Essays in Development Economics

The Role of Information and Access to Financial Services in Changing Financial and Employment Behaviour and the Resulting Impact on Household Welfare

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BY

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Declaration

I declare that this thesis has not been submitted as an exercise for a degree at this or any other university and it is entirely my own work.

Chapter 2 is based on joint work. Details are given in the acknowledgements.

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Tara Bedi

Summary

In developing countries, financial markets tend to be incomplete, particularly for formal savings and credit products (Banerjee and Duflo 2007). Additionally, there are gaps in access to social safety nets for managing income shocks, particularly for poor households (World Bank 2015b). For those more vulnerable to poverty, including women and those living in rural areas, the constraints to moving out of poverty can be particularly formidable. Furthermore, in the context of a changing climate, prior coping mechanisms may no longer work. This leaves households more vulnerable to poverty and makes these constraints more dynamic to address (Abeygunawardena et al. 2009), creating an imperative for further research.

This thesis contains three essays that analyse the financial access and information awareness constraints faced by individuals and household in developing countries for managing income uncertainty and income shocks. The underlying theme throughout this thesis is about addressing how people manage these constraints and how this affects their welfare outcomes. Each essay uses micro level data at the individual and household level in different country contexts to analyse constraints to poverty alleviation. Through this analysis, these essays add to the empirical evidence by providing some insight into the potential tools and mechanisms that help households manage income variability and shocks. In chapter 1 of this thesis, a more detailed introduction is provided.

Chapter 2 considers the impact of income uncertainty on the types of savings products agriculture dependent individuals in Kenya use. For the analysis, this chapter uses the FinAccess 2016 individual level cross-sectional data from Kenya, which is nationally representative, merged with the Famine Early Warning Systems Network (FEWSNET) rainfall data, from Kenya. Income uncertainty is proxied by recent changes in the coefficient of variation (CV) of rainfall for both the long and short rainy seasons. This chapter shows that agricultural dependent individuals respond to income uncertainty through differentiated revealed preferences for formally regulated, credit enabling and less liquid savings products. The results are robust to different time periods, various model specifications, and find that for non-agriculture dependent individuals, changes in climate variability has no effect on preferences for formally regulated or credit enabling savings products. The findings of this paper add a new dimension to our understanding of the use of financial products in

low and unpredictable income settings. It also adds to the literature on strategies for consumption smoothing.

Chapter 3 exploits an exogenous change in the availability of credit for women in Viet Nam to look at the gender differentiated impact of credit on individual and household welfare. This chapter uses the Vietnamese Access to Resources Household Survey (VARHS), an extensive panel dataset that tracks the same 2,108 households for the period 2008 to 2016. Household fixed effects analysis is used to explore how access to credit and the level of credit supplied impacts on the allocation of female time and their investment decisions on their income generating activities. This chapter also explores whether credit in the hands of women versus men affect household welfare outcomes differently. From the analysis, this chapter finds that greater access to credit increases the number of days that women work in agriculture and home enterprise activities, while also changing the inputs women use in their income generation activities. Next, it finds that an increase in the relative amount of loans in the hands of women versus men negatively impacts income per capita and food expenditure in the current period. It may be the case that it will take some time before this increased credit access has a positive impact on household welfare outcomes. Though, this chapter does find some positive correlations between increased credit access for women with child welfare outcomes, where increased access for women is correlated with a decrease in the time male children spend working in agriculture activities. This chapter contributes to a growing literature that explores the impact of access to credit on individual and household welfare outcomes and in particular whether credit in the hands of women compared with men matters.

Chapter 4 addresses whether the direct and public provision of information to citizens affects the implementation of anti-poverty programs. This chapter uses the Young Lives panel dataset from India for the period of 2006 to 2014, that collects detailed demographic, consumption, income and occupation information at the individual and household level. This chapter exploits the variation in the implementation of an accountability intervention on the Mahatma Gandhi National Right to Employment Guarantee Act (MGNREGA) in Andhra Pradesh, India, to explore empirically whether MGNREGA outcomes change for households who are exposed to the information campaign and organised into groups. The main result from this chapter is that households in treatment areas are given more days of work, which translates into a smaller gap between the days they are entitled to and the the days they get supplied to work. The days worked under MGNREGA by households in treatment areas are rationed 24.6 percent less than for those in non-treatment areas. This essay adds to the empirical literature by showing that an information intervention to beneficiaries, who are empowered through being in a group setting, can increase the accountability of a program and how it is implemented.

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Dedication

And Dominic, this one is for you. For always encouraging me, even when I doubted myself. Some stars always shine bright.

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Chapter 1

Introduction

1.1 Thesis motivation and chapter previews

Increasingly, ending extreme poverty seems like a tangible goal. Today, 10.7 percent of the global population lives on less than $1.90 a day, which is a significant decrease from over 35 percent who lived below the poverty line in 1990 (World Bank 2017c). Additionally, the distance for these poor households to the poverty line is also shrinking (Chandy et al. 2016).

Individual, household and regional characteristics all play a determining role for the movement in and out of poverty. Furthermore, the interactions between these factors make poverty more complex to tackle. It is not just that households are poor because of a low income, but their individual characteristics, such as their gender, their education level and their occupation, make them even more vulnerable to being caught in a poverty trap.

These dynamics make certain groups, such as women from ethnic minority households, or agricultural households, even more vulnerable. Often, these vulnerable groups lack awareness of their rights and entitlements from anti-poverty programmes, limiting their ability to benefit from these programmes (Shankar et al. 2010). Additionally, they often exist outside the formal financial system, lacking access to saving and credit products which, if used correctly, can help cope with risks and crises, such as an increasingly variable climate (Beck et al. 2004, 2007; Ghosh et al. 2000; Klapper et al. 2016). These groups also face hidden barriers. Aterido et al. (2013) find that women pay a higher interest rate and are, simultaneously, less likely to be financed by formal institutions. These constraints make it harder for these groups to break the poverty trap.

Research on poverty has analysed the constraints poor households face in escaping poverty and managing shocks. While progress has been made on tackling these constraints, these constraints are evolving and dynamic. This creates an imperative for more in depth research on these constraints to poverty.

The focus of this thesis is to address these constraints by looking at how the easing of barriers to information, and financial access constraints, impact savings, credit and employment behaviour. It looks at these constraints in new contexts, where prior research has not focused, or with a new angle. Where feasible, the essays then look at what effect there is on household welfare outcomes, such as income, food expenditure and the allocation of children's time (including their time spent working).

Through three essays, this thesis looks at the constraints in three different developing country contexts, Kenya, Vietnam and India. In the last decade, all three economies have seen rapid annual real GDP growth.[1] Even with this growth, poverty levels range from 20 percent to 30 percent in these countries (World Bank 2017a,b, 2013).[2] These figures mask further inequality within the country. For instance, in Viet Nam, the ethnic minority account for 50 percent of poor households, while only accounting for 15 percent of the population.

At the micro level, households within these countries face similar constraints in managing income vulnerability. Through looking at these constraints across these different contexts, this thesis adds to the empirical literature on how these constraints are evolving and what country specific dynamics there are to these constraints. Through the findings from each essay, this thesis provides new insights into mechanisms for tackling poverty and highlighting gaps that need further research.

The main contribution of this thesis to the empirical literature is to show that income uncertainty, gender and being informed on anti-poverty program entitlements affect how and where individuals and households save, how they allocate their time and their ability to benefit from anti-poverty programs. More specifically, chapter two shows that agriculture dependent individuals increase their use of savings products that were institutionally less risky and enabled credit access when faced with an increase in future income uncertainty. Chapter three shows that increased access to credit for women increases the number of days they work and the types of inputs they use in the income generation activities. Finally, chapter four shows that when individuals are informed and empowered, they are better able to access their entitlements from anti-poverty programs.

The remainder of this chapter focuses on the specific issues this thesis hopes to address, and summarises the following chapters.

[1] Since 2010 all the three countries have seen annual real GDP growth above 5 percent. In 2016, all three countries had a growth rate of 6 percent (IMF 2017).

[2] For Kenya this figure was from 2005, for India it was from 2011 and for Viet Nam it was from 2010.

1.1.1 Income Uncertainty and Preferences for Formal Financial Products: Evidence from Rural Kenya

Chapter 3 contributes to the empirical research by showing how income uncertainty, proxied by climate variability, influences the type of savings devices individuals use.

The literature is clear that income plays a key role in determining the types of savings devices individuals use (Demirguc-Kunt and Klapper 2012; Kibet et al. 2009; Camara et al. 2014). The link though, between income uncertainty and the types of savings products individuals use, is currently not well developed. While a number of papers look at the impact of income shocks on savings, there is limited empirical research that has focused on how future income uncertainty influences how and where individuals decide to store their liquid wealth *ex ante*. In this chapter, I examine how future income uncertainty, proxied by climate variability, affects the savings devices individuals use. The mechanisms through which I analyse how uncertainty affects the use of savings products is by the savings product's level of regulation, whether the product is credit enabling and how liquid each savings device is.

I use the FinAccess 2016 individual level cross-sectional data from Kenya, which is nationally representative, and has detailed information from 8,665 individuals on where they save, their self-perceived behaviours, income and expenditure data, and other socio-economic and demographic information. I merge this individual data with the Famine Early Warning Systems Network (FEWS-NET) rainfall data from 2002 to 2015, from Kenya, which is produced from daily precipitation reanalysis data at a spatial resolution of 0.1 degrees. I create a proxy from this rainfall data for income uncertainty in the coefficient of variation (CV) of rainfall for both the long and short rainy seasons. I use this measure as an estimate of the probability of a shock occurring in the future and analyse how a change in this measure affects the types of savings devices individuals use in Kenya.

The findings from this chapter suggest that a recent increase in climate variability results in the increased use of savings products that are institutionally less risky and enable credit access. Another key finding of this chapter is that increased variability decreases the probability of saving in products where liquid wealth is easily accessible. This could imply that savings which are accessible to an individual are also easily accessible to those in their network. As climate variability is covariate in nature, individuals facing increased variability may protect themselves by decreasing their use of savings in such products.

A number of robustness checks are carried out, which help confirm that the variability of the rainy seasons are an important influence in what savings products agriculture dependent individuals use. Households who face an increase in climate variability are more likely to report using savings products that facilitate credit and that are less risky. These results hold even when one looks at different time periods or uses different time frames to measure the rainfall variability.

1.1.2 Access to credit and welfare outcomes: does the gender of the recipient matter?

Chapter three contributes to a growing literature that looks at the impact of credit access on individual and household welfare. In particular, this essay considers how credit access changes the time allocation of women, their investment choices and whether welfare outcomes associated with credit are differentiated by gender.

While it is well established that resources held in the hands of women have a positive impact on individual, household and child welfare outcomes, few studies have explored whether credit held in the hands of women has similar effects. This chapter examines whether credit extended to women has a different effect on individual and household welfare than credit extended to men. This chapter uses an extensive panel dataset from Vietnam for the period 2008 to 2016 and exploit the recent implementation of the Gender Equality Law in 2010, which aimed to significantly expand access to credit for women in rural households, to identify the effect of female credit on individual, household and child welfare outcomes.

This chapter finds evidence that access to formal loans increases the number of days that women work overall, particularly in agriculture and home enterprise activities. The chapter explores if this changes the inputs women use in their income generation activities and find that it does, resulting in women substituting hired inputs with their own labour. At the household level, however, the chapter finds that the proportion of formal loans held by women in the household has: a negative impact on household income and food expenditure; is positively correlated with the ratio of female working days to male working days; and is negatively correlated with the working days of men. This suggests that credit to women may in fact increase their burden in household income generating activities with no positive consequences for household welfare. This chapter does find, though, that increased access to credit for women is significantly correlated with a decrease in the time male children spend working in agriculture activities, suggesting that there may be some positive benefits for children.

1.1.3 Accountability of public works entitlements in rural India

Chapter four investigates the role of information awareness in improving the implementation of anti-poverty programs. It uses panel data on 2,056 households from three Young Lives survey rounds from Andhra Pradesh, India. It supplements this with Government data on the implementation of an accountability campaign rolled out in 2011 and 2012 in AP.

Accountability is often identified as one of the reasons anti-poverty programs fail in developing countries. The lack of awareness by beneficiaries on their entitlements is often cited as a reason for this lack of accountability. Yet, there is debate in the empirical literature on the effectiveness of information campaigns on increasing accountability. This chapter addresses this issue of accountability through analysing the impact of a public information campaign on employment outcomes under the Mahatma Gandhi National Right to Employment Guarantee Act (MGNREGA) in the state of Andhra Pradesh, India. It contributes to the empirical literature through estimating the impact of information in the hands of mobilised individuals on the supply of guaranteed employment.

The main finding from this chapter is that households exposed to the information campaign had a smaller gap between the total days of work they were entitled to and the days they got supplied to them. The number of days households get rationed in treatment areas are 24.6 percent less than the days rationed for households in non-treatment areas. The results also show that households in treatment areas are more likely to work more days in the non-lean agriculture season. On the other hand, this chapter finds evidence that households in treatment areas have a 3 percent higher probability of working more than the 100 days entitlement.

1.1.4 Concluding chapter

In Chapter 5 I discuss some concluding remarks and implications for policy.

Chapter 2

Income Uncertainty and Preferences for Formal Financial Products: Evidence from Rural Kenya

2.1 Introduction

In developing countries, financial markets tend to be incomplete and with access gaps to financial products for the very poor (Banerjee and Duflo 2007; Ersado et al. 2003). With gaps in the credit, insurance and savings markets, these individuals have limited access to financial tools that enable them to cope with income shocks. This has important implications for households who are agriculture dependent, as they are particularly vulnerable to income shocks through their exposure to unexpected weather variability (Morduch 1995). In order to manage these shocks households turn to other mechanisms, including crop diversification, savings in the form of liquid and semi-liquid assets, borrowing from money lenders and remittances from family (Ashraf et al. 2003; Rosenzweig and Wolpin 1993; Suri 2003). These mechanisms, such as buffer stocks in the form of bullocks (Rosenzweig and Wolpin 1993), helps households self-insure against negative income shocks (Paxton and Young 2011).

To enable households to manage income shocks better, there have been numerous policy efforts to increase financial inclusion for poor households. These efforts are supported by research findings, which confirm when faced with income shocks, including from seasonal fluctuations due to the agriculture cycle, financial tools can help households smooth consumption (Ghosh et al. 2000). Farmers with access to savings and credit tools are able to manage the down times from seasonal fluctuations and increase output (Klapper et al. 2016). Some success has been made on this front, with the number of unbanked households in developing economies decreasing by 20 percent (Demirguc-Kunt et al. 2015).

This increase in financial inclusion has been paralleled by the growth in the penetration and diversity of savings products, including savings accounts through mobile phones. The significance of these new financial products is that they are changing the ease of access, affordability and the risk involved in storing wealth. For households building a liquid buffer stock, devices that are accessible, affordable and trustworthy can play a key role in determining usage of that device for storing savings (Kendall 2010). For instance in Kenya, Suri and Jack (2016) find that access to mobile money changes poor households financial behaviour through enabling them to save more with mobile money accounts. Mbiti and Weil (2011) find that increased use of mobile money positively impacts the likelihood of being banked.

Within the context of increased gain in financial inclusion and growth in the portfolio of savings devices, there is a question on how income uncertainty affects the demand and use of these financial products. To answer this question, I use a change in climate variability, as a proxy for income uncertainty, to analyse how it affects an individual's decision on how and where they save in Kenya. I take a recent change in the rainfall Coefficient of Variation (CV) of the two rainy reasons in Kenya as the measure of climate variability. In line with Alem and Colmer (2014), I differentiate between a climate shock and climate variability. Unlike a climate shock, which is an actual deviation from the long-term mean, climate variability is the likelihood of rainfall deviating from a certain time-defined average.[1] Therefore climate variability provides an estimate of the probability of a shock occurring in the future.[2]

I test whether an increase in the CV, as a proxy for an increase in the expectation of future income uncertainty, produces evidence for the following three hypotheses: first, income uncertainty increases the likelihood of individuals using savings devices that are credit-enabling; second, income uncertainty increases the use of savings products that are regulated; and third, income uncertainty increases the use of savings devices that make it easy to convert savings into cash in hand. The direction and impact of climate related income uncertainty on savings product usage will depend on which mechanism is stronger.

I test these three hypotheses *ex ante*, climate related income uncertainty could impact an individual's usage of savings devices via the following channels. First, it could increase the demand for ease of access products, where users save in devices that the stored wealth can be easily accessed. Second,

[1] In explaining this difference I also draw from the following website, which provides a clear outline of what the Coefficient of Variation is and how it can be interpreted, `http://tornado.sfsu.edu/geosciences/classes/m356/RainfallVariability/TempVar.htm`. (SFSU 2015)

[2] A change in climate variability not only affects the change in future shocks, it also has a differing effect on the ecosystem as argued by Gherardi and Sala (2015).

it could have the the 'portfolio effect', where given the fear of loss in more risky savings devices users move their portfolio to save in products that are regulated and therefore less risky to losing stored wealth. Third, it could increase the demand for products that facilitate credit, where users save in products that enable the savings to act as collateral for credit access (Colmer 2013). One might expect that higher future income uncertainty results in increased usage of savings devices that are deemed to be institutionally less risky and credit enabling, such as savings through banks. Through testing these hypotheses, I contribute to the empirical research by showing how income uncertainty, proxied by climate variability, influences the type of savings devices individuals use.

For regions like Sub-Saharan Africa, climate variation, especially precipitation and temperature variability, is a major determinant of agricultural outcomes (Herrero et al. 2010; Ray et al. 2015; Thurlow et al. 2009). This makes the income of those in agriculture (i.e. the majority of the rural poor) in these regions highly vulnerable to changing weather and climate patterns. For countries like Kenya, this is important given agriculture accounts for over 75 percent of people's livelihood and contributes 26 percent to the GDP (Herrero et al. 2010).

Since the 1950s there have been strong regional variations in precipitation trends (Thornton et al. 2014). This variation is important because changes in the number of extreme weather events and in the intensity and distribution of rainfall has important implications for agriculture dependent households. Thurlow et al. (2009) estimate that climate variability in Zambia cost the country over US$4.3 billion over 10 years. In addition, this variability in climate is cited as a contributory factor for why 300,000 people in Zambia would remain below the national poverty line in 2016 (Thurlow et al. 2009).

A significant body of research looks at the role of climate in generating income shocks and its effect on consumption and savings behaviour (Udry 1995; Duflo and Udry 2004; Paxson 1992). These papers demonstrate that climate shocks affect the income and savings behaviour of agriculture households. Paxson (1992) finds that agriculture households save a much greater amount of transitory versus non-transitory income. Given this importance of savings in managing income uncertainty, it is critical to understand what drives the usage of savings products in the context of a changing climate where savings, both formally and informally, are one of the main mechanisms used to deal with income shocks.

There is a gap in the research on how future income uncertainty affects the types of savings products used by individuals. Therefore, the motivation behind this paper is to explore the effect *ex ante* beliefs on future income uncertainty, proxied by climate variability, have on current financial decision-making.

I explore this issue in the context of Kenya for two reasons. First, Kenya has seen a rapid and significant change in its financial product portfolio over the last 10 years. With the introduction of mobile money in Kenya in 2007, 10 years later over 96 percent of households have at least one individual who uses mobile money and this has led to the introduction of other products like M-Shwari, a paperless saving product accessed through a mobile phone (Suri and Jack 2016).[3] Second, Kenya is facing an increasingly unpredictable climate, where it has experienced a higher number of droughts and floods and will see a rise in high rainfall events (Herrero et al. 2010; DFID 2009).

For the analysis, I use the FinAccess 2016 individual level cross-sectional data from Kenya, which is nationally representative. This dataset includes information from 8,665 individuals on where they save, their self-perceived behaviours, income and expenditure data, and other socio-economic and demographic information. The dataset also has information on nine savings devices that individuals can use. I categorise these nine savings products by their level of regulation, whether they enable credit and how easily they enable savings to be changed into cash in hand. I also create groupings of savings products based on these characteristics. I merge this individual level data with the Famine Early Warning Systems Network (FEWSNET) rainfall data from 2002 to 2015, from Kenya. The rainfall data is produced from daily precipitation reanalysis data at a spatial resolution of 0.1 degrees.

In the analysis, I base the approach on the theoretical frameworks by Claessens (2005), Beck and de la Torre (2006) and King (2014), which lay out the economic and socio-cultural determinants of the demand for financial products, particularly formal banking products. Based on these demand factors, I outline a simple usage model to estimate the probability of an individual using the different types of savings products on offer. I use a multivariate probit regression to estimate how a change in climate variability affects the usage of savings products. I include a rich set of control variables to minimise the potential for factors, such as omitted variable bias, to confound the analysis. I include individual, household and location controls. However, it is possible that some omitted bias is still present in the results, particularly since the data is cross-sectional. I address this through using rainfall data over a 12 year period.

The main finding in this chapter is that an increase in future income uncertainty, proxied by climate variability in the short and long rainy seasons, increases the likelihood of agriculture dependent individuals using more regulated and credit enabling savings products, such as banks and SACCOs. This result suggests that a recent increase in climate variability led to an increase in the use of savings products that were institutionally less risky and enabled credit access. This result is robust to the different time periods and model specifications.

I also find that increased variability decreases the probability of saving in products where liquid wealth is easily accessible. As climate variability is covariate and affects others in the same climate zone, individuals facing increased variability may ring fence their resources by decreasing their use

[3] I refer to products like M-Shwari in this paper as Mobile Bank Saving products.

of savings products that may be accessible to others in their social network. This reasoning seems to be supported by the fact that this result also holds for non-agriculture dependent individuals.

I run a number of robustness checks to test the validity of the results and find that the findings hold. First, as mentioned above, I re-run the empirical specification for data from the FinAccess 2013 survey and find that the results hold. Second, I test whether annual rainfall variability has a similar effect on savings products and find that it does not. This is line with our expectation, as it should be the variability of the rainy seasons linked to the agriculture cycle that affect income uncertainty. Third, I estimate the effect of rainfall variability for non-agriculture dependent individuals and find that it decreases the likelihood of using liquid savings products. Fourth, I run the estimations with a wealth index, instead of an income measure, and find that the results hold. Fifth, I leave out the income shock measure and find that the results remain consistent. Finally, I test whether it is the time frame of the CV measure that is driving the results by using the level value of the CV and changing the time frame in calculating the CV. I find that the level effect of the CV is predominantly insignificant. In regards to the time frame for calculating the CV, when I use a 4 year time frame versus a 3 year frame, the results holds and if anything, the size of the effect gets stronger.

The rest of the paper is organised as follows: Section 2 provides an overview of the conceptual framework used in this analysis. Section 3 provides an overview on the savings devices used in Kenya and links this to the role of climate in affecting the usage of savings devices. Section 4 examines the data and summary statistics. Section 5 presents the empirical approach. Section 6 describes the results from the multivariate probit regressions, while section 7 outlines a number of robustness checks. Finally, Section 8 concludes.

2.2 Conceptual Framework: The link between income uncertainty and savings

Increased financial access has a pivotal role in eliminating poverty for rural households as they are often the least likely to have a formal bank account (Honohan and King 2013; Klapper et al. 2016). Burgess and Pande (2005) found that the expansion of bank branches into rural unbanked areas in India decreased rural poverty by 17 percentage points. Brune et al. (2016) show how access to a bank account helped farmers in Malawi increase their expenditure on equipment by 13 percent and crop output by 21 percent compared to the control group.

As mentioned earlier, in developing countries there are often gaps in the savings, credit and insurance markets. As a result, individuals have limited access to financial tools that enable them to cope with income shocks. Due to the imperfections evident in these markets, financial management strategies that help smooth consumption, based on the Life Cycle Model and the Permanent Income Hypothesis, generally do not prevail in developing countries (Deaton 1992; Ersado et al. 2003). In the context of developing countries, savings take on a critical role in buffering between income and consumption (Ashraf et al. 2003).

For agriculture dependent households, climate variability is a central determinant of income fluctuations (Cline 2009; Herrero et al. 2010). For countries where agriculture is still predominantly rainfall fed, variability in rainfall is a determining factor in crop output (Thurlow et al. 2009). A pivotal study found that the overall impact of climate change would result in a reduction in agricultural productivity, with the most severe losses realised in developing countries (Cline 2009). Thurlow et al. (2009) find that climate variability has negatively affected the growth of maize in Zambia and has decreased agriculture's annual GDP growth rate by at least 1 percentage point. Focusing on Kenya specifically, a study by Herrero et al. (2010) found that there would be losses in the production of key staples at a national level in Kenya as a result of changing climate variability. A more recent climate change study finds that a third of the global crop yield variability can be explained by climate variation (Ray et al. 2015). For Kenya, this study found that the variability of maize production could be explained by a mix of precipitation and temperature variability (Ray et al. 2015).

With countries like Kenya facing increased climate change, there is a need for more work exploring how climate variability, separate to the role of climate shocks, affects where individuals save.[4] This is especially important given that some of the biggest barriers cited by farmers in a study on Agricultural Enterprise and Land Management in the Highlands of Kenya was the lack of cash due to variability of income flows, credit market gaps and high expenditure demands (Place et al. 2006). Consequently, households were often unable to purchase needed inputs, such as high yielding seed varieties or fertilisers, and had to work for others instead of on their own land (Place et al. 2006).

Furthermore, risk and uncertainty related variables should be central to determining the liquidity of savings (Paxton 2009). Therefore, understanding how climate variability affects the usage of savings devices by individuals dependent on agriculture will build a clearer picture on what individuals in more uncertain climate environments are using and how to better tailor savings products to their needs.

One of the few papers that examines the issue of income uncertainty via a climate channel is Colmer (2013) for Ethiopia. This paper looks at how uncertainty affects investment decisions through the

[4] In this paper a climate shock is an actual deviation from the long-run average of rainfall for a particular month. So a negative shock is registered if for a particular month, a household has a defined amount of rainfall that is at least one standard deviation less than that month's long run average. A positive shock is registered if the monthly rainfall for a specific month is at least two standard deviations more than the month's long run average. This long run average for a particular month is calculated based on the rainfall for that month from 2002 - 2014. As Suri (2003) lays out, if the mean rainfall is a household's expectation in regards to the rainfall, a shock is a deviation from this.

channel of child labor and human-capital accumulation. Using rainfall as an exogenous determinant of the household's level of risk, Colmer (2013) shows that historical climate variability impacts current child labour and educational decisions. Colmer (2013) finds that increased variability increases the hours spent by children on farming activities, while decreasing the hours spend on domestic chores. This paper establishes the importance of climate variability on household investment decisions and provides a strong rationale for why it may also influence where people save.

One measure that captures climate variability is the Coefficient of Variation (CV). Colmer (2013) shows how the CV can be taken as an indicator of the probability of future shocks. The CV is calculated by dividing the standard deviation by the average rainfall. It provides a measure of the likelihood of rainfall varying from some time defined average (SFSU 2015). Lower CV values indicate more dependable rainfall patterns, while higher values mean the rainfall in an area is defined by more extreme patterns. This helps farmers estimate how frequently rainfall in their area can vary from a time-defined mean.[5] For instance, rainfall in an area with a CV value of 0.1 can vary $+/-10$ percent from its average, while rainfall in an area with a CV value of 0.5 can vary $+/-50$ percent from its average.[6] This implies a more dependable rainfall pattern for the first case, while the second case has a higher probability of a big deviation from the average. Therefore the higher the CV, the more likely it is the individual will face a climate shock in the future.

In the paper I take a recent change in the CV as a measure of the change in the probability of future shocks. Based on this, I outline three channels through which a change in the CV *ex ante* can influence the type of savings devices individuals use.

First, prior literature has emphasised the role of credit access as a key-deciding factor in the usage of savings devices (Kibet et al. 2009). *Ex ante*, a change in the CV can affect the demand for a savings product through the precautionary motive, where individuals use savings devices that help them mitigate the effects of future shocks. Savings through formal banks, Micro-Finance Institutes (MFIs) or credit unions, could facilitate credit to members. This could enable them to invest in new technology, watering mechanisms and so forth, which decreases their exposure to the impact of future shocks. It could also enable people to smooth consumption when a shock occurs through a loan, due to the individual already having savings in that device to act as security against any borrowings. Savings devices that facilitate credit, e.g. loans from a bank or a savings group, will cause individuals to shift savings from other savings devices into these credit-friendly savings devices (Rogg 2000). Informal groups are also important, especially for those with higher levels of social capital, in facilitating credit (Mwangi and Ouma 2012).

[5]The CV is calculated again by dividing the standard deviation of rainfall by the mean rainfall for a particular length of time.

[6]This example is drawn from "Measures of Temporal Precipitation Variability" from the following website: `http://tornado.sfsu.edu/geosciences/classes/m356/RainfallVariability/TempVar.htm`

Second, not all savings products are regulated in the same way. Individuals facing higher climate variability may move away from savings devices that are more informal and associated with higher risk levels of losing stored wealth to devices that are more regulated and potentially better able to safeguard savings from losses, the 'portfolio effect'. In this case, options like a formal bank product or more recent products like mobile money accounts provide a less risky option than more informal savings options. This is because these products are regulated with some form of implicit guarantee. They also offer security to users as there are checks for users to access the funds, for instance to access saved resources in a mobile money product in Kenya users need to use their pin. Finally, these products also have protocols in place that can, in the case of mobile money, recover funds when a SIM is lost (Mbiti and Weil 2011).

Third, certain savings mechanisms facilitate easier access to stored wealth. For instance, with over 110,000 mobile money agents in Kenya (Suri and Jack 2016), it is easy for an individual to find an agent. Once an individual is at a mobile money agent, it takes about one minute to cash out resources from one's mobile money account (Mbiti and Weil 2011). This would suggest that it is quite easy to convert savings on a mobile money account into cash in hand. If access to savings is important in the face of future income uncertainty, then one would expect increased use of savings options, such as mobile money, in the face of increased climate variability.

Yet, it may be the case that such products could also make these savings too easily accessible for the individual, as well as to others in the individuals network. Mbiti and Weil (2011) find that 17 percent of people who reported that they did not want to receive money on M-Pesa, were worried that their money would be too easily accessible. Baland et al. (2011) find that individuals in Cameroon take out a loan and pretend to be poor even when they have significant savings, the reason they propose for this behaviour is that the individuals are trying to hide these resources from others in their network. Given that many individuals are dependent on agriculture, it is possible that an increase in climate variability may decrease the usage of savings products through which others can also easily access the stored wealth of the individual. If this is the case, it is feasible that an increase in climate variability also influences the types of savings devices non-agriculture individuals use who are located in agriculture areas or have family dependent on agriculture. Therefore, if the motive was to safeguard these resources from others, one would expect to see all individuals less likely to save with products like mobile money that make remitting money easier.

Based on these channels, I propose three hypotheses;

Hypothesis 1: An increase in income uncertainty leads to an increase in the use of savings devices that are credit-enabling

Hypothesis 2: An increase in income uncertainty leads to an increase in the likelihood of using savings devices that have some form of regulation - the 'portfolio effect'

Hypothesis 3: An increase in income uncertainty leads to an increase in demand for liquid savings products.

It is important to propose an alternative outcome in regards to *Hypothesis 3*, where an increase in income uncertainty leads to an decrease in demand for liquid savings products. One would see this occur if the need to protect these resources from family and friend networks outweigh the demand for easily accessible savings.

2.3 Kenyan context

Agriculture in Kenya is predominantly rainfall dependent (Speca 2013). Kenya is also one of the countries facing an increasingly unpredictable climate. Climate change studies predict a rise in precipitation during the rainy seasons, especially the short rainy season, with an increased intensity of high rainfall events (Herrero et al. 2010; DFID 2009). Herrero et al. (2010) found that it is most likely that Kenya will become wetter, though this will not be evenly distributed across the country. In addition, Kenya has experienced a higher number of extreme events, with a rising occurrence of droughts and floods (DFID 2009). Between 1991 and 2008, Kenya experienced over seven droughts that impacted over 35 million people (Rourke 2011). Understanding how individuals in such contexts respond to changing income uncertainty is important in helping people to better cope with future income shocks and to help keep households out of poverty.

2.3.1 Usage of savings devices in Kenya

In a context, like Kenya, where credit and insurance markets are imperfect, savings devices are central components of household strategies to build up liquid assets to help safeguard consumption from an income shock. The Kenyan Financial Diaries found that low-income Kenyans try to keep some liquid savings in order to deal with income uncertainty (Zollmann 2014).

Households in developing countries often do not have access to formal savings products and are left to rely on informal mechanisms to build this buffer stock. In 2013, only 24.8 percent of households in Kenya had access to a formal savings product (Bedi and King 2015), by 2016 this increased to 30 percent.[7] The majority of households tend to use informal savings products to store wealth. While Kenyan Financial Diaries found that savings are more important than borrowing in the respondent's financial strategy, only 9 percent of these savings were held in formal bank accounts,

[7] FinAccess 2016 classifies the following as a formal savings product: Postbank account, bank account for savings or investment, Current account and finally a Bank account for everyday needs.

the rest were held in informal savings devices (Zollmann 2014). Households therefore often rely on informal financial products, potentially at a higher cost and risk level.

The problem with these informal savings mechanisms, such as savings with family and friends, is that they can often fail to help when the risk is covariate (Barnett et al. 2008). This results in farmers investing in low-return and low-risk options, which is often identified as one of the core reasons for why farming households remain in low equilibrium poverty traps (Barnett et al. 2008).

At the same time, the range of savings devices in Kenya on offer has been rapidly changing with a growing number of semi-formal devices, particularly products linked to mobile phones, such as M-Shwari. The implication of this is that it may affect what devices household use to store their savings to manage future income shocks.

2.3.2 Agriculture & rainfall in Kenya

If one looks at the national level rainfall data for Kenya from 1900 to 2012 in Figure 2.1, one can observe at the aggregated level a slight increase in the linear trend for the annual rainfall for Kenya over this 112 year time period.[8]

Most of Kenya is bimodal, receiving the bulk of its rain in the long rainy season, from March to May, and the short rainy season, from October to December (Kerandi and Omotosho 2008; Place et al. 2006; Chemin et al. 2013).[9] These two rainy seasons are therefore the main seasons for agriculture and for those dependent on agriculture activity, these two seasons are an important determinant of income. Both crop production and livestock related agriculture are dependent on the two rainy seasons. In figure 2.2, one can see that food crops like barley, maize, millet, sorghum and beans are grown during both long and short rains.

Delays and changes in the level of rainfall affect the moisture levels of soil and water bodies (IPCC 2014). This not only impacts the growth of crops but also the distance livestock have to travel for food and water. As a result, the delay and change in the rainfall level also affects the physical conditions of the livestock and their milk production. For example, a lack of adequate rainfall in the 2010 short rainy season affected the production of milk and the conditions of livestock in the pastoral areas of North Kenya (GIEWS and FAO 2011).

[8] The figures presented here are based on the dataset produced by the Climatic Research Unit (CRU) of University of East Anglia (UEA) for rainfall at the national level. It was downloaded from the following website `http://sdwebx.worldbank.org/climateportal/index.cfm?page=downscaled_data_download&menu=historical`

[9] It is important to note that while one is called the long and the other the short, both rainy seasons have the same timeframe of three months. In addition, this means that 6 months of the year are the non-rainy months and 6 months are the rainy months.

The variability in both rainy seasons is important given that the amount and distribution of the rainfall affects agriculture productivity and therefore income fluctuations (Chemin et al. 2013). The behaviour of these rainy seasons is different though, with the short rains being more variable than the long rains and accounting for more of the inter-annual rainfall variability in Kenya (Herrero et al. 2010). It is therefore important to look at these two rainy seasons separately, as combining them would average out this difference in variability.

If one looks specifically at the trends in these two seasons over time in Figure 2.3, one can see that for the short rainy season, the trend for the average rainfall received is increasing over the time period of 1901 to 2006. This is in line with recent climate studies (Herrero et al. 2010). On the other hand the trend for the average rainfall in the long rainy season, is slightly decreasing over the same time period.

Yet, this does not tell us whether Kenya has been facing increased variability in rainfall. If one looks at what the CV is for ten-year period blocks for Kenya in Figure 2.4a, one can see that over time the CV has been changing for the country both for annual rainfall and for the two rainy seasons.[10] When one breaks this into smaller blocks, one can see in Figure 2.4b that within a three-year time frame the CV changes. There is even more variability within this shorter timeframe. Regardless of which measure and timeframe I use, a key point that stands out from this is that there is high variability in rainfall in Kenya.

2.4 Data description & summary statistics

This paper uses individual level data from the FinAccess 2016 survey, collected between August 18 - October 15, 2015, by Financial Sector Deepening (FSD) in collaboration with the Central Bank of Kenya and the Kenyan National Bureau of Statistics. This survey used a three-stage cluster stratified probability sampling approach so that the data is nationally representative. A total of 8,665 individuals were interviewed from August to October 2015.[11] In order to ensure national representativeness the sample is weighted at the individual and household level to the total adult population (Central Bank of Kenya et al. 2016).

The data collected by the FinAccess survey includes information about the devices people save through, about self-perceived behaviours, income and expenditure data, self-reported shocks and

[10] This change is important because it enables us to look at how a change in income variability, proxied by this exogenous increase or decrease in CV, affects savings behaviour.

[11] The 2015 FinAccess sample is nationally representative based on the KNBS NASSEP V national household sampling frame. The target sample is 10,008 interviews from 834 clusters across Kenya (14 households were targeted in each cluster). On respondent per household (+16 years) was selected randomly. Estimates from the 2015 FinAccess sample are representative to 13 sub-regional county clusters (of Kenya et al. 2016).

other socio-economic and demographic information. In terms of savings products, on average individuals from the survey used 2 savings devices from a total of 9 saving product options. Over 18 percent used no savings device, which is a decrease from 27 percent who reported not using any device in FinAccess 2013.

In Table 2.1, I define how I characterise each savings product from FinAccess 2016 in terms of how regulated, credit enabling and liquid it is. If a product has government oversight, I deem it regulated. If a product has an identified way that individuals are able to apply for credit through the institution that provides the product, it is considered credit enabling. Finally, I define a savings product as liquid if it is relatively quick and easy to convert into cash in hand. For instance, I term mobile money liquid given that over 69 percent of individuals have access to a mobile money agent where they can convert money on their phone to cash within 30 minutes from their house. On the other hand, just over 38 percent have access to a bank branch within this time distance, though if I include access to a bank agent as well, this figure goes up to 52 percent. Furthermore, bank branches and agents have more limited hours that users can access funds compared to a kiosk from where mobile money agents operate. Finally, banks often have more requirements for withdrawing funds.

Table 2.1: Savings Products and their level of credit, liquidity and regulation.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8))
Savings Products	Formal Bank	SACCO	Mobile Bank	Mobile Money	Secret Place	ROSCA	ASCA	Save with Friends/Family
Facilitates Credit	Yes	Yes	Yes	No	No	No	Yes	Potentially
Instand Liquidity	No	No	No	Yes	Yes	No	No	Depends
Government Regulation	Yes	Yes	Yes	Yes	No	No	No	No

In Table 2.2, I group all the 9 savings devices depending on how regulated, credit enabling and liquid they are. The grouping of these products changes depending on these characteristics. For instance while half of the products have some form of regulation, the others are informal and have no legal oversight. In terms of credit access, there are four products that facilitate credit, including informal ones, like ASCAs, and new formal products, like Mobile Bank products including M-Shwari. Finally, in terms of liquidity the two main forms of liquid savings products are saving in a secret place and saving with Mobile Money like M-Pesa.

Table 2.2: Savings Products Grouped by their level of credit, liquidity and regulation.

	(1)	(2)	(3)	(4)
Credit Enabling	Regulated that Enable Credit	Regulated	Informal	Liquid
Formal Bank	Formal Bank	Formal Bank	Secret Place	Secret Place
SACCO	SACCO	SACCO	ROSCA	Mobile Money
MFI	MFI	MFI	ASCA	
Mobile Bank	Mobile Bank	Mobile Bank	Save with Friends & Family	
ASCA		Mobile Money		

Analysing the usage of these savings products by occupation provides interesting insight into the linkages between how individuals earn their income, where they store their savings and the characteristics of the savings products they use. Over 42 percent of all FinAccess 2016 respondents depend on agriculture for at least one of their two main income sources, which is a decrease from 56 percent in 2013.[12] Agriculture dependent individuals save across a number of different savings devices, as can be seen from Table 2.3. Interestingly, the two most common reported products used for savings are the ones that are most liquid, with 37 percent reporting saving in their mobile money account and saving in a secret place. This is then followed by saving in a ROSCA at 35 percent.

Table 2.3: Proportion of individuals using the different savings products, FinAccess 2009, 2013 and 2016

	All		Agriculture	
Year	2013	2016	2013	2016
Number of Adults	6,186	8,124	3,582	3,347
TotalSavingsDevices	1.391	1.939	1.377	1.860
Saver*	0.681	0.780	0.686	0.762
Formal Savings	0.237	0.286	0.198	0.227
SACCO	0.100	0.119	0.114	0.137
MFI	0.032	0.032	0.031	0.033
Mobile Bank	NA	0.150	NA	0.100
Mobile Money	0.264	0.404	0.236	0.370
Secret Place	0.318	0.351	0.327	0.370
ROSCA	0.228	0.330	0.252	0.352
ASCA	0.061	0.143	0.066	0.165
Friends & Family	0.151	0.124	0.154	0.118

Note: People can have more than one savings device therefore TotalSavingsDevices add to more than 1. The sum of all the individual savings devices, aside from the category Saver, add up to the figure in TotalSavingsDevices. *Saver is a dummy variable where the individual has at least one saving device that they use.

Not surprisingly, agriculture dependent individuals are less likely to save through a formal bank product, at 22.7 percent, compared to 28.6 percent for all respondents. However, from Table 2.3 there appears to be no particular pattern between formal and informal products or regulated and unregulated products. While agriculture dependent individuals are less likely than the average respondent to report savings with mobile money or mobile bank products, they are more likely to report savings with a SACCO or an ASCA.

I supplement the FinAccess 2016 data with rainfall data from the Famine Early Warning Systems

[12] In this paper agriculture dependent refers to these individuals who are dependent on agriculture as their primary or secondary source of income.

Network (FEWSNET). I do this because while FinAccess 2016 collects self-reported income shocks, it does not include data on sources of future income uncertainty. Furthermore, as the FinAccess data is cross-sectional, one can only analyse one point of time. The FEWSNET data provides an exogenous climate variability measure.[13]

The FEWSNET rainfall data is from 2002 - 2014, and provides an average rainfall figure for a ten-day cycle.[14] Satellite rainfall estimates (RFE2) are produced from daily precipitation reanalysis data at a spatial resolution of 0.1 degrees, which corresponds to a 10 km resolution (NOAA Climate Prediction Center 2001). As the FinAccess 2016 data includes GPS coordinates for each household, the GPS location for each individual is used to merge rainfall data from the corresponding grid point into one dataset. More specifically, households are matched to the rainfall grid that their latitude and longitude coordinates intercept.

If one looks at Table 2.4, one can see that on average the long rainy season receives a larger amount of rain than the short rainy season.[15] At the same time though, the standard deviation for the short rainy season tends to be almost as high as the long rainy season. This could suggest that there is more variability in the short rainy season in the total amount of rainfall, which is supported by the literature (Herrero et al. 2010). Both of these two seasons, which account for 6 months of the year, constitute around 60 percent of the annual rainfall.

Table 2.4: Mean rainfall (mm), 2002 - 2015, FinAccess households with FEWSNET data

Year	Long Rains Average	Long Rains SD	Short Rains Average	Short Rains SD	Yearly Rainfall Average	Yearly Rainfall SD
2002	448	136	381	119	1223	358
2003	407	129	286	103	1144	418
2004	397	110	327	100	1137	299
2005	470	130	206	101	1123	368
2006	423	102	410	129	1178	284
2007	399	125	276	112	1158	308
2008	345	114	277	77	1088	311
2009	379	109	362	107	1188	362
2010	438	125	294	100	1224	346
2011	401	147	418	109	1331	352
2012	470	140	483	144	1454	417
2013	514	178	341	134	1277	439
2014	463	188	430	143	1430	475
2015	505	204	498	174	1431	457

Table 2.5 reaffirms that the short rainy season has more variability than both the long rainy season and the annual rainfall, with its 2002 - 2013 CV value of 0.32. This CV value indicates that the

[13] `www.fews.net`. This data was downloaded in September 2016, and at a spatial resolution of 0.1, which is around 10 km spatial resolution. More information on this rainfall data can be found at: `http://www.cpc.ncep.noaa.gov/products/fews/RFE2.0_tech.pdf`. It is important to note that while all estimates may have a bias, in a recent study done on Uganda comparing different rainfall estimation models, the RFE2 was found to be better performing than reanalysis models and to have higher correlation with gauge data from rainfall stations (Maidment et al. 2013).

[14] There is data till 2015 and report it in the average rainfall calculations for 2015.

[15] Rainfall is measure in mm which is in depth unit.

spread of rainfall can vary up to 32 percent from the long-run average. The spread of annual rainfall can only vary up to 13 percent from its long-run value, suggesting much lower levels of variability in the annual rainfall.[16] So if there is a deficit or excess rain during the rainy seasons, the yearly total may even out as this rain could fall either within or outside of the rainy season. This would suggest that if one is interested in using the variability of rainfall as a proxy for future income uncertainty, then looking at the rainy seasons, versus the yearly rainfall, would be the right approach.

Table 2.5: Average Coefficient of Variation Values, FEWSNET

Rainy Season	Time Frame	Mean	St. Dev.	Min	Max
Long Rains	2003 - 2005	0.137	0.072	0.003	0.447
	2006 - 2008	0.187	0.102	0.012	0.562
	2009 - 2011	0.151	0.093	0.016	0.785
	2012 - 2014	0.145	0.096	0.002	0.598
	2002 - 2014	0.193	0.051	0.093	0.459
Short Rains	2002 - 2004	0.212	0.146	0.002	1.024
	2005 - 2007	0.437	0.303	0.013	1.494
	2008 - 2010	0.223	0.136	0.002	0.875
	2011 - 2013	0.242	0.121	0.011	1.095
	2002 - 2013	0.321	0.127	0.118	1.059
Yearly Rains	2002 - 2004	0.115	0.062	0.002	0.334
	2005 - 2007	0.119	0.064	0.003	0.447
	2008 - 2010	0.111	0.053	0.005	0.330
	2011 - 2013	0.105	0.041	0.014	0.314
	2002 - 2014	0.133	0.037	0.060	0.338

In order to capture a change in rainfall variability, I estimate the CV for the most recent three year period (2012- 2014), along with a separate estimate for the three years prior to that (2009-2011). I then take the difference between the two and use this difference to capture the change in the variability the individual faces.[17]

I do this for two reasons. First, just calculating and using the CV for a particular time period does not capture changes in recent rainfall. While a CV based on rainfall data for ten years gives us a longer-term indicator of the level of variability in rainfall for a particular area, if there has been a more recent trend, that long run trend will mask the more recent trend. If one estimates the CV in three year time blocks and compare it to a long-run CV value, one can see that the long-run CV value masks a lot of the variability in rainfall. Looking at Table 2.5, one can see that for the short

[16]It is important to remember that this is an average of the values from all individuals across the different regions in Kenya, the actual variability between regions may be higher.

[17]For example, to calculate a change faced by an individual in their CV value for the long rainy season, I first calculate the CV for the long rainy season months from 2012 to 2014. I calculate the CV for 2009 to 2011 and then take the difference between these two values and this is value for the change in the variability faced by the individual.

rainy season, the average CV value ranged between 0.19 to 0.41 depending on what time range was used to calculate it.

I argue that it is the recent history that influences the savings product behaviour of agriculture dependent individuals. It is more likely that people will be taking recent history into their decision making process on how to manage future shocks, including on where to save. Therefore, for the main estimation of the CV, I use the shorter timeframe to calculate the CV, as it is this information which will influence people's expectation on future variability.

Second, while farmers may be aware of the long-term variability of rainfall in their areas, financial product suppliers may also be more aware of long-term trends and use this information in the location of their product placement. Therefore any results just based on the long run CV may actually reflect where financial providers have located themselves, versus individual decisions on what savings device to use. Using the difference between two relatively recent CV values goes some way to address this.

With over 42 percent of individuals in the FinAccess 2016 survey being dependent on agriculture as one of their two main income source, looking at the measure of rainfall variability defined above provides us with an important insight into how climate, as a proxy for future income uncertainty, influences what savings devices such households use.

2.5 Empirical approach

A lot of the empirical and theoretical work looking at the demand for savings devices has focused on the demand for formal financial services. Claessens (2005), Beck and de la Torre (2006) and King (2014) develop theoretical frameworks that help analyse the demand for formal banking products and where problems of access may lie. These three papers lay out the main economic and social-cultural factors, including income, age, cost, accessibility of the product, that determine the demand function for formal banking products. I use the main factors they identify in the estimation model identified in equation 2.2 below. In term of access, Morduch (1995) identify reliability, convenience, continuity and flexibility as key determinants of access. These factors therefore have a central role to play in what savings devices people use. Within these models, the factors determining demand are dependent on both individual and institutional attributes.

A number of papers have used a Random Utility model (RUM) to look at what determines usage of and demand for financial products. For instance Orosco (2007) specifies a RUM to analyse the demand for deposit accounts in the US. Mwangi and Ouma (2012) and Kibet et al. (2009) use a similar model for analysing determinants of credit and savings products respectively in Kenya. In this model, the utility an individual i derives from savings product j is expressed in equation 2.1 as:

$$(U_{ij}) = U(z_j, \varepsilon_j, x_i; \theta) \tag{2.1}$$

From equation 2.1, the utility from savings product j for individual i is determined by the savings institution's observable (z_j) and unobservable (ε_j) characteristics, a vector of observable and unobservable individual characteristics (x_i), while θ is a set of parameters.

In deciding which savings product to use, individuals in Kenya have access to a number of institutions ranging from the formal to the informal, offering some form of savings product. I assume that individuals choose the savings products that give them the highest utility. Yet, as one can not observe the utility derived from the savings products and as information on product pricing and interest rates are missing from the FinAccess 2016 dataset, one is left only observing the outcome of the individual's choice.

In order to separate out the impact of income uncertainty on savings products usage, this paper specifies a simple usage function that builds on the components of the utility function where households face a decision on where to store their liquid wealth. This decision is dependent, as discussed above, on individual and institutional characteristics and could change as new types of products enter the market. Also critical, is how this usage is affected by the future income uncertainty of the individual. For those who are agriculture dependent, this is heavily influenced by recent climate shocks and the variability of the climate they live in.

I exploit the exogenous rainfall variation across the regions in Kenya to assess whether a recent change in the CV for rainfall influences the probability that an individual uses savings devices that are regulated, credit enabling and provide easy access to stored wealth. I estimate equation 2.2 as follows:

$$(Usage_{ij}) = \beta_1(\Delta CV_i) + \beta_2(NShock_i) + \beta_3(PShock_i) + X_i\delta + Z_{ij}\gamma + \eta_{ij} + e_{ij} \tag{2.2}$$

where ($Usage_{ij}$) is a dummy indicator for whether individual i uses savings product j; ΔCV_i is the change between the two most recent CV values for individual i;[18] $NShock_i$ is a dummy variable

[18] This change in CV is the difference between the CV value from the most recent three year period and the CV value for the three years prior to that.

for whether the individual i experienced a negative rainfall shock in the last 12 months;[19] $PShock_i$ is a dummy variable for whether the individual i experienced an excessive rainfall shock in the last 12 months;[20] X_i are individual specific control variables including income, age, gender and household size; Z_{ij} are institutional controls for product j for individual i including whether the financial provider is the most trusted financial service provider and if it is the closest financial service provider; η_{ij} are location dummies including dummies on access to public infrastructure and regional dummies; and finally e_{ij} is the statistical noise term.

A number of different multivariate probit regressions are run to estimate equation 2.2, where the dependent variable is a cohort of savings devices grouped according to three characteristics, regulation, credit access and ease of liquidity, as laid out in Table 2.2.[21] These regressions help capture the effect of the change in climate variability on the usage of products in these groupings. It also enables us to analyse the relationship between using different types of grouped savings devices and the identified climate, institution and individual characteristics for the FinAccess 2016 survey respondents. The results will help estimate the likelihood of storing wealth in particular types of savings device (such as credit enabling savings products versus liquid savings products) and help identify key drivers of use.

The paper then estimates equation 2.2 for each individual savings device. The reason for this is that as Table 2.1 shows, each savings device differs slightly on the level of risk they present to users, the liquidity of stored wealth and their ability to facilitate credit. This provides a deeper understanding on how future income uncertainty is driving usage across the different devices. This will also help identify whether any particular savings device is driving the results of the grouped categories.

In terms of the independent variables, the main interest lies in the coefficient β_1 in equation 2.2, which captures the effect of a change in the variability of rainfall on the likelihood of using a particular savings device. This is important because in making a decision on what savings product to use, individuals do not know if they will experience an income shock in the future. Yet, for those dependent on agriculture, they know the most recent average level of rainfall over a particular period,

[19] A negative shock is defined if for any months during either rainy season the individual received total rainfall for that month that was less than one standard deviation from the long-run average of rainfall for that month that the individually usually received. These values are generated from the rainfall data.

[20] A positive shock is defined if for any months during either rainy season, the individual received total rainfall for that month that was more than 2 standard deviations above the long-run average of rainfall for that month that the individually usually received. The reason a positive shock has to be bigger to register than a negative one is that I assume that unlike less rainfall, receiving more rainfall is to the farmers advantage up to some level. I assume the crop threshold after which it suffers is lower for receiving less rainfall. Again these values are generated from the rainfall data.

[21] From Table 2.2 one can see that four of these products appear in groups 1 to 3. I still analyse these three groups separately as they vary by at least one product. In the case of regulated products, given that 37 percent of agriculture dependent individuals report saving with mobile money, this is then a different product usage that I am trying to estimate compared to the credit grouping of savings products.

as well as the variability of its distribution. Combining this information creates the Coefficient of Variation (CV).

As laid out earlier, I use a change between two recent CV values for the individual for the long-rains, short-rains and annual rainfall as the measure of a change in rainfall variability.[22] This enables us to investigate why agriculture dependent individuals facing an exogenous change in income uncertainty use particular types of savings products.[23] I expect both the long and short rain CV measures to be important for savings product usage. This is due to the fact that both rainy seasons are important for agriculture productivity. I do not expect the CV measure for the annual rainfall to be important, as it doesn't reflect the seasonality for the agriculture seasons.

Recent income shocks are also important for the usage of particular savings products. Both β_2 and β_3 capture the effects of a recent (the 12 months prior) negative income shock on the use of savings products, as they reflect deviations in rainfall from the mean monthly rainfall during the rainy seasons. I control for these deviations in order to separate the effects of these income shocks from future income uncertainty.

The inclusion of individual characteristics in X_i and institutional characteristics in Z_{ij} control for any factors related to the individual and the device that may be driving the usage of a specific savings device. In particular, controlling for an individual's income is important because it enables us to look at the effect of future income uncertainty, proxied by a change in climate variability, separate to the the effect of current income.

In addition, I control for regional factors that maybe influencing the use of any particular savings device through the inclusion of η_{ij}. A specific concern here is the role of market access, given that this paper focuses on how climate variability affects agriculture dependent individuals. Those who are closer to big markets and agriculture buyers may be more likely to diversity and commercialise, therefore unless this is controlled for, the result could be due to omitted variable bias.

While recognising that market access variables are context dependent and that it is hard to capture all dimensions of market access, this paper attempts to address potential omitted variable bias through including two different dimensions of market access. First, region level dummies are used to control for location effects, with Nairobi as the base dummy. This partially captures this concept of

[22]The cumulative rainfall for each of these seasons is calculated for each year of data, as well as the annual rainfall figure. Then the average rainfall and standard deviation for each of the seasons is calculated. Then the CV for three year periods are calculated. As a robustness check, the long run CV value is also calculated using data from 2002 to 2014. The 12-month period before the FinAccess 2016 survey was fielded is left out. The non-rainy months are: January, February, June, July, August, and September. The long rainy season months are: March, April, May. The short rainy season months are: October, November, and December.

[23]This change in CV may affect users behaviour on savings product usage, it is less likely to affect the placement of savings products by suppliers. Therefore, it enables one to look more clearly at how income variability, proxied by the variability of rainfall, affects the demand for savings products.

market access as almost all main agricultural firms are located in Nairobi (Place et al. 2006). Farmers based in locations further away from Nairobi and therefore further away from the main agriculture firms have a lower level of market access. Second, distance to public infrastructure, proxied by distance to secondary schools from the FinAccess 2016 survey, captures another dimension of market access. The further away a respondent is from a public structure, the more remote the respondent is assumed to be. Important for this proxy is the finding of Escobal and Torero (2005), that access to primary and secondary schools affected household welfare in rural Peru. Though a recent study on Kenya points out that controlling for market access may not be an issue, as regardless of whether a village is remote or accessible, the number of traders buying grain directly in the village is the same across both groups (Chamberlin and Jayne 2013).

2.6 Results and discussion

In this analysis, I look at how increasing climate variability affects the types of savings devices individuals use. To do this the paper estimates equation 2.2 to determine whether agriculture dependent individuals, who saw an increase between their two most recent CV values from the two rainy seasons, are more likely to use saving products that are credit enabling, regulated or liquid.[24] Each model is estimated controlling for individual and location characteristics. The standard errors are clustered at the sub-location level and take into account the stratification and the weighting used in the survey sample design.[25]

2.6.1 Future income uncertainty and the use of different types of savings products for agriculture dependent individuals

In Table 2.6, I look at how a change in the recent climate variability faced by an agriculture dependent individual influences the probability of the respondent using savings products that are credit-enabling, regulated and ease access to stored wealth. From column 1 in Table 2.6, one can see that there is no significant relationship between increased variability in either rainy season and the likelihood of an individual using any saving device. Yet, as discussed previously, each savings device differs on their level of regulation, credit access and liquidity. Simply looking at whether any savings device is being used could mask more nuanced relationships between the particular characteristics of savings devices and income variability.

[24] I define it liquid when accessing stored wealth is quick and relatively easy

[25] The sub-location level is two levels above the household level to account for the fact households within a particular area will share rainfall values.

Table 2.6: Probit Estimates for Grouped Savings Devices, FSD survey 2016

VARIABLES	(1) Save Any Device	(2) AllSaving WCredit	(3) RegulatedSaving WCredit	(4) Regulated Saving	(5) Informal Saving	(6) Liquid Saving
lnIncome	0.044***	0.080***	0.084***	0.080***	0.033***	0.035***
	(0.007)	(0.010)	(0.010)	(0.012)	(0.010)	(0.011)
RecentLRCVDif	-0.077	-0.069	0.039	0.004	-0.202	0.070
	(0.086)	(0.169)	(0.181)	(0.155)	(0.132)	(0.116)
RecentSRCVDif	0.063	0.264*	0.376**	0.397***	-0.093	-0.128
	(0.102)	(0.148)	(0.146)	(0.142)	(0.158)	(0.122)
NWeatherShock12mon	-0.076***	-0.056	-0.087**	-0.062	-0.069*	-0.049
	(0.027)	(0.038)	(0.037)	(0.039)	(0.037)	(0.030)
PWeatherShock12mon	0.040**	-0.031	-0.014	-0.002	0.028	0.051*
	(0.019)	(0.031)	(0.032)	(0.032)	(0.029)	(0.028)
Observations	3,269	3,269	3,269	3,269	3,269	3,269
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Location Controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The main findings from Table 2.6 are that an increase in the climate variability of the long and short rainy seasons increased the likelihood of using regulated savings products that were credit enabling. This positive relationship is consistent for both rainy seasons, though only significant for the short rainy season. This result supports both *hypothesis 1* and *2*. What this suggests is that *ex ante* beliefs about future income uncertainty, proxied by an increase in climate variability, increases the likelihood of using savings devices that offers access to credit but that is also less risky (because of having some form of formal regulation governing it).

It is important to note that these two characteristics are individually significant for the short rainy season. For credit enabling savings products, this result is significant at the 10 percent level, while for regulated savings products at the 1 percent level. This relationship for regulated savings products is consistent for the long rainy season, but is not significant. For credit enabling savings products there is a negative but again a non significant relationship.

No significant evidence from these estimations are found for *hypothesis 3* regarding the use of liquid savings products in the face of increased climate variability.

Next, I investigate whether any individual savings product may be driving this result. If these results are due solely to one product, then the product may better explain the result versus the characteristics proposed by the various hypotheses. Conversely, I also want to confirm that the relationship between these characteristics, credit-enabling, regulation and liquidity, and the use of a savings product with these characteristics hold at the individual level.

The multivariate probit results from the usage of individual savings devices in Table 2.7 are in line with *hypothesis 1 & 2*, individuals with greater climate variability use savings devices that are credit enabling but that also have some form of regulatory oversight. An increase in climate variability, both in the short and long rainy seasons, increases the likelihood of savings with formal bank products, this result is significant for the short rainy season at the 5 percent level. Holding all else constant, an increase in climate variability by a value of 0.01 would increase the probability of using a formal bank saving product by 0.0023. An increase in the rainfall variability in the short rainy season also leads to a similar increase in using SACCOs for savings, which is also significant at the 5 percent level. Both these products are regulated and also can enable credit.

Table 2.7: Probit Estimates for Individual Savings Devices, FSD survey 2016

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Formal Savings	SACCO	Mobile Bank	Mobile Money	Secret Place	ROSCA	ASCA	Family & Friends
lnIncome	0.071***	0.013***	0.019***	0.052***	0.011	0.037***	0.016**	0.021***
	(0.008)	(0.005)	(0.005)	(0.011)	(0.009)	(0.011)	(0.007)	(0.006)
RecentLRCVDif	0.119	-0.128	0.078	0.048	0.020	-0.142	-0.130	0.076
	(0.117)	(0.091)	(0.051)	(0.127)	(0.118)	(0.140)	(0.113)	(0.071)
RecentSRCVDif	0.192*	0.207**	0.011	0.094	-0.211	0.120	0.008	-0.031
	(0.098)	(0.083)	(0.047)	(0.121)	(0.130)	(0.152)	(0.099)	(0.077)
NWeatherShock12mon	-0.048**	-0.002	0.004	-0.012	-0.025	-0.047	0.025	-0.033*
	(0.021)	(0.021)	(0.012)	(0.030)	(0.027)	(0.033)	(0.026)	(0.018)
PWeatherShock12mon	-0.036*	0.016	-0.010	-0.003	0.023	-0.025	-0.031	0.007
	(0.019)	(0.018)	(0.010)	(0.027)	(0.028)	(0.027)	(0.020)	(0.018)
Observations	3,269	3,269	3,015	3,269	3,269	3,269	3,015	3,269
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institutional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

There is also some initial evidence for the alternative to *hypothesis 3* on the use of more liquid savings devices. Increased variability of future income, proxied by a change in climate variability, decreases (not increases) the likelihood of saving in a secret place. There could be a number of reasons for this negative relationship, including that not only are these savings more easily accessible to the person saving, but also to others they are connected with. In a case of a covariate shock, it may be hard to ring fence resources from others in their individual network.

What these results suggest, is that for agriculture dependent individuals, in the face of increasing income variability access to savings products that protect liquid wealth through regulating the product, while facilitating access to credit are important in helping them manage future income shocks. Furthermore, helping individuals' ring-fence savings may be important and this would be in line with the body of literature on commitment mechanisms for encouraging savings (Dupas and Robinson 2013).

Overall, the findings show that an increase in future income uncertainty, proxied by climate variability in the short and long rainy seasons, increases the likelihood of using more regulated savings products that are also credit enabling, such as banks and SACCOs.[26] This result suggests that a

[26] An increase in climate variability means that the variability of the climate is rising.

recent increase in climate variability results in the increased use of savings products that are institutionally less risky and enable credit access. Another key finding of the paper is that increased variability decreases the probability of saving in products where liquid wealth is easily accessible. This could imply that savings which are accessible to an individual are also easily accessible to those in their network. As climate variability is covariate in nature, individuals facing increased variability may protect themselves by decreasing their use of savings in such products.

2.7 Robustness checks

In order to ensure that these findings are robust to how I specify the variables and also to different time periods, a number of checks are carried out to test the validity of the results discussed above. First, I rerun the main regressions specified above with the 2013 FinAccess data to see if these relationships hold for different time periods. Second, I use the CV measure for annual rainfall versus the CV for the two rainy seasons. If variability in rainfall is a key determinant of income variability for agriculture dependent individuals, then it is the variability of the rainy seasons that should matter and not the average rainfall for the year. Third, I estimate equation 2.2 for non-agriculture dependent individuals, as one would expect climate variability not to affect the savings product usage of individuals who have occupations outside of agriculture.

Fourth, I run two additional specifications to address the potential collinearity between income and recent climate related income shocks. In the first, I drop the recent climate shock variable from the regressions. In the second, I leave recent climate shocks in but replace the income variable with an index of assets owned by the individual. I am looking to see if these changes affect the size and significance of the results.

Fifth, I check the robustness of the CV measure by testing whether varying the timeframe for the base CV value affects the results. For this, I calculate the difference between the most recent CV value and the prior 10 year CV value.[27] I also run the regressions for the long run CV value, this is to check whether it is the level or the change in CV value which is driving these results. Finally, I change the time frame for calculating the change in the CV value, instead of the three year specification I use a four year specification. I use the difference between the CV for the four most recent years and the CV of the four years prior to that to see if this changes the results.

[27] The most recent CV value is based on the most recent three years rainfall values for the respective rainy season, while the prior CV value is based on 10 years of rainfall values for the respective rainy season.

2.7.1 Repeat Analysis for the Previous Round of FinAccess

First, I reestimate equation 2.2 using data from FinAccess 2013 merged with rainfall data from FEWSNET.[28] What I find from the results is that increased income uncertainty, proxied by climate variability, increases the likelihood of using savings products that are regulated and credit-enabling. These results confirm the findings from the 2016 data.

From Table 2.8, when looking broadly at the use of any savings device in the face of increased climate variability, there is still no significant relationship. Similar to the 2016 data, it is only once savings products are grouped together based on their characteristics of credit-enabling, regulation and liquidity that the relationship emerges between income uncertainty and the types of savings products used. The consistent relationship across both datasets is the relationship between a change in rainfall variability and the use of savings products that are both regulated and credit enabling. This result suggests that this combination of regulated and credit-enabling is also an important factor for individuals from the 2013 dataset. This relationship is positive for both rainy seasons, where an increase in variability increases the likelihood of an individual reporting the usage of these types of savings products. Interestingly though, for the 2013 data this is only significant for the long-rainy season at the 10 percent level but not for the short rainy season.

For the long rainy season, both these two characteristics of regulation and credit-enabling are individually important. The relationship between regulated savings products and increased climate uncertainty is positive for the long rainy season and significant at the 5 percent level. For the 2013 data, the relationship has become negative for the short rainy season but is not significant. For saving with any device that could facilitate credit, this relationship is now positive and significant for the long rainy season at the 10 percent level.

Finally, the results from the 2013 data suggests a negative relationship between increased climate variability and using liquid savings products. While an increase in the CV value for the short rainy season had a negative but non-significant relationship with the use of liquid savings devices, in the 2013 data this relationship seems to be stronger and is also significant at the 5 percent level. So it would seem that households who face increased variation in future income uncertainty are less likely to store savings in products that make accessing these savings easy. As discussed earlier, this could be because it is not only easier to access it for themselves but also for others in their network.

2.7.2 Annual rain variability

Next, I look at the whether the change in the variability of annual rainfall affects the use of savings products for agriculture dependent individuals. One would expect that it is the variability of the

[28]I follow the same protocol for merging the rainfall data with the 2013 FinAccess data as I did for the 2016 data.

Table 2.8: Probit Estimates for Grouped Savings Devices, FSD survey 2013

VARIABLES	(1) Save Any Device	(2) AllSaving WCredit	(3) RegulatedSaving WCredit	(4) Regulated Saving	(5) Informal Saving	(6) Liquid Saving
Income	0.061***	0.082***	0.079***	0.089***	0.043***	0.047***
	(0.009)	(0.008)	(0.008)	(0.010)	(0.009)	(0.009)
RecentLRCVDif	0.112	0.266*	0.249*	0.308**	-0.053	0.034
	(0.120)	(0.140)	(0.135)	(0.148)	(0.136)	(0.139)
RecentSRCVDif	-0.032	0.041	0.070	-0.156	-0.139	-0.207**
	(0.096)	(0.088)	(0.086)	(0.110)	(0.107)	(0.105)
NWeatherShock12mon	-0.015	-0.029	-0.037	-0.053*	0.003	-0.030
	(0.024)	(0.025)	(0.023)	(0.028)	(0.029)	(0.028)
PWeatherShock12mon	-0.035	-0.075***	-0.077***	-0.049*	-0.027	-0.038
	(0.024)	(0.024)	(0.023)	(0.027)	(0.027)	(0.027)
Observations	3,281	3,281	3,281	3,281	3,281	3,281
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Location Controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

rainy seasons that is important for the income variability of agriculture dependent individuals, not the CV of yearly rainfall. This is because planting seasons are based around the timing of the rainy seasons. Therefore, rainfall occurring outside of these seasons should have less influence on the crop cycle, especially if it comes in the lean seasons, and therefore the future income variability of these individuals.

When I estimate equation 2.2 for the use of grouped savings products, I find no significant relationships. From Table 2.12, one can see that a recent change in the variability of annual rainfall has no significant effect on the usage of any grouped savings devices. This finding would support the idea that it is the variability of the rainy seasons linked to crop cycles that are driving the results.

2.7.3 The effect of variability in rainfall for non-agriculture dependent individuals

The next step in the robustness checks is to rerun all the estimations for all non-agriculture dependent individuals.[29] One would expect future income uncertainty for non-agriculture related occupations, such as working for the Government or in manufacturing, not to be dependent on climate variability. As a result, climate variability in the rainy seasons should not affect where these individuals save.

From Table 2.9, as expected, one can see that an increase in the climate variability of either rainy season has no significant effect on most of the grouped savings products. However, it does decrease

[29]For this section I refer to all non-agriculture individuals who live in both rural and urban areas. I run the same regressions for non-agriculture individuals in rural areas and find similar results.

Table 2.9: Probit Estimates for Grouped Savings Devices, Non Agriculture Dependent Individuals, FSD survey 2016

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Save Any Device	AllSaving WCredit	RegulatedSaving WCredit	Regulated Saving	Informal Saving	Liquid Saving
lnIncome	0.060***	0.157***	0.164***	0.135***	0.044***	0.047***
	(0.006)	(0.012)	(0.014)	(0.011)	(0.008)	(0.008)
RecentLRCVDif	-0.080	0.144	0.199	-0.080	-0.151	-0.228**
	(0.068)	(0.135)	(0.135)	(0.101)	(0.126)	(0.109)
RecentSRCVDif	-0.034	0.088	0.119	0.014	-0.074	-0.150
	(0.060)	(0.134)	(0.136)	(0.109)	(0.108)	(0.103)
NWeatherShock12mon	0.003	0.031	-0.009	-0.029	0.050*	0.012
	(0.018)	(0.038)	(0.041)	(0.031)	(0.027)	(0.029)
PWeatherShock12mon	0.003	-0.028	-0.040	-0.019	0.035	0.013
	(0.016)	(0.034)	(0.035)	(0.028)	(0.025)	(0.026)
Observations	4,665	4,665	4,665	4,665	4,665	4,665
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Location Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Only individuals who are NOT dependent on agriculture are included in this analysis. This means that agriculture is not the primary or secondary source of their income. RecentLRCVDif refers to a recent change in the long rainy season CV, while RecentSRCVDif refers to a recent change in the short rainy season CV. A three year time frame is used to calculate the CV. Reported here are the marginal coefficients from the probit regression. This is calculated at a baseline for where the predictor variables equal their mean value. Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

the probability of these individuals using more liquid saving products, which is similar to the finding for agriculture dependent individuals. In line with the previous reasoning, for individuals in an area that experiences similar variability, liquid savings are not only more easily accessible to the individual saving but potentially to others in their network who may make financial requests on them. So while this individual may not be agriculture dependent, others in their network may be agriculture dependent and may have financial requests from these non-agriculture dependent individuals. Therefore, an increase in future income variability of their network could also increase their income variability through the expectation of increased financial obligations, which then affects whether they use more liquid savings products.

Interestingly, when investigating further and looking at the relationship between the usage of individual savings products and increased climate variability, from Table 2.13, one can see that increased climate variability in both rainy seasons for non-agriculture dependent individuals decreases the likelihood of these individuals using mobile money as a savings product. This result is significant at the 1 percent level for the long rainy season and at 5 percent for the short rainy season. Given that mobile money provides easy access to stored wealth and is a key remittance mechanism, this may help explain this negative relationship. Tools like mobile money provide easier and more efficient ways for households to manage shocks by accessing a wider network of social support (Suri and Jack 2016).

I argue that this could be due to these non-agriculture dependent individuals having agriculture dependent individuals in their network. Therefore, increased climate variability increases their future income uncertainty due to the potential demands put on their stored wealth by those in their network. They respond by decreasing their likelihood of using savings products that make remitting these saved resources easier. Interestingly, the other significant relationship, which may give further evidence to this network demand relationship, is that increased variability in the long rainy season decreases the likelihood of saving with family and friends. This is significant at the 5 percent level.

2.7.4 Income and income shocks

Income is one of the biggest and most significant determinants of financial product usage. There are two concerns with the specification, first that households who experienced income shocks, proxied by climate shocks, in the last twelve months, had their most recent income affected by the shocks. So recent income shocks is affecting not only the product usage, but also the reported income. Second, income in itself maybe affected by product usage. So households with higher incomes maybe more likely to use a particular product, but the use of these products may also lead to higher incomes. To address these two potential concerns I run two checks. I first rerun the regressions without controlling for recent climate shocks, second, I rerun the regressions but replace income with an index of the total assets owned by the individual. From Table 2.10 and 2.14, what one can see is that the results hold. While there is a slight decrease in the coefficient size, the sign of the coefficients do not change and the results remain significant.

Table 2.10: Probit Estimates for Grouped Savings Devices, without Income Shocks, FSD survey 2016

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Save Any Device	AllSaving WCredit	RegulatedSaving WCredit	Regulated Saving	Informal Saving	Liquid Saving
lnIncome	0.052***	0.079***	0.083***	0.080***	0.033***	0.036***
	(0.007)	(0.010)	(0.010)	(0.012)	(0.010)	(0.011)
RecentLRCVDif	-0.074	-0.016	0.086	0.026	-0.184	0.054
	(0.087)	(0.170)	(0.185)	(0.152)	(0.128)	(0.114)
RecentSRCVDif	0.035	0.240*	0.309**	0.353***	-0.163	-0.196
	(0.100)	(0.142)	(0.137)	(0.134)	(0.159)	(0.123)
Observations	3,269	3,269	3,269	3,269	3,269	3,269
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Location Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Only individuals dependent on agriculture are included in this analysis. RecentLRCVDif refers to a recent change in the long rainy season CV, while RecentSRCVDif refers to a recent change in the short rainy season CV. A three year time frame is used to calculate the CV. Recent rainfall deviations, i.e. rainfall shocks in the prior twelve months, from the long run average rainfall is not controlled for. Reported here are the marginal coefficients from the probit regression. This is calculated at a baseline for where the predictor variables equal their mean value. Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

2.7.5 The time frame of the CV value

Another important check is to see whether the results are being driven by how I calculate the CV value. One question lies on how one defines the base CV value, in particular would having a different time frame for the base CV value affect the results. In the current regressions, the base CV value is calculated using the three year time period prior to the most recent three year CV value. For instance, for the long rainy seasons the base CV is calculated using the data from the long rainy season for 2009 to 2011, while the recent CV is calculated using 2012 - 2014 rainfall data.

Given that I have rainfall data from 2002, I use a base CV value calculated on a longer time frame. For these regressions, I calculate the base CV value for the long rainy season from 2002 to 2011, while for the short rainy season and annual rainfall I calculate it from 2002 to 2010. I then calculate the change between the recent CV value and the longer run base. I use this difference in the specifications to estimate the probit regressions. What I find in Table 2.11 is that the main results hold, if anything the coefficients become slightly larger and more significant. In addition, there is now a well defined negative relationship between increased variability in the long rainy season and informal savings products. This is in line with *hypothesis 2*, where individuals move away from using unregulated products.

Table 2.11: Probit Estimates for Grouped Savings Devices using a Long run CV base value, FSD survey 2016

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Save Any Device	AllSaving WCredit	RegulatedSaving WCredit	Regulated Saving	Informal Saving	Liquid Saving
lnIncome	0.052***	0.094***	0.098***	0.099***	0.037***	0.043***
	(0.007)	(0.010)	(0.010)	(0.011)	(0.010)	(0.011)
CVLongRainDifLong	-0.038	0.041	0.253	0.176	-0.378**	-0.054
	(0.118)	(0.180)	(0.178)	(0.171)	(0.177)	(0.152)
CVShortRainDifLong	0.075	0.258**	0.359***	0.369***	-0.090	-0.091
	(0.065)	(0.110)	(0.114)	(0.094)	(0.114)	(0.085)
NWeatherShock12mon	-0.075***	-0.054	-0.084**	-0.050	-0.073*	-0.048
	(0.028)	(0.040)	(0.038)	(0.039)	(0.038)	(0.031)
PWeatherShock12mon	0.041**	-0.035	-0.021	-0.012	0.034	0.046*
	(0.019)	(0.032)	(0.033)	(0.031)	(0.028)	(0.028)
Observations	3,269	3,269	3,269	3,269	3,269	3,269
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Location Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Only individuals dependent on agriculture are included in this analysis. CVLongRainDifLong refers to a change between the most recent long rainy season CV value, based on three years of rainfall measures, from the long run value of the CV, calculated based on 10 years of rainfall figues for the long rainy season months. Similarly, the CVShortRainDifLong refers to a change between the most recent short rainy season CV value, based on three years of rainfall measures, from the long run value of the CV, calculated based on 10 years of rainfall figues for the short rainy season months.Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Another concern is whether one is looking at the right variable, the change in the CV value versus using the long term CV value. I argue that it is the change in CV which is important and not the level affect in influencing product usage. I run the empirical specifications with the level value of the CV and in Table 2.15 find that the level effect on the usage of the different types of savings devices is predominantly insignificant. The one key relationship I do find is that individuals in areas with higher climate variability in the long rainy season are less likely to use savings devices that are credit-enabling and this is significant at the 10 percent level. This result, I argue may actually reflect financial provider placement versus user decisions. So credit-enabling savings providers may be less likely to locate in areas that have more climate variability, therefore it is not surprising that the individuals in these areas are less likely to use such savings devices.

Finally, the time frame for calculating each CV block is a three year period. One concern could be that it is this timeframe that is driving the results versus the variability itself. I therefore recalculate the CV values for a four year time period and take the change in CV between the most recent CV value and the four year period before that. In Table 2.16, one can see that the results hold and if anything the coefficients become slightly larger and the level of significance increases. It important to note though, that now increased variability in the long rainy season has a negative and significant relationship with regulated products. Though this effect is smaller and at a lower level of significance than the effect of increased variability in the short rainy season.

In summary, one can see from the robustness checks that the rainy season variability is an important influence in what savings products agriculture dependent individuals use. Households who face an increase in climate variability are more likely to report using savings products that facilitate credit and that are less risky. This combination of features together seem to be important for individuals facing further income uncertainty. These results hold even when one looks at different time periods or uses different time frames to measure the rainfall variability.

2.8 Conclusion

Exposure to weather uncertainty makes agriculture dependent individuals vulnerable to income shocks (Morduch 1995). Access to the right financial tools can play a critical role in helping manage this uncertainty. Through tools that facilitate saving, borrowing or insurance, these individuals are better able to cope with any shocks they face in the future.

The motivation behind this paper was to understand what role *ex ante* beliefs on future income uncertainty has on financial decision-making. By introducing future income uncertainty, proxied by climate variability, this paper shows the importance of *ex ante* future income uncertainty beliefs on where agriculture dependent individuals save. Interestingly, it also shows how climate variability may affect where non-agriculture dependent individuals save.

This paper utilises data from Kenya to show that individuals who faced increased climate variability were more likely to use savings products that were institutionally less risky and that enable credit access. Through using FinAccess 2016, a nationally representative sample with data from 8,665 individuals, merged with satellite rainfall estimates, this paper analysed how rainfall variability influences an individual's financial decision-making. It uses a multivariate probit model to analysis agriculture-dependent individuals decisions on what types of savings device to use for storing their liquid wealth.

The main finding from the analysis is that *ex ante*, future income uncertainty beliefs increase the likelihood of using savings devices that are credit enabling and regulated, i.e. savings with formal banks and SACCOs, which provides support for *hypothesis 1* and *2*. In addition, in regards to *hypothesis 3*, this chapter finds evidence for the alternative proposed effect, where increased climate variability decreases, instead of increasing, the likelihood of using savings products that are more liquid, i.e. where saved wealth is more accessible. One reason for this is that such devices are not only more accessible to the individual but also to others in their network. This reasoning seems to be supported by the fact that this result also holds for non-agriculture dependent individuals.

One important constraint for this analysis is the lack of individual level panel data. While I use nationally representative cross-sectional data, I am able to incorporate a factor of variability through the rainfall data in estimating the relationships. Through the use of the rainfall data over a 12 year period, I create a change in the CV value that introduces an element of variability in the analysis.

Finally, this paper adds to the literature by showing how income uncertainty, proxied by climate variability, influences individual financial product usage. Furthermore, through showing which characteristics are driving product usage, it helps explain where households save in the face of increasing income uncertainty. The results add support to the empirical literature that regulated savings products that enable credit are important for agriculture dependent individuals who are facing greater income uncertainty in a developing country context. These devices can help households smooth consumption through the credit arm of the savings device, while also offering safeguards against the loss of savings stored in the device. Therefore, initiatives that help increase access to such savings devices are important for policy makers to investigate further.

2.A Figures

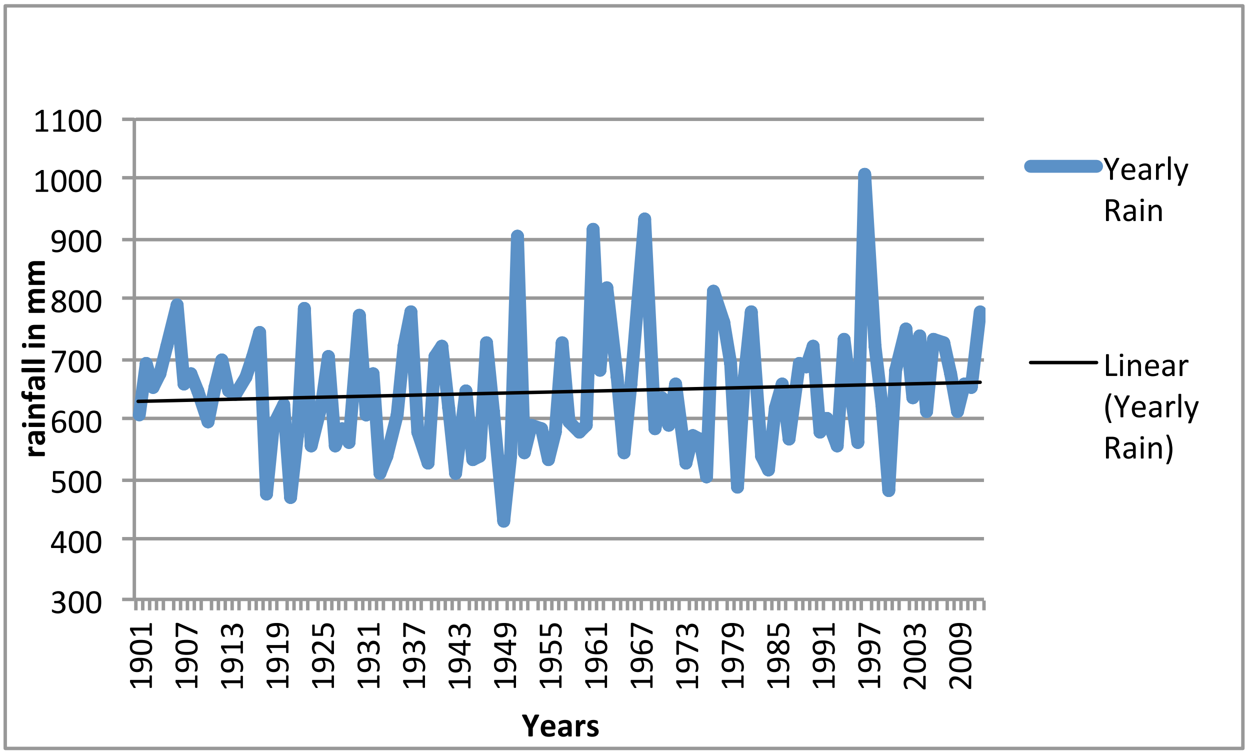


Figure 2.1: Total Yearly Rainfall in Kenya: 1901 - 2012

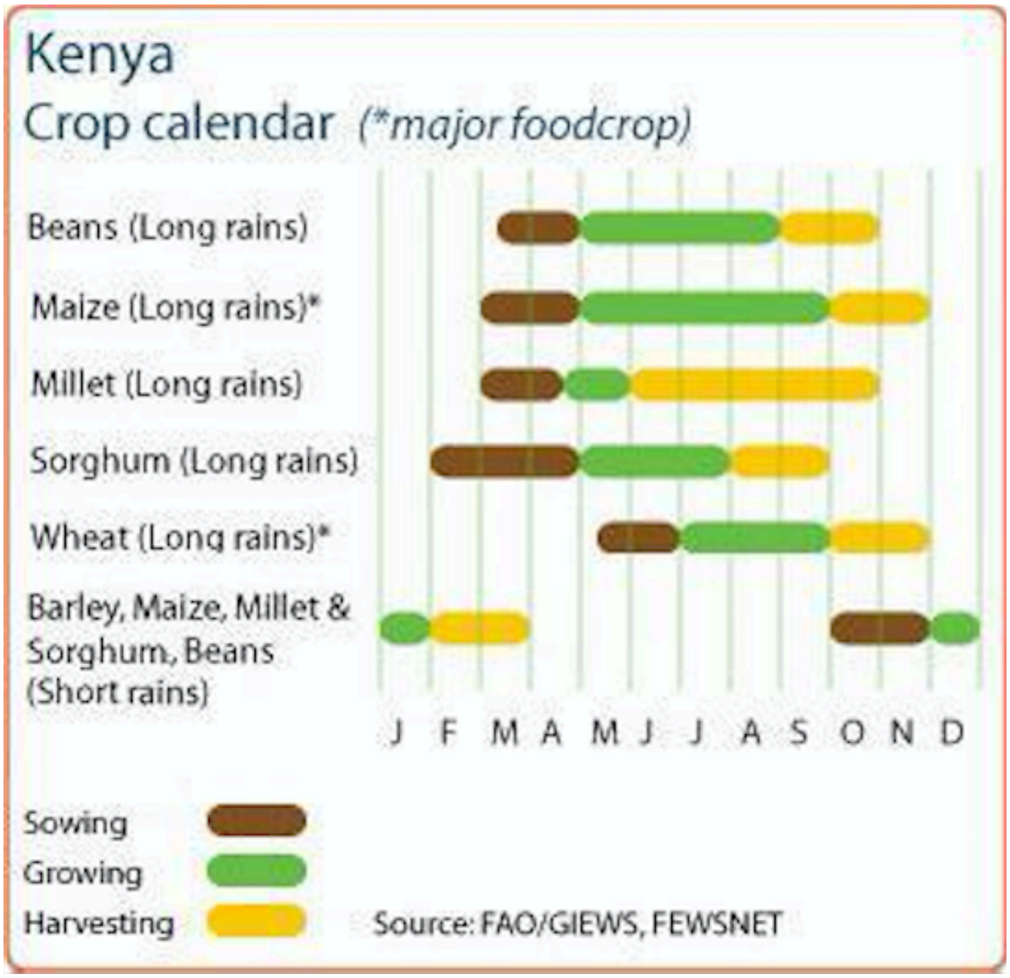
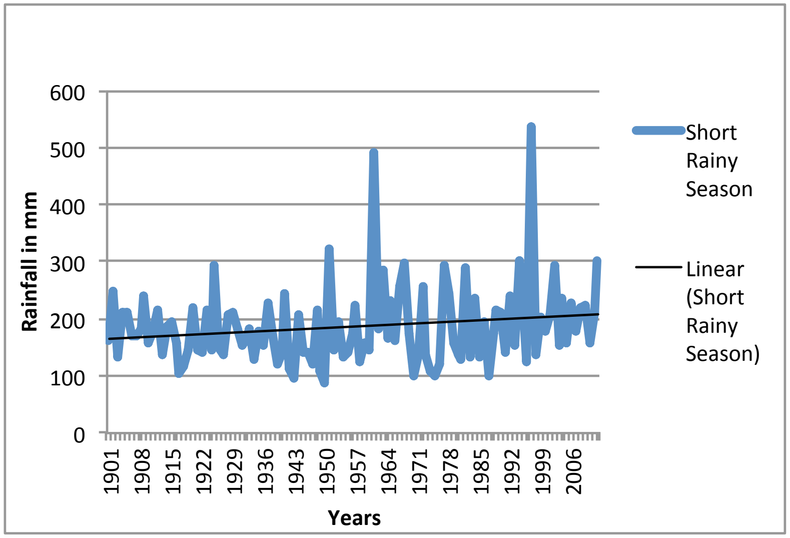
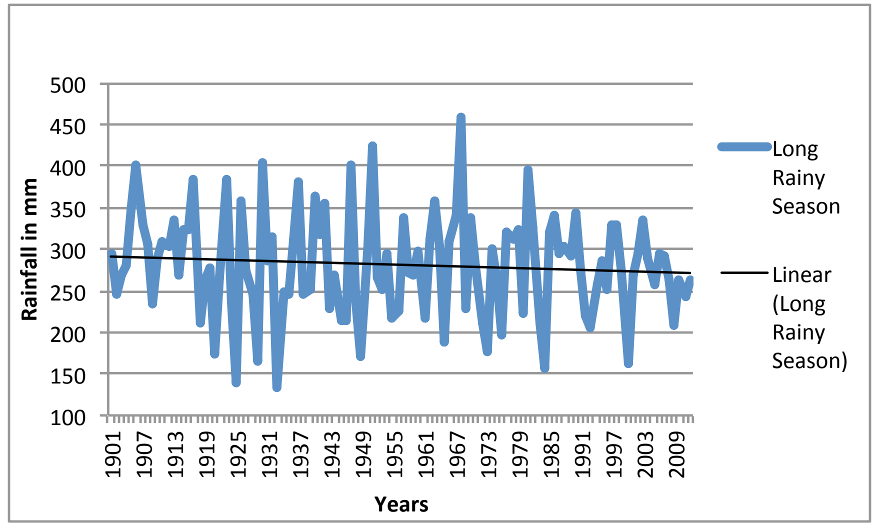


Figure 2.2: Kenya Crop Calendar

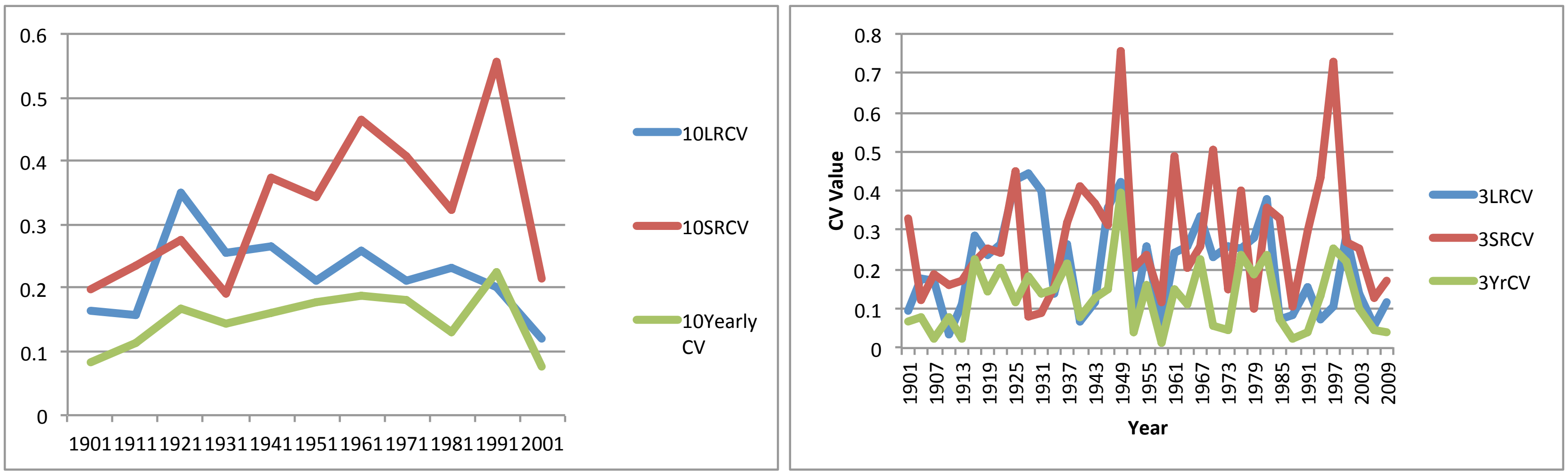


(a) Short Run Rainy Season



(b) Long Run Rainy Season

Figure 2.3: National Rainfall for the Short and the Long Rainy Seasons, 1901 - 2012



(a) CV calculated every 10 years

(b) CV calculated every 3 years

Figure 2.4: Coefficient of Variation for Short, Long and Annual Rainy Seasons, 1901 - 2012

2.B Tables

Table 2.12: Probit Estimates for Grouped Savings Devices, Yearly rainfall, FSD survey 2016

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Save Any Device	AllSaving WCredit	RegulatedSaving WCredit	Regulated Saving	Informal Saving	Liquid Saving
lnIncome	0.052***	0.093***	0.096***	0.097***	0.038***	0.043***
	(0.007)	(0.010)	(0.010)	(0.011)	(0.010)	(0.011)
RecentYrCVDif	0.086	-0.250	-0.201	-0.177	0.317	0.095
	(0.148)	(0.259)	(0.268)	(0.213)	(0.231)	(0.205)
NWeatherShock12mon	-0.067**	-0.038	-0.066*	-0.030	-0.062	-0.051*
	(0.029)	(0.041)	(0.039)	(0.038)	(0.038)	(0.031)
PWeatherShock12mon	0.039**	-0.043	-0.030	-0.023	0.036	0.049*
	(0.019)	(0.032)	(0.033)	(0.031)	(0.028)	(0.028)
Observations	3,269	3,269	3,269	3,269	3,269	3,269
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Location Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Only individuals dependent on agriculture are included in this analysis. RecentYrCVDif refers to a recent change in the annual CV value. A three year time frame is used to calculate the annual CV value. Reported here are the marginal coefficients from the probit regression. This is calculated at a baseline for where the predictor variables equal their mean value. Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2.13: Probit Estimates for Individual Savings Devices, Non Agriculture Dependent, FSD survey 2016

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Formal Savings	SACCO	Mobile Bank	Mobile Money	Secret Place	ROSCA	ASCA	Family & Friends
lnIncome	0.132***	0.035***	0.064***	0.079***	0.007	0.054***	0.034***	0.029***
	(0.012)	(0.004)	(0.007)	(0.010)	(0.007)	(0.008)	(0.006)	(0.005)
RecentLRCVDif	0.109	-0.022	-0.043	-0.316***	-0.121	-0.051	0.046	-0.174**
	(0.129)	(0.056)	(0.079)	(0.111)	(0.117)	(0.114)	(0.080)	(0.079)
RecentSRCVDif	0.157	0.010	0.034	-0.223**	-0.085	-0.160	0.017	-0.045
	(0.119)	(0.056)	(0.084)	(0.112)	(0.110)	(0.103)	(0.073)	(0.078)
NWeatherShock12mon	-0.011	0.003	-0.012	0.004	0.051*	0.038	0.041*	-0.036*
	(0.037)	(0.013)	(0.021)	(0.031)	(0.026)	(0.032)	(0.024)	(0.020)
PWeatherShock12mon	-0.052	-0.000	0.004	-0.006	0.027	0.001	0.021	-0.007
	(0.032)	(0.014)	(0.020)	(0.028)	(0.025)	(0.024)	(0.018)	(0.016)
Observations	4,665	4,665	4,665	4,665	4,665	4,665	4,665	4,665
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institutional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Only individuals who are NOT dependent on agriculture are included in this analysis. This means that agriculture is not the primary or secondary source of their income. RecentLRCVDif refers to a recent change in the long rainy season CV, while RecentSRCVDif refers to a recent change in the short rainy season CV. A three year time frame is used to calculate the CV. Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.14: Probit Estimates for Grouped Savings Devices, Asset Index, FSD survey 2016

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Save Any Device	AllSaving WCredit	RegulatedSaving WCredit	Regulated Saving	Informal Saving	Liquid Saving
AssetLevel	0.051***	0.067***	0.063***	0.068***	0.024***	0.012*
	(0.008)	(0.011)	(0.010)	(0.010)	(0.008)	(0.007)
RecentLRCVDif	-0.047	-0.039	0.062	-0.014	-0.153	0.074
	(0.087)	(0.172)	(0.185)	(0.158)	(0.132)	(0.117)
RecentSRCVDif	0.084	0.251*	0.357**	0.393***	-0.126	-0.162
	(0.099)	(0.147)	(0.147)	(0.142)	(0.157)	(0.123)
NWeatherShock12mon	-0.083***	-0.058	-0.089**	-0.062	-0.080**	-0.058*
	(0.028)	(0.037)	(0.035)	(0.038)	(0.037)	(0.030)
PWeatherShock12mon	0.044**	-0.011	0.007	0.015	0.031	0.051*
	(0.019)	(0.030)	(0.032)	(0.031)	(0.028)	(0.029)
Observations	3,328	3,328	3,328	3,328	3,328	3,328
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Location Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Instead of income, an index of assets are created, where the lowest number in the index is 0 and the the maximum number of assets an individual could have is 22. Only individuals dependent on agriculture are included in this analysis. RecentLRCVDif refers to a recent change in the long rainy season CV, while RecentSRCVDif refers to a recent change in the short rainy season CV. A three year time frame is used to calculate the CV. Reported here are the marginal coefficients from the probit regression. This is calculated at a baseline for where the predictor variables equal their mean value. Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.15: Probit Estimates for Grouped Savings Devices, long run CV Level, FSD survey 2016

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Save Any Device	AllSaving WCredit	RegulatedSaving WCredit	Regulated Saving	Informal Saving	Liquid Saving
lnIncome	0.052***	0.092***	0.096***	0.097***	0.038***	0.043***
	(0.007)	(0.010)	(0.010)	(0.011)	(0.010)	(0.011)
CVLongRains	-0.025	-0.827*	-0.682	-0.204	0.128	-0.002
	(0.234)	(0.469)	(0.478)	(0.388)	(0.375)	(0.307)
CVShortRains	0.092	0.353	0.373	0.075	0.071	-0.079
	(0.114)	(0.260)	(0.251)	(0.187)	(0.225)	(0.153)
NWeatherShock12mon	-0.068**	-0.036	-0.064*	-0.028	-0.066*	-0.053*
	(0.028)	(0.039)	(0.037)	(0.038)	(0.038)	(0.031)
PWeatherShock12mon	0.039**	-0.046	-0.031	-0.023	0.035	0.048*
	(0.019)	(0.033)	(0.033)	(0.031)	(0.028)	(0.028)
Observations	3,269	3,269	3,269	3,269	3,269	3,269
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Location Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Only individuals dependent on agriculture are included in this analysis. CVLongRains refers to the level value of the CV calculated on 10 years of rainfall measurements from the long rainy season months, this doesn't include the 12 months prior to the survey. Similarly, the CVShortRains refers to the level value of the CV calculated on 10 years of rainfall measurements of the short rainy season months, this also doesn't include the 12 months prior to the survey. Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2.16: Probit Estimates for Grouped Savings Devices, 4 Year CV, FSD survey 2016

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Save Any Device	AllSaving WCredit	RegulatedSaving WCredit	Regulated Saving	Informal Saving	Liquid Saving
lnIncome	0.053***	0.084***	0.087***	0.083***	0.034***	0.035***
	(0.007)	(0.010)	(0.010)	(0.012)	(0.010)	(0.011)
ChLRCV4Yr	-0.083	-0.227	-0.217	-0.294*	-0.206	-0.082
	(0.115)	(0.165)	(0.186)	(0.167)	(0.180)	(0.144)
ChSRCV4Yr	0.149	0.512***	0.566***	0.451***	-0.026	-0.143
	(0.097)	(0.170)	(0.168)	(0.156)	(0.165)	(0.135)
NWeatherShock12mon	-0.078***	-0.069*	-0.097**	-0.071*	-0.075**	-0.054*
	(0.030)	(0.040)	(0.038)	(0.039)	(0.038)	(0.032)
PWeatherShock12mon	0.045**	-0.018	-0.007	0.005	0.040	0.050*
	(0.019)	(0.031)	(0.032)	(0.031)	(0.028)	(0.028)
Observations	3,269	3,269	3,269	3,269	3,269	3,269
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Location Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Only individuals dependent on agriculture are included in this analysis. ChLRCV4Yr refers to a recent change in the long rainy season CV, while ChSRCV4Yr refers to a recent change in the short rainy season CV. Unlike the main specification, a four year time frame is used to calculate the CV. For example the change in variability for the long run rainy season is the difference in the CV values between 2010-2013 and 2006 - 2009. Reported here are the marginal coefficients from the probit regression. This is calculated at a baseline for where the predictor variables equal their mean value. Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix

Table 2.17: Variable definitions and summary statistics

Variable	Description	Obs	Mean	Std. Dev.	Min	Max
lnIncome	This is the log of self-reported income. In the case that someone didn't give an exact amount but the range within which their income was, the mid-point of the range was taken as their income.	8007	8.760	1.326	0	16.534
CVLongRains	This is the coefficient of variation (CV) for the long rainy season (March - May). This is calculated by dividing the standard deviation of total rainfall during the long rainy season for 2002 - 2014 by the mean rainfall for the same time period.	8586	0.185	0.045	0.105	0.415
RecentLRCVDif	This is the difference in the CV value between the most recent three year period (2012- 2014) and the three years prior to that (2009-2011) for the long rainy season.	8586	-0.006	0.106	-0.560	0.383
CVShortRains	This is the coefficient of variation (CV) for the short rainy season (October - December). This is calculated by dividing the standard deviation of total rainfall during the short rainy season for 2002 - 2013 by the mean rainfall for the same time period.	8586	0.321	0.127	0.118	1.06
RecentSRCVDif	This is the difference in the CV value between the most recent three year period (2011- 2013) and the three years prior to that (2008-2010) for the short rainy season.	8586	0.018	0.122	-0.395	0.458

Negative Rainfall Shock	This is a dummy variable that takes a value of one if a person experienced a deficit amount of rainfall during any of the rainy season months in the 12 months prior to the survey, i.e. March - May 2012. A deficit is if an individual gets one standard deviation of rainfall below the 10-year monthly average for that month.	8660	0.241	0.428	0	1
Positive Rainfall Shock	This is a dummy variable that takes a value of one if a person experienced an excess amount of rainfall during any of the rainy season months in the 12 months prior to the survey, i.e. October - December 2011. Excess rainfall is defined by whether an individual gets 2 standard deviations of rainfall above the 10-year monthly average for that month.	8660	0.291	0.454	0	1
CV non-rainy season	This is the coefficient of variation (CV) for the non-rainy season (January, February, June, July, August and September). This is calculated by dividing the standard deviation of total rainfall during the long rainy season for 2002 - 2013 by the mean rainfall for the same time period.	8586	0.201	0.106	0.073	0.950
RecentNRCVDif	This is the difference in the CV value between the most recent three year period (2011- 2013) and the three years prior to that (2008-2010) for the non rainy season.	8586	0.021	0.1452	-0.842	0.425
CVAllYear	This is the coefficient of variation (CV) for the year. This is calculated by dividing the standard deviation of total rainfall for 2002 - 2013 by the mean rainfall for the same time period.	8586	0.133	0.037	0.060	0.338
RecentYrCVDif	This is the difference in the CV value between the most recent three year period (2011- 2013) and the three years prior to that (2008-2010) for the annual rainfall.	8586	-0.006	0.073	-0.220	0.264

FSavings	Formal Savings Product: This is a dummy variable that takes the value of one if a person has: Postbank account; Bank account for savings or investment; Current account; Bank account for everyday needs; ATM/Debt Card	8124	0.286	0.452	0	1
TrustBanks	Banks are the most trusted financial provider: This is a dummy variable that takes the value of one if the person selected banks as their most trust financial provider.	8124	0.375	0.484	0	1
BankClFinProv	This is a dummy variable that takes the value of 1 if a Bank is the closest financial provider to them.	8124	0.069	0.254	0	1
mobilephone	This is a dummy variable that takes the value of one if they report having a mobile phone with.	8124	0.761	0.426	0	1
MMSave	Self-reported savings with mobile money: This is a dummy variable that takes the value of one if they report using mobile money to save with.	8124	0.404	0.491	0	1
TrustMM	Mobile Money providers are the most trusted financial provider: This is a dummy variable that takes the value of one if the person selected mobile money providers as their most trusted financial provider.	8124	0.234	0.424	0	1
MPESAClFinProv	This is a dummy variable that takes the value of 1 if a mobile money agent is the closest financial provider to them.	8124	0.765	0.424	0	1
MBankSave	Mobile Bank Account: This is a dummy variable that takes the value of one if a person has a mobile bank account like M-Shwari.	8124	0.150	0.357	0	1
TrustMBank	Mobile Bank providers are the most trusted financial provider: This is a dummy variable hat takes the value of one if the person selected mobile bank providers as their most trusted financial provider.	8124	0.025	0.156	0	1

SaveSACCO	Save with SACCO: This is a dummy variable that takes the value of one if they report savings with a SACCO.	8124	0.119	0.324	0	1
TrustSACCO	SACCOs are the most trusted financial provider: This is a dummy variable that takes the value of one if the person selected SACCOs as their most trust financial provider.					
SACCOClFinProv	This is a dummy variable that takes the value of 1 if a SACCO is the closest financial provider to them.	8124	0.013	0.112	0	1
SecretPlace	This is a dummy variable that takes the value of one if the person reports saving in a secret place.	8124	0.351	0.477	0	1
SaveWFriendsFamily	This is a dummy variable that takes the value of one if the person reports saving with friends and/or family.	8124	0.124	0.330	0	1
SaveASCA	This is a dummy variable that takes the value of one if they report savings with an ASCA.	8124	0.143	0.350	0	1
SaveROSCA	This is a dummy variable that takes the value of one if they report savings with a ROSCA.	8124	0.330	0.470	0	1
TrustROSCAASCA	ROSCAs and ASCAs are the most trusted financial provider: This is a dummy variable that takes the value of one if the person selected ROSCAs or ASCAs as their most trust financial provider.	8124	0.001	0.035	0	1
ROSCAClFinProv	This is a dummy variable that takes the value of 1 if ROSCA or ASCAs are the closest financial provider to them.	8124	0.055	0.229	0	1
CreditPlusProduct	This is a dummy variable that takes the value one if an individual reports saving with any product that facilitates credit, such as a bank product, SACCO, ASCA or mobile bank like M-Shwari	8124	0.462	0.499	0	1

RegulatedPlusProduct	This is a dummy variable that takes the value one if an individual reports saving with any product that is regulated, such as mobile money, SACCO, MFI, mobile bank or a bank product.	8124	0.566	0.496	0	1
LiquidSProducts	This is a dummy variable that takes the value one if an individual reports saving with any product where it is easy and quick to convert savings into cash in bank, such as mobile money or savings in a secret place.	8124	0.588	0.492	0	1
VGoodSSAccess	This is a dummy variable taking the value of one if a person lives up to 30 minutes away from a secondary school and 0 otherwise.	8124	0.712	.453	0	1
GoodSSAccess	This is a dummy variable taking the value of one if a person lives between 30 - 60 away from a secondary school and 0 otherwise.	8124	0.182	0.386	0	1
AvgSSAccess	This is a dummy variable taking the value of one if a person between 60 - 180 minutes away from a secondary school and 0 otherwise.	8124	0.034	0.181	0	1
BadSSAccess	This is a dummy variable taking the value of one if a person lives over 2 hours away from a secondary school and 0 otherwise.	8124	0.073	0.260	0	1
GenderD	This is a dummy variable that takes the value of 1 if a person is a woman and a value of 0 if a person is a man.	8124	0.613	0.487	0	1
age	This is the age of the respondent. Those below 18 are dropped from the analysis.	8124	38.4	16.3	18	100
age2	This is the square term of the age of the respondent	8124	1737	1563	324	10000
MinimalEducation	This is a dummy variable that takes the value of 1 if a person has no education or some primary education, otherwise it is 0.	8124	0.186	0.389	0	1

Primary	This is a dummy variable that takes the value of 1 if a person has completed primary education and/or done some secondary, otherwise it is 0.	8124	0.447	0.497	0	1
Secondary	This is a dummy variable that takes the value of 1 if a person has completed their secondary education and a value of 0 otherwise.	8124	0.267	0.442	0	1
Tertiary	This is a dummy variable that takes the value of 1 if a person has completed their university or technical degree and a value of 0 otherwise.	8124	0.100	0.300	0	1
Central	This is a dummy variable for the Central region	8124	0.10	0.31	0	1
Coast	This is a dummy variable for the Coast region	8124	0.08	0.27	0	1
UpperEastern	This is a dummy variable for the Upper Eastern region	8124	0.05	0.21	0	1
MiddleEastern	This is a dummy variable for the Middle Eastern region	8124	0.08	0.28	0	1
LowerEastern	This is a dummy variable for the Lower Eastern region	8124	0.09	0.28	0	1
NorthEastern	This is a dummy variable for the North Eastern region	8124	0.07	0.25	0	1
Mombasa	This is a dummy variable for the Mombasa region	8124	0.04	0.19	0	1
Nyzanza	This is a dummy variable for the Nyzanza region	8124	0.10	0.30	0	1
NorthRift	This is a dummy variable for the North Rift Valley region	8124	0.05	0.21	0	1
CentralRift	This is a dummy variable for the Central Rift Valley region	8124	0.11	0.31	0	1
SouthRift	This is a dummy variable for the South Rift Valley region	8124	0.089	0.28	0	1
Nairobi	This is a dummy variable for the Nairobi region	8124	0.06	0.24	0	1
Western	This is a dummy variable for the Western region	8124	0.09	0.29	0	1

Chapter 3

Access to credit and welfare outcomes: does the gender of the recipient matter?

3.1 Introduction:

In low income countries, the economic choices of poor households are often constrained by the inefficient operation of local financial markets (Banerjee and Duflo 2007). In particular, access to formal credit can be very limited, especially for poor and vulnerable groups (Ersado et al. 2003). Providing access to borrowings that can be put to productive uses, has the potential to lead to long-term economic growth by helping farmers and investors to build economies of scale in production and generate the profits necessary to lift themselves out of poverty.[1]

While there is extensive literature on access to credit, there is no consensus on the gender differentiated impact of credit on individual and household welfare. Therefore, in this paper, we explore the extent to which there are differences in the impact of credit depending on whether it is men or women that have access to loans. We exploit an exogenous change in the availability of credit for women in Viet Nam, to examine how it impacts women's behaviour and use of time. We also look at whether it leads to improved household welfare outcomes. Finally, we look at how these changes are correlated to the allocation of men's time in the household, as well as the time allocation of children.

Within poor households, the constraints to accessing credit can differ based on gender, both in terms of the extent of access and the costs associated with credit. For example, Agier and Szafarz

[1] Beck et al. (2004, 2007) found that the provision of credit is correlated with the income growth of the poor and that relaxing a credit constraint can also reduce income inequality.

(2011) found that women in Brazil receive smaller loans from Microfinance institutions than men, even though they actually incur smaller losses. In a cross-country study, Demirguc-Kunt et al. (2013) found a four-percentage point gender gap in borrowing across low and high-income countries. Aterido et al. (2013) also find that across studies, women are less likely to be financed by formal institutions and those that are, pay a higher interest rate than men.[2]

Furthermore, the decisions regarding how resources are used within households can also differ based on gender, as these decisions are determined by a number of different factors including, the preferences of individuals in the households and their bargaining power (World Bank 2012). When household members do not have the same preferences and partially pursue individual interests, the allocation of available resources is based on bargaining, where individual bargaining power determines the outcome (Manser and Brown 1980; McElroy and Horney 1981). Based on this, one would expect that external factors that change women's access to resources, including credit, should affect women's bargaining power within the household.

There are two possible mechanisms through which credit can affect the bargaining power within a household. First, increasing access to credit, as well as the supply of credit, directly provides more resources for women that can be used to increase consumption. Second, it may enable women to generate income separate to men, through enabling investments in income generation activities or financing an off-farm job (Swaminathan et al. 2010). The income earned from these activities, if controlled by the women, could increase their bargaining power within the household and therefore their agency (World Bank 2012).

If indeed men and women do use credit differently, then differences in the extent of access and level of loans may have heterogeneous effects on an individual's time allocation, how the resources are used and on household welfare outcomes. There is a large literature which suggests, for example, that resources held in the hands of women rather than men is more beneficial for household welfare, in particular the well-being of children (Duflo 2003; Pitt and Khandker 1998; Qian 2008; World Bank 2012). The findings are strongest for conditional and unconditional cash transfers (Yoong et al. 2012).

Yet, research findings on the Graduation model bring into question how these resources benefit women.[3] While women seemed to retain control of the initial asset transfer, any further assets bought from income generated from the initial transfer seem to belong to the men (Roy et al. 2015). These resources also have implications on how men and women allocate their time. For

[2]This does not appear to be unique to low-income countries. Alesina et al. (2013) find that women in Italy pay more for credit, even though they are less risky borrowers than men.

[3]The graduation model is an anti-poverty program that aims to move households out of poverty through five elements: The targeting of poor households, provision of consumption support, a saving program, skills training and regular coaching, and an asset transfer. This program is targeted to women in the household.

instance, Asadullah and Ara (2016) find that while women initially increased their time spent on self-employment activities, in the long run they seemed to slip back into spending more time carrying out household chores. Men on the other hand, seemed to increase their time on micro-enterprise activities, suggesting a take over by men on these activities (Asadullah and Ara 2016). Finally, there may also be unintended effects, such as decreasing the mobility of women outside the house (Roy et al. 2015).

If we turn to credit access, the evidence on the impact of credit on welfare is mixed and few studies consider gender differences. Angelucci et al. (2015) find that increased access to credit has a positive effect on income while Islam and Maitra (2012) find it reduces the need to sell livestock in the face of a health shock. Angelucci et al. (2015) also find that it reduces the need for households to sell assets in order to pay for existing debts. On the other hand, in the review of six randomised evaluations, Banerjee et al. (2015c) report no impact of increased access to credit on consumption expenditure but in some contexts (in this case Ethiopia) a negative impact on food security was observed. In a separate study from South Africa though, those with increased access to credit were less likely to feel hungry suggesting that credit may well be a means to reducing hunger (Karlan and Zinman 2010). Similarly, in relation to educational outcomes, predominantly most papers find no impact of increased credit access on the educational attainment of children (Banerjee et al. 2015a; Karlan and Zinman 2010). Though again, in Banerjee et al. (2015c) review paper, they highlight a study from Bosnia that found significant and negative effects on the school attendance of 16-19 year olds for households who had increased access to credit.

Similarly, the findings from the papers that focus on the impact of credit in the hands of women compared with men are inconclusive; while Pitt and Khandker (1998) found strong gender effects as a result of access to microcredit, these findings were later challenged (Yoong et al. 2012). Some evidence points to increased levels of female decision-making power in households where access to credit for women increases (Angelucci et al. 2015). On the other hand, it seems that increased access also increases the level of stress experienced by women more than that felt by men (Karlan and Zinman 2010).

There is also a question on the length of time needed for the impact of credit on individual and household welfare outcomes to be realised. Khandker and Samad (2014) find that the benefits of microcredit loans were only realised over a 20 year time-frame, where a 10 percent increase in credit access for women saw a 5 percentage point decrease in poverty. In fact, a paper by Islam et al. (2016) suggests that participation in microcredit programs may initially have a negative impact or no impact at all on food security, before having a long-run positive effect on it. Overtime, this increased access to capital may encourage more risk-taking, which could explain why initially credit in the hands of women has no effect on household welfare (Buvinic and O'Donnell 2017). It is also important to note that it takes time to pay off the loan. Given these factors, this suggests that it may take a number of years before increased access to credit positively affects individual and household welfare outcomes.

Overall, the findings from the literature suggest that men and women do face different constraints in accessing credit, which can also affect the size of their loan. Regardless of gender, this credit can affect individual behaviour, household welfare and well-being outcomes, but these results are not consistent across all studies. Very little is known on whether credit in the hands of men versus women matters for individual time allocation, household welfare and well-being outcomes. Furthermore, no study has looked at this question in the context of Viet Nam. Finally, understanding the impact of laws, such as the Gender Equality Law, is critical in helping understand how to increase financial inclusion for women. Our aim in this paper is to empirically explore each of these aspects using the illustrative example of Viet Nam and the change in the Gender Equality Law that aimed to extend access to credit for women.

In this paper, we explore whether individual and household welfare outcomes associated with access to credit and the amount of credit supplied are different if credit is held by women or men within households. We use the Vietnamese Access to Resources Household Survey (VARHS), an extensive panel dataset that tracks the same 2,108 households for the period 2008 to 2016. As well as household-level information, our data include information on the individuals within each household, as well as extensive data on the loans that individuals hold.

The recent implementation of the Gender Equality Law in Viet Nam provides an ideal case for exploring gender differentials in the impact of credit on outcomes. This law aimed to significantly expand access to credit for women in rural households, particularly among the poor and ethnic minorities. This law acts as a regime switch that enables us to identify the independent impact of access to credit, as well as the level of credit, for women on a range of household outcomes. The implementation strategy for the law, which was passed in 2007, runs from 2011 to 2020 and so using the VARHS data provides us with two periods before, and three periods after the implementation of the law, as well as the within-household variation in access to credit for men and women.

We begin our analysis by exploring empirically whether there is evidence, for our sample, that the Gender Equality Law was, in fact, implemented. We use a household fixed effects analysis to explore whether access to credit and the level of credit increased for women relative to men in the post-2010 period. We also consider whether access to and the level of credit increased for poor households and ethnic minority households.

The second part of our analysis considers how access to credit and the level of credit supplied impacts on the allocation of female time and their investment decisions on their income generating activities. To address potential endogeneity issues, we use the regime switch provided by the Gender

Equality Law to instrument the level of loans held by females by the exposure of the individual to the implementation of the Gender Equality law in the post-2010 period. An individual is considered as affected by the law if the individual is female, not the household head, and is either in a poor or an ethnic minority household.

The final part of our analysis looks at how this credit access affects household welfare (measured as household income and consumption). We analyse the effect of the relative bargaining power via loan access through looking at the relative share of formal loans in the hands of women compared to men. We create a ratio of aggregate loans held by women in the households to the aggregate loans held by men at the household level. We use household fixed effects to determine whether access to credit for women has a different effect on household welfare than access to credit for men. We address potential endogeneity issues by instrumenting for the ratio of female versus male formal loan access with the regime switch from the Gender Equality Law. A household is considered as affected by the law if there is at least one woman present, who is not the household head, and the household is either a poor or an ethnic minority household. The final part of our analysis looks at how this change in female time allocation is correlated to the time allocation of males and children in the household.

We find evidence of the implementation of the law at work in our data with women, particularly poor and ethnic women, in the post-2010 period accessing more loans through formal institutions compared with men. In our analysis, we find that access to formal loans and access to a greater supply of credit increases the number of days that women work in agriculture and home enterprise activities. We also find that this changes the inputs women use in their income generation activities. Next, we find that an increase in the relative amount of loans in the hands of women versus men negatively impacts income per capita and food expenditure in the current period. It may be the case that it will take some time before this increased credit access has a positive impact on household welfare outcomes. Though, we do find some positive correlations between increased credit access for women with child welfare outcomes. We find that this increased access for women is correlated with a decrease in the time male children spend working in agriculture activities.

The paper is structured as follows. Background to the Gender Equality Law and its implementation strategy are provided in Section 3.2. Section 3.3 presents the data and Section 3.4 the empirical approach. The results are presented in Section 3.5 and Section 3.6 concludes.

3.2 Vietnamese Context

In Southeast Asia, Viet Nam is seen as a leader on gender equality. It has made remarkable strides in tackling gender disparities. In areas such as education, data on school enrolment reflects little difference between boys and girls and Viet Nam has a decreasing gap between male and female literacy levels (Newman 2016; Wells 2005). In addition, it has one of the highest proportions of women in the parliament (Wells 2005). Women are also largely economically active with a high female-male labor participation rate at 89 per cent Viet Nam (World Bank 2015a).

Despite significant progress over the last decade, women still face a number of barriers. In fact, Viet Nam is one of the few countries that has seen a widening gender pay gap, where female salaries are around 70-80 percent of their male counterparts (ILO 2013). There is also a gap in opportunities, with men more likely to have access to vocational training than women (JICA 2011). Women also have a double role to play as not only do they have to fulfil their work duties, but they also carry out the majority of household domestic jobs (JICA 2011). As a result of this, if they run an enterprise, due to their multiple other commitments, women often lack the time they need for their enterprise (IFC 2007). They are also more likely than men to participate in lower return activities, such as agriculture and informal sectors (Wells 2005).

In terms of credit access, women face multiple constraints in accessing credit largely due to the fact that they lack credit history and collateral (IFC 2007). Even when they do access credit, often the size of the loans they can access is smaller than their needs (IFC, 2007). Compared to male entrepreneurs, women find it more challenging to access finance (IFC 2007).

To address these disparities, in 2006 Viet Nam took an important step forward in introducing its first gender law, the Gender Equality Law. Article 12 of the Law, Gender Equality in the field of economy, had important implications for credit access as it emphasised the need for equal opportunities for men and women to access capital and the need for female workers in rural areas to be given credit aid (Socialist Republic of Viet Nam 2006). Responsibility for developing and then monitoring the implementation of the law was given to the Ministry of Labor, Invalids and Social Affairs (MoLISA). It was not until 2010, however, when the implementation details of this law became clear. On December 24, 2010, Decision No. 2351/QD-TTg was passed, approving the 2011 - 2020 national strategy for Gender Equality. Objective 2 of this strategy had significant implications for women's credit access, as it specified the need 'to narrow the gender gap in the economic, labor and employment domains; to increase access to economic resources and labor market for rural poor women and ethnic minority women' (Socialist Republic of Viet Nam 2010). Norm 4 under this objective laid out the specifics of what this meant for women: 'the rate of poor female labourers in rural areas or ethnic minority regions who wish to borrow preferential capital from employment or poverty reduction programs and official credit sources will reach 80 per cent by 2015 and 100 per cent by 2020' (Socialist Republic of Viet Nam 2010).

Not only was this the first time Viet Nam had a Gender Equality Law, but it was also the first time core state funds were assigned to a gender program (UN Women 2011). Assigning a budget line to this implementation strategy signalled a political commitment to the strategy. Furthermore, if implemented as designed, this law aims to change the status quo for women and men in Viet Nam. Laws that address control or access to resources are important because they outline and formalise the national framework in which women are able to excise their agency, which can be important for improving household outcomes (World Bank 2012).

This strategy on the Gender Equality Law was an important step up from previous strategies of this kind in Viet Nam. The prior strategy (2001-2010) focused solely on female-headed households, while the 2011-2020 strategy shifted focus to women more generally. Moreover, the intensity of the targeting increased with the former aiming for access to credit for 50 per cent of women while the latter was more ambitious at 80 per cent. Finally, the 2011-2020 strategy specifically targeted poor women who live in rural or ethnic minority regions.

The implementation of this strategy is likely to have impacted which women access credit, with more credit going to poor and ethnic minority households in rural areas, how many women access formal credit, and the size of the loans that women receive in the post-2010 period. One would not expect to see a similar increase in the number of males receiving credit. As such, the implementation of the Gender Equality Law can be seen as an exogenous shock to access to credit for particular types of women that did not affect men in the post-2010 period. This is the variation that we exploit in this paper to identify the relationship between credit held in the hands of women and individual and household outcomes.

Most credit in rural areas of Viet Nam is provided through two state banks, the Viet Nam Bank for Social Policy (VBSP) and the Viet NamBank for Agriculture and Rural Development (VBARD). The former is the main vehicle that the government uses for the provision of credit to disadvantaged and vulnerable groups, while the latter works on a commercial basis. The implementation of the Gender Equality Law is carried out through the VBSP. A description of the evolution of the formal state-banking sector in Viet Nam is provided in the Appendix.

3.3 Data

Table 3.1 presents the proportion of households and individuals in our sample with loans by source, formal and informal. We gather data on the three most important loans that a household has. We know which individuals in the household are responsible for each loan and so we can determine the gender of the loan recipients. For the purpose of our analysis we only consider the most important loan held by each individual within households.[4]

[4]The maximum number of loans held by a household is 3. On average between 14 to 35 percent of households held more than one loan, depending on the year. By focusing on the main loan we potentially exclude two other loans for these households when conducting our analysis.

Table 3.1: Number and proportion of households with loans by source

Year	2008	2010	2012	2014	2016
% of households with at least one loan	44.4	47.2	38.3	33.6	27.3
% of households with at least one formal loan	26.6	31.5	24.8	20.1	19.6
% of households with at least one informal loan	18.6	16.7	13.6	13.8	7.7
Number of adults	6,405	6,379	6,538	6,529	6,476
% of individuals with at least one loan	15.31	16.35	12.69	11.08	9.06
% of women with at least one loan	4.94	5.42	4.06	3.28	2.59
% of men with at least one loan	10.36	10.92	8.62	7.90	6.47
% of individuals with at least one formal loan	8.93	10.65	8.07	6.49	6.43
% of women with at least one formal loan	2.21	2.47	2.37	1.4	1.43
% of men with at least one formal loan	6.69	7.71	5.70	5.08	5.00
% of individuals with at least one informal loan	6.37	5.59	4.49	4.56	2.56
% of women with at least one informal loan	2.66	2.46	1.63	1.77	1.14
% of men with at least one informal loan	3.65	3.13	2.86	2.78	1.42

Note: Authors' own calculations based on VARHS data. The total number of households in each year is 2,108 and the total number of individuals, of the age 18 or older, are 32,327.

Our data reveal a high level of access to credit compared with other developing countries. Of particular note is that the majority of loans are through formal institutions. These include the two main banks serving rural communities in Viet Nam, VBSP and VBARD, along with other state-owned and private banks. Overall, however, the proportion of households with loans declined between 2008 and 2016. This decline is likely due to the introduction of other government policies in rural areas during this period, in particular the provision of direct financial supports for households in times of crisis, and also an increase in the role and importance of private remittances. This overall decline could also be because of monetary and fiscal tightening in early 2011, which negatively affected availability of credit in the economy (OECD13). In addition, if we compare formal versus informal credit one can see that this decline is partially driven by a decrease in informal access, particularly since 2014. The proportion of households with formal credit access has remained relatively constant around 20 percent for 2014 and 2016.

In our analysis, we are interested in determining whether the change in implementation of the Gender Equality Law (2007) at the end of 2010 increased access to credit and the supply of credit for women in poor or ethnic minority households. The change in the law also shifted focus away from female-headed households towards female household members more generally. In Table 2, we examine the extent to which there is evidence of this policy being implemented in our raw data.

Examining the individual level data, we find that within households approximately 15 per cent of adults have a loan in 2008. This fell to 9 per cent by 2016. While both formal and informal access has decreased, similar to the household level analysis, since 2014 this decrease appears to be driven by a decrease in informal loan access. In line with the literature, we find a much greater proportion of loans are held by men than women, particularly formal loans. Though the gap in access to formal credit between men and women decreased between 2008 and 2016.

Table 3.2: Access to credit by household characteristics

Year	2008	2010	2012	2014	2016
Number of adults	6,405	6,379	6,538	6,529	6,476
% of loans in one household name	95.41	95.21	84.22	88.40	80.41
% of loans in two household names	4.59	4.79	15.78	11.60	19.59
% loans held by women	4.94	5.42	4.06	3.28	2.59
Value of loans received by females	23,396	23,116	27,290	30,334	63,699
% loans held by men	10.36	10.92	8.62	7.90	6.47
Value of loans received by males	38,515	41,624	59,993	71,126	86,981
% loans held by female heads	1.77	1.94	1.56	1.14	0.99
Value of loans received by female heads	26,729	25,855	29,015	34,745	72,109
% loans held by female non-heads	3.16	3.48	2.50	2.04	1.61
Value of loans received by females non-heads	21,524	21,585	26,216	27,846	58,524
% loans held by females in poor hhs	1.18	1.11	0.84	0.75	0.37
Value of loans received by females in poor hhs	12,601	12,970	17,896	19,384	22,300
% loans held by females in non-poor hhs	3.76	4.31	3.22	2.43	2.22
Value of loans received by female in non-poor hhs	26,800	25,735	29,738	33,708	70,599
% loans held by females in ethnic minority hhs	0.62	0.61	0.52	0.60	.41
Value of loans received by females in ethnic minority hhs	13,529	15,435	18,842	17,246	37,296
% loans held by females in non-ethnic minority hhs	4.32	4.81	3.54	2.59	2.17
Value of loans received by females in non-ethnic minority hhs	24,821	24,091	28,527	33,354	68,755

Note: Authors' own calculations based on VARHS data. Calculations based on individual level data. The total number of households in each year is 2,108 and the total number of individuals, of the age 18 or older, are 32,327. All loan amounts are reported in VND (000). .

While the proportion of men and women with loans declined between 2008 and 2016, particularly after 2010, the value of loans received increased for both. This increase was much greater for men. While the size of women's loans increased, the gap in loan size with men increases over time. In 2008, the gap between the average male and female loan was VND 15,119,000 but by 2016, this gap had widened to VND 23,282,000.

Interestingly, when we look at the number of loans with more than one name on the loan, we see an increase post 2010. In 2008 and 2010, over 95 percent of loans were held in just one household member's name, by 2016, this figure was down to 80 percent.

Female-headed households hold a lower level of loans than other female adult household members, however, by 2016 this gap in access decreases. On the other hand, the value in loans held by female-headed households was higher than the value of loans held by other female adult household members. Both groups saw an increase in loan size, though by 2016 female-headed households still had larger loans.

To explore the extent of access to credit for poor households, we classify households as poor if they are regarded as such by the Ministry of Labor, Invalids and Social Affairs (MoLISA).[5] We find no evidence of increased access to credit for women in these households. While the size of the loans to women in poor households increased post-2010, the increase was much lower than for women in non-poor households.

There is some evidence of the policy at work in relation to ethnic minority households. While the proportion of women with access to credit declined post 2010, it remained relatively stable among ethnic minority women. It should be noted though, that the level of access for ethnic women was already at quite a low level in 2008.

Since 2010, it would seem that all households saw a decrease in loan access, though this was accompanied by an increase in the size of the loans. While both men and women were affected by this decrease, it would seem that men saw a bigger decrease in loan access than women between 2010 and 2016. If we look at the number of households who have two names on the loans, we see an increase in this figure post 2010. This may signal an increase in the number of women co-signing for the loan. If we compare loan access for female head households to loans held by other female adult household members, we see a decrease for both groups, though both also see an increase in loan value. If we look at this access by household characteristics, poor and ethnic women do face lower levels of credit access and obtain smaller size of loans than non-poor or non-ethnic households. There is little evidence from the raw data that suggests this has changed.

Overall, the implementation of the law seems mixed. On one hand, it would seem to have ensured that access to credit for women has fallen less than men post-2010. It also seems to have increased the likelihood of women co-signing for a loan. On the other hand, access to credit for poor and ethnic women has not caught up with their non-poor or non-ethnic counterparts.

The second part of our analysis focuses on the extent to which increasing access to credit and the level of credit for women through the implementation of the Gender Equality Law impacted on individual behaviour and household welfare outcomes. As discussed in Section 3.2, a large body of evidence points to positive impacts on individual, household and child welfare when resources are

[5]In Viet Nam, the allocation of aid resources and other benefits depends on being 'identified' as poor by the MoLISA. The classification process takes into account household income, wealth and other factors in determining whether a household is poor or not. For a full description of the classification process see Newman and Zhang (2015).

Table 3.3: Use of credit by household characteristics

Year	2008	2010	2012	2014	2016
% loans used for agriculture purposes*	47.6	33.6	27.0	30.1	27.6
% loans used for agriculture purposes by women	44.0	24.5	23.7	19.7	19.6
% loans used for agriculture purposes by men	49.3	38.1	28.5	28.5	30.8
% loans used to buy assets**	14.5	14.5	13.5	10.2	16.2
% loans used to buy assets by women	13.8.	16.7	14.3	9.6	19.0
% loans used to buy assets by men	14.8	13.5	13.1	13.1	15.0
% loans used for consumption	26.9	39.7	46.3	47.2	43.1
% loans used for consumption by women	30.8	45.9	50.0	59.6	51.2
% loans used for consumption by men	25.0	36.7	44.5	44.5	39.8
% loans used for education***	1.21	14.4	13.6	10.9	4.6
% loans used for education by women	1.25	13.9	13.1	12.5	7.1
% loans used for education by men	1.2	14.6	13.8	13.8	3.6
% households hired labour****	79.4	44.1	57.8	48.9	49.7
% households with credit hired labour****	80.6	48.8	66.6	56.9	56.7
% households without credit hired labour ****	78.5	39.9	52.3	44.8	47.1
% households hired machinery ****	79.5	49.5	58.1	56.5	48.5
% households with credit hired machinery****	81.5	53.2	64.5	64.1	57.2
% households without credit hired machinery ****	78.2	46.2	54.1	52.6	45.2
% households hired cattle ****	75.9	19.0	21.2	8.7	10.0
% households with credit hired cattle****	76.8	17.2	22.7	8.2	8.7
% households without credit hired cattle ****	75.1	21.1	21.2	9.0	10.5

Note: Authors' own calculations based on VARHS data. Calculations based on data from 32,327 individuals of the age 18 or older. *Agriculture purposes include loans used for rice, other crops and animal husbandry. **Buying assets include build/buy house, buy land, buy another asset. *** Consumption includes expenditure for a wedding/funeral, paying for education and other general consumption items. ****Education is also one of the consumption components. ***** These figures are based on household level data, which is a balanced panel of 2,108 observations per VARHS round.

held in the hands of women rather than in the hands of men. If the implementation of the Gender Equality Law was effective, it should have increased the credit available to women relative to men. We explore whether there is evidence that this in turn changes individual's time allocation, their investment decisions and household and child welfare.

First, we look at how this credit was used by individuals. While it is hard to confirm exactly how this credit was used, as the categories of use are broad (such as crops, livestock, forestry, education and so forth), the VARHS data does provide some indication of how the loans were used. For instance, if we look at how both men and women report using the credit, crop production and livestock are the top two cited uses of the main loan for either gender. From Table 3, when we aggregate across all agriculture categories, it is initially the predominant reported use of the loans from individuals in the dataset. Over time though, it declines from over 47 percent percent in 2008 reporting the use of their loans for agricultural purposes, to just over 27 percent by 2016. On the other hand, when we look at the use of loans for consumption, by 2016 this is the most commonly cited use of loans. Furthermore, women are more likely to report using loans for consumption than men. Over time, this gap in using loans for consumption become larger between men and women. In regards to the use of loans to buy assets, while men and women self report a similar level of use initially, by 2016 women are more likely to report using a loan for the purchase of an asset. Finally, while a similar level of men and women self-report using loans for education over the years, in 2016 this changes with twice the percent of women report using it for education then men. What these statistics seem to suggest is that men and women use the loans for different purposes and these purposes change over time.

In addition to this, the VARHS data enables us to look at what type of inputs households used for their agriculture activities based on whether they had access to credit or not. This gives us some degree of insight to the level of capital and labour individuals in Viet Nam use for their agriculture activities over time. From Table 3, we see that households with credit are more likely to report hiring machinery and labor for agriculture purposes than households without credit. This is consistently higher across the VARHS survey time periods. This could provide some insight into the channels that contribute to the household outcomes we see as a result of credit access.

Having a better understanding of how loans were used by individuals, we now consider a number of different individual and household welfare outcomes in our analysis. Table 4 provides a description of each outcome considered along with summary statistics. We first consider the number of days worked by individual members of the household in different types of activities. Access to credit for women may be important in this regard, as it may provide investment capital for agriculture or enterprise activities or may provide women with the means to find or travel to find work. Furthermore, it may provide women with greater freedom to decide how to allocate their time.

Table 3.4: Individual and household welfare outcomes

Outcome	Description	2008	2010	2012	2014	2016
Individual- Male:						
Work days	Number of days worked	169	157	156	147	153
Work days ag	Number of days worked in agriculture	73	60	57	43	44
Work days job	Number of days worked in job	68	69	74	79	85
Work days ent	Number of days worked in hh enterprise	23	21	21	21	21
Individual-Female:						
Work days	Number of days worked	152	137	132	126	132
Work days ag	Number of days worked in agriculture	82	70	62	48	47
Work days job	Number of days worked in job	41	40	44	52	58
Work days ent	Number of days worked in hh enterprise	26	23	24	23	24
Individual- Male:						
Days Maize	Number of days worked growing maize	6	5	3	2	2
Days Rice	Number of days worked growing rice	25	21	21	15	14
Days Other Crops	Number of days worked growing other crops	19	15	12	12	14
Individual-Female:						
Days Maize	Number of days worked growing maize	6	4.6	3.2	2.6	2.6
Days Rice	Number of days worked growing rice	24.3	20.6	21	15	15
Days Other Crops	Number of days worked growing other crops	19.2	15.3	15.3	12.3	12.3
Household:						
Income	Log of household income per capita	6.83	7.36	7.24	7.40	7.52
Food Exp	Log of food expenditure per capita	5.50	5.65	5.95	5.92	5.97
Households with children:						
Kids sick days	Number of days children were sick	7	7	4	4	4
Kids attend school	Proportion of children attending school	0.63	0.61	0.63	0.60	0.56
Kids work days	Number of days children worked	40	31	18.5	11	11
Kids work days ag	Number of days children worked in agriculture	29	21	12	7	6.5
Kids work days job	Number of days worked in job	6	5	4	2	2.5
Kids work days ent	Number of days worked in hh enterprise	2	1	1	0.56	0.68

Note: Authors' own calculations based on VARHS data. Calculations based on data from 32,327 individuals of the age 18 or older and a total of 2,108 household observations per VARHS round.

The descriptive statistics presented in Table 3.4 reveal that the total number of days worked by adults in our sample declined between 2008 and 2016. The majority of the decline is due to fewer days worked in agricultural activities. We also observe that men work more days than women mainly due to a higher number of days worked in waged employment.[6] Women on the other hand work more days in agriculture, though when we look within the type of crops cultivated, men and women have a similar pattern of time allocation between crops. In addition, women also allocate more work days in home enterprise activities. This suggests that women are more likely to be in the informal sector.

At the household level, we consider the level of income of the household and the level of food expenditure. Food expenditure is generally considered a more reliable and accurate measure of household welfare than income given that it is less likely to be under-reported and is less likely to suffer from measurement error (Meyer and Sullivan 2003). The variable is constructed by aggregating the value of a set of food items consumed by the household in the previous month and is converted to real terms using a national food price index.[7] We see from Table 4 that both household income and expenditure increase between 2008 and 2016.

For children's outcomes, we consider the number of days children are reported as being sick (in the last 30 days), whether or not the children attend school, and the number of days that children spend working in different activities.[8] Overall we see the welfare of children, on the basis of these measures, improving over the sample period. Over 60 percent of children attend school and the proportion remains relatively constant between 2008 and 2016. Moreover, the number of days children worked declined by a large proportion between 2008 and 2016.

3.4 Empirical Approach

Our analysis focusses on the extent to which access to credit in the hands of women has a different effect on outcomes than access to credit in the hands of men. There are two main potential mechanisms: i) access to credit may provide women with resources that they personally can use in their own income-generating activities; or ii) women may be more likely to use credit to directly increase household consumption than men. The former could have the knock-on effect of increasing household welfare and, in particular, children's outcomes, if higher income levels improve women's relative bargaining power within the household. Income earned by women, separate to

[6]See Newman 2016 for a full description and discussion of gender relations in rural Viet Nam in the 2008 to 2014 period.

[7]Our data does not include a full food expenditure diary but a list of foods that are considered quite typical for most rural households. They include pork, beef, chicken, fish, shrimp, fruit, candy, powdered or canned milk and eating and drinking outside the home or processed foods.

[8]An individual is counted as a child if they are under 17 years of age at the time of the survey.

their husbands, enables them to potentially gain greater control of their income and with this control, increase their bargaining power within the household (Swaminathan et al. 2010). Through this mechanism, women could increase their ability to make decisions in the household, particularly around household welfare outcomes (Swaminathan et al. 2010).

The first part of our analysis considers the impact of credit access on women's behaviour in relation to income-generating activities. Around 45 percent of the female working population in rural Viet Nam work in agriculture (World Bank 2015a). Access to credit could have important implications for their behaviour in relation to their agricultural activities. For example, since women often only have access to primitive production and harvesting tools (Ha et al. 2015), access to credit could allow them to hire better machinery or inputs. Inequality in resource access also often affects the participation of women in cultivating cash crops (Hill and Vigneri 2014). Access to credit might also influence the types of crops that women grow if it provides them with the capital or inputs necessary. As mentioned previously, in the VARHS data, we see that one of the biggest uses of loans reported by women is for crop production.

We examine how credit access affects women's allocation of time and what they decide to invest in. We consider the number of days that women work in various occupations, as well as the days spent cultivating different types of agriculture crops. In addition, we look at whether access to credit changes the likelihood of women hiring or buying additional inputs into agricultural production and home enterprise activities.

To explore the impact of credit for women on their decisions in relation to income-generating activities, we estimate the empirical specification given in equation (3.1),

$$\begin{aligned} Y_{ijt} = \beta_1 loans_{ijt} + \beta_2 female_{ijt} \times loans_{ijt} + \beta_3 female_{ijt} + X_{ijt}\delta \\ + Z_{jt}\gamma + \alpha_j + \eta_t + e_{ijt} \end{aligned} \tag{3.1}$$

where Y_{ijt} is the number of days individual i in household j in time t works in a particular activity; $loans_{ijt}$ is the log value of loans received by individual i; $female_{ijt}$ is a dummy indicator for whether the individual is female; X_{ijt} are individual specific control variables including age and whether the individual is the head of household; Z_{jt} are time-varying household control variables including the age, education level and marital status of the head of household, whether there are children in the household, household size, household income and assets, including the land area owned, the value of durable goods owned by the household, an indicator for whether the household has an enterprise and whether the household has a property right to the land they farm, and indicator variables for whether the household suffered any income shocks in the previous two years; α_j are household fixed effects; η_t time dummies; and e_{ijt} the statistical noise term. The identification of the impact of female credit on days worked comes from the within-household variation in credit received for men and women. The main parameters of interest are β_1 and β_2, where β_1 captures the impact of credit on outcomes for men, while the estimate for β_2 indicates how this differs for women in the same household.

We also examine whether credit access affects the type of investments women make in their income generation activities. We examine whether access to credit affects the likelihood of women investing in different types of inputs for these activities. Given that we have information on whether households hired cattle, machinery or labour for agriculture related activities, as well as the amount of resources they invested in their home enterprise, we are able to see if increased credit access for women affects the likelihood of using any of these inputs. Understanding how loans affect the usage of these inputs will help us better understand the mechanisms through which these loans impact the time allocation of women and potentially subsequent household outcomes. Using the same specification in equation (3.1), we consider a range of outcome variables that capture investments and input usage by women. This includes investment in cattle, machinery or labour for agriculture and the total amount of resources invested in home enterprises.

The identification challenge faced in this analysis is that access to credit is likely to be endogenous to each of these outcomes. Lenders could give credit to households that are better-off or due to unobservable characteristics, such as how entrepreneurial households are (Khandker and Faruqee 2003; Quach 2017). The inclusion of household fixed effects and a rich set household time-varying control variables goes some way to alleviating concerns about the endogeneity of credit access in general but the identification of the impact of female and male credit on welfare may still be confounded by other unobserved time-varying household specific factors. For example, women who become more engaged in agriculture over time or who start to invest more in certain inputs might be more likely to get a loan.

To address these endogeneity concerns we use an exogenous policy change that affects the loan eligibility criteria of women but not the outcome of interest, household welfare. We use the regime switch of the Gender Equality Law to instrument female access to credit with a dummy indicator for whether or not that particular woman was targeted by the implementation of the Gender Equality Law in 2011. In other words, all poor (MoLISA defined) and ethnic minority women who are not a household head are considered treated by the law in the post-2010 period.

For this to be a valid instrument, we first need to provide evidence that the Gender Equality Law was actually implemented in practice. We exploit the variation in access to credit for men and women within households to ascertain whether women had more access to credit (also in terms of loan size) post 2010. To achieve this we estimate equation (3.2):

$$\begin{aligned} credit_{ijt} = \beta_1 females_{ijt} + \beta_2 female_{ijt} \times post2010_{ijt} + \beta_3 post2010_{ijt} + \\ X_{ijt}\delta + Z_{jt}\gamma + \alpha_j + \eta_t + e_{ijt} \end{aligned} \tag{3.2}$$

where $credit_{ijt}$ is a dummy indicator for whether individual i in household j in time t has access to credit; $post2010_{ijt}$ is an indicator which takes a value of one in 2012, 2014 and 2016; and all other terms are as defined in equation 3.1.

The coefficient β_1, in equation (3.2), captures the extent to which women are more or less likely than men to have loans in the pre-2010 period. Our main interest lies in the coefficient on the interaction term, β_2, which tells us the extent to which this differential changes in the post-2010 period. Including household fixed effects controls for all time invariant household specific factors that could impact on the gender differential in the extent of access to credit. The inclusion of other time-varying household characteristics in Z_{jt}, controls for any changes in circumstances of the household, aside from the implementation of the law, that might impact on women's access to credit relative to men. The inclusion of individual specific controls in X_{ijt}, controls for the fact that age and whether the individual is the head of household are likely to be important determinants of access to credit that may confound the gender differential.

To capture the nuances of the law, we also consider whether there was a change in the extent of access to credit for women that are not head of households, which was the target of the law. In addition, we explore whether or not there are differential effects for poor and ethnic minority households. We also consider other aspects of loans as dependent variables, using the same empirical specification in equation (3.2), including the size of the loans received, whether the loan was from a formal or informal source, the interest rate, whether the loan required collateral and whether the loan required a guarantor. In doing so, we aim to determine whether the terms on which women were offered credit improved post-2010, thus providing further evidence of the implementation of the law in practice.

The second criteria for exposure to the law to be a valid instrument is that it should be exogenous to the outcome variables, namely the number of days worked by women and to the amount of inputs used in their income-generation activities. The only purpose of Norm 4 of the implementation strategy of the Law was to extend credit to women in rural areas. Yet, it could be argued that other elements of the Gender Equality Law affected an individual's time allocation and their investments. Potential features of the law that could confound our instrument are initiatives that increased women's access to employment. Indeed, the law aimed to increase vocational training for women under 45. If this was rolled out, then vocationally trained women are potentially more employable and therefore more likely to find work implying that the mechanism through which the law impacts on hours worked is through training and not access to credit. To rule out this possibility, as a robustness check, we run a second specification for equation (3.1) and (3.3), where we control for whether a particular commune received funding for vocational training post 2010.

In the final part of our analysis, we explore whether women's access to credit impacts household welfare. We would expect that depending on who experiences increased access to credit within the household, the outcomes for households could vary. Women may be more likely than men to use credit to increase consumption, or, as discussed earlier, through its impact on bargaining power, increased access to credit could lead to women's preferences within the household being favoured (Swaminathan et al. 2010). To explore the effect of female credit on household level outcomes, we aggregate the individual-level data to the household level. We use the share of formal loans in the hands of women compared to men as the indicator for increased credit in the hands of women compared to men. For each household level welfare outcome (income and food consumption), we estimate the specification given in equation (3.3).

$$W_{jt} = \beta_1 credprop_{jt} + \beta_2 formalcredit_{jt} + \beta_3 informalcredit_{jt} + Z_{jt}\gamma + \alpha_j + \eta_t + e_{ijt} \tag{3.3}$$

where W_{jt} is the welfare outcome for household j in period t; $credprop_{jt}$ is the ratio of the total value of formal loans held by women to the total value of loans held by men in the household; $formalcredit_{jt}$ is a dummy variable for whether the household has formal credit; $informalcredit_{jt}$ is a dummy variable for whether the household has informal credit; and all other variables are as before. The parameter of interest is β_1, which captures the impact of the relative share of credit in the hands of women versus men in the household on the household welfare outcome of interest.

The same endogeneity concerns relating to women's access to credit arise in this specification. It is also likely that household access to formal credit suffers from similar endogeneity problems. We instrument for these two variables using three instruments. First, we use the regime switch of the Gender Equality Law to instrument female access to credit with a dummy variable for whether or not that particular household was targeted by the implementation of the Gender Equality Law in 2011. In other words, all poor (MoLISA defined) and ethnic minority households in the post-2010 period, that have at least one female member who is not a household head, are considered impacted by the law. Second, we use the total number of mass organisations that lend to households in the commune. Third, we use the total number of banks, state and private, that lend in the commune.

3.5 Results

3.5.1 Instrument validation

We begin by testing the validity of our instrument by exploring the extent to which there is evidence in our data that the Gender Equality Law was implemented. We estimate equation (3.2), to determine whether within households, women have more access to credit than men in the post-2010 period. Only households with more than one adult are included in the analysis. We disaggregate between female-headed households and other female household members, given that it is only the latter that were targeted by the law. The results for access to credit (that is the individual has a loan) and the size of the loans received are presented in Table 3.5. Each specification is estimated using household fixed effects and includes time dummies and individual-level and household-level control variables.

Columns (1) and (2), from Table 3.5, consider whether women have more access to credit in general than men. Column (1) suggests that the extent of access to credit for women is significantly different than men, while column (2) suggests that in value terms women are in receipt of less credit than men. In columns (3) and (4), we consider whether the extent of access to credit for women changed post-2010, when the Gender Equality Law was implemented by including an interaction term between gender and the dummy indicator for the 2012, 2014 and 2016 time periods. In the case of credit access, the interaction term is statistically significant suggesting that access to credit is different in 2012, 2014 and 2016 than in 2008 and 2010. We find no evidence that the size of the loan changed post the implementation of the Gender Law.

In columns (5) and (6), we separate out female head of households from other females within the household. In both cases, we find that before the implementation of the law (in 2008 and 2010), females within households that were not the head of household received less credit than men. This coefficient is not significant though. The interaction term with the post-2010 dummy, however, is positive and statistically significant. This suggests that after 2010, access to credit for female non-heads increased compared to men in regards to credit access and the value of their loans. We have to interpret this with caution, as the level effect is not significant. These results provide some evidence that the law targeted women, particularly females who are not the head of the household, in the post-2010 period.

The implementation of the Gender Equality Law also targeted two other types of households, ethnic minority households and poor households. To explore the extent to which there is evidence for the implementation of the law on these dimensions in practice among our sample, we estimate a similar

Table 3.5: Impact of the Gender Equality Law on access to credit — individual level analysis

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	credit_i	lamt_received	credit_i	lamt_received	credit_i	lamt_received
female_i	-0.009*	-0.127**	-0.020***	-0.202***		
	(0.005)	(0.054)	(0.007)	(0.073)		
femxpost10			0.017**	0.123		
			(0.008)	(0.075)		
post_10			-0.047***	-0.347***	-0.049***	-0.358***
			(0.008)	(0.083)	(0.008)	(0.082)
femalenonhead_i					-0.008	-0.068
					(0.007)	(0.066)
femnonheadxpost10					0.023***	0.176**
					(0.007)	(0.073)
femalehead_i					-0.076***	-0.822***
					(0.019)	(0.195)
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes	Yes	Yes
F-test of joint significance			0.22	1.78	9.21	4.34
			(0.637)	(0.182)	(0.002)	(0.37)
Observations	31,473	31,473	31,473	31,473	31,473	31,473
R-squared	0.147	0.149	0.147	0.149	0.149	0.151
Number of HHID	2,051	2,051	2,051	2,051	2,051	2,051

Note: Authors' own calculations based on VARHS. Analysis is at the individual level.

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

specification to equation (2) focusing on ethnic minority and poor (MoLISA defined) individuals and whether access to credit for them changed post-2010. The results are presented in Table 3.8.

Columns (1) and (2), from Table 3.8, reveal that before the implementation of the law, ethnic minority households did not differ from other households in terms of access to credit, in terms of both whether they have any loans and the size of those loans. Post-2010, there is evidence that both the level of access to credit for these households and the value of loans changed. The interaction with the post-2010 indicator is positive and significant for the value of the loan. This suggests that after 2010, ethnic minority households saw an increase in their loan access and the size of their loans compared to other households. This provides further evidence that the law was indeed implemented in practice, although again some caution should be exercised given that the level and interaction term are not jointly significant.

In relation to poor households (as classified by MoLISA), we find that, in general from columns (3) and (4), they are more likely to have loans and have larger loans than non-poor households. This is not surprising, given that one of the benefits of being classified as poor is gaining access to formal credit. The interaction term, however, is not statistically significant suggesting that there was no change in the extent of access to credit post-2010.

While the law does not appear to have had an impact on the extent of access to credit for poor households, there is evidence to suggest that women, including those who are not the head of the family, and ethnic minority households, have better access to credit in the post-2010 period.

As a final check, we look to see whether there is evidence that women who are poor and an ethnic minority have better access to credit post-2010. Columns (1) and (2), from Table 3.6, show significant difference in access to credit for this group post-2010, particularly in regards to access to credit, as both the level and the interaction variables are significant. The size of the interaction term is bigger than the level effect, suggesting an increase in access for this group.

When we focus on non-household head females who are poor and/or ethnic in columns (3) and (4), the interaction term post-2010 indicates that they are more likely to receive credit and a higher loan value after the implementation of the Gender Equality Law. Again this interaction term is statistically significant and indicates that poor and/or ethnic non-household head women receive more credit post-2010. Again though, some caution should be exercised given that the level and interaction term are not jointly significant. This gives further evidence of the Gender Equality Law being implemented and being reflected in the data.

In Table 3.7, we explore the extent to which there was a change in the source and terms of credit that women have access to after 2010. The law targeted formal credit for women and so should have impacted access to formal credit in particular. Column (1) of Table 3.7, reveals this to be the case

Table 3.6: Impact of the Gender Equality Law on access to credit for poor, ethnic females

VARIABLES	(1) credit_i	(2) lamt_received	(3) credit_i	(4) lamt_received
femPEth_i	-0.013*	-0.080		
	(0.008)	(0.079)		
femPEthxpost10	0.020***	0.200***		
	(0.007)	(0.067)		
post_10	-0.044***	-0.333***	-0.043***	-0.321***
	(0.007)	(0.070)	(0.007)	(0.070)
femnhPEth_i			-0.011	-0.044
			(0.008)	(0.077)
femnhPEthxpost10			0.016**	0.163**
			(0.007)	(0.067)
Household FE	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes
F-test of joint significance	0.46	1.61	0.27	1.64
	(0.499)	(0.205)	(0.603)	(0.200)
Observations	31,473	31,473	31,473	31,473
R-squared	0.147	0.149	0.147	0.149
Number of HHID	2,051	2,051	2,051	2,051

Note: Authors' own calculations based on VARHS. Analysis is at the individual level.
Robust standard errors clustered at household level presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1
F-test relates to the joint significance of the level and interaction term in each case.

with female non-heads more likely to access formal credit post-2010 than pre-2010 and overall more likely than men to access credit post-2010. We do not find a similar result for informal loan access, as the interaction term post-2010 is not significant. Overall, however, women are more likely to access informal credit than men. Columns (3) to (5) consider whether the terms of credit in relation to formal loans are different for women and men before and after the implementation of the law, including the rate of interest that they are charged, whether they needed collateral and whether or not they required a guarantor. We find that the law has no statistically significant impact on these terms for women compared with men.

Table 3.7: Impact of the Gender Equality Law on source and terms of credit

	(1)	(2)	(3)	(4)	(5)
VARIABLES	formal	informal	int_rate	collateral	guarantor
femalenonhead_i	-0.015***	0.007*	33.955	0.017	0.021
	(0.005)	(0.004)	(45.520)	(0.094)	(0.055)
femnonheadxpost10	0.022***	0.002	-25.243	0.080	0.167***
	(0.006)	(0.005)	(39.794)	(0.062)	(0.045)
post_10	-0.017**	-0.032***	8.330	-0.009	0.064**
	(0.007)	(0.005)	(10.891)	(0.046)	(0.027)
femalehead_i	-0.069***	-0.006	-0.549	-0.056	-0.196**
	(0.015)	(0.010)	(26.091)	(0.134)	(0.084)
eth_min	0.009	-0.020	4.033	0.031	-0.019
	(0.020)	(0.013)	(12.000)	(0.158)	(0.083)
poor	0.011**	0.006	13.410	-0.033	-0.024
	(0.006)	(0.005)	(11.221)	(0.036)	(0.027)
Household FE	Yes	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes	Yes
F-test of joint significance	2.90	8.98	0.36	1.20	12.08
	(0.089)	(0.003)	(0.546)	(0.27)	(0.000)
Observations	31,473	31,473	2,538	2,541	2,541
R-squared	0.114	0.035	0.134	0.037	0.234
Number of HHID	2,051	2,051	1,223	1,224	1,224

Note: Authors' own calculations based on VARHS. Analysis is at the individual level.

Robust standard errors clustered at household level presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1

F-test relates to the joint significance of the level and interaction term in each case.

For the regressions on interest rate, collateral and guarantor there are a lot of missing observations as this information is only collected for individual with credit and therefore they are dropped.

3.5.2 Analysis of female time-use decisions

Having established that there is indeed evidence of the Gender Equality Law being implemented in our sample we move to our main analysis, which focuses on how increased access to credit, for women, affects their individual time allocation and investment decisions. To understand the impact of credit access on the time allocation of women, we consider the number of days that women work in various occupations. To explore the impact of credit for women on the extent to which they work, we use the individual level data and estimate the empirical model given in equation (1). Our focus is on the gender differential in the impact of formal loans on days worked but we also control for the level of informal loans that the individual receives to control for changes in the substitutability of formal and informal credit. The results are presented in Table 3.9. We present the OLS results and then the results when female credit (i.e. the interaction term between the female indicator and the access to credit indicator) is instrumented by whether the woman was exposed to the implementation of the Gender Equality Law.

Columns (1) and (2), from Table 3.9, present the results for the impact of formal credit on the total days worked by individuals for the OLS and IV specifications. The interaction term in our OLS result is not statistically significant but once we instrument for total loans in the hands of women, we find that it increases the total number of days they work. The coefficient on the IV estimator is much larger, suggesting that OLS underestimates the true impact of credit in the hands of women on their time allocation and investment decisions. This is not surprising given the potential selection effect, whereby women in higher-welfare households have less demand for credit and so have a lower level of loans than in lower-welfare households.

Our results suggest that overall for women with formal loans, the number of working days increases by almost 10 days a year. When we disaggregate by the type of work, we find that it is driven by an increase in the number of days that women work in agriculture and home enterprise activities. We do not find any statistically significant impact on days worked in a waged job.

In Table 3.10, we explore whether, within agriculture, credit impacts the type of crops women cultivate. We find that credit increases the number of days women spend cultivating maize and rice. As a cash crop, maize in particular has the potential to boost household income.

In Table 3.11, we examine whether credit impacts on the investments that women make in their income generation activities. We estimate the impact of the credit access on their likelihood to hire machines, cattle or labour for the production of agriculture, as well as their level of investment in their home enterprises. If we see that women are more likely to use inputs that help in their income generating activities, this will suggest that access to loans provides women with greater financial freedom to take some risk through changing what inputs they use.

As revealed in Table 3.11, once we instrument for credit access, we find that increased credit in the hands of women decreases their likelihood of renting cattle and hiring labor for their agriculture activities. This suggests that access to credit for women frees up women's time allowing them to use their labor on-farm instead of hired labor. Moreover, access to credit for women also appears to allow households to substitute away from cattle production into maize and rice production.

This leaves a question though on what women exactly use their credit for, especially as we see that they are more likely to use their own labor in agriculture production and less likely to hire external inputs. To address this question, we return to Table 3.3, which laid out how loans are used by women versus men. What we see from Table 3.3 is that women are more likely to use the loans for consumption purposes, which includes paying for a wedding/funeral, and education and health expenses. This gap between women and men, on the use of loans for consumption, increases over time. In 2008, 30.8 percent of women used their loans for consumption versus 25 percent of men. By 2016, this gap in usage had widened, with 56.7 percent of women using loans for consumption versus 39.8 percent of men. Similarly, women are more likely to use their loans to buy assets than men, which includes buying a house and land. Men, on the other hand are much more likely to use their loans for agriculture purposes. In 2016, 30.8 percent of men used their loans for agriculture compared to only 19.6 percent of the women.

These loan usage patterns may help explain why we do not see women increasing their use of external inputs and relying on their own labor instead. They are potentially using the credit to pay for current expenses, like health expenses or for events like a wedding, additionally, they are investing them in purchases like land and housing. By using these loans in this way, it can then also influence how credit impacts outcomes at the household level. For instance, if loans are used for consumption, it is unlikely that there will be an immediate, if any, return from the use of the loan. If the loans are used for the purchase of land, it will take time before the return from the loan is realised.

3.5.3 Analysis of household outcomes

The observed change in time allocation can have two possible consequences for women and household welfare. On the one hand, if the change in time allocation and in the types of crops produced leads to higher incomes, this will directly improve household income. Moreover, if higher income strengthens women's bargaining power within the household, it could have knock on effects for household welfare, by increasing food expenditure and improving children's welfare.

The final part of our analysis examines whether there is a difference in the impact of (formal) credit in the hands of women compared with men on household welfare outcomes. We measure household welfare using food consumption per capita and the log level of the monthly per capita household income (see Section 3.3). We estimate the model presented in equation (3.3) with a fixed effects linear estimator. Given the potential issues of endogeneity identified earlier, we instrument for our relative bargaining measure, the ratio of formal loan values of women to men. We also instrument for whether this credit access is from a formal source. Our instruments are whether or not the household was exposed to the implementation of the Gender Equality Law during the post-2010 period, the total number of mass organisations lending in the Commune and the total number of banks lending in the commune. The results are presented in Table 3.12.

From our OLS estimates in columns (1) and (3), we find that increased credit in the hands of women decreases log income per capita and food expenditure of the household. The results hold when we use IV estimation but it should be noted that our estimates are somewhat weak. The F statistic for our instruments is low, although the p-value suggests that the F test is significant at the 10 percent level.

These results suggest that the change in time allocation and input usage of households has a negative effect on household income and on household consumption. It may be that the impact of these changes takes some time to be realised, but at least contemporaneously, the effect of increasing access to credit to women relative to men has a negative effect on household welfare.

To try to understand this result further, we consider whether increasing access to credit to women also changes the time allocation of men. We explore this possibility in Table 3.13, by examining how the ratio of female to male loans in the household impacts on the ratio of female to male working hours overall, and for different activities. We find that the more credit women have relative to men, the greater the ratio of female to male working days, specifically in agriculture and waged employment. We also consider the total days worked by men and find a negative coefficient on the number of days men work in most activities, although this relationship is only well determined for enterprise activities. Increased credit for women appears to reduce the amount of days men spend working. As discussed by Mayoux (2000) in a review of microcredit and the empowerment of women, access to credit can increase the burden of work of women, have little impact on the bargaining power of women within households, and consequently no impact on household welfare.

Finally, we examine whether increased credit in the hands of women filters through to improved welfare outcomes for children. Narciso and Newman (2016) find that where women spend more of their time working for a wage compared with other types of activities, children work significantly fewer days in agricultural activities. To explore whether the increased credit in the hands of women reduces the burden placed on children to work, we examine the relationship between the ratio of female to male credit within the household and the days worked by children in different activities. The results are presented in Table 3.14. We find a negative correlation between more credit in the hands of women and the number of days that children work. The correlation is only statistically significant, however, for days spent by boys working in agriculture.

3.6 Conclusion

This paper explores the impact of credit on welfare outcomes for individuals and households and considers in particular whether the impact of credit on outcomes is different if loans are received by women or men within households. We use an extensive panel dataset on 2,108 households in Vietnam for the period 2008 to 2016 and exploit the recent implementation of the Gender Equality Law in 2010, which aimed to expand access to credit for women in rural households, to identify the effect of female credit on welfare outcomes. We find evidence of the implementation of the law in practice in our data and use this to instrument for loans held by women within households to explore the impact on individual and household welfare outcomes.

We find that access to formal loans increases the number of days that women work overall, which is driven by an increase in the days they work in agriculture and home enterprise activities. Within agriculture, we find that women increase the time they allocate to higher return agricultural activities, such as growing maize, but also to growing rice. This increase in loan access seems to result in women substituting hired inputs, such as cattle rental and the hiring of labor, with their own labor. This substitution helps explain the increase in the number of days women work. By looking at loan usage patterns, we are also able to see that women are more likely to use their loans for consumption purposes, as well as for purchasing assets. This could be why we do not see them hiring external inputs and using their own labor instead. In addition, we find that this increase in total days worked by women is correlated with a decrease in the total days men work in home enterprise activities.

When we look at how these individual changes impact at the household level, we find that the relative increase of loans in the hands of women has a negative effect on income per capita and food expenditure in the current period. This result is in line with the findings by Islam et al. (2016), where initially access to credit can have a negative effect on food security. We do find some positive correlations between this increased credit access for women with child welfare outcomes. We see that this increased access is correlated to male children working fewer days in agriculture.

Our paper contributes to a growing literature that explores the impact of access to credit on individual and household welfare outcomes and in particular whether credit in the hands of women compared with men matters. Our results suggest that in the provision of credit to women, it is important to consider issues such as the time burden on women from this increased credit and how they will use this credit. If household welfare outcomes are the goal, then the time frame to capture this impact will need to be considered. This is important given that Khandker and Samad (2014) find that the impact of credit was realised over 20 years and Islam et al. (2016) find that the credit has a non-linear effect on household welfare outcomes, such as food security, where it may first negatively affect it before having a positive effect in the long-run. Though, even in a short time frame this increased access can positively relate to household outcomes, given that increased access to credit for women is correlated to improved outcomes for male children within the household.

In the Vietnamese case, the Gender Equality Law was successful in regards to increasing credit access for women. Yet, in order to identify its impact on household outcomes, one will need to look at a longer time frame for these impacts of be realised.

3.A Tables

Table 3.8: Impact of the Gender Equality Law on access to credit for poor, ethnic individuals

	(1)	(2)	(3)	(4)
VARIABLES	credit_i	lamt_received	credit_i	lamt_received
eth_min	-0.014	-0.134	-0.006	-0.061
	(0.023)	(0.225)	(0.023)	(0.223)
poor	0.017**	0.152**	0.018**	0.149*
	(0.006)	(0.061)	(0.009)	(0.086)
ethminxpost10	0.015**	0.144**		
	(0.008)	(0.073)		
poorxpost10			-0.002	0.015
			(0.010)	(0.093)
post_10	-0.058***	-0.474***	-0.054***	-0.444***
	(0.007)	(0.069)	(0.007)	(0.068)
Household FE	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes
F-test of joint significance	0.01	0.00	5.14	5.063
	(0.914)	(0.967)	(0.026)	0.018
Observations	32,250	32,250	32,250	32,250
R-squared	0.026	0.023	0.026	0.023
Number of HHID	2,108	2,108	2,108	2,108

Note: Authors' own calculations based on VARHS data at the individual level. Analysis is at the individual level. Robust standard errors clustered at household level presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1. F-test relates to the joint significance of the level and interaction term in each case

Table 3.9: Impact of loans on days worked

VARIABLES	(1) totdays_work OLS	(2) totdays_work IV	(3) totdays_ag OLS	(4) totdays_ag IV	(5) totdays_job OLS	(6) totdays_job IV	(7) totdays_ent OLS	(8) totdays_ent IV
femxlamtf_i	-0.092	40.095***	0.208	26.781***	-0.422	-10.402	0.097	21.219***
	(0.551)	(15.535)	(0.378)	(9.918)	(0.468)	(12.859)	(0.349)	(7.859)
lfamt_received	3.229***	-6.894*	2.042***	-4.652*	0.322	2.835	0.809***	-4.511**
	(0.274)	(3.891)	(0.185)	(2.493)	(0.249)	(3.237)	(0.178)	(1.979)
lifamt_received	3.114***	2.630***	2.026***	1.706***	-0.274	-0.154	1.245***	0.991***
	(0.291)	(0.351)	(0.211)	(0.251)	(0.260)	(0.295)	(0.217)	(0.237)
female_i	-8.628***	-30.250***	16.083***	1.785	-27.590***	-22.220***	3.826***	-7.539*
	(2.001)	(8.221)	(1.067)	(5.234)	(1.883)	(6.764)	(1.156)	(3.907)
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage F-stat		32.69		32.69		32.69		32.69
		(0.000)		(0.000)		(0.000)		(0.000)
F-test of joint significance	18.84	1.74	222.53	32.47	225.11	23.88	11.09	10.26
	(0.000)	(0.188)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0. 001)
Observations	31,473	31,473	31,473	31,473	31,473	31,473	31,473	31,473
R-squared	0.065	-0.121	0.102	-0.136	0.110	0.096	0.093	-0.084
Number of HHID	2,051	2,051	2,051	2,051	2,051	2,051	2,051	2,051

Note: Authors' own calculations based on VARHS. This is based on individual level analysis. Results are similar when access to credit rather than size of loans is considered. Robust standard errors clustered at household level presented in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. First-stage F-test refers to the test of excluded instruments in the IV regression. F-test of joint significance relates to the joint significance of the level and interaction term in each case.

Table 3.10: Impact of loans on time spent on different crop options

VARIABLES	(1) LAmtMaize OLS	(2) LAmtMaize IV	(3) LAmtRice OLS	(4) LAmtRice IV	(5) LAmtOCrop OLS	(6) LAmtOCrop IV
femxlamtf_i	-0.086	6.805***	-0.148	9.171**	0.008	3.263
	(0.053)	(2.345)	(0.153)	(4.025)	(0.183)	(4.033)
lfamt_received	0.093***	-1.642***	0.763***	-1.585	0.551***	-0.269
	(0.028)	(0.590)	(0.087)	(1.013)	(0.099)	(1.027)
lifamt_received	0.050*	-0.033	0.699***	0.586***	0.496***	0.457***
	(0.028)	(0.042)	(0.093)	(0.107)	(0.098)	(0.111)
female_i	0.732***	-2.976**	5.707***	0.692	2.550***	0.798
	(0.140)	(1.282)	(0.455)	(2.121)	(0.441)	(2.108)
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes	Yes	Yes
First stage F-stat		32.69		32.69		32.69
		(0.000)		(0.000)		(0.000)
F-test of joint significance	23.86	11.84	148.36	23.33	32.50	4.05
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.044)
Observations	31,473	31,473	31,473	31,473	31,473	31,473
R-squared	0.031	-0.653	0.066	-0.091	0.034	0.015
Number of HHID	2,051	2,051	2,051	2,051	2,051	2,051

Note: Authors' own calculations based on VARHS. This is based on individual level analysis. Results are similar when access to credit rather than size of loans is considered. Robust standard errors clustered at household level presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1. First-stage F-test refers to the test of excluded instruments in the IV regression. F-test of joint significance relates to the joint significance of the level and interaction term in each case.

Table 3.11: Impact of loans on current agriculture and home enterprise investment

VARIABLES	(1) RentMachines OLS	(2) RentMachines IV	(3) RentCattle OLS	(4) RentCattle IV	(5) HireLabour OLS	(6) HireLabour IV	(7) lhhent_input OLS	(8) lhhent_input IV
femxlamtf_i	-0.001	0.077	0.002	-0.293***	-0.000	-0.159**	-0.006	-0.076
	(0.002)	(0.065)	(0.002)	(0.082)	(0.002)	(0.070)	(0.005)	(0.168)
lfamt_received	0.000	-0.020	-0.000	0.074***	-0.001	0.039**	0.003	0.021
	(0.001)	(0.016)	(0.001)	(0.021)	(0.001)	(0.018)	(0.002)	(0.042)
lifamt_received	0.001	-0.000	-0.000	0.003**	0.001	0.003**	0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.004)
female_i	0.001	-0.041	-0.002	0.157***	0.003	0.088**	0.002	0.039
	(0.002)	(0.035)	(0.002)	(0.044)	(0.003)	(0.038)	(0.008)	(0.091)
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage F-stat		32.69		32.69		32.69		32.69
		(0.000)		(0.000)		(0.000)		(0.000)
F-test of joint significance	0.01	1.45	0.01	11.46	1.02	4.51	1.02	4.51
	(0.93)	(0.23)	(0.93)	(0.00)	(0.31)	(0.03)	(0.31)	(0.03)
Observations	31,473	31,473	31,473	31,473	31,473	31,473	31,473	31,473
R-squared	0.116	0.055	0.394	-0.277	0.113	-0.100	0.897	0.896
Number of HHID	2,051	2,051	2,051	2,051	2,051	2,051	2,051	2,051

Note: Authors' own calculations based on VARHS. This is based on individual level analysis. Results are similar when access to credit rather than size of loans is considered. Robust standard errors clustered at household level presented in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. First-stage F-test refers to the test of excluded instruments in the IV regression. F-test of joint significance relates to the joint significance of the level and interaction term in each case.

Table 3.12: Impact of relative share of female versus male loans on current household welfare

VARIABLES	(1) Household Income OLS	(2) Household Income IV	(3) Food expenditure OLS	(4) Food expenditure IV
credprop_fem	-0.008***	-0.226*	-0.005**	-0.261**
	(0.002)	(0.124)	(0.002)	(0.106)
formal_credit	-0.094***	-4.077**	-0.036	-2.244
	(0.024)	(1.865)	(0.023)	(1.563)
informal_credit	0.002	-0.715*	0.033	-0.071
	(0.022)	(0.395)	(0.021)	(0.327)
Household FE	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes
First stage F-stat for credprop_fem		4.33		4.14
		(0.005)		(0.006)
First stage F-stat for formal_credit		2.63		2.62
		(0.045)		(0.050)
Observations	7,908	7,853	7,924	7,869
R-squared	0.239	-3.247	0.152	-2.000
Number of HHID	1,710	1,655	1,710	1,655

Note: Authors' own calculations based on VARHS. This is based on household level analysis. Analysis excluded households with just one adult. Results are similar when access to credit rather than size of loans is considered. Robust standard errors clustered at household level presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1. First-stage F-test refers to the test of excluded instruments in the IV regression.

Table 3.13: Impact of female to male loan ration on the ratio of female versus male days worked &total days worked by men, OLS

VARIABLES	(1) Total Days (Ratio W/M)	(2) Agriculture Days (Ratio W/M)	(3) Enterprise Days (Ratio W/M)	(4) Wage Days (Ratio W/M)	(5) Total Days (Men)	(6) Agriculture Days (Men)	(7) Enterprise Days (Men)	(8) Wage Days (Men)
credprop_fem	0.699**	0.229*	0.176	0.427**	-0.975	0.112	-0.400**	-0.687
	(0.302)	(0.135)	(0.189)	(0.182)	(0.760)	(0.304)	(0.201)	(0.487)
formal_credit	6.211*	1.111	0.281	3.755*	-10.466	6.437*	-4.713**	-12.190**
	(3.292)	(1.441)	(2.302)	(2.049)	(8.557)	(3.484)	(2.302)	(5.590)
informal_credit	0.728	-1.664	-0.111	0.205	6.984	2.718	7.035**	-2.769
	(1.882)	(1.120)	(2.110)	(1.106)	(8.901)	(3.515)	(2.733)	(5.552)
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,924	7,924	7,924	7,924	7,924	7,924	7,924	7,924
R-squared	0.007	0.008	0.094	0.006	0.105	0.091	0.191	0.089
Number of HHID	1,710	1,710	1,710	1,710	1,710	1,710	1,710	1,710

Note: Authors' own calculations based on VARHS. This is based on household level analysis. Robust standard errors clustered at household level presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.14: Impact of ratio of female versus male loans on total days worked by children, OLS

VARIABLES	(1) Total Days	(2) Agriculture Days	(3) Enterprise Days	(4) Wage Days	(5) Agriculture Days (Girls)	(6) Agriculture Days (Boys)
credprop_fem	-0.309	-0.207	-0.036	-0.035	-0.002	-0.204*
	(0.301)	(0.210)	(0.043)	(0.153)	(0.138)	(0.116)
formal_credit	-2.848	-1.252	-0.682	-0.612	0.627	-1.879
	(3.545)	(2.632)	(0.731)	(1.607)	(1.785)	(1.605)
informal_credit	0.254	0.420	0.404	-0.448	0.451	-0.031
	(2.998)	(2.360)	(0.661)	(1.534)	(1.667)	(1.436)
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,145	4,145	4,145	4,145	4,145	4,145
R-squared	0.036	0.039	0.012	0.005	0.032	0.019
Number of HHID	1,224	1,224	1,224	1,224	1,224	1,224

Note: Authors' own calculations based on VARHS. This is based on household level analysis. Robust standard errors clustered at household level presented in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.15: Variable dictionary and summary statistics for control variables

Variable	Mean	Std. Dev.	Min.	Max.
Individual:				
Female_i	= 1 if individual is female	0.511	0.499	
Age_i	Age of individual	42.90	17.92	
Head_i	=1 if individual is head of household	0.301	0.460	
Household:				
Age	Age of head of household	54.57	12.47	
Married	=1of household head is married	0.84	0.40	
Higher Education	=1 if head of household has third-level education	0.19	0.39	
HHsize	Size of the household	4.8	1.8	
Ethnic	=1 if head of household is an ethnic minority	0.23	0.42	
Poor	=1 if household classified as poor by the authorities	0.14	0.34	
lnInc	Log of household income	7.26	0.83	
Lninc_pub	Log of household income from public transfers	3.67	4.18	
lnArea	Log of total land area	8.92	1.45	
HHEnt	=1 if household enterprise	0.27	0.44	
lnDurables	Log of value of durable goods	9.7	1.78	
redbook	=1 if household has a property right to the land	0.87	0.33	
Natural shock	=1 if household has experienced a natural shock	0.345	0.47	
Economic shock	=1 if household has experienced an economic shock	0.16	0.37	
Commune:				
EnterpriseDummy	=1 if Commune has an enterprise	0.84	0.36	
Vocational Program	=1 if Commune has a vocational program	0.12	0.32	

3.B Appendix A1

Provision of Formal Credit in Viet Nam

In Viet Nam, formal credit is provided to households in rural areas through two main state-owned banks, the Viet Nam Bank for Social Policy (VBSP) and the Vietnamese Bank for Agriculture and Rural Development (VBARD). Preferential lending by government to the poor began in 1995 with the establishment of the Fund for the Poor, which operated through the VBARD, the main state commercial bank. The Fund was soon replaced with a Viet Nam Bank for the Poor (VBP) managed by the VBARD. The VBP was established as a non-profit entity, which aimed to target poverty reduction through providing no-collateral, low interest loans to the poor for the purpose of investing in agricultural production or other enterprises. Households eligible for loans were those households classified as poor by the Ministry of Labor, Invalids and Social Affairs (MoLISA) and Ministry for Agriculture and Rural Development (MARD) and applications for loans were certified by the local commune's People's Committee. Loans were managed through local savings and credit groups who fulfilled the responsibility of repaying the bank and in this way operated very much like microfinance institutions. The savings and credit groups were certified by the commune People's Committee and were organized through the Farmer's Union and Women's Union. Mass organizations also played a role in mobilizing and delivering loans directly to poor households.

The VBP operated up to 2001 and was successful in increasing the number of poor households that had access to credit. There were, however, significant limitations to its effectiveness. Management problems emerged due to the fact that the VBP operated under the VBARD and was not an entity in its own right. It also became apparent that monitoring the use of loans by households was problematic. The most notable limitation, however, was the question over the sustainability of offering low-interest credit to a high-risk group, even on a non-profit basis.

To overcome these difficulties the VBSP was established in 2003 and is now the only bank that offers credit on a social policy basis. The VBSP is fully independent from the VBARD and so its establishment allowed for the full separation of preferential credit from commercial credit. The method of lending centrally involves the four main mass organizations, the Women's Union of Vietnam, the Farmer's Union of Vietnam, the War Veteran's Union of Viet Nam and the Youth Union of Viet Nam. The mass organizations are responsible for establishing savings and credit groups that is the main channel of delivery of funds. They are also responsible for certifying poor households, and for supervising and encouraging borrowers to use loans for their intended purpose. The VBSP deals directly with loan disbursement, loan collection and safe treasury management.

Chapter 4

Accountability of public works entitlements in rural India

4.1 Introduction

Accountability is often cited as one of the reasons for the failure of anti-poverty programs and laws in developing countries (Burguet et al. 2016). The Mahatma Gandhi National Right to Employment Guarantee Act (MGNREGA) in India, is one of the world's biggest public works program, covering around 11 percent of the world's population, and accounts for a significant proportion of India's annual public spend (Niehaus and Sukhtankar 2013a). Yet, even 10 years into its implementation, the Act suffers from leakages, low participation rates and the rationing of work (Sukhtankar 2016; Dutta et al. 2014; Shariff 2009).

Common cited reasons for this lack of accountability include lack of awareness by beneficiaries (Dutta et al. 2014; Jha et al. 2015), lack of independent monitoring of agent behaviour, and dis-empowered clients. For instance, Dutta et al. (2014) and Jha et al. (2015) identify the lack of information awareness on MGNREGA by potential beneficiaries as one the barriers in ensuring the successful implementation of the Act. Ravallion et al. (2013) found that while people were aware that MGNREGA existed, there was low knowledge on the details of the Act.

There is a debate in the empirical literature on the effectiveness of information campaigns on corruption in the delivery of development programs. On one hand, Banerjee et al. (2015b) find that providing information directly to citizens is cost effective in improving government performance. The findings by DiRienzo et al. (2009) indicate that information awareness by beneficiaries can limit the discretionary powers of the government, as it increases the transparency of the scheme, enabling a more efficient delivery of the scheme. On the other hand, Banerjee et al. (2010) and Ravallion et al. (2013) find that awareness, while necessary, may not be sufficient to overcome issues of accountability. In a randomised control trial study on an information awareness campaign on MGNREGA in Bihar, India, Ravallion et al. (2013) found that while the information campaign increased knowledge of the act, it did not translate into improving the performance of the scheme in Bihar. Ravallion et al. (2013) argue that for information to influence the implementation of a program, individuals need to be empowered enough to actually put the information to use. There is little empirical research that looks specifically at how information in the hands of individuals mobilised to use the information affects the prevalence of corruption.

This paper addresses this lack of accountability in the context of households who are exposed to an information campaign and who are also organised to use this information. Making use of an accountability intervention on MGNREGA in Andhra Pradesh (AP), India, this papers contributes to the empirical evidence by estimating the impact of a public information awareness campaign on program implementation. This accountability initiative is embedded within a broader state wide initiative that also reorganises individual households into groups of 10 to 15 households. I argue that such groups should have increased their level of empowerment, as they no longer act as individual households but as a collective. This enables them to use the information provided to them.

In this paper, I ask whether the direct and public provision of information to citizens changes the expected cost of deviation from the program rules and regulations for local implementing agents, resulting in a change in the supply of guaranteed employment. More specifically, I first test whether an information campaign narrows the gap between a household's entitled benefit and their supplied benefit. Next, I look at whether the there is a change in the supply of work by agriculture seasons. Finally, I test whether the information campaign affects the likelihood of local agents engaging in favouritism.[1]

I analyse the implementation of MGNREGA in a principal-agent-client setting. The implication of not having accountability between these relationships is that the government designated agent, who implements the program at the local level, could take self-motivated decisions that result in the mis-allocation or loss of resources to the client (Burguet et al. 2016). This agent can collude with local elites or engage in favouritism with clients (Dutta 2015). As a result of this, an agent may shirk their duty (Reinikka and Svensson 2011), take bribes (Mauro 1995) or mis-use their role for private gains (Treisman 2000). The costs of this misalignment undermine the benefits of the intended programs. Jha et al. (2015) find significant program capture of the Rural Public Works and Food for Work Programs in India, reflecting a degree of collusion between program agents and the village elite.

[1] I define favouritism as when the local agent provides more than the entitled days of work to some but not all households.

Ravallion et al. (2013) argue that any such initiative has to make the client's demand of program entitlements binding. For this, political will is a critical determinant, as it can ensure the successful implementation of a program (Imbert and Papp 2012). This political will has to influence the implementation structure of the program, to ensure that local implementers act according to the programs intended design. From the supply side, one way to do this is through increasing the expected cost of deviating for local implementers.

At the local level, the decision to deviate from the program design by those implementing the program depends on their expected cost of deviating. The discretionary power of these local agents, along with the clarity and complexity of the program, are factors that can influence their expected cost of deviating. Therefore, an accountability initiative that makes the rules of a program clearer and simpler, while also decreasing the discretionary power of a local agent can increase their expected cost of deviating (Ryvkin and Serra 2012). The objectives of the information campaign rolled out in AP on MGNREGA aimed to do this.

In order to investigate the impact of the information campaign on program outcomes, I use the Young Lives panel survey from AP, India.[2] The survey in India collects extensive demographic, consumption, income, assets and job information at the individual and household level since 2002, from 2056 rural household in 15 mandals.[3] This is merged with data from the Government on the MGNREGA information campaign, which was carried out in 6 of these 15 mandals between 2011 and 2012.[4]

The identification strategy in this paper exploits the variation in the implementation of the information campaign to analyse the effect of the information campaign on MGNREGA outcomes. I compare MGNREGA outcomes of households who are exposed to the information campaign, to those in non-treatment areas. In order to make this comparison, the empirical analysis in this paper uses a fixed effects negative binomial estimation model.

For the validity of the identification strategy, I show that the treatment is orthogonal to the outcome variables. The summary statistics, based on pre-intervention data, shows us that treatment and non-treatment households display no difference between the number of days worked but do display a statistically significant difference in the number of days rationed and total days worked in the

[2]On June 2, 2014 Andhra Pradesh was split into two states, Andhra Pradesh and Telengana. This happened 5 months after the field work for Round 4 of the Young Lives survey was completed. Prior to the split, MGNREGA was being implemented by the same department for all areas and there should have been no different approach in implementation.

[3]It also collects information on 811 urban households and from another 5 urban mandals.

[4]A mandal is the second layer of the three-tier local Panchayat Raj system within an Indian State. A mandal has an administrative capital, other villages and potentially other towns within its boundaries. capital state. The population size and number of villages in a mandal can vary. For instance, Mandasa mandal in Andhra Pradesh has 83 villages with a total of over 17,000 households (`http://vlist.in/sub-district/04776.html`), while Bukkapatnam has a total of 41 villages with a total of 40,000 households (`http://www.onefivenine.com/india/villag/Anantapur/Bukkapatnam`).

non-lean season at baseline. I therefore run a fixed effects negative binomial estimation model with the two pre-treatment rounds, 2006 and 2009, to test for parallel trends. I am unable to test for parallel trends on days worked during the non-lean season, as this data was not collected in Round 2. From the results, I find no significant difference between the total number of days households are rationed from their total entitlements. On the other hand, I do find that non-treatment areas display a higher growth in total days worked that is statistically significant. This tells us that there may be a negative bias, which may result in an underestimation of the results from the analysis. This finding provides evidence though, that I can use the variation in the implementation of the information campaign for the identification strategy.

The analysis explores empirically whether the provision of information on MGNREGA to job seekers, who are organised into groups, affects MGNREGA outcomes. The main result from this essay is that post treatment, households in treatment areas are given more days of work under MGNREGA than households in non-treatment areas. For treatment households, this translates into a smaller gap between the days they are entitled to and the days they got supplied to them. The number of days households work get rationed are 24.6 percent less than the days rationed for households in non-treatment areas.[5] This suggests that the demand constraint for what local agents supply is getting more binding. The implications for poor households managing income vulnerability is sizeable, as this means that they are more likely to receive the days of work they need, particularly when other work is not available.

I find that households in treatment areas get more days of work in the non-lean agriculture season. Interestingly, I also find evidence that households in treatment areas have a 3 percent higher probability of working beyond the 100 days entitlement. What these results point to is that an information intervention to workers, who are empowered through being in a group setting, can increase the accountability of a program, helping improve how it is implemented. At the same time, it could also enable beneficiaries to access a greater number of days than their entitlement.

I carry out a number of different robustness checks and confirm that the results hold. I get consistent results when I run the same estimation for households where MGNREGA was started in 2006. When I drop Scheduled Tribe (ST) households, as the implementation of MGNREGA for these households may be different, I still find that the results hold, though the size of the coefficient becomes smaller. Similarly, when I drop areas that were allocated but not treated, the results remain significant and the size of the coefficient becomes larger. Finally, when I test the estimation model used in the paper through using two different estimation specifications, the fixed effects linear estimator and a Poisson robust estimation, I find that the results hold.

[5] In this paper rationing refers to the gap between the total days a person is entitled to work and the actual number of days they are allocated to work.

The remaining part of this paper is structured as follows. Section 4.2 provides a short overview of MGNREGA in India, its impacts and the barriers in implementation. As part of this section, I go into detail on how MGNREGA is implemented in AP and on the accountability intervention rolled out by the state. Section 4.3 goes over the conceptual framework used in the paper and how it applies to the implementation of MGNREGA in AP. This is followed by Section 4.4, which provides an overview of the data used in this paper and summary statistics of the main variables of interest. Section 4.5 lays out the identification strategy and Section 4.6 presents the empirical specification used to study the impact of the accountability intervention on MGNREGA outcomes. The main results are presented in Section 4.7, which includes the robustness checks. Section 4.8 concludes the paper.

4.2 The role of accountability in guaranteeing employment in India

In 2005, the Indian Government passed the National Rural Employment Act, where any adult in a rural household could demand up to 100 days of employment per year. The Mahatma Gandhi National Right to Employment Guarantee Act (MGNREGA), as it was later called, was designed as a demand driven act with universal access across rural India.[6] Through this act, the Government provides the rural poor with a type of social safety net underpinned by guaranteeing work for a set number of days.[7] Over the last decade, MGNREGA generated a lot of interest because it is one of the largest anti-poverty programs in the world, between 2010 to 2011 US $8.9 billion was allocated to it, which is 3.6 percent of government spending (Niehaus and Sukhtankar 2013b).

Yet, there are glaring problems in the implementation of the Act even 10 years after it was started. For instance, participation remains uneven across India. For states such as Haryana and Maharashtra, the participation rate for MGNREGA is between 9 - 12 percent for those classified as poor (Dutta et al. 2014).[8] Interestingly even States that share a border can have a disparity between them on the take-up of the scheme and the number of work days obtained. In Karnataka, the participation rate was only 8.2 percent, while for AP it was over 38 percent (Shariff 2009). Yet,

[6] In 2006, MGNREGA was rolled out in the 200 poorest districts in India, 130 more districts were added in 2007 - 2008, while the remaining districts came under MGNREGA in 2008-2009 (Azam 2012).

[7] Other critical features of this Act was that it set a minimum wage and conferred other workplace rights on workers, including childcare, workplace insurance, distance to work and so forth.

[8] Any household in a rural area is eligible to work under MGNREGA. The participation rate captures the extensive margin of the outcome variable, as it tells us the percent of rural households who take up work under MGNREGA. This does not tell us anything about the number of days a household works under MGNREGA. It only reflects from all eligible households, what percent actually takes up work under the Act. The percent of poor in Haryana is 11.16, while in Maharashtra it is 17 percent (Reserve Bank India 2013).

given that Karnataka has a higher percent of households below the poverty line than AP, one would expect it to actually have more households taking up MGNREGA work.[9]

On the intensive margin, the total number of work days obtained through the scheme is also low for a number of states.[10] For instance in West Bengal, rural households obtained an average of 25 days of work from the potential 100 days available to them (Shariff 2009).[11] This could suggest that for those who are participating, the demand for work is not being met. From the total number of rural households who demanded work, only 56 percent received work (Dutta et al. 2014). One of the cited reasons for this gap is that local officials are rationing the number of work days given (Sukhtankar 2016). In the 200 poorest districts of India, only 3.2 percent of the registered households received the guaranteed annual 100 days of employment (Gaiha et al., 2010).[12]

Even with implementation gaps, the impact of MGNREGA on poverty is substantial. Zimmermann (2013) found that MGNREGA acts as a safety net for households in the face of a negative economic shock. In the short run, Deininger and Liu (2013) found that participants increased their level of protein and energy intake, while in the medium term they accumulate more non-financial assets. Imbert and Papp (2015) found that in the early implementation districts private sector wages increased, which resulted in large welfare gains for the poor even beyond the program effects of MGNREGA. This impact by MGNREGA is not just limited to the current generation. MGNREGA increases the educational outcomes of children whose mother's participated in the scheme (Afridi et al. 2012).

Given that the existing research points to the important welfare impacts of MGNREGA on households, a key question is why do households who want to work not obtain this work and what can be done to facilitate this demand. Making MGNREGA function more efficiently can have a significant impact on poverty, as Ravallion et al. (2013) argue that under ideal assumptions poverty could be reduced by a 12 percent point reduction, as a result of income gains from MGNREGA in Bihar.

4.2.1 Implementation of MGNREGA in Andhra Pradesh

Andhra Pradesh (AP), in South India, is one of the star MGNREGA performers (Dutta et al. 2014). With over 65 percent of AP's 15 to 61 year olds rural based (Office of the Registrar General & Census Commissioner, India 2011), MGNREGA has important implications for rural households and poverty levels in AP.

[9]In 2013, 9.2 percent of households in AP were classified as poor, compared to 20.9 percent in Karnataka (Reserve Bank India 2013).

[10]In this paper when talking about the intensive margin, I am referring to the the number of days worked.

[11]This means that these households had 75 more days that they were entitled to work but did not obtain.

[12]Even in 2015, only 3 percent of households in India had obtained the 100 days of guaranteed employment (Tewari 2016).

AP has been very active in trying to prevent corruption in MGNREGA. First, it took a transparent approach by making all program information accessible and traceable online for the public (Deininger and Liu 2013).[13] In addition, to minimise potential for corruption in payments the state streamlined the payment system (Deininger and Liu 2013). Furthermore, in 2008, AP started implementing social audits to uncover any fraud or other problems in the delivery of the Act.

Yet, even with these steps, Masiero and Maiorano (2017) argue that the power structure of MGNREGA at the village level left scope for local capture. In AP, as per the Act, any rural house can demand up to 100 days of paid work per year. Also per the guidelines of the Act, any rural household in AP who wants to work must first apply for a job card.[14] Once this household receives a job card, they can apply for work.

It is in the application for work where these power dynamics become important. In order to apply for work, until 2010, a household had to submit a written application to the Field Assistant, the key government appointed official at the village level.[15] A duplicate of the submitted application is created, which the Field Assistant signs and then submits to the Mandal level official.[16] At this point the application is computerised and entered into the MGNREGA monitoring system. Within 15 days of the application, work must be sanctioned, if not the household has the right to apply for unemployment benefits.[17]

Therefore in AP, the Field Assistant has a certain degree of discretionary power on what information is being inputted into the computer system (Masiero and Maiorano 2017). Masiero and Maiorano (2017) argue that the system in AP enables the Field Assistant to retain their power at the village level and undermine the empowerment of households who want to work. This then has important implications for the number of work days supplied to these jobseekers and when these work days are supplied.

[13] As early as 2007, this online system was piloted in AP (Kumari et al. 2008).

[14] It is important to note that a job card can only be obtained at the household level and that the 100 days is per household, not individual.

[15] The Field Assistant is the main government official who manages the scheme at the village level and is the point of contact for the households (Masiero and Maiorano 2017). The roles and responsibilities of the Field Assistant: "Assists the Panchayat Secretary, supervises the works, maintains the muster rolls, gives mark outs at work sites, maintains the register of material procured, maintains the village information boards." (Government of Andhra Pradesh 2013).

[16] As mentioned earlier, a mandal is the second layer of the three-tier local Panchayat Raj system within an Indian State.

[17] It would seem that there is little incentive to push for an unemployment payment given the following two points. First according to the Act this unemployment payment is only at the rate of 0.05 per cent of MGNREGA wages per day (Babu et al. 2014). Second, each day of unemployment payment counts towards the 100 days of entitled work days.

Andhra Pradesh's Information Awareness Campaign

In their commitment to ensuring that MGNREGA benefits reached the intended beneficiaries, in 2010, the AP Government took their approach one step further. The Government ran an intervention that reformed how clients could access work under MGNREGA and that provided information to clients on MGNREGA, including on their entitlements and how to address their MGNREGA related grievances. They set up a civil society and government collaboration called the AP NGO Alliance (APNA) to deliver this intervention. APNA's original objective was to "mobilize the rural poor and empower them to fully use the entitlements provided by the Act" (Government of Andhra Pradesh 2010b).

This intervention had significant potential to change the power structure of MGNREGA implementation through changing how informed jobseekers in AP were, how they applied for work and how Field Assistants were monitored. First, as part of this intervention, the structure of how work was applied for was reformed. Starting in mid 2010, APNA NGOs organised individual households into fixed labour groups (FLGs). Between 10 to 15 households were brought together into one FLG. Once a household was part of an FLG, they had to apply for work together to the Field Assistant and they no longer could apply for work as an individual household. This step is very important because it addresses a supply side constraint, in order for work to be sanctioned a minimum number of households have to apply to make the work feasible. By having a group that fulfils the minimum threshold number, it makes it easier to supply the work. This step of the intervention was done in all areas of AP. If there was no alliance NGO covering a particular mandal, the Field Assistant reorganised participating households into FLGs.

The next component of the intervention was to increase knowledge on the Act. Between May 2011 and August 2012, APNA NGOs were to carry out an information awareness campaign to the FLGs. The purpose of this campaign was to educate the workers on their rights and entitlements under the Act and on how they could access their entitlements from the Act (Government of Andhra Pradesh 2010b).[18] Each FLG had to select four representatives that would attend these sessions. A total of five FLG groups would attend each session, so the NGO trained 20 people per session.[19] The NGO would send their staff member (who was not from the village) to the village to provide the training. Guidelines from the government were provided to each NGO on the materials to be covered but how the NGO delivered the information was up to themselves. At the end of the training, the NGO had to submit a completion report back to the government. As for the FLGs representatives, their role was to share the information with those not at the training and then apply the information for the benefit of their FLG group.

[18] Information in the paragraph is based on GO 80 (Government of Andhra Pradesh 2010b).

[19] Around 50 to 65 households would be covered by each training that was given as part of the information campaign.

The final component of the intervention was that NGOs were required to carry out regular monitoring of MGNREGA implementation. The NGOs were to have monthly meetings with the groups to check in on grievances, observe irregular behaviour and carryout fact finding missions on any complaints brought up. While this component of the intervention was only initiated at the end of 2013, outside the timeframe of this study, local officials, including the Field Assistant, were informed about it and knew that it was going to be initiated.

An example of activities by APNA NGOs will help create a clearer understanding of the intervention. According to the Watershed Support Services And Activities Network (WASSAN), activities they did as part of APNA included the following. First, to train FLGs on how to maintain and update records at the village level. Second, to conduct regular FLG meetings at the village level. Third, to support FLGs in identifying what works could be done on their land. Fourth, to encourage job card holders to use the toll-free complaint number in voicing their grievances. Fifth, to mobilise wage seekers to be involved in following up with the mandal administration for the implementation of identified works.[20]

Each of these steps had the potential to change the power structures at the village level, particularly the level of discretionary power of the Field Assistant.

4.3 Conceptual Framework

The impact of information on corruption and program delivery depends on its nature, in particular whether it is given publicly (Reinikka and Svensson 2011; Banerjee et al. 2015b) and whether it targets citizens or government employees (Banerjee et al. 2015b). The question then becomes whether the type of accountability intervention implemented in AP can change the outcomes of MGNREGA.

In this paper, I view accountability in the setting of a principal-agent-client. In the implementation of MGNREGA, the principal, the central and state government, laid out the conditions under which the client can access work and the total days they are entitled to each year. The principal wants the program to be implemented as designed, yet is unable to oversee the rollout of the program in all locations, especially at the village level where the program is implemented.

To ensure implementation, the principal can hire an agent to oversee the implementation and to ensure that clients accesses the program according to the rules and regulations of the program. In

[20]This information is taken from National Consortium on MGNREGA (2016) at `http://www.nregaconsortium.in/wassan-and-partners/`.

this way, the principal delegates decision making power to the agent.[21] The agent, appointed by the principal for the implementation of MGNREGA at the village level in AP, is the field assistant. This field assistant serves for as many years as the field assistant wants unless the principal decides to replace the assistant. The field assistant receives a wage, w, for each period for performing the delegated responsibilities.[22] If the field assistant is replaced, any future income is forfeited.

The client is entitled to a certain level of benefit, which I term E. If a client chooses to work under MGNREGA, they have the right to demand 100 days of entitled work per household per year at a guaranteed wage.[23] Therefore under MGNREGA, their entitlement is 100. The field assistant can provide the number of days demanded or not.

There is a problem of information asymmetry, as the client and agent may have vested interests which the principal does not know about and can not observe. The field assistant though knows what the clients entitlements are and what level of benefits the client can demand. The client on the other hand, may not be fully aware of the all the rules and regulations governing their entitlements.

Given this lack of clarity on the rules by the client, there is a possibility that the field assistant could deviate from the behaviour that the principal wants adhered to, which has implications for the client (Burguet et al. 2016). More specifically, the field assistant can collude with local elites or engage in favouritism with clients (Dutta 2015). As a result, a field assistant may shirk their duty (Reinikka and Svensson 2011), take bribes (Mauro 1995) or mis-use their role for private gains (Treisman 2000). In addition, depending on the type of decision made by the field assistant, the outcome may also be beneficial to some of the clients (Burguet et al. 2016), for instance if they engage in favouritism. However, from the principals perspective, any deviation from the program design is not viewed as optimal (Burguet et al. 2016).

If the field assistant decides to deviate, there are two potential deviation options in regards to total days of work supplied, S, under MGNREGA. First, the field assistant could decide to engage in favouritism through providing over the maximum number of days permitted to certain households, but not all households, in return for some share, s, taken from the total days supplied. Therefore, $S > 100$, which is the maximum entitlement the client can access per year.

[21]For this section I draw on the paper "The Microeconomics of Corruption. A Review of Thirty Years of Research." by Burguet et al. (2016).This paper reviews microeconomic research on corruption, laying out the main theoretical models of corruption, the incentives for bureaucracies, the measurement of corruption and finally the empirical evidence on corruption.

[22]The government has two purposes in setting w, to prevent corruption and to ensure that the official implements the program as designed (Burguet et al. 2016). I assume that the type of wage provided is a reservation wage that doesn't stop corrupt officials from accepting bribes or deviating from program design (Burguet et al. 2016). In order to prevent officials from taking any bribe Burguet et al. (2016) state that the principle has to pay an efficiency wage

[23]Supply of work in terms of projects should not be an issue because the Act stipulates that there should always be a shelf of works that can be given when work is requested. Furthermore, as found by Leelavathi and Hanumantha Rao (2010) there were sufficient number of works on the shelf to meet the demand for work in Andhra Pradesh.

The other option is the field assistant could decide to ration the days of work, where the field assistant provides less than the days demanded by the client, where $R = 100 - S, R \geq 0$. The field assistant may do this if this behaviour avoids some cost, c, by adhering to the wishes of a powerful local constituency, such as the local farmers, if the delivery of the benefit is not in their interest (Ravallion et al. 2013).[24] In addition to rationing, as a result of the collusion with local farmers, the field assistant may not provide work during harvest times, even if the client has demanded work.[25] As a result, one would see very few days of work being provided in the non-lean seasons, July - February, and the majority of the days during the lean season, March - June.

Based on these points, I propose that the behaviour of the local field assistant is determined by the profits and costs from their individual behaviour identified above. In addition, their behaviour is also determined by the likelihood of the field assistant getting punished. Pulling this into an equation gives us the field assistant's generic expected income, $E(Y_A)$, which is captured in equation 4.1

$$E(Y_A) = w - d(E - S)^2 + (s - c)S \tag{4.1}$$

.

In equation 4.1, if the field assistant deviates from the program design the field assistant incurs a non-linear cost. This non-linear cost is the expected cost of deviation, d, times the squared size of deviation.[26] I assume that each field assistant has some level of risk aversion that feeds into this cost of deviation (Shenje 2016). The size of the expected cost of deviation is determined by the difference between the amount of benefit the client is entitled to, E, and the amount of benefit the field assistant supplies to the client, S. The larger the size of deviation, the more likely that a deviation will be noticed, therefore I assume the expected cost changes depending on the size of deviation. For small deviations, it may go undetected, incurring no costs. For slightly bigger deviations, the cost incurred could be a warning. I assume above a certain thresh hold of deviation, the cost of deviation is job loss. Depending on the size of deviation, the associated risks and the field assistant's level of risk aversion, this should then increase the cost of deviation.

Ideally, the amount each client is entitled to is binding on what is supplied by the field assistant. Maximising and solving for the amount of benefit supplied, from equation 4.2 gives us:[27]

$$S = E + \frac{s - c}{2d} \tag{4.2}$$

[24]The cost of not adhering to the wishes of such a group could be a combination of things, such as reputation slander or putting additional pressure on the field assistant by various members of the group.

[25]The reason for this is that this is the time when local farmers need to hire people, so they may put pressure on the field assistant not to provide work during these times.

[26]I square the term $(E - S)$ because if not, in the optimisation of equation 4.1, the outcome would either be an infinite number of days provided or no days of work provided at all. This term also helps capture discrete changes in the size of the field assistant's expected punishment.

[27]Please find the solution for this in appendix 4.C.

.

If equation 4.2 is applied to MGNREGA, the field assistant's decision on the total days to supply is laid out in equation 4.3:

$$S = 100 + \frac{s - c}{2d} \tag{4.3}$$

In this model, there is always some deviation. What determines this deviation is the ratio between the field assistant's net benefit from deviating, $s - c$, and the field assistant's expected cost of deviation times two. As the expected cost of deviation gets bigger, the demand constraint becomes increasingly binding. Furthermore, as the expected cost of deviation goes to infinity, this deviation should go towards zero.

The size of deviation may decrease or increase, if the ratio changes. One way to change this ratio is if the expected cost of deviation changes. Factors that affect this cost is the level of discretionary power the field assistant holds, how clear the rules of the program are, and how simple it is to access the entitlements under the program. The higher the discretionary power of the field assistant or if the rules of the program are unclear or complicated, this cost d is lower for the field assistant (Ryvkin and Serra 2012). Therefore, any initiative that makes the rules of a program clearer, that makes knowledge on the program more public or that decreases the discretionary power of a field assistant should impact this expected cost of deviation (Ravallion et al. 2013; Ryvkin and Serra 2012). I therefore propose in equation 4.4 that the expected cost of deviation is a function of the following factors:

$$d = f(K_p, Emp, G_{mech}, P) \tag{4.4}$$

.

Where K_p is the level of public knowledge on the program by clients, Emp is how mobilised clients are to use this information, G_{mech} is the strength of the grievance mechanism, and P is the size of punishment. If any of these terms increase, the expected cost of deviation should also increase.

Based on this, I argue that any intervention that changes the bargaining dynamics between the principal - agent - client can change the outcome of the game. This is particularly true for an intervention that increases the bargaining power of one of the actors compared to another by changing the expected costs incurred from their behaviour. An accountability initiative that can change any of these factors, should impact the expected cost of deviation and therefore the outcome of a program. Furthermore, if this initiative changes future expectations about discretionary power and the cost of deviation, this would then also impact current behaviour (Niehaus and Sukhtankar 2013a).[28]

Given that one of the components in determining the cost of deviation is public knowledge, K_p, I argue that as a result of the APNA information campaign the expected cost of deviating should increase.[29] The clients now have increased knowledge on the Act, including on what their entitlements are, the associated rules and regulations and a better understanding on the complaints mechanism. Furthermore, given that the information was provided through a public training, both the field assistant and the client now know that the other knows the entitlements and rules of the program. This should decrease the discretionary power of the field assistant.

Ravallion et al. (2013) state that for an information intervention to be effective, in addition to being informed, clients need to be empowered to use this information. As discussed earlier, prior to the information campaign being rolled out in 2011 - 2012, AP had a change in how households applied for work, resulting in a change in group size in 2010. This change from households applying individually for work, to applying as a group of 10 - 15 households, should have affected the expected cost of deviation in all areas. The reason for this, I argue, is that this change in group size should have increased the empowerment factor, Emp, in equation 4.3.

The level of empowerment of workers should increase, as they no longer act as individual households but as a collective. If the group decides to react to a deviation by the field assistant, they no longer need to act alone. The second implication of this is that if the field assistant was previously favouring one client (household), given that client is now part of a group of 15 households, the field assistant has to deal with all 15. If the field assistant continues to favour one, it is very likely that the others will complain. Furthermore, if the field assistant favours one group, it is likely other groups will notice and complain as a group.

While the monitoring component of APNA could also have important implications on the expected cost of deviation, it was not implemented within the data time frame. Therefore, given that neither the grievance mechanism nor the size of punishment changed in the time frame I am focusing on, any change in MGNREGA outcomes should be driven by the change in group size and public knowledge.[30] Yet, given that the APNA information campaign was not rolled out in all areas, any

[28]The intuition laid out by Niehaus and Sukhtankar (2013a) is that the expectation of future wages affects the incentive to cheat today. In their paper, they find that future rent expectations matter on the agents current behaviour.

[29]This is because if the field assistant is engaging in an indiscretion it is more likely post intervention that the behaviour comes to light and the punishment at the village level is usually the replacement of the agent.

[30]The size of the punishment did not change but post-intervention clients were mobilised and informed about their rights, entitlement and that there was a grievance mechanism. In addition, now the field assistant knows that the clients know their rights. Therefore, the likelihood of punishment changes, i.e. the expected cost of deviation, even if the size of punishment has not. As it is more likely in the case of an indiscretion that clients complain or act on it, or for the field assistant the threat point of complaining and punishment has increased. Therefore the expected cost of deviation changes through the channel of increased knowledge and level of empowerment.

differences between MGNREGA outcomes between treatment and non-treatment areas should be due to an increase in the knowledge of MGNREGA, K_p, as a result of the information intervention.[31] I argue that as a result of this increase in knowledge in treatment areas, the expected cost of deviation in these areas for the field assistant should increase. I test the following hypotheses:

- *Hypothesis 1:* An increase in the expected cost of deviation decreases the rationing effect in treatment areas, where the gap between entitled days and supplied days of work is narrowed;
- *Hypothesis 2:* An increase in the expected cost of deviation increases the number of days worked in the non-lean season in treatment areas.
- *Hypothesis 3:* An increase in the expected cost of deviation decreases the number of households who have worked over the entitled number of annual days in treatment areas.

4.4 Data Description

For this paper, I use three of the four rounds of the Young Lives panel survey from Andhra Pradesh (AP), India.[32] Since 2002, the survey in India has collected extensive demographic, consumption, income, assets and job information at the individual and household level from 3000 households in 87 villages from 20 mandals across 7 districts in AP.[33] Of these mandals, 5 are urban and are therefore not included in this study as MGNREGA is only implemented in rural areas. The survey is representative of the geographical regions and the poverty distribution within the two states (Galab et al. 2011).

Since 2006 (Round 2), the survey collected information on MGNREGA, including total number of work days obtained under MGNREGA for each individual in the household. In 2009 (Round 3) and 2013 (Round 4), they also provided a breakdown of total days worked by lean and non-lean seasons. Therefore, for the main estimation model I use the two most recent Young Lives rounds, as I am able to look at the total days worked under MGNREGA and by season.

The other reason for using the two most recent rounds of the Young Lives data is because MGNREGA was only implemented in all areas by 2008. At the time of the Round 2 survey, MGNREGA had only been running for six months in 4 of the 6 rural districts included in the Young Lives survey.[34] In the identification strategy and the robustness checks, I make use of the data from Round 2.

[31] All areas were affected by the change in group size, therefore if there is any difference in MGNREGA outcomes, this difference should be driven by the change in level of awareness.

[32] As mentioned earlier, in 2002 when the first round of the survey was collected, AP and Telangana were one State. In 2014 they were split into two, though this is after the time frame for this study

[33] In each village around a 100 households are surveyed. The attrition rate for the households is relatively low, with an overall attrition rate of between 0% - 2% between the four rounds already fielded (Galab et al. 2014).

[34] In 2006, Anantapur, Kadapa, Mahbubnagar and Karimnagar were brought under MGNREGA, in 2007 Srikakulam was brought in and West Godavari was only brought in by 2008 (Government of India 2005)

Table 4.1: Descriptive Statistics: MGNREGA outcome variables at the household level, Round 3 & 4, Young Lives Survey Data

	Round 3		Round 4	
MGNREGA Statistic	Mean	St. Dev.	Mean	St. Dev.
Has a JobCard %	80	0.40	83	0.37
Worked in MGNREGA last 12 months %	70	0.46	64	0.48
Total Days Rationed	60	35	54	38
Worked over 100days %	10.0	0.30	9.0	0.29
Worked over 150days %	3.9	0.19	3.4	0.18
% Treated	0.0	0.00	41	0.49
Total Number of Days Worked by Household in NREGA				
Annually	46.4	55	49.5	48
Lean Season	27	36	39	44
NonLean Season	20	32	11	26
Total Number of Days Worked by Women in NREGA				
Annually	27	33	29	32
Lean Season	16	21	22	28
Non Lean Season	11	19	6	15
Total Number of Days Worked by Men in NREGA				
Annually	20	32	21	31
Lean Season	12	20	17	25
Non Lean Season	9	18	5	16

From Table 4.1, one can see that between 2009 (Round 3) and 2013 (Round 4), the number of households working in MGNREGA increased and the total days worked also increased.[35] In 2009, 80 percent of the households had job cards while 70 percent of them had worked in MGNREGA in the prior 12 months. By the time of the Round 4 survey in 2013, a total of 83 percent of households had a job card, though the percent who worked in the prior twelve months decreased to 64 percent.

Table 4.1 also shows us that the total days worked in the non lean seasons, which is from July to February, decreased between the two rounds, while the days worked during the lean season increased. Additionally, the days worked by women is higher than those worked by the men, though the total days for both increased between the rounds.

In terms of whether one can see any signs from the raw data that local agents are deviating from the entitled days of work per household, one can see that between 2009 and 2014, the total days rationed decreases,[36] as does the percent of households who worked over the total entitled days. This would suggest, that field assistants are increasingly complying with the rules regarding the total number of days households are entitled to work per year. On the other hand, field assistants are less likely to provide work during the non-lean seasons, which would be in line with the demands of farmers who may not want competition during the seasons when they need to hire labour to work on their lands. Overall, these numbers point to a decrease in the percent of households working under MGNREGA but an increase in the number of days for those who are working. Given the reorganisation of households into working groups, one would expect the increase in number of days worked as the group application makes it easier for work to be supplied. In addition though, throughout this analysis, I will examine whether this change was also driven by the changed level of public knowledge in the treatment areas.

I merge the survey information with data obtained from Government orders on which mandals were allocated to receive the information campaign and with Government data on which areas actually received the information campaign, when and by which NGO.[37] A significant number of NGOs applied to be part of APNA and 330 NGOs were accepted into the initiative (Government of Andhra Pradesh 2010a). A total of 703 out of the 1098 mandals in Andhra Pradesh were included in this initiative (Government of Andhra Pradesh 2010a). Of the 15 rural mandals in the Young Lives survey, 9 were allocated to receive the treatment, while 6 were not. When I match the survey data with government data on which areas were treated, I see that from these 15 mandals, a total of 6 areas received the information awareness campaign, while the other 9 did not. So of the 9 areas allocated to receive treatment, 3 allocated areas did not receive treatment.

[35] Round 3 was collected at the end of 2009 and the beginning of 2010

[36] As mentioned previously, in this paper rationing refers to the gap between the total days a person is entitled to work and the actual number of days they are allocated to work.

[37] NGO's had to deliver the training to all villages in a mandal from June 2011 to May 2012.

4.5 Identification Strategy

For the identification strategy, I use the variation in the implementation of the information campaign to analyse the impact of information awareness on MGNREGA outcomes. From the data on the APNA initiative, I only know which areas were allocated for treatment and which areas were actually treated.[38] The main concern with using this identification strategy is whether the treatment is orthogonal to the outcome variables: is the treatment driving the results or are the results due to the initial difference between the mandals. In particular, were areas that were assigned to NGOs and treated different to non-treatment areas.

In this section, I lay out the main issues of potential endogeneity and address each point individually. As discussed above, the first issue of concern is whether pre-treatment there were differences between the implementation of MGNREGA across allocated/treatment areas. If there are large differences between these areas, particularly on the intensive margin for MGNREGA outcomes, this is an indicator that these areas are different in regards to the research question and therefore these differences maybe driving the results.

Another issue is whether treatment areas are more open to being organised and mobilised by NGOs, and are therefore more able to act on information provided to them. An additional issue is if treatment is determined by the reach of NGOs, where NGOs are more likely to apply to treat areas if their presence in the area is high. It could also be an issue of access which determines treatment, harder to reach areas are less likely to be treated. I address each of these points below by first comparing pre-treatment summary statistics and then I run a fixed effects negative binomial with the two pre-treatment rounds, 2006 and 2009, to test for parallel trends.

Table 4.2: Descriptive Statistics on MGNREGA, Non-treatment versus treatment, Round 3, 2010

Intervention	Area Allocated	Area Not Allocated			Treated	Not Treated		
MGNREGA Outcome Variables	Mean	Mean	t-test	p value	Mean	Mean	t-test	p value
Has a Job Card	82%	77%	-2.35**	0.02	81%	79%	-0.97	0.33
Worked for NREGA in last 12 months	72%	68%	-1.88*	0.6	72%	69%	-1.56	0.12
Days Rationed (less than 100)	60.5	59	-0.82	0.42	58	61	1.78*	0.08
Worked for Over 100 days	28%	27%	-1.00	0.31	10%	11%	-0.77	0.44
Total Days Worked	47	46	-0.32	0.74	49	45	-1.43	0.15
Total Days Worked Non-Lean Season	22	17	-3.14***	0.0	22	17	-3.17***	0.00
Total Days Worked Lean Season	26	29	1.8*	0.07	27	28	0.47	0.64

Allocated areas are mandals that were allocated to NGOs for treatment. Treated areas are areas that were both allocated to NGOs for treatment and that received the treatment. There are a total of 1,089 households in allocated areas and 967 in non allocated areas, while there are a total of 835 households in treatment areas versus a total of 1,221 in non-treatment areas.

I first address the performance of MGNREGA in treatment versus non-treatment areas. When one looks at MGNREGA outcomes, both on the extensive and intensive margins, one can see from Table 4.2 that on the extensive margin, households in allocated areas, as well as treatment areas, are

[38] I do not know if an area did not get allocated because no NGO applied to work in that area or because the Government decided not to allocate the area for treatment even if an NGO applied for it.

more likely to have job cards and have worked in the 12 months prior to the survey. The difference between the means is only statistically significant between allocated and non-allocated areas. For treatment versus non-treatment areas, the difference is not statistically significant.

Now turning to the intensive margin, once a household works, there is no difference in the total number of days they work between allocated and non-allocated areas, as well as for treatment versus non-treatment areas. Yet, there is a statistically significant difference in the number of days households work in the non-lean season and the total days rationed between treatment versus non-treatment areas. This difference pre-treatment is important because it puts into question whether the treatment versus non-treatment areas are comparable on these two outcomes. Yet, as I will show further on this section, when I run a parallel trends regression to test for pre-intervention trends, I find that there is no difference between the total days rationed between the areas, once I control for all other factors. I can not test for parallel trends in regards to days worked in the non-lean season, as Round 2 of the Young lives data did not collect information on this metric in 2006.

The next issue I look at is whether the households in treatment areas are different in their ability to be mobilised and act on information provided to them. If households have taken action collectively in a community, participated in an awareness campaign or taken part in a protest, they are potentially more likely to respond to such types of information interventions. The Young Lives survey collects information on how mobilised households are to act collectively. If one looks at this information, as a measure of activism across the different areas, from Table 4.3, one can see there is little difference between allocated and non-allocated areas and treatment versus non-treatment areas in the percent of households who have taken action on a serious issue, and participated in an awareness campaign.

Another concern one may have is that NGOs are more likely to apply to cover a mandal and treat areas if their reach is high in the area. In the set up of the alliance, the criteria for participation in APNA from the Government was that while any NGO could apply to be part of APNA, they had to have experience working on rights based issues. In addition, when they applied they also had to state which mandals they wanted to cover.[39] If they got the mandals they requested, they had to deliver the intervention to all villages in the mandal. This is important because as stated earlier if NGOs are already working in a community, particularly rights based NGOs, it could be that they are more likely to treat that area. Round 2 of the Young Lives survey collected information on whether a locality had an NGO working there in 2002 and in 2006, and what type of NGO it was. If one looks at Table 4.3, one can see that there is no significant difference in NGO presence between allocated versus non-allocated areas. On the other hand, treatment areas have a significantly higher percent of NGOs than non-treatment areas. This difference is driven by NGOs engaged in children

[39] In talking to NGOs in Mahabubnagar, the feedback was that as they were unsure how many areas they were going to get allocated by the government, they applied to work in more areas than they could cover.

activities, not rights based activities. This would suggest that this difference is not the driving reason for why areas which were allocated got the treatment.[40]

Table 4.3: Descriptive Statistics, Non-treatment versus treatment, Round 3, 2010

Intervention	Area Allocated	Area Not Allocated			Treated	Not Treated		
Households level of Activism	Mean	Mean	t-test	p-value	Mean	Mean	t-test	p-value
Taken Action With Others	0.25	0.16	-1.22	0.22	0.25	0.18	-0.83	0.40
Participated In An Awareness Campaign	0.17	0.15	-1.32	0.19	0.16	0.16	-0.07	0.94
NGO presence in the area								
Local NGO present in 2002	0.49.5	0.46	-1.63	0.102	0.59	0.40	-8.8***	0.0
Local NGO present in 2006	0.49	0.48	-0.62	0.53	0.56	0.44	-5.58***	0.0
Rights Based NGO Working in Area	0.09	0.18	5.59***	0.0	0.14	0.12	-1.46	0.14
NGO working on children issues	0.13	0.22	5.24***	0.0	0.26	0.11	-8.8***	0.0
Locality level of Access								
Village Accessible by Road	0.96	0.98	2.8***	0.01	0.96	0.98	2.5**	0.02
Paved Road	0.41	0.31	-4.7***	0.0	0.17	0.50	16.5***	0.0
Motorized Unpaved Roads/Tracks	0.80	0.83	1.96**	0.05	0.89	0.76	-8.1***	0.0
Religion								
Hindu %	98	95	3.7***	0.0	97.6	95	3.2***	0.0
Scheduled caste %	17.5	25.1	-4.17***	0.0	22.6	20.4	1.22	0.22
Scheduled tribe %	14	16	-1.27	0.20	7.4	26.2	-12.16***	0.0
Backward caste %	56	42.5	6.32***	0.0	58.3	35.3	10.5***	0.0
Other Caste %	12.00	16	-2.6***	0.01	11.3	18.00	-4.3***	0.0

Finally, it could be an issue of access. Allocated and treatment areas maybe physically different to non-allocated and non-treatment areas, which also could impact the outcome variables. Accessibility is key because areas that were less accessible may be harder to treat but also harder to implement MGNREGA in. To see if this is determining the delivery of the treatment I look at access to these areas and the size of the mandals. When I look at accessibility, the key take away from Table 4.3 and Table 4.4 is that all these areas are accessible. A total of 96 percent of households in both allocated and treatment areas can be accessed by a vehicle, compared to 98 percent of non-allocated and non-treatment households. This is critical as an NGO would have to visit and train all parts of the mandal over the intervention year.

In regards to size, in Table 4.4, one can see that the while the mean number of households in allocated mandals are larger, the total number of villages in these mandals is almost the same. So the number of villages that an NGO would need to cover in their training would be the same. While the mean number of villages for treatment areas is higher than non-treatment mandals, the number of households in non-treatment areas is larger. On average allocated and treatment mandals are not very different in terms of accessibility or size to non-allocated and non-treatment mandals.

[40] These questions were only asked in the 2006 Young Lives survey, not in the follow-up surveys. While these numbers may have changed by Round 3 in 2009, I do not expect to see a big change given that the changes between 2002 and 2006 were relatively small.

Table 4.4: Mandal Descriptive Statistics, Non-treatment versus treatment, Round 3, 2010

Mandal Name	Total Number of Households	Total Number of Village	Distance from District HQ	Year MGNREGA Started
Amrabad	10,469	44	115 km	2006
Atlur	6,771	99	33 km	2006
Bukkapatnam	9,712	41	68 km	2006
Buttayagudem	12,813	50	69 km	2008
Chapad	9,595	62	40 km	2006
Devarkadara	9,851	39	25 km	2006
Dharur	7,851	56	37 km	2006
Gudibanda	9,917	58	110 km	2006
Kataram	7,719	58	100 km	2006
Kotabommili	15,871	43	39 km	2007
Mandasa	17,814	75	103 km	2007
Nawabpet	8,929	72	27 km	2006
Regidiamadalavalasa	15,451	66	37 km	2007
Seethampeta	11,388	113	55 km	2007
Vajrakarur	9,671	28	51 km	2006
Mean of Allocated Mandals	9,911	61	54 km	
Mean of Non-Allocated Mandals	12437	59	71 km	
Mean of Treated Mandals	9,992	66	53 km	
Mean of Non-Treated Mandals	11,541	58	66 km	

Note: Allocated Mandals are Bukkapatnam, Vajrakarur, Atlur, Chapad, Amrabad, Buttayagudem, Nawabpet, Devarkadara, Seethampeta. Treated . Mandals are: Bukkapatnam, Vajrakarur, Atlur, Chapad, Buttayagudem, Seethampeta. The following are neither allocated nor treated : Regidiamadalavalasa, Kotabommili, Mandasa, Gudibanda, Kataram, Dharur. Information on each mandal comes from `http://www.onefivenine.com/` and `http://www.census2011.co.in/census/state/andhra+pradesh.html`

One additional issue in accessibility could be determined by the Caste origin of the household. In particular, Scheduled Tribe (ST) communities tend to be slightly more remote and therefore harder to access (Motkuri 2013). When I look at the breakdown of Caste between treatment and non-treatment areas, what one can see is that non-treatment areas have a significantly higher number of ST households. This may be important in determining the outcomes of interest, therefore I deal with this as part of the robustness checks.

Given that there are differences between the treatment and non-treatment areas, including on the total number of days rationed, it is important to compare pre-treatment trends to help test how comparable the areas are to each other. Once I add in the Round 2 data from the Young Lives survey, which was collected 6 months into the implementation of MGNREGA, I have two rounds of data pre-treatment. This enables us to look at the pre-treatment trends and test the parallel trend assumption.[41]

At the time of the Round 2 data collection in 2006, MGNREGA was implemented in over 66.5 percent of the treatment localities compared to 63.1 percent of those in non-treatment areas. By 2009, when Round 3 of the Young Lives survey was collected, MGNREGA was implemented in all rural areas covered by the survey. When one looks at Table 4.5, one can see that for areas where MGNREGA was implemented, there is no significant difference between the percent of households with job cards or who have worked in the prior 12 months. Households in treatment areas though work more days in 2006 and 2009 but this difference is only statistically significant for 2006. By

[41]It is important to note that this only gives us 6 months of data on how MGNREGA was implemented in these areas. Yet, what I am interested in is whether the trends in total days worked and rationed changed. So even with 6 months of data one should be able to look at these trends.

2009, households in non-treatment areas have a similar number of days that they work compared to treatment areas. This suggests that the growth rate at which households in non-treatment areas are accessing work is higher than treatment areas between the two survey rounds. When one looks at total days rationed, one can see that treatment areas are rationed fewer days than non-treatment areas. This is statistically significant for both rounds. Therefore, it is important to check for parallel trends prior to treatment to see what the implications of this are for the identification strategy.

Table 4.5: Round 2 and 3 MGNREGA Descriptive Statistics, Non-treatment versus treatment, Round 2 & 3, 2010

Round	2, 2006 - 2007				3, 2009 - 2010			
Intervention	Treated	Not Treated			Treated	Not Treated		
MGNREGA Outcome Variables	*Mean*	*Mean*	*ttest*	*p-value*	*Mean*	*Mean*	*ttest*	*p-value*
NREGA operates in locality %	67	63	-1.61	0.11	100	100		
Has a Job Card %	70	73	1.37	0.17	81	79	-0.92	0.36
Worked for NREGA in last 12 months %	58	55	-1.14	0.26	72	69	-1.5	0.13
Total Days Worked	33.4	14.7	-9.73***	0.0	49.5	45.2	-1.57	0.11
Total Days Rationed	69	85	9.73***	0.0	58	61	1.75*	0.08

As a final step, I apply the empirical strategy laid out in equation 4.5, from Section 4.6 below, to the two pre-intervention rounds of data.[42] A fixed effects negative binomial estimation model with an interaction variable for treatment areas and time allows us to capture any trend changes in treatment areas pre-treatment. In particular, one is looking to see if there is any upward trends in treatment areas compared to non-treatment areas prior to the treatment, as this would make it hard to argue that the difference in MGNREGA outcomes is due to the treatment. Ideally, the parallel trends assumption would hold where the change in outcomes is not significantly different between the two areas.

From Table 4.6, I find that while the number of households with a job card is statistically higher in treatment areas, there is no statistical difference between whether households worked in the prior 12 months once they had a job card pre-treatment. Importantly for this paper, I find no statistical difference between the number of days households get rationed between treatment and non-treatment areas. I do find that household in non-treatment areas had a higher growth rate in the number of days they worked between 2006 and 2009, which is line with discussion above. Pre-treatment, controlling for all other factors, households in treatment areas increased the number of days they worked at a rate of 0.76 compared to those in non-treatment areas.[43] This suggests

[42]The sample size for this analysis is smaller as only 64.1% of the households were in areas where MGNREGA was implemented by end of 2006. Furthermore, in 2006 only 56% of the households worked in the prior 12 months.

[43]I report the incidence rate ratios (IRR). The IRR from a fixed effects negative binomial is interpreted as the log of the ratio of expected counts. It can be thought of as the number of times (rate) at which those in treatment areas experience a particular event compared to those in areas that did not experience that event. On the other hand, the coefficient is interpreted as the difference between the log of expected counts (UCLA: Statistical Consulting Group 2017).

a slower positive trend in total days worked in treatment areas, pre-treatment, compared to non-treatment areas. The implication for this paper is a potential negative bias in the estimation strategy, where I underestimate the results.

Table 4.6: Pre-Information Campaign trends of Total Days worked, Round 2 & 3

VARIABLES	(1) Has Job Card OLS2	(2) Work last12 Months OLS2	(3) Ration	(4) DaysWorked
InterventionxRound3	0.055**	-0.031	1.045	0.711***
	(0.026)	(0.032)	(0.108)	(0.0772)
HHGen	-0.031	-0.105	1.407**	0.843
	(0.058)	(0.071)	(0.197)	(0.177)
hhsize	0.008	0.016	0.916***	0.976
	(0.009)	(0.010)	(0.0221)	(0.0277)
HHHEduc	-0.002	-0.002	1.004	1.001
	(0.002)	(0.003)	(0.00813)	(0.00630)
HHHAge	0.000	-0.001	0.998	1.001
	(0.001)	(0.001)	(0.00239)	(0.00219)
irrgt	0.029	-0.043	1.168*	0.798**
	(0.025)	(0.033)	(0.105)	(0.0757)
MotorAccessible	0.010	0.071	0.828**	1.390**
	(0.038)	(0.048)	(0.0755)	(0.193)
Factory	0.110***	0.029	0.761*	0.712*
	(0.040)	(0.047)	(0.123)	(0.128)
EconShock	0.008	0.004	0.988	1.005
	(0.020)	(0.024)	(0.0650)	(0.0860)
NaturalDisaster	0.001	0.045*	0.932	1.160*
	(0.021)	(0.025)	(0.0704)	(0.0993)
3.Round	0.075***	0.147***	0.621***	1.669***
	(0.024)	(0.030)	(0.0534)	(0.171)
Household FE	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes	Yes
Observations	2,647	2,557	1,772	1,694
R-squared	0.074	0.105		
Number of childid	1,325	1,325	886	847

We report the Incidence Rate Ratios (IRR) from the fixed effects NBREG regressions.
Standard errors are clustered at the household level and are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Given these results, using the variation in treatment enables us to look at how increased public knowledge on the Act affects the expected cost of deviation for the field assistants in treatment areas and the amount of program benefit they supply to clients.

4.6 Empirical Specification

The conceptual framework laid out that the information campaign should affect the expected cost of deviating for the field assistants who supply work to the clients. Ideally, I would have a measure to capture this increased cost of deviating for the field assistant but as I do not, I look at MGNREGA outcomes. If the information campaign succeeded in increasing the cost of deviation, this should then affect the MGNREGA outcomes.

I first estimate the impact of the campaign on the extensive margin, the likelihood of the household having a job card and participating in MGNREGA, but I expect it not to affect the take up of MGNREGA given the structure of the information campaign. The information was provided to existing working groups of MGNREGA households, therefore these are workers who are already working in MGNREGA. Households who do not participate in either round are unlikely to have been exposed to the training and therefore unlikely to be affected by the training.

If successful the information campaign should affect the total days supplied to households who want to work. If one can see a decrease in the number of days rationed (Hypothesis 1), an increase in the total days provided during the non lean season (Hypothesis 2) or any evidence of favouritism (Hypothesis 3), this would signal an increase in the field assistant's expected cost of deviating.

To investigate this further, I estimate *hypothesis 1*, the total number of days rationed, which is the gap between the entitled days of work, 100, and the total days obtained. I am testing *hypothesis 1*, by looking at whether the total number of days rationed is different for treatment areas. If I find that there is a decrease in the rationing of MGNREGA work days in treatment areas, this provides evidence that the expected cost of deviation for agents has increased and that the demand constraint has become more binding.

Next, I estimate *hypothesis 2* and look at the days worked by seasons, to see if there is any difference in days worked in treatment areas by season. If I see an increase in non-lean days worked this also signals that the demand constraint is becoming more binding. Households are better able to obtain work when they want work.

Finally, I estimate whether there is any evidence of favouritism, which is what *hypothesis 3* predicts. I estimate this as a dummy that takes the value of 1 if a household receives more than the entitled days of 100.[44]

[44] I also test for over a 150 days of work supplied as at certain time periods, such as a bad drought year, the total number of entitled days can go up for certain areas to 150 days.

For each of the dependent variables I use the empirical specification laid out in equation 4.5:

$$Y_{jt} = \beta_1(TreatArea_j * Post2012_t) + \beta_2(Post2012_t) + \delta X_{jt} + \gamma Z_{kt} + \alpha_j + e_{jt} \tag{4.5}$$

where Y_{jt} is the dependent count and dummy variables for household j in time t for each specification, including total days rationed, total days worked annually, total days worked in the lean season, total days worked in the non-lean season, the probability of having a job card, working under MGNREGA in the last 12 months and working over the 100 day annual entitlement; $TreatArea_j$ is a dummy variable for whether household j was in the treatment areas; $Post2012_t$ is an indicator that takes the value of 1 if it is Round 4 of the survey and therefore post the APNA intervention; X_{jt} are time-varying household control variables including the education level, age and gender[45] of household head, the total size of the household, the total amount of land owned by the household, whether this land is irrigated, and whether the household experienced an economic shock or a natural disaster; Z_{kt} are village level control variables which include whether there is a factory in or within 5 km of the village and whether the village is accessible to motorised vehicles; α are household fixed effects; and e_{jt} is the statistical noise term.

The coefficient β_1, in equation 4.5, is the main coefficient of interest. This coefficient tells us to what extent there is a differential change in the work obtained by households in treatment areas after the APNA information campaign. The coefficient, β_2, captures the common time trend in the post treatment period.

I include household fixed effects so that any time invariant household specific factors that affect the amount of days households work under MGNREGA could be controlled for. I also include time-varying household characteristics in X_{jt} as this helps control for any changes in the household's circumstances, separate to the impact of the information campaign, that affects their take up of work under MGNREGA. The inclusion of village specific controls in Z_{kt} helps address whether there are any time varying factors at the village level that could affect the likelihood of work alternatives and therefore the take up of work under MGNREGA.

Given that the main outcome variables I am trying to estimate are count variables that are bound by 0, with a number of people who work zero days, a Poisson estimation provides a better fit to the relationships I am trying to estimate than an OLS. If one looks at Figure 4.1, it confirms a significant number of households who work 0 days. The average mean days worked under MGNREGA is 46 for Round 3 and 49 for Round 4, while the variance is over 2907 for Round 3 and above 2281

[45] Between Round 3 and 4 some household heads changed, including going from a male to a female headed household

for Round 4. This tells us there is over dispersion in the data. In such cases, it is better to use the negative binomial as unlike the Poisson, the assumption of the mean and variance being equal does not have to hold. Therefore, I estimate equation 4.5 with a fixed effects negative binomial for all count outcome variables. I use a household fixed effects linear estimation model for the other outcome variables, including having a job card, working in the last 12 months under MGNREGA, and whether households work over the total number of entitled days.

In order to ensure the robustness of this approach, I run a number of different checks that I report in the results section. First, I check to ensure that these results hold if I include both pre-treatment rounds. Next, given some initial differences between the percent of households classified as Scheduled Tribes (ST) in treatment and non-treatment area, a concern could be that this difference is driving the results as implementation of MGNREGA maybe different for ST households. I therefore reestimate the results but without ST households. There could still be concerns that allocated but non treatment maybe driving the results. I therefore drop these areas and re-estimate the results for the remaining areas. I also check the robustness of the estimation model by running three additional estimation models to ensure that the results hold.

4.7 Results

The first part of the analysis explores the extent to which the APNA information campaign changes the likelihood to participate in MGNREGA on the extensive margin. From column (1) and (2) in Table 4.7, I see that households in treatment areas see no significant increase in the likelihood of having a job card or having worked under MGNREGA in the prior 12 months compared to households in non-treatment areas. As discussed previously, given how the information campaign was targeted to households already working, this result is not surprising. I expect the information campaign to influence the MGNREGA outcomes on the intensive margin.

Hypothesis 1: A decrease in total days rationed

I find evidence for *hypothesis 1*, which predicts that an increase in the expected cost of deviation should decrease the rationing effect in treatment areas. Households in treatment areas are less likely to be rationed in total work days obtained under MGNREGA compared to households in non-treatment areas. Column (A1) of Table 4.8 shows us that there is a negative relationship between total days rationed and households in treatment areas. From column (A2) in Table 4.8, I can see that the days worked by households in treatment areas are rationed at a rate of 0.764.[46] This is 24.6 percent fewer days of work not obtained compared to households in non-treatment areas. This result is significant at the 1 percent level.

The demand constraint of treatment households is getting more binding for the field assistant, enabling the implementation of MGNREGA to be in line with the principal's design of the program. For these households, post intervention, they are being given more days of work. This translates into a smaller gap between the days they are entitled to and the days they got supplied to them. This result is important because it means that the program is more able to act like a demand driven program, where households receive the work they demand.

In support of this finding, I look at whether total days worked by households in treatment areas actually increases. In column (2), from Table 4.9, I find that the rate at which households in treatment areas obtain days of work under MGNREGA increased by a factor of 1.325 (32 percent) compared to households in non-treatment areas, holding all else constant. This result is significant at the 1 percent level. I also check whether this change in total days is being driven by women or men in column (5) and (6) and find that both genders see an increase in total days worked. Given that treatment households are working more days, this supports *hypothesis 1*, that the total days rationed is decreasing for treatment areas.

In order to ensure the robustness of these results, I carry out the various checks identified earlier in the empirical section. For the first check, I reestimate the regressions using the three rounds of the Young Lives data that included questions on MGNREGA. As discussed earlier, I do not use round two (2006) in the main specification as it decreases the number of observations and does not enable one to analyse the work supplied by lean and non-lean seasons. What I find in column (A3) in Table 4.8, is that the size and the significance of the results hold and are very consistent.

For the next robustness check, I drop ST households from the analysis. The reason for this is that non-treatment areas had a higher percent of ST households than treatment areas. This difference could be important to the results, as it could be the case that implementation of MGNREGA for ST households is different from non-ST houses. Therefore, the lower rate of days given to non-treatment areas may be due to this difference versus the effect of the program. What I find in column (A4) from Table 4.8, is that the sign and significance of the results hold. At the same time, the size of the coefficient does become smaller, suggesting some of the effect is driven by the difference in access to MGNREGA by ST households. Understanding why the rate of change for total days worked for ST households may be different than non-ST houses would be an important angle for further research.

[46] As mentioned previously, the IRR is interpreted as the log of the ratio of expected counts, while the negative binomial coefficient is interpreted as the difference between the log of expected counts (UCLA: Statistical Consulting Group 2017).

Next, I drop all allocated but non treatment areas as there still maybe some concerns that areas which were allocated but not treated are different to allocated and treated areas. In column (A5) from Table 4.8, one can see that post intervention allocated treated households are less likely to be rationed then non-allocated non-treated households, this result is significant at the 1 percent level. As another check, I also drop households that were rationed 100 days in both Rounds. This limits the analysis to households that worked at least one day in either Round but also to those who didn't obtain the total entitled days of work in both Rounds. From column (A6), one can see that the results are consistent.

I now address the main concern with using the fixed effects negative binomial model, which is that it may not pick up time-invariant factors. This effects the results and undermines the purpose of the fixed effects model controlling for unobservable time-invariant factors (Allison and Waterman 2002).[47] One suggested solution to this is to run an unconditional estimation of a negative binomial model with household dummies.[48] From column (A7) in Table 4.8, I find that the direction, size and sign of the result holds when I run the unconditional NBREG with household dummies.

I also reestimate total days rationed using both a fixed effects linear estimation model and a fixed effects robust Poisson estimator. If one looks at column (A8) in Table 4.8, one can see that the size of the OLS coefficient on the number of days rationed is relatively similar to column (A1), that reports the original NBREG coefficients. This result is also significant, though at the 5 percent level versus the 1 percent level from the fixed effects NBREG estimator. This suggests that using the NBREG model is correct as it more precise in being able to estimate the count outcome at hand.[49] Finally, in regards to the Poisson estimator, in column (A9) in Table 4.8, I find that the direction of the results hold though at a lower level of significance of 10 percent and the coefficient becomes smaller.

Hypothesis 2: An increase in days worked during the non-lean season

The next part of this analysis looks at *Hypothesis 2*, which predicts that an increase in expected cost of deviation increases the number of days worked in the non-lean season in treatment areas. The lean season is when there is little agriculture work to be obtained, hence any MGNREGA work created at this time creates little competition for other employers looking to hire the same workers. The non-lean season accounts for 9 months of the year and during this time there is the planting,

[47] Please also refer to the discussion by P. Allison on this at `https://statisticalhorizons.com/fe-nbreg`

[48] Though there is debate on this and whether the suggested approach of an unconditional estimation NBREG actually correct for it (Phelps 2007).

[49] In the NBREG fixed effects model households that only have one observation or that never work, where the total days worked are 0 for both rounds, are dropped as this model looks at what factors drive the change in total days worked.

harvesting and other farm related activities. One would expect that not all these months have a need for labour, therefore there will be times during the non-lean seasons where people would want work under MGNREGA.

From column (2) in Table 4.10, I see that in the non-lean season the rate at which households work under MGNREGA increased by a factor of 1.8 (80 percent) compared to non-treatment households. This means households in treatment areas are more likely to obtain work during non-lean season months. From Table 4.11, I see no similar relationship for the lean season.

To ensure the robustness of the result, I reestimate the relationship using a fixed effect linear estimator and a Poisson estimator. The size of the coefficient for the NBREG estimator is double the size of the coefficient from the OLS estimator, though similar in size to the IRR value from the Poisson estimator. One reason for this could be that the NBREG estimator drops observations where both rounds have zero values. This enables us to look more specifically at why households increase or decrease days. The OLS considers all observations and therefore this may influence the size of the coefficient.

Given that I could not test the parallel trend assumption for this dependent variable and I found an initial difference at baseline on this metric, one has to be cautious about this result.[50]

Hypothesis 3: No work over the 100 day entitlement

Finally, I examine *Hypothesis 3*, which predicts that an increase in the expected cost of deviation decreases the number of households who have worked over the entitled number of annual days in treatment areas. In column (1) of Table 4.12, I find that households in treatment areas are more likely to obtain over 100 days of work annually. This finding is opposite to what *Hypothesis 3* predicts. When I look at the likelihood of a household obtaining more than their entitled days, one can see that households in treatment areas have a 3 percent higher probability of working over the 100 days entitlement. This result also holds when I include all rounds of the Young Lives survey that ask questions on MGNREGA.

I also test whether these households are more likely to receive over 150 days of work each year. The reason I look at this figure is that if an area experiences a particular shock, such as an extended drought, the number of days entitled can be increased by the government up to 150 days per year. In column (3) of Table 4.12, I find no significant difference between treatment and non-treatment households in their likelihood of working over 150 days per year.

[50] Round 2 of the Young Lives survey did not collect the days worked by the agriculture season. They only collected total days worked annually.

One potential explanation for this is that being informed enables workers to push for a greater number of days to work, as they are more organised to collectively push for the threshold increase in reaction to a problem, such as drought in their area. The reason that this may be driving the result is that I find no difference between treatment and non-treatment households obtaining above 150 days of work each year.

I run similar robustness tests to those discussed previously and find that the results hold. More specifically, it holds for when the analysis is conducted for all three Rounds, for non-ST households, for households in allocated and treatment areas and for households that work in at least one Round.

From these results, I find support that areas which received the information campaign were able to access work at a higher rate than areas that did not receive the information campaign. Furthermore, I find support for *hypothesis 1* and *hypothesis 2*, where treatment areas were less likely to be rationed in the number of work days and are more likely to receiving work during the 9 month non-lean season. Though, opposite to *hypothesis 3*, I find that households in treatment areas, who are now informed, are more likely to access more than their 100 day entitlement. I propose that the reason for this is that they are able to collectively push for an increase in the entitlement threshold to 150. Yet, this only applies within the limit of 150 days, as I find no evidence that these households are more likely to work over 150 days compared to households in the non-treatment areas.

I run a number of robustness checks and find that the effect of the information campaign treatment holds even with the robustness concerns identified above. Yet, it is important to recognise the impact of the ST households on the results, where the coefficient decreases when these households are dropped from the analysis.

4.8 Conclusion

In this paper, I explore how an information campaign provided to beneficiaries, who are empowered to use the information, affects the implementation of a national employment guarantee Act. The empirical literature has been inconclusive on the role of information in increasing accountability in the delivery of anti-poverty programs. On one hand increased beneficiary awareness has been found to increase the accountability and delivery of programs (Banerjee et al. 2015b; DiRienzo et al. 2009), on the other hand, others have found that increased awareness by itself is not enough (Banerjee et al. 2010; Ravallion et al. 2013). This second body of research has suggested that for information campaigns to be effective, individuals need to be empowered to use the information (Ravallion et al. 2013).

This paper contributes to the empirical literature by showing that an information campaign can affect the delivery of a program when individuals are organised to use the information. Exploiting the variation in the implementation of an information campaign in Andhra Pradesh, India, this paper analyses the difference in MGNREGA outcomes for households who were exposed to this information campaign versus household who were not exposed to it. Prior to the roll out of the campaign, all rural households accessing MGNREGA work were reorganised into working groups. I argue that the structure of the working groups make these households more mobilised to use this information.

I find that as a result of this intervention, when households are better informed of their rights under MGNREGA, the total number of days they get rationed decreases, the total days they work in the non-lean season increases and their likelihood of obtaining more than the 100 days of entitled work increases. These findings have important implications for the implementation of MGNREGA in other states of India. It provides evidence that the combination of informing and empowering workers to use the information can address issues of accountability in implementation. This also has implications for other anti-poverty programs in developing countries. In addition, it would be important to conduct research on similar interventions, in order to see if these results hold across different programs and country contexts.

One question for further analysis, as a result of the findings in this paper, is why the size of the effect changes for ST households. Ensuring that ST households are able to use MGNREGA as the safety net it is designed as, is critical in helping these households tackle poverty.

4.A Figures

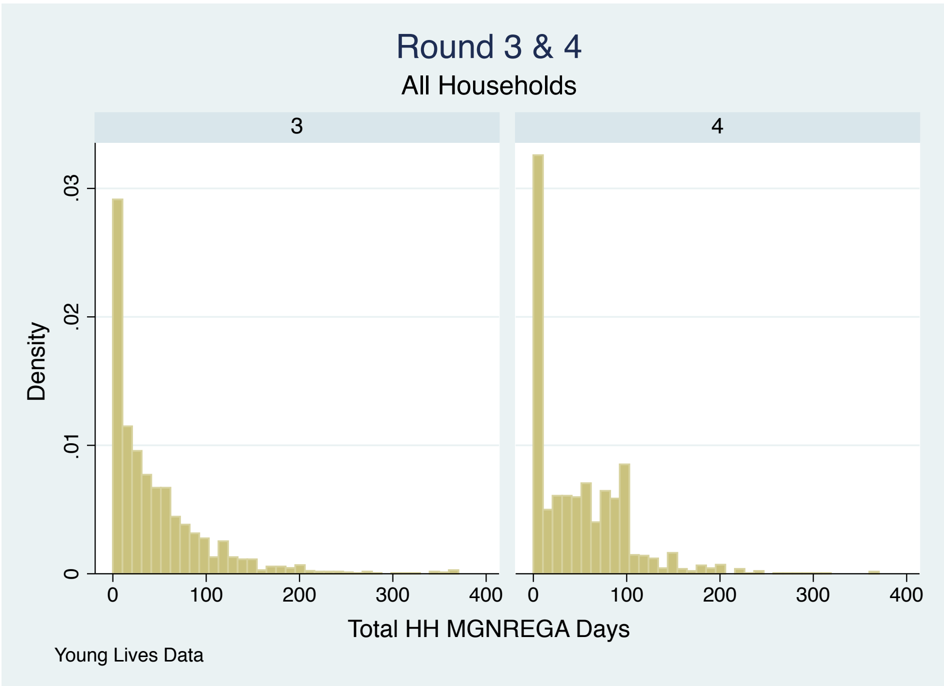


Figure 4.1: Total days worked under MGNREGA in the last 12 months

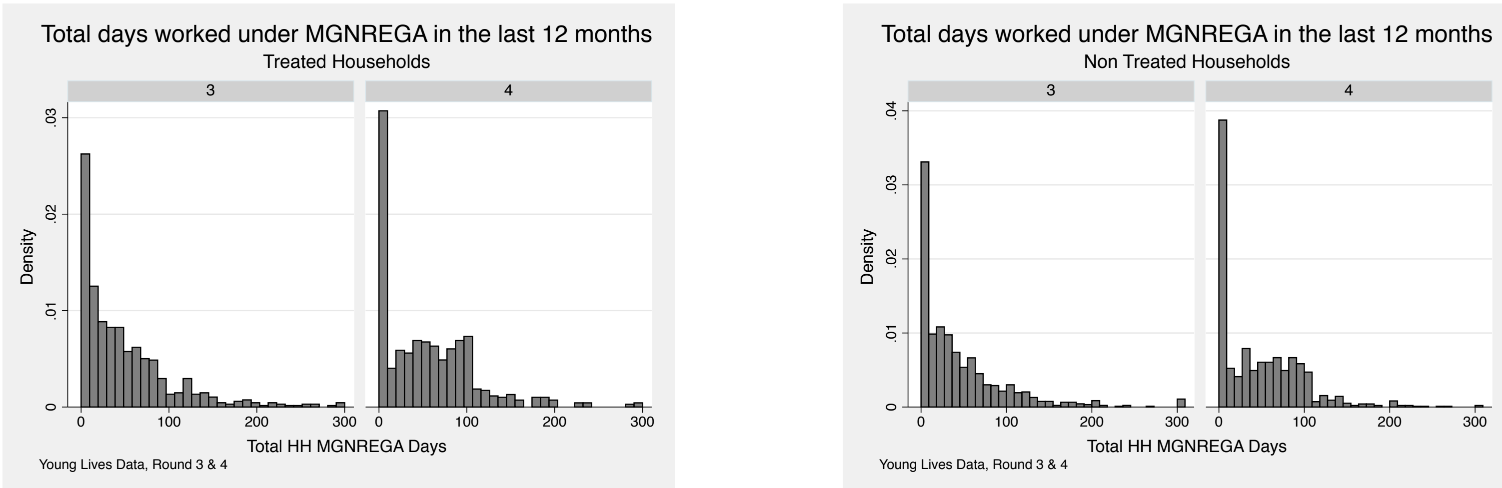


Figure 4.2: Total Days Worked for treatment and Non-treatment Households by Survey Round

4.B Tables

Table 4.7: Impact of Information Campaign on Participation in MGNREGA

	(1)	(2)
VARIABLES	Has Job Card	Work last12 Months
InterventionxTime	-0.024	0.030
	(0.018)	(0.023)
4.Round	0.009	-0.150***
	(0.019)	(0.023)
Household FE	Yes	Yes
Household Controls	Yes	Yes
Village Controls	Yes	Yes
Observations	4,112	4,112
R-squared	0.027	0.054
Number of childid	2,056	2,056

Standard errors are clustered at the household level and are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.8: Hypothesis 1: Impact of the Information Campaign on Total Days Rationed

VARIABLES	(A1) NBREG Coefficient◇	(A2) IRR◇	(A3) AllRounds◇	(A4) Non ST Households◇	(A5) Allocated Treated◇	(A6) Worked At least One Round◇	(A7) Unconditional IRR◦	(A8) OLS log	(A9) Poisson
InterventionxTime	-0.303***	0.738***	0.735***	0.800***	0.701***	0.722***	0.797**	-0.260**	0.918*
	(0.069)	(0.0734)	(0.0569)	(0.0665)	(0.0511)	(0.0601)	(0.0752)	(0.111)	(0.0415)
3.Round			0.624***						
			(0.0397)						
4.Round	0.057	1.058	0.641***	1.038	1.092	1.094		-0.159	0.908**
	(0.074)	(0.0893)	(0.0363)	(0.0810)	(0.107)	(0.0909)		(0.105)	(0.0382)
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,988	2,988	4,058	2,506	2,414	2,702	3,383	3,383	2,988
Number of childid	1,494	1,494	1,615	1,253	1,207	1,351		1,840	1,494
R-squared								0.030	

◇ Uses a fixed effect NBREG estimation model; ◦ Uses an unconditional NBREG with household dummies. IRR is the incidence rate ratios

Standard errors are clustered at the household level and are in parentheses *** p<0.01, ** p<0.05, * p<0.1. For the NBREG model we run a vce(boot) for our standard errors. The Poisson is a vce(robust) standard errors.

Table 4.9: Impact of the Information Campaign on Total Days Worked

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	NBREG Coeff	OLS log	NBREG IRR	Poisson	Female Days	Male Days
InterventionxTime	0.324***	0.287**	1.383***	1.166**	1.309***	1.501***
	(0.063)	(0.119)	(0.122)	(0.0838)	(0.125)	(0.150)
4.Round	-0.277***	-0.341***	0.758***	0.999	0.783***	0.764***
	(0.076)	(0.122)	(0.0594)	(0.0745)	(0.0627)	(0.0659)
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,048	3,521	3,048	3,048	2,926	2,424
R-squared		0.021				
Number of childid	1,524	1,870	1,524	1,524	1,463	1,212

Note: Authors own calculations based on Young Lives Data.

Standard errors are clustered at the household level and are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 4.10: Hypothesis 2: Impact of the Information Campaign on Non-Lean Days Worked

	(1)	(2)	(3)	(4)
VARIABLES	NBREG Coeff	NBREG IRR	OLS log	Poisson
InterventionxTime	0.653***	1.921***	0.249**	2.062***
	(0.116)	(0.221)	(0.117)	(0.291)
4.Round	-1.440***	0.237***	-0.999***	0.389***
	(0.123)	(0.0261)	(0.108)	(0.0569)
Household FE	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes	Yes
Observations	2,254	2,254	3,481	2,254
Number of childid	1,127	1,127	1,863	1,127
R-squared			0.182	

Standard errors are clustered at the household level and are in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4.11: Impact of the Information Campaign on Lean Days Worked

	(1)	(2)	(3)	(4)
VARIABLES	NBREG Coeff	NBREG IRR	OLS log	Poisson IRR
InterventionxTime	0.162	1.176	0.207	1.076
	(0.099)	(0.116)	(0.128)	(0.0927)
4.Round	-0.072	0.931	-0.029	1.343***
	(0.082)	(0.0761)	(0.126)	(0.114)
Household FE	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes	Yes
Observations	2,888	2,888	3,483	2,888
Number of childid	1,444	1,444	1,862	1,444
R-squared			0.016	

Standard errors are clustered at the household level and are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.12: Hypothesis 3: Impact of the Information Campaign on Working over 100 and 150 days

VARIABLES	(1) Days100+	(2) Days150+	(3) Days100+ All Rounds	(4) Days150+ All Rounds	(5) Days100+ Non ST HH	(6) Days150+ Non ST HH	(7) Over100+ Allocated Treated	(8) Days150+ Allocated Treated
InterventionxTime	0.033*	0.012	0.031**	0.014	0.043**	0.012	0.046**	0.009
	(0.019)	(0.012)	(0.015)	(0.010)	(0.022)	(0.013)	(0.020)	(0.012)
3.Round			0.104***	0.030***				
			(0.011)	(0.007)				
4.Round	-0.041**	-0.002	0.063***	0.026***	-0.038**	-0.001	-0.041**	0.004
	(0.018)	(0.011)	(0.010)	(0.006)	(0.019)	(0.012)	(0.021)	(0.012)
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,112	4,112	6,167	6,167	3,494	3,494	3,326	3,326
R-squared	0.013	0.013	0.052	0.022	0.016	0.017	0.016	0.012
Number of childid	2,056	2,056	2,056	2,056	1,753	1,753	1,663	1,663

Standard errors are clustered at the household level and are in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4.13: Regression build up: Impact of the Information Campaign on Total Days Rationed

VARIABLES	(1) Ration	(2) Ration HhChar	(3) Ration Village Controls
InterventionxTime	0.772***	0.786***	0.739***
	(0.0534)	(0.0518)	(0.0528)
4.Round	0.964	0.949	0.997
	(0.0427)	(0.0454)	(0.0427)
HHGen		1.077	1.091
		(0.111)	(0.142)
hhsize		0.920***	0.922***
		(0.0186)	(0.0141)
HHHEduc		0.991	0.989
		(0.00970)	(0.00964)
HHHAge		0.999	0.999
		(0.00328)	(0.00415)
Land		0.938	0.924
		(0.0607)	(0.0467)
irrgt		0.920	0.918
		(0.0623)	(0.0715)
MotorAccessible			1.123
			(0.183)
Factory			0.772***
			(0.0737)
EconShock		1.087	1.102
		(0.0608)	(0.0698)
NaturalDisaster		0.968	0.969
		(0.0492)	(0.0589)
Household FE	Yes	Yes	Yes
Household Controls	No	Yes	Yes
Village Controls	No	No	Yes
Observations	2,988	2,988	2,988
Number of childid	1,494	1,494	1,494

Note: Authors own calculations based on Young Lives Data.
Standard errors are clustered at the household level and are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 4.14: Variable Description and Summary Statistics

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Economic Shock	0.666	0.472	0	1	4112
Factory	0.272	0.445	0	1	4112
Total Female MGNREGA Days	27.699	32.415	0	360	3503
Does your household have job card under the NREGS?	0.814	0.389	0	1	4112
HH Head Age Round	28.903	21.659	0	87	4112
HH Head Education Level	4.045	4.993	0	28	4112
HH Head Gender	0.083	0.276	0	1	4112
Household size	5.146	2.105	1	30	4112
(mean) irrgt	0.288	0.453	0	1	4112
lLand	0.876	0.773	0	7.314	4112
Accessible to motorized Vehicles	0.961	0.193	0	1	4112
Total Male MGNREGA Days	20.711	31.038	0	360	3482
Natural Disaster	0.421	0.494	0	1	4112
Days Rationed less than 100	56.961	36.881	0	100	3383
Total HH MGNREGA Days	47.96	53.388	0	360	3521
Total HH MGNREGA NonLean Days	15.195	29.329	0	360	3481
Total HH MGNREGA Lean Days	33.346	40.954	0	360	3483
Over the past 12 months, has anyone in your household worked for the NREG scheme	0.677	0.468	0	1	4030

4.C Appendix A

$$
\begin{aligned}
(P_S) : maxE(Y_A) = w - d(E-S)^2 + (s-c)S \\
\frac{\delta E(Y_A)}{\delta S} = 2dE - 2dS + s - c = 0 \\
2dS = 2dE + s - c \\
S = E + \frac{s-c}{2d}
\end{aligned}
\tag{6}
$$

Chapter 5

Conclusion

This thesis examines the constraints faced by households vulnerable to poverty across three countries, Kenya, Viet Nam and India. Each of the essays employ micro-level data to examine three different issues that could impact the tools and mechanisms individuals and households use to move out of poverty. First, the effect of income uncertainty on saving behaviour of agriculture dependent individuals. Second, whether it matters if credit is held in the hands of women versus men for individual and household welfare outcomes. Third, whether increased information awareness in households who are empowered to use this information enables a more efficient delivery of anti-poverty programs.

From these essays, one can draw the following conclusions. In the context of Kenya, income uncertainty plays a critical role in determining what savings products individuals use. Their differentiated and revealed preference for regulated and credit enabling products indicate the type of tools these individuals turn to for managing future income shocks. The findings from this chapter also highlight the preference for savings products that allow the user to ring-fence the saved wealth. The research has been pretty clear that households with access to financial tools are better able to manage income shocks (Ghosh et al. 2000; Klapper et al. 2016). The findings from this chapter potentially indicate a need to address this demand for regulated, credit-enabling products that are also able to ring fence stored wealth. Given the changing portfolio of savings products and the emergence of new technologies, there is potential to facilitate the design and supply of products that incorporate these characteristics in an accessible and affordable way. Some potential avenues to do this include policy initiatives that encourage innovation and competition between providers.

In the context of Viet Nam, the research from this thesis provides insight into the effectiveness of a law designed to increase credit access for women. It accentuates the issue that credit can potentially create an increased work burden for women. It also highlights that in order for credit to impact household welfare, the time frame to capture this impact must be considered, as emphasised by the research findings from Khandker and Samad (2014). In addition, the impact of credit can be mixed, where in the current period it has a negative impact on household income but is positively correlated to children welfare outcomes. As highlighted by Islam et al. (2016), credit can have a non-linear effect on household welfare outcomes. Each of these points emphasise a need to think through the design of credit products and the different channels they could impact the individual.

Finally, in the context of India, the essay shows that if clients are provided with information on their rights and entitlements, as well as being empowered to use this information, this can affect how local agents implement programs. This finding buildings on the conclusions by Ravallion et al. (2013), who found that while the information campaign increased knowledge of the act, it did not translate into improving the performance of the scheme in Bihar. One reason Ravallion et al. (2013) propose for this, is that in order for information to influence the implementation of a program, individuals need to be empowered enough to actually put the information to use. The findings from this paper show that an information campaign can increase the cost faced by the agent from deviating from the principle's program design. This can have meaningful impacts on the implementation of anti-poverty programmes. It is crucial for policy makers to incorporate information and empowerment mechanisms in the accountability design of their programs. This also has implications for existing programs.

These findings highlight a need to for further research that further disaggregates vulnerability by households' characteristics. There is also a need to look at these hypotheses in different contexts. Through such analysis, common lessons and gaps will emerge that can provide further insight into how we can better address the barriers faced by poor households. This tests whether these findings are transferable across different developing country contexts.

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