enrolment from kindergarten - First stage equation

	All	Female	Male	All	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
Age child	0.041***	0.046***	0.039***	0.042***	0.048***	0.039***
	(0.004)	(0.007)	(0.005)	(0.005)	(0.008)	(0.005)
Eligible	0.799***	0.772***	0.814***	0.801***	0.771***	0.816***
	(0.009)	(0.015)	(0.011)	(0.010)	(0.018)	(0.011)
Eligible × Age child	-0.040***	-0.042***	-0.040***	-0.040***	-0.043***	-0.038***
	(0.004)	(0.007)	(0.005)	(0.005)	(0.008)	(0.006)
White				-0.018***	-0.011	-0.022***
				(0.005)	(0.008)	(0.005)
Age respondent				-0.002***	-0.002***	-0.001***
				(0.000)	(0.000)	(0.000)
Male (d)				-0.002		
				(0.004)		
Constant	0.095***	0.105***	0.090***			
	(0.007)	(0.013)	(0.009)			
Adjusted R^2	0.669	0.642	0.684	0.672	0.645	0.686
Observations	29462	10602	18860	29462	10602	18860
Control	No	No	No	Yes	Yes	Yes
State-Year FE	No	No	No	Yes	Yes	Yes

Notes: Model estimated through Linear Probability Model. Dependent variable equals to 1 if the child is attending school, 0 otherwise. Significance levels * 10% ** 5% *** 1%. Columns 1-3 standard errors robust, Columns 4-6 clustered at state-year level. Source: Current Population Survey (CPS).

Chapter 5

Conclusions

This thesis has provided an empirical analysis of effectiveness of social protection programmes in Ethiopia, and of kindergarten eligibility and provision of elder care in the United States, identifying the trade-off between child care and elder care. The findings from the three studies offer important contributions as they provide valuable insights for policy-making while contributing to the economics literature.

The first chapter examined whether targeting of two main aid interventions has improved since the introduction in 2005 of a major safety net programme, the PSNP. Using the last two rounds of the ERHS, one just before the implementation of the PSNP (2004) and the other after a few years of its implementation (2009), I directly compared the differences in targeting with a focus on three main variables that capture food insecurity, poverty and political connections. While the results for 2004 are in line with previous studies (Broussard et al., 2014; Caeyers and Dercon, 2012), which found political connections to be particularly important in selection of beneficiaries for both public works and food aid, the results for 2009 point to an overall improvement in targeting, especially for public works. In particular, wealth in 2009 is a strong predictor of targeting while political connections no longer appear to play a role. For food aid, while there are similar encouraging trends for political connections, indicators related to demographics are only significant in villages that implement also public works, suggesting the possibility of some externalities from public works to food aid targeting.

In the second chapter, I assess whether the distribution of food aid in the aftermath of a severe drought that hit Ethiopia in 2011 protected children's health two years later from

the shock. The analysis focuses on height-for-age of children 0-36 months old at the time of the drought. Using a doubly-robust matching estimator to address the selection into emergency aid, I find that the drought had a detrimental effect on the health of children that did not receive aid by lowering their height-for-age by 1.76 standard deviations. This effect was found to be totally offset by emergency aid, which proved effective in protecting the long-term health of children from households that were hit by the drought.

The last chapter analysed the trade-off between child rearing and provision of elder care. Exploiting the age eligibility for public kindergarten, in combination with state-level variation in the offer of full-time kindergarten, I estimate the Intention to treat (ITT) for the effect of age eligibility to kindergarten on provision of elder care. The results show that having the youngest child aged 5 in states that offer full-time kindergarten increases the probability of providing elder care by around 9 percentage points, corresponding to an increase of 63 percent to the baseline. The results, as expected, are higher for females (9.2%) than for males (8.1%). Assessed in conjunction with results on labour force participation, the time freed up from having the youngest child eligible for kindergarten has differential effects on females and males. While men mostly invest the additional time on labour supply, both at the extensive and intensive margins, females employ the extra-free time to provide elder care.

The results above summarised provide a number of implications for empirical analysis and policy, and contribute in understanding the effectiveness of economic policies aimed at improving the well-being of vulnerable groups of the population. In particular, as the debate on targeting has recently regained the attention of researchers and practitioners (del Ninno and Mills, 2015; Brown et al., 2016; Kidd et al., 2016) following the rapid growth in the number of cash transfer programmes implemented in Sub-Saharan Africa, evidence to inform on how a targeting method works is needed. In particular, the results suggest that, notwithstanding the improvements in targeting since the introduction of the PSNP, using a combination of CBT with other targeting methods can be effective in reaching the chronically poor. The creation of more specific and clear eligible groups or criteria would in fact reduce the room for elite capture, and it would help with the transparency of the process and its perceived fairness by the community. In addition, targeting procedures designed to improve transparency, monitoring and to address grievances can be beneficial to reduce the potential drawbacks associated with CBT. While cash transfers and safety nets

targeting receive the attention of researchers and policy-makers, targeting of programmes designed to respond to shocks have been under investigated, with only few exceptions.The results also suggest that more structured targeting is needed for programmes dealing with shock responses. As far as the empirical analysis is concerned, by comparing targeting during two different periods, I provide a first evidence of changes in targeting performance over time. However, notwithstanding the delays that emergency aid has criticised for, the evidence shows that it still plays an important role in protecting the health of the most vulnerable.

By examining the trade-off between child rearing and elder care, the third chapter provides first evidence on kindergarten policies and their effect on the provision of elder care. The implications for policy purposes are highly relevant as the cost-benefit assessments of child-care subsidies have so far overlooked at this additional benefit. Traditionally, in fact, the main benefits considered the direct effects on children's outcomes, and the indirect effects on the labour supply.

Although this thesis sheds light on the effects of policies aimed at alleviating the poverty and supporting vulnerable groups of the population, there are some limitations that are worth to be highlighted, and research suggestions to address them and fill the gaps are here discussed.

In the first chapter, the measures introduced by the government to improve targeting could not be tested as information on implemented procedures was not available in the Ethiopian Rural Household Survey. Information at the village level on what procedures were implemented would provide useful insights as to whether procedures and guidelines were correctly implemented and would allow for an empirical test on which measures were most effective in improving targeting. In addition, information on subjective perception of the fairness of the programme would also add an important value to the overall assessment of this particular form of targeting.

A further limitation relates to the fact that the data set used for this analysis is not nationally representative, therefore the results should be interpreted with caution, and policy implications from them should take into account this limit. The data set, while being a rich source of data on several topics, is collected only in 15 rural villages. Further, one of the variable of main interest in the analysis, political connections, was collected only in 2004, which constrained in the analysis. In particular, I had to assume that political

connections have not changed in the five years period. While this assumption seems to be reasonable in the Ethiopian context of the period covered by the analysis, it could still not realistically depict political affiliations in 2009. As targeting continues to be a top issue on the policy agenda, research on further refinements in criteria used to target beneficiaries would increase the effectiveness of aid, particularly given the limited resources at disposal.

In the second chapter, the collection of anthropological data in the Ethiopian Rural Socioeconomic Survey, allows to assess the long-term impact of aid on children's health. However, the large number of measurement errors in gender and age entries prevented to create a larger child-level panel data set. Since the survey tracked households, to create an individual-level panel, gender and age were needed. In addition, the measurement of the anthropometrics was successfully recorded only for a subset of children, further restricting the children in the final sample.

These two issues left me with a reduced sample of 168 children, which constrained the analysis in a number of ways. First, it was not possible to further investigate heterogeneous effects, for instance disaggregating by sex of the child. These data limitations had implications also in terms of econometric approaches used. In particular, issues related to unobservables correlated to health status could have been addressed with mother fixed effects, which in fact could not be implemented. The lack of suitable instrument for health measure reflects a further limitation of this study. The use of instrumental variables is used in the literature on child health to address issues of endogeneity but also of measurement error related to health measures. However, a valid instrument was not found. While concerns raised by the us of self-reported drought are partially addressed in the study, a larger data set would have allowed to test the robustness of the results by using a drought variable derived from rainfall data. The analysis would have therefore benefited from higher quality of health data. Additional information on the time of the drought and the distribution of aid would have also provided additional evidence on the importance of timing in shocks response. Further research on the modalities of aid distribution would inform on issues related to aid distributed in times of shocks.

The data did not allow to assess the impact of the PSNP on children's long-term health. The sample size was in fact even smaller for PSNP beneficiaries. The lack of an empirical assessment of the role of PSNP in protecting households hit by shocks reveals an important gap, particularly in light of the importance of the PSNP as part of the shocks

response, with a system set up in the recent years to scale-up PSNP in times of crises.

With the availability of a third round of data soon to be released, it would be interesting to investigate the impact of drought and aid in the longer term on health and on other child outcomes, such as schooling. Further, a development of the current analysis would be to disentangle the effect of the drought between the immediate effect, which in the literature (Rabassa et al., 2014; Tiwari et al., 2013) has been referred to as the "disease effect", and the lagged "income effect". Separating the two would provide an indication of the more appropriate timing for delivering aid and also of what other interventions could be provided. Lastly, it would be interesting to assess aid role in reducing mortality and morbidity, particularly in relation to chronic diseases.

In the third chapter, evidence on the trade-off between child-rearing and elder care was provided. The use of a neat identification was implemented through the age of the youngest child via a reduced form approach. However, a more robust strategy would have been possible if the exact child birth date or the quarter were available. In addition, if information on school attendance by the child was available, an instrumental variable approach would have been possible, providing additional strength to the results. In addition, information on time spent providing care was only recorded with a 24 hour recall period, a short time span, which produced a fuzzy variable. The results on the extensive margin therefore can only be interpreted with caution. Additional information on time spent providing elder care would allow to produce more informative estimates on the extensive margin.

An additional contribution to the study of child and elder care could extend to pre-kindergarten policies to see whether access to this level of education have similar effects on provision of elder care. This would allow to ascertain whether publicly funded schooling had an effect on the provision of elder care only from the age of five or whether similar effects were found also at earlier ages.

In summary, this thesis provide valuable insights on issues related to social protection programmes, both in developing and developed countries. The analysis conducted in the three chapters contributes to the relevant ongoing debates and suggests research development to further contribute to the literature. Notwithstanding the specific countries in which the analysis has focused, some external validity to similar contexts is produced.

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