THREE ESSAYS ON ACCESS TO CREDIT AND FINANCIAL SHOCK IN NIGERIA

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DECLARATION

I hereby declare that this thesis has not been and will not be, submitted in whole or in part to another university for the award of any other degree.

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Three Essays on Access to Credit and Financial Shock in Nigeria

Summary

This thesis comprises three essays on access to credit and the impact of financial shocks in Nigeria. In the first essay, we examine the impact of the Central Bank of Nigeria's (CBN) development fund initiative on access to credit for Micro, Small and Medium (MSM) firms using data from the World Bank Enterprise survey for Nigeria. The key findings reveal that the Micro, Small and Medium Enterprise Development Fund (MSMEDF) had a positive effect on access to bank credit by firms. The programme is estimated to have increased the incidence of loan take-up by firms during the period of the study. The second essay investigates whether there is a gender dimension to small- and medium-sized enterprise (SMEs) credit market participation and loan success in Nigeria. The findings reveal evidence of an unequal treatment in loan success in those firms that were 100% female owned. The findings reveal that exclusively female-owned firms are not constrained in applying for loans, but are less likely to be successful in their loan application compared to male-owned firms. The third essay examines the impact of unanticipated shocks on household welfare measures in Nigeria. Using the General Household Survey (GHS) data for Nigeria, we exploit the increase in prices of food items as a measure of financial shock in combination with personal and local shocks to investigate their impact on household-level food and non-food expenditures, household assets, savings and food poverty. The empirical analysis reveals that financial shocks exert the most influence on household welfare measures, and household assets play a key role as a shock absorber in providing some resilience to households in the event of a shock.

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Chapter One - Introduction

This thesis consists of three independent essays related in turn to an empirical analysis of access to credit, credit market participation and gender, and the impact of shocks on household welfare in Nigeria. The themes of the essays are broadly related and centred on firm-level financial constraints and household-level financial resilience. One of the critical factors that determine a firm's productivity and growth is access to credit. However, it has been well documented that Micro, Small and Medium Enterprises (MSMEs) face significant credit constraints in developing economies due to the presence of underdeveloped financial systems (Banerjee and Duflo, 2014). The reluctance of intermediaries to lend to firms is not surprising given the information asymmetry in the credit market that sometimes leads to unequal treatment in the market. Financial resilience, on the other hand, is a concept that refers to the ability to manage and mitigate risks associated with financial adversity (NAB and Centre for Social Impact, 2018). Therefore, financial institutions play a significant role in promoting both access to credit for firms and building financial resilience for households. A healthy financial system supports robust economic growth, and enables firms and households to improve their efficiency and mitigate risk. While financial deepening helps firms reduce volatility and improve long-term investment returns, thus helping to minimise capital constraints (Aghion *et al*., 2010), it also provides households with a platform to accumulate economic resources, such as savings that can be used as shock absorbers in an emergency. One area of policy intervention necessary to mitigate financial shock and stimulate the growth of firms, especially MSMEs, is to build resilience and improve access to external finance.

Following the global financial crisis in 2008, many countries took various measures to support the MSME sector and build household-level resilience. The financial crisis highlighted various areas of policy responses. These actions were followed by the implementation of financial and social reforms that strengthened firms and households. One of these was the implementation of policies aimed at helping SMEs obtain access to finance through the expansion of credit guarantee schemes and direct lending programmes. The creation of these instruments became important in driving the financial inclusion strategy and other government development strategies.

The general contribution of these essays is focused on the impact evaluation of intervention funds on access to credit, quantifying the magnitude of the gender gap in financial access to credit, and determining the effect of shocks on household welfare indicators in a developing economy. All three themes revolve around the process inherent in developing countries' economic plans. These development goals are articulated in the Millennium Development Goals (MDGs), and include increasing the standard of living, poverty reduction, gender equality, and reduced welfare inequality. The objective of a country's economic policies is to provide a favourable environment for its citizens and businesses to prosper. However, if businesses are starved of finance and the country experiences an economic shock, it inevitably affects firms and households and, consequently, the rate of economic development. The mechanisms through which firm growth and household welfare are affected can differ, but the established findings corroborate the importance of access to credit for firms and the effect of shocks on household welfare. Therefore, understanding the magnitude of these effects is important for policy interventions, given it will help shape the design of such policies both at the firm-level and household-level.

In 2013, the Central Bank of Nigeria introduced a N200 Billion Micro, Small and Medium Enterprise Development Fund (MSMEDF), designed to channel low-interest funds to the MSME sub-sector in order to increase productivity and output, create jobs and engender inclusive growth in Nigeria. The first essay explores this unique scheme by investigating the impact of the Central Bank development funds on the access to credit of Small- and Medium-Sized Enterprises (SMEs). The main objective of the Fund was to increase access to credit for the country's MSMEs. However, it is unclear if this was the case in practice, as anecdotal evidence reveals strong criticisms of the programme for failing to achieve its stated objectives. The general criticisms argued that the stringent conditions attached to accessing the Fund were counter-productive. However, there is no empirical evidence to support these claims. It is therefore important to empirically examine whether the programme had any significant impact on the incidence of firm-level loan take-up. A significant strand of the existing micro-level empirical analysis focuses on the impact of SME credit schemes on performance measures, such as sales, employment, and growth, which impact the performance of firms over time, with less research investigating the direct impact on bank loans. This first essay attempts to fill this gap and add to the literature on the impact of such credit schemes on SME loan take-up rates.

The second essay takes the issue of access to credit further by investigating whether there is a gender dimension to SME credit market participation and loan success in Nigeria. The existing literature reveals that the availability of credit is a supply-side concept linked to the role of lenders in credit rationing in the market with imperfect information (Stiglitz and Weiss, 1981). The problem of information asymmetry encourages banks to rely on the observable attributes of firms when assessing borrowers for their credit-worthiness and riskiness. Among the attributes of a firm that lenders may use to screen potential borrowers is the gender of the owner. A growing literature suggests that gender plays a decisive role in securing (or otherwise) access to credit, and this may lead to unequal treatment in the credit market. Therefore, this essay contributes to the existing literature on the theme of gender and unequal treatment in the credit market. It is important to understand the factors that influence a firm's participation in credit markets, and the factors that lenders use in screening borrowers, and whether there is a gender dimension underlying these processes.

The third and final paper is focused on household financial resilience in its investigation of the impact of unanticipated shocks on household welfare. In order to gain a deeper understanding of the welfare effect of a financial shock (measured in this current research as an increase in food prices), it is important to examine the effect in combination with other shocks; this is because households are prone to various forms of environmental, personal and economic shocks that affect their welfare and increase vulnerability to household poverty. Shocks are unanticipated events that occur in an economy that potentially have significant and sudden welfare losses on the affected individual or population. Therefore, the household-level impact of a food price hike, in combination with other shocks on household welfare measures, is investigated for Nigerian households. This empirical analysis contributes to the existing literature relating to the impact of financial shocks on socio-economic indicators at the micro level. It also highlights the mechanism through which a financial shock adversely affects socio-economic outcomes, and the measures taken by households to cushion the effect of such shocks.

Following this introductory chapter, the remainder of the thesis is structured as follows. The next chapter examines the impact of the CBN development fund on access to credit (bank loan take-ups) for Nigerian firms. We evaluate the programme participation of eligible borrowers on the key outcome variable of interest. The chapter uses two rounds of the World Bank Enterprise survey data from Nigeria (one before the intervention in 2010, and one after the intervention in 2014) in an attempt to identify the impact of the intervention on bank loan take-ups. The empirical strategy exploits a propensity score matching (PSM) technique in conjunction with a difference-in-difference (DID) framework. Overall, the evidence in the empirical work undertaken in this chapter confirms a positive effect of the CBN development fund intervention on MSMEs bank loan take-up rates. The empirical evidence contradicts the widespread negative perception contained in anecdotal accounts extensively reported in print and broadcast media in Nigeria.

The second essay (Chapter 3) builds on the first by investigating gender differences in credit market participation and loan success, where access to credit (i.e., bank loan success) captures whether or not a firm successfully acquired credit. First, we use a bivariate probit model with partial observability to control for selectivity bias and then, jointly, model loan application and loan success. In this way, two probability processes are investigated and empirically formulated. First, an equation for whether a firm participates in a loan application, which is fully observable, is specified and, conditional on this, an equation for whether the loan application was successful using the sub-sample of firms that participated in the credit market. The findings reveal evidence of discrimination in loan success for those firms that are exclusively female-owned. A key finding of this chapter is that fully female-owned firms are not constrained in applying for loans, but are less likely to be successful in their application compared to their male-owned firm counterparts.

In the third and final essay (Chapter 4), the distributional effects of shocks on household welfare measures are estimated using a recentred influence function (RIF) to motivate the unconditional quantile regression models. A probit model is also employed to measure the impact of shocks on both household savings and the household food poverty rate. The empirical analysis reveals that financial shocks, among all sets of shocks, exert the most impact on household welfare measures, and that the depletion of household assets act as shock absorbers providing resilience to households in the event of a shock.

The novel contributions of the three essays are as follows. With respect to SME policy reforms in Nigeria, the study in the first empirical chapter is the first to provide empirical evidence of the impact of the MSMEDF credit scheme for Nigeria. Over the years, the Nigerian government has established many SME intervention programmes; to date, however, no studies have attempted to evaluate the impact of such programmes. As the MSMEDF was a unique development fund intervention aimed at addressing the problem of financial constraints facing MSMEs and the gender financial gap in Nigeria, it provides a unique opportunity to evaluate the impact of the programme. Therefore, this study is the first to attempt a systematic evaluation of the SME programme by quantifying the impact of the MSMEDF on lending behaviour. The second essay makes a further unique contribution to the literature of gender and access to credit by using an array of different techniques to investigate this theme. The study provides a methodological contribution to the existing literature. It does this by proposing the use of a bivariate probit model with partial observability to deal with the potential problem of selection bias when the probability of borrowing from a formal financial institution may be jointly determined by the demand for and supply of credit. To the best of our knowledge, there is only one prior study that attempted to address the issues of credit discrimination as experienced by women at the SME level in Nigeria. Specifically, Nwosu *et al*. (2015) modelled access to credit as an independent concept from credit market participation.

The third essay also provides an innovative contribution to the literature by investigating the impact of a financial shock across welfare distribution using RIF-based functions, and examines how assets and savings are being used as shock absorbers for household resilience. The few existing studies on the impact of unanticipated shocks in Nigeria focused mainly on measuring the average (homogeneous) effect of shocks on selected household welfare indicators, such as total consumption expenditure and the poverty rate. None to date have examined the distributional effect of shocks on the expenditure welfare measures in Nigeria. This study is therefore not only the first to provide a distributional analysis of the impact of financial shock across household welfare distribution in Nigeria, but also provides insights on the role resilience metrics, such as household assets, perform in the face of these shocks.

The structure of the thesis is now laid out. The next three chapters contain the separate empirical essays. A final chapter provides a summary of the key findings and contains suggestions for future research.

Chapter Two - The Impact of the Central Bank of Nigeria Development Fund on Access to Credit for Micro, Small and Medium Nigerian Firms (Essay 1)

2.1 Introduction

Recognition of Small and Medium Enterprises (SMEs) as a critical contributor to economic growth has attracted research attention in recent years. In most countries, SMEs account for both a substantial share of gross domestic product (GDP) and the labour force. Available data reveal that SMEs account for more than 90% of enterprises across the world, an average of 60-70% of total employment, and 50% of GDP (Ayyagari *et al*., 2010; International Council for Small Business, 2019). The vast majority of businesses in Africa are SMEs (Quartey *et al*., 2017). In Nigeria, the MSME sector provides, on average, 85% of employment in the private sector and contributes approximately 50% to GDP (National Bureau of Statistics, 2017a).

Many SMEs struggle to compete in the global market due to various constraints, such as access to finance and weak managerial skills. This is also believed to be the result of their inability to exploit the opportunities presented by the changing market environment. In response to this issue, many developed and low-income countries have established intervention programmes offering financial support and subsidised credit to SMEs. Like other countries, the Nigerian government, through the Central Bank of Nigeria (CBN), The World Bank, private banks and individuals, have put in place various interventions, bilateral arrangements and the establishment of various programmes to support SMEs in Nigeria. These include the Small-Scale Industries Credit Scheme (1971), World Bank US$41 million SME I Loan Scheme (1984), World Bank US$270 million SME II Loan Scheme (1990), Small and Medium Enterprises Equity Investment Scheme (2001), Small and Medium Enterprises Credit Guarantee Scheme (2010), and Micro, Small and Medium Enterprises Development Fund (2013).

Although many developed countries have evaluated the impact of credit schemes (Uesugi *et al*., 2010; Kim *et al*., 2015; de Blasio *et al*., 2018; Cowling *et al*., 2018), most developing countries rarely evaluate their SME programmes. Recently, there have been some evaluation studies for low-income countries in Latin America, the Caribbean and Asia (Tan and Lopez-Acevedo, 2011a). However, there are limited studies in Africa and most rely on anecdotal evidence, qualitative surveys and simple case studies that are not empirically rigorous or capable of capturing a causal effect.

In Nigeria, many studies have reviewed key policies introduced in the past to support MSME development, investigating the challenges facing implementation and attempting the impact evaluation of these intervention programmes using descriptive analysis (Joseph *et al*., 2021; Oduntan, 2014; Isa and Terungwa, 2011). While most of these studies are interested in evaluating the weaknesses and successes of the programmes without an impact evaluation of the intervention funds, a study by (Agusto and Co, 2015) identified the dearth of evaluation mechanisms as the biggest drawbacks of evaluation studies of government intervention funds in Nigeria. Their study designed an evaluation framework, the Agusto MSME intervention funds' performance evaluation model (Agusto MIFPEM), to help determine the economic and social impact of these intervention funds. To the best of our knowledge, however, no study heretofore has attempted a rigorous isolation of the causal effects of the programmes in Nigeria. This study is especially relevant because of the attempt to evaluate the causal impact of the CBN Micro Small and Medium Enterprise Development Fund (MSMEDF).

In 2013, the CBN established the N220 billion MSMEDF in Nigeria (the equivalent of US$600 million). The Fund was designed to channel low-interest funds to the MSME sub-sector to enhance access to finance by MSMEs, increase productivity and output, increase employment, and engender inclusive growth. What sets the MSMEDF apart from other credit schemes is that it was accessible by all state-level governments that met the eligibility criteria. However, the programme has been criticised for not achieving its objectives. The detractors generally alleged that politicians hijacked the programme and that the stringent conditions attached to accessing the Fund were counter-productive. However, there is no empirical evidence to support these claims. This study explores this unique credit intervention scheme, the Micro, Small and Medium Enterprises Development Fund (MSMEDF), to investigate whether the programme had any significant impact on firm-level incidence of loan take-up.

As one of the first studies, we use World Bank Enterprise Survey data for 2010 (which pre-date the intervention year) and 2014 data (that post-date the intervention). The surveys contain information on whether a firm had access to credit as well as a set of demographic information. The question on access to credit allows us to study the incidence of loan take-ups. Despite the concerns about the limitations of our dataset, the World Bank Enterprise Survey data remain the most comprehensive micro-level firm dataset in Nigeria.

In order to measure the impact of the programme (MSMEDF), we exploit the variability in compliance in the programme given that not all states in Nigeria satisfied the eligibility requirements, meaning they could not participate in the programme. The sample was then divided into two groups. The firms located in the states that participated in the programme were assigned to a treatment group, while those located in the non-participating states were assigned to a control group. We then employed the Propensity Score Matching techniques combined with a difference-in-difference method to control for selection into the programme.

Our results suggest that MSMEDF has a positive effect on access to bank credit by firms. The programme is estimated to have increased the incidence of loan take-up by firms by approximately 10 to 14 percentage points, and the results are robust to the use of different sub-samples. The chapter is structured as follows. Section 2.2 provides a detailed description of the scheme. Section 2.3 reviews the relevant literature on the determinants of access to credit, and the empirical literature on SME programmes more generally. Section 2.4 describes our dataset and the construction of the treatment and control groups. Section 2.5 explains the identification strategy and empirical methodology. Section 2.6 describes the findings. Section 2.7 concludes, and outlines some issues for future research.

2.2 Context

2.2.1 The Intervention Scheme

The MSMEDF, which began operation in 2013, is endowed with 220 billion Naira (equivalent to US$600 million) seed capital. The Fund[1] is divided into two components: 10% is devoted to developmental programmes, such as grants for capacity building and administrative costs, while 90% is for commercial purposes. The commercial part of the Fund is released to Participating Financial Institutions (PFIs) at a 2% rate for on-lending to MSMEs at a maximum interest rate of 9% per annum. Eligible activities under the scheme

[1] See Central Bank of Nigeria (2015b) for the MSMEDF guideline.

include manufacturing, services, trade and general commerce, cottage industries and other economic activities prescribed by the CBN. To address the existing gender disparity in access to loans, 60% of the Fund is earmarked for providing financial services to women.

The primary objective of the intervention is to channel low-interest funds to the MSME sub-sector through formal financial institutions. The low-interest-rate funds aim to close the enormous financial gap that has hindered the development of the SME sub-sector in Nigeria through enhancing access to formal financial services, increasing productivity and output of micro-enterprises, increasing employment, creating wealth, and encouraging inclusive growth.

The Central Bank of Nigeria (CBN) is responsible for the management of the Fund, and the participating institutions in the MSMEDF are state governments, specialised banks, and deposit money banks. The first stage of the programme is for the participating institutions to meet the requirements of the Central Bank of Nigeria (CBN). The second stage is for businesses to apply to the banks after meeting the lending criteria.

If the State decides to participate in the programme, it is required that they provide a resolution in the State House of Assembly authorising participation in the scheme. This requirement emphasises the importance of building a good working relationship between the two arms of government, the executive and the legislative. Given the political composition of Nigeria, with 36 federal states and a Federal Capital Territory (FCT), each federal state has an executive governor, the Head of Government, and a legislative body, the State House of Assembly. While the governor is responsible for policy formulation and execution, the legislators enact the laws. The governor must receive the resolution of the legislature to execute any new policy. Therefore, the relationship between the governor and the state legislature will significantly affect a state’s participation in the programme.

In addition, the States must establish a Special Purpose Vehicle (SPV) responsible for the co-ordination, appraisal, disbursement and recovery of loans under the scheme. Also, they are required to provide a Bank Guarantee/Irrevocable Standing Payment Order (ISPO) equivalent to the principal and interest charge; the ISPO is structured to allow for a pre-payment plan in the event of default. The states must sign a Memorandum of Understanding (MoU) with the CBN on the modalities for implementing the programme, and present an annual framework for the empowerment programme targeted at eligible firms. The framework aims to create a sustainable demand for financial services and provide the basis for measuring the performance of the Fund. The State government is expected to provide capacity-building opportunities for eligible firms by providing relevant skill acquisition agencies.

Deposit Money Banks (DMBs) and non-bank financial institutions (which comprise microfinance banks, finance companies and financial cooperatives), are eligible to participate in the scheme. The non-bank financial institutions must be registered with the Corporate Affairs Commission (CAC) and must be a registered member of the Apex association of the institution. Furthermore, they are expected to provide a Board Resolution or trustee consent to participate in the scheme. Collateral to cover a minimum of 75% of the loan amount, a 12-month statement of accounts, and the submission of current audited accounts of the institution also constitute part of the eligibility criteria. On the other hand, the DMBs are required to sign an MoU with the CBN and agree to bear all the credit risks associated with the take-up of the loans.

The state-owned banks eligible to participate in the programme are the Bank of Industry and the Bank of Agriculture; the eligibility criteria are the same as in the case of the DMBs. After meeting the eligibility criteria, the participating institutions can then access the Fund from the CBN. What differentiates eligible States from ineligible States is their participation in the programme. Eligible States satisfied all CBN eligibility criteria and participated in the programme either through the State government, DMB or specialised banks. The Fund must be distributed to the participating States from a financial institution within a State.

For businesses, the disbursement mechanism begins when a borrower applies to borrow from either the State government through the PFIs approved by the CBN in their respective States, DMB or through specialised banks. After receiving the loan application, participating banks appraise the application for economic and financial viability; this is intended to reduce the credit risk associated with unviable ventures. The participating institution then forwards the applications through the SPV to the CBN. The CBN undertakes a pre-disbursement assessment of the loan request and disburses funds to the approved participating institutions' corresponding bank account. Once the applications are approved, the participating institutions disburse the funds to the borrower, usually within one month of completing documentation, and the borrower undertakes to use the funds strictly for the permissible activities under the condition of the scheme.

The PFIs are responsible for providing the list of prospective borrowers, and evidence of submission of names of borrowers to the licensed Credit Bureaux for credit checks. This is designed to reduce market failure sourced in information asymmetry. In addition, the PFIs are also required to submit periodic reports on disbursement to the CBN. The CBN undertakes regular ‘on-site’ and ‘off-site’ checks to determine the veracity of the reports submitted by the banks.

In addition, the participating firms must meet eligibility criteria. The banks require that the borrowing firms have some form of fixed asset as collateral. Although banks have some sets of borrowing criteria that are standard across banks, the eligibility criteria required to access the fund are at the discretion of the individual banks.

One major setback of the scheme was low participation in the programme. Since the programme was launched in 2013, the implementation has met with low demand. Only 19 State governments and 124 PFIs participated in the programme, with a disbursement rate of 27%; 25,210 beneficiaries received loans in 2015 (Central Bank of Nigeria, 2015b). Failure to participate, also known as no-shows (Card *et al*., 2011) in programme evaluation may compromise the impact evaluation design. Therefore, it is important to understand the reasons for non-compliance among participants.

Non-participation by some State governments may be because these States may have enrolled in a similar programme with higher interest rates than the Fund offers. For example, some States participated in the Bank of Industry (BoI) matching Fund with States by lending at 12.5% and the BoI earned a management fee of 2.5% from the States. The States have a spread of 10% interest rate (Bank of Industry, 2015), which is in excess of the 7% available in the MSMEDF. In addition, there is anecdotal evidence from firms that the loan rate for the nation-wide Dangote scheme for SMEs, which pre-dated the MSMEDF, was fixed at 5% interest rate. Banks were allowed to charge any interest rate from 3% to 9% for the MSMEDF, but most banks were charging a 9% interest rate to absorb the risk associated with SME financing. This renders the MSMEDF rates less favourable to States and SMEs at the time of its introduction.

The eligibility criteria, requiring state governors to get the State House of Assembly to participate in the scheme, may be another source of non-compliance. As stated earlier, the type of relationship between the executive and the legislature may increase or reduce their chances of participation in the scheme, given that some governors may not have a good working relationship with their legislators.

Furthermore, the Fund may not have been given the desired legislative backing due to lack of support from male-dominated assemblies fearful of empowering women. The allocation of 60% of the Fund to women was expected to increase the availability of credit among female entrepreneurs in Nigeria. However, there appears to be a challenge in getting a resolution from a male-dominated House of Assembly. According to the National Bureau of Statistics (2016), only 5.6% of the seats in the State House of Assembly in Nigeria are occupied by women.

Studies have shown that women in States with higher percentages of female representatives have the highest success rate of successful legislation affecting women than their female counterparts in low female representation legislatures (Thomas, 1991).

The spread of the interest rate may be another reason why some banks failed to participate in the scheme. Before the scheme was introduced, the prime lending rate was 26% (Central Bank of Nigeria, 2014), and the Fund was disbursed at 2% for on-lending at 9%. The banks have a spread of 7%, far less than they get from lending at the prevailing market rate. In addition, placing the risk on the bank is likely to make the Fund unattractive, as the banks were expected to bear the credit risk associated with providing the loan. Some banks were therefore unwilling to fund MSMEs due to the high risk attached to them.

There have been myriad complaints and extensive anecdotal evidence suggesting the programme has not achieved its objectives, and the expected benefits remain well out of reach. To provide some insights on the media and public perception of the programme, Table A2.1 in the Appendix to this chapter presents summaries from some articles in the national dailies covering the period between 2015 and 2018.

This provides a snapshot of the type of complaints from stakeholders and expected beneficiaries of the Fund. Based on these extracts, the consensus is that the programme is perceived to have failed, with stakeholders citing different challenges in accessing the Fund. The above review of the anecdotal evidence identified five key themes as the major reasons for the supposedly low MSMEDF uptake among firms. Stringent criteria for accessing the Funds have been identified as the primary reason for the low participation rate. Collateral and some documents were required to access the Fund, including tax clearance certificates, statements of bank account, and certificate of incorporation; this also made it difficult for firms to access the Fund. Firms anticipated that the collateral and documentation required under the scheme would be lower than that required for a conventional loan. However, because the commercial banks were forced to bear the credit risk from the loans, they were unwilling to reduce the requirements for lending to MSMEs. Therefore, the requirements for accessing the Fund by commercial banks may have been counterproductive, as the primary aim of the programme was to increase access to credit to MSMEs with limited collateral for a formal loan. Also, it is believed that the banks may not have been well disposed towards the programme, as they were compelled to lend at a 9% interest rate. This is lower than the 21-30% for commercial banks, to as high as 48-60% per annum for microfinance banks (Agusto and Co, 2015).

It is a widely held view, although not substantiated, that State governors and some government officials were subverting the programme, disbursing the Fund to their relatives and political associates by setting up proxy companies to access the funds. One possible implication is that the Fund may not get to the targeted group. Furthermore, a lack of awareness of the programme has been identified as one of the reasons the programme has not achieved its aim. Many MSMEs claimed they were not aware of the programme. Agusto and Co (2015) stated that although the Fund has a relatively high awareness rate compared to other similar programmes in Nigeria, only 16% of respondents were aware of the MSMEDF.

The anecdotal evidence presented above, which is generally based on perceptions of the MSMEDF, suggests the programme has been a failure and has not achieved its primary aim. Whether this is true or not is the subject of empirical investigation in this study. The primary aim of this research is to provide empirical evidence of whether the MSMEDF has achieved its objective of increasing the uptake of loans by SMEs in Nigeria.

2.3 Literature Review

Access to credit has been identified as one of the major constraints in the start-up, survival and growth of SMEs (Cavalcanti and Vaz, 2017; Fowowe, 2017). Stiglitz and Weiss (1981) argued that a loan market might be characterised by credit rationing due to adverse selection and moral hazard. Because borrowers have a different probability of loan repayment, and banks cannot identify good and bad borrowers, the banks tend to use interest rates as a screening mechanism. However, due to asymmetric information, the banks do not have prior knowledge of the risk type of the borrower. Therefore, charging an average interest rate may hurt low-risk borrowers, forcing them to exit the market, while high-risk borrowers may invest in risky ventures with a high probability of bankruptcy. Interest rates cannot efficiently allocate credit (Aga and Reilly, 2011), so banks use alternative measures to screen borrowers. This section will review relevant literature on the determinant of access to credit following the conceptual framework provided in Aga and Reilly (2011). We will further review empirical work on the impact of MSMEs credit schemes in developed and developing countries, although the availability of such studies is somewhat limited for Africa.

2.3.1 Determinants of access to credit

The size and age of the firm have been identified as key indicators used by lenders to screen potential borrowers. Firm size may be an indicator of firm growth, as larger firms are likely to have audited financial statements, which can be used to infer firm transparency. The size of the firm may infer market power (Majocchi *et al*., 2018), due to diversification in exploiting economies of scale. Therefore, lenders are more likely to lend to larger firms given their size. Similarly, older firms are less likely to be credit constrained, as they are considered more reputable than younger firms (Nguyen *et al*., 2021), have gone through more years of learning, and may also have a track record of credit worthiness. For these reasons, therefore, younger firms are more likely to be credit constrained.

Another critical factor is the attribute of the owner. Among the attributes of the owners are gender, educational level and experience. Evidence from studies reveals that female entrepreneurs have less access to credit than their male counterparts (De Mel *et al*., 2009; Asiedu *et al*., 2013). In contrast, Hansen and Rand (2014a) provide mixed evidence for the access to credit of female-owned and male-owned manufacturing firms in sub-Saharan African countries. Small and micro-enterprises owned by females are less likely to be credit-constrained than those owned by males. While for medium-sized businesses, the likelihood of being constrained is lower when owned by male. On the other hand, Aga and Reilly (2011), and Wellalage and Locke (2017) found that male-owned enterprises are more credit constrained than female-owned ones. Another study by Moro *et al*. (2017) found no evidence of gender discrimination, as credit allocation was based on the credit worthiness of the firm, which is independent of the manager’s gender. They argued that a female-run firm is less likely to apply for credit due to an anticipated rejection. One possible reason for the mixed result may be that gender may not be the only factor as to why women are credit constrained, but that participation in a credit market may depend on the managers' willingness to participate in borrowing activity and their perception of the possible success or rejection of the application process.

Intellectual resources (Ogubazghi and Muturi, 2014), that is, the educational level of the manager and the relevant workplace experience, can also affect the likelihood of firms accessing credit. Education provides managers with broad skills and capabilities that are valuable in preparing a convincing business proposal for a bank loan. Similarly, managers with quality work experience may influence a firm's accessibility to bank financing. Nofsinger and Wang (2011) demonstrated that banks rely on the experience of managers to reduce the problem of information asymmetry and moral hazard in funding, given that experienced managers perform better than inexperienced ones.

Collateral is a critical determinant of access to credit. Various studies indicate that collateral plays a crucial role in firm financing, as banks use its many forms as a screening mechanism. Chan and Kanatas (1985) and Bester (1987) find that the quality of collateral pledged depends on the borrower's risk. High-risk borrowers pledge less collateral than low-risk borrowers, and this may generate an adverse selection effect (Stiglitz and Weiss, 1981). Other studies revealed that the requirement for higher collateral provides perverse investment incentives (Niinimäki, 2018), as borrowers hold on to a poor project to prevent the loss of assets (e.g., buildings, lands and machines) in the event of liquidation.

The location of businesses in clusters, such as industrial or export-processing zones, may also be an important factor in access to credit. Research has revealed that business clustering leads to business networks and an integrated support system (Foghani *et al.*, 2017). With close interactions within the cluster, the firms develop a certain level of trust, and this enables the firm to obtain credit support in the form of trade and supplier credits. Also, firms with an international quality certification (e.g., International Organization for Standardization – ISO) are more likely to gain access to credit, as banks may use the certification to infer a firm's credibility.

Finally, informality also plays a vital role in access to credit. Several studies find informality increases a firm's credit constraint (Aga and Reilly, 2011; Wellalage and Locke, 2017), as business registration often plays a vital role in access to credit.

2.3.2 Impact evaluation of SMEs credit schemes

Credit schemes are widely used in many countries as a policy instrument to mitigate the problem of access to credit for SMEs. There have been several studies analysing the impact of SME programmes using impact evaluation methods. However, not all studies adopted a rigorous evaluation technique, as pointed out in Storey's six-level classification of impact evaluation analysis (Storey, 2017). The author identified the first three steps as a simple qualitative analysis, while the last three involve a rigorous analysis of the effectiveness of the policy initiative; it does this by comparing the performance of treated and non-treated firms, matching firms, and taking selection bias into account. The literature search has identified rigorous impact evaluation studies in developed and developing countries around the world. However, there are limited evaluation studies for Africa. By reviewing literature from these other developing countries, we assume that these countries have similar characteristics to Africa, and this can provide lessons and experiences useful for evaluating a similar programme in Nigeria.

The bulk of literature in the impact evaluation of SME programmes is empirical. Some SME credit scheme studies, especially in Africa, used simple descriptive statistical methods (Jibrilla, 2013; Mthimkhulu and Aziakpono, 2012) to analyse programme participation. The most commonly used method of analysis is quasi-experimental design. This method evaluates the performance of firms before and after the introduction of a programme or intervention, comparing the treatment and control groups by using non-experimental observation data, often from different periods.

The studies that used non-experimental designs relied on regression analysis (Roper and Hewitt-Dunda, 2001) to control the differences in treatment and control groups. Others used instrumental variables, fixed effects regression with selectivity correction (Mole *et al*., 2008). Most studies tend to use propensity score matching combined with DID methods (Wren and Storey, 2002; Morris and Stevens, 2010; Čadil *et al*., 2017; Dvouletý, *et al*., 2019) to control for observed (and indirectly unobserved) firm-level characteristics. Some studies used a Regression Discontinuity Design (de Blasio *et al*., 2018; Pellegrini and Muccigrosso, 2017) to identify the causal effect of interest. The analytical approaches used in these studies reveal some methodological evolution over time in estimating programme impact effects, and in controlling for (among other things) selection into programme participation (see Dvouletý *et al*., 2021; Kersten, *et al*., 2017 for a systematic review).

There have been several studies on the economic impact of credit programmes in developed and developing countries around the world, with some earlier studies undertaken by Levitsky and Prasad (1989). Their study outlines the main elements, scope and problems in the operation of credit schemes in 27 developed and developing countries using an analytical approach. Tan and Lopez-Acevedo (2011a) provided a more recent review of the impact evaluation studies of the SME programme from 19 high income and developing countries. Their review provided an overview of the scope of credit schemes, the period covered by the analysis, and the methodologies used. They restricted their review to studies with a non-experimental design, and addressed issues of selectivity bias associated with programme participation. One major significant limitation to programme evaluation identified in their study, especially in developing countries, is the general lack of data and short panel data, making it difficult for researchers to evaluate the impact of SME programmes.

2.3.3 Studies on impact evaluation of SME programmes in developed countries

The programmes evaluated in these studies covered two distinctive outcomes: intermediate and performance outcomes. Intermediate outcomes are short to medium term outcomes that can be measured within a short period after the introduction of an intervention. Performance outcomes, such as sales, output, exports, investment, employment and labour productivity, are better measured over the longer term.

Most studies in developed countries used long-term performance outcomes to assess the impact of credit schemes. SME intervention programmes are relevant to some of these measures, such as employment, sales, increased growth, revenue, and profit. Cowling *et al*. (2018) evaluated the impact of a UK small firm loan-guarantee scheme on employment and sales revenue. Using a natural experiment, they find that the five-year policy rule[2] has a positive effect on the performance of firms in terms of employment, and exerted no significant effect on sales. Similar results are reported for several other countries, including

[2] The five-year rule focuses on start-ups and young businesses under five years old, as these are the businesses that have the least amount of time to develop a strong financial record.

Canada (Chandler, 2012), Czech (Dvouletý, 2017), Italy (de Blasio *et al*., 2018), New Zealand (Morris and Stevens, 2010) and Japan (Uesugi *et al*., 2010).

Using World Bank Enterprise Survey data, Oh *et al.* (2009) evaluated the impact of a credit guarantee policy in Korea. The study adopted the propensity score matching technique, and the results reveal a significant increase in employment and survival rate of firms, but no effect on R&D, investment and productivity. In another study, Kim *et al*. (2015) assess the economic impact of public-private matching fund programmes using firm-level data: the study adopts the Propensity Score Matching (PSM) technique. The findings reveal that firms with public-private support tend to invest more capital in research and development (R&D). The support also yields some positive impacts on their assets, in addition to R&D expenditure from private investment in the matching fund. However, the study shows that there is an insignificant relationship between sales and fixed assets.

Using a similar methodology, Asdrubali and Signore (2015) examine the economic impact of the Multi-Annual Programme (MAP) for enterprises and entrepreneurship for the European Union SME Guarantee Facility in Central, Eastern and South-Eastern European (CESEE) countries covering 2005-2012. The study adopts Propensity Scores and Difference-in-Difference estimation techniques. The findings from the study reveal that the European Union SME Guarantee Facility in the CESEE region positively influenced firm employment levels. The study also shows that MAP beneficiaries had lower productivity because of the limited guarantee loan. It is worthy to note that micro and young SMEs have benefited the most from MAP-guaranteed loans. Overall, the European Union SME Guarantee Facility has been successful in its effectiveness for beneficiary firms in CESEE countries.

In general, high-income country studies tend to have more extensive panel data, and most studies indicate a positive impact of programmes on certain performance outcomes[3], but not others. PSM and DID are mostly adopted in these studies.

[3] For positive effects see Srhoj *et al*. (2021), Koski and Pajarinen (2013), and Chandler (2012). For non-effects see Srhoj *et al*. (2019), and Capelleras *et al*. (2011).

2.3.4 Studies on impact evaluation of SME programmes in other developing countries

The studies in developing countries also present mixed results for firm performance indicators. Some studies indicated that intervention schemes exert some positive impact on sales, R&D, employment and the overall productivity of firms (Tan and Lopez-Acevedo, 2011b; Özçelik and Taymaz, 2008), while others suggest no impact on these outcomes (Chudnovsky *et al*., 2006; De Negri *et al*., 2006).

Evidence from empirical studies in developing countries suggests that most studies have little effect on performance measures. Tan and Lopez-Acevedo (2011a) attributed the lack of a positive treatment effect on performance indicators to the short panel data used in these studies. The studies for high-income countries generally used extensive panel data exceeding ten years. Studies for developing countries follow firms over a shorter period. Since the impact of an intervention may take longer to influence firm performance, this may explain why some studies have found no effect on performance measures at all.

2.3.5 Studies on impact evaluation of SME programmes in Africa

Jibrilla (2013) investigates the impact of government interventions on small-scale enterprises (SSEs) in Mubi, Nigeria. The study adopts descriptive techniques to analyse the sourced data from interviews and administered questionnaires. Based on the empirical results, the findings reveal that government intervention schemes/programmes lack the awareness of the SSE operators because of the stringent measures in accessing the intervention, and therefore the SSE operators interpret that government interventions are irrelevant.

Oyefuga *et al*. (2008) evaluate the impact of an earlier SME funding intervention scheme in Nigeria. The study adopts a questionnaire and interview method to obtain the necessary information from the sampled firms and commercial banks in Lagos, which is the industrially active environment for the analysis. The study reveals that a lack of well-structured business plans and organised projects were the main reasons why small-scale enterprises could not access funds from the scheme. However, the scheme was supportive of some SMEs, while most seemed unaware of the operation of the scheme.

Mthimkhulu and Aziakpono (2012) undertake an analysis of credit guarantee schemes and credit market failures for small businesses in South Africa. The study used data on the

Khula Credit Guarantee Fund from 1996 to 2012 and concluded that there was indication of improvement in capital flows to the target groups between 1996 and 1998. The findings also confirm findings from previous studies, that access to finance may not be the most critical constraint for small business development in South Africa.

Most studies on SME intervention schemes in developing economies, especially Africa, are based on Storey's (2017) first three steps of analysis using analytical and descriptive methods, with none of these studies using rigorous evaluation methods. Therefore, the current study examines the MSMEDF in Nigeria by using an impact evaluation method to empirically examine the effect of the scheme on firms' loan receipt. We focus on the incidence of loan take-up by firms as a short-term outcome, because the period after the introduction of the intervention fund was too short to measure performance outcomes, such as sales growth, employment and investment, which are the most commonly used outcome measures in the literature.

2.4 Research Question

As noted in Section 2.2, the MSMEDF was introduced to channel low-interest credit to the MSME sub-sector through formal financial institutions. The primary objective was to close the sizeable financial gap that has hindered the development of the sector. The CBN revealed 19 states participated in the scheme with an approximate 27% disbursement rate.[4] However, there have been several claims in the print media that the intervention funds have not achieved their purpose (see Table A2.1 for details). Observers cited stringent criteria as the main reasons why the fund had low participation rates. It is necessary to investigate the impact of the programme on bank loan take-up by firms to address whether the claims inherent in the anecdotal evidence have any content. Specifically, this research intends to answer the question whether or not the MSMEDF intervention fund had an impact on loan take-up of SMEs in Nigeria. Since it is not known from the data if firms obtained credit from the Fund, the research intends to examine this indirectly through determining whether the MSMEDF exerted an impact on the bank loan take-up of SMEs located in States that participated in the Fund. In other words, did the intervention enhance the loan take-up rate of Nigerian SMEs in States participating in the MSMEDF? This is the primary research question interrogated in this empirical chapter.

[4] This is equivalent of 59bn naira out of the total of 220bn (CBN, 2015). This represents the 27% cited in the text.

2.5 Data Description

The World Bank Enterprise Survey (WBES) data for Nigeria in 2010 and 2014 are the main data sources for this study. The two years represent the pre-intervention survey conducted in 2010 and post-intervention survey conducted between April 2014 and February 2015. Respondents to the survey are mainly business owners and senior managers of companies. In World Bank enterprise surveys, standardised survey instruments and a uniform sampling methodology are used to randomly select firms from different strata based on their sector of activity, employment size and geographical location. The industries were stratified into the manufacturing, retail, and services sectors. For stratification according to the size of the company, the population of the companies was divided into three strata: small businesses (5-19 employees), medium enterprises (20-99 employees) and large enterprises (100 or more employees). Geographical location stratification was defined on the basis of 26 states in 2010 and 19 in 2014.

The regions surveyed represented cities with the highest level of economic activity. They were selected based on their level of economic activity and other factors, such as the number of establishments, their contributions to both employment and value added in GDP. This particular selection criterion explains why some states were surveyed in 2010 and not in 2014, with the number of states decreasing from 26 in 2010 to 19 in 2014. As a result, the sample size for the years changed from 3,157 establishments in 2010 to 2,676 establishments in 2014. This could raise some concerns about randomisation in the survey sample. However, the surveyed states represent a reasonable national representation of the country, with all six geo-political zones contained within the sample. Also, the states excluded from the 2010 sample were replaced with states with similar characteristics to those surveyed in 2014. Most importantly, the firms, as the main observation units for this analysis, were randomly selected from within these states. Therefore, we have confidence that using a sample of randomly selected firms in the states minimises the potential problem of bias in the use of the states for both years.

As earlier stated, participation in the programme was not random; the states were allowed to participate or not in the programme. Thus, there is self-selection into the programme. This non-random assignment to the treatment makes the empirical investigation of causal effects challenging. Our basic identification strategy is to use the state-level variation in MSMEDF (treatment), and participation in the programme to attempt to identify the causal effect of interest. The treatment group consists of states that satisfied the eligibility criteria and actively participated in the programme by complying with CBN regulations. The control group is comprised of those states that did not fulfil the CBN's eligibility requirements and could not participate in the scheme.

The cross-sectional Enterprise Survey (ES) of firms in 2010 and 2014 provides information for the pre- and post-periods of the intervention respectively. As previously stated, Nigeria has 36 states with a Federal Capital Territory. Out of the 36 states, 19 states[5] participated in the programme as of February 2015.[6] Figure A2.1 in the Appendix to this chapter shows the treatment assignment by states. Because the sampled states in both years are not the same, the key assumption here is that the states dropped from the sample by the survey proprietors have the same characteristics as those sampled in the later year. Therefore, the main sample used for the baseline model consisted of all the states that participated and those who did not participate in the programme but were surveyed in either of the two years.

A major concern in the impact evaluation of credit schemes is that many other credit programmes are concurrently available. In general, no credit scheme happens in isolation; therefore, it is difficult to disentangle the impact of a particular programme from several other similar credit funding programmes. This poses a problem for our identification strategy, as the introduction of the MSMEDF coincided with similar programmes targeted at MSMEs, some of which were introduced before this scheme. To address this problem, we isolated the states participating in other programmes that coincided with the introduction of MSMEDF. The BOI/State matching funds for MSMEs in Nigeria have been identified as the primary scheme concurrent with the MSMEDF. The matching fund is based on a partnership between the Bank of Industry (BoI) and some State Governments. Under this scheme, entrepreneurs of MSMEs with production activities and business carried out within the State have access to the Fund at an interest rate of between 10-12.5% per annum, compared to the prevailing lending rate of 26% per annum (CBN, 2010).

[5] States eligible for the MSMEDF are: Benue, Borno, Gombe, Taraba, Osun, Zamfara, Ondo, Bauchi, Kwara, Enugu, Delta, Oyo, Sokoto, Abia, Akwa Ibom, Cross River, Bayelsa, Jigawa and Kebbi.
[6] This period coincided with the period for the ES in 2014.

Our analysis takes advantage of the fact that the BoI[7] in Nigeria is responsible for implementing the alternative MSME credit schemes in operation over the period under review. This allows us to statistically isolate the states that participated in other programmes from our analysis. To get a clean treatment and control group free of contamination, it is possible to exclude all the states that participated in both the MSMEDF and BoI fund schemes from both groups. It is worth mentioning that we have only eliminated the states that participated in the BoI fund on or before February 2015[8], which coincides with the period in which Enterprise Survey data are available.

Since the intervention focuses exclusively on MSMEs, we restricted our sample size to include only formal SMEs. In other words, large firms were excluded from the analysis. Given that the sample of states differs across the two datasets, this may pose a problem for the internal validity of the research. We attempt to resolve this problem by estimating the effects of interest using three different samples.

SAMPLE 1

This comprises all the observations in our pre-intervention and post-intervention data. It consists of all the treated and non-treated states with no restriction as to whether or not the firms participated in other similar programmes, or whether or not the sampled states overlap in both years. There are 26 states surveyed in 2010 and 19 states in 2014. The treatment group in 2010 consisted of 14 states with a sample size of 1,595 firms, and a control group of 12 states with a sample size of 1,337 firms. In the post-intervention year, the treatment group consisted of 10 states with 1,064 firms and a control group of 9 states with 1,432 firms.

SAMPLE 2

This reduced sample is comprised only of the states that participated in the MSMEDF without being exposed to other credit schemes. The control group are non-treated States without MSMEDF and exposure to other schemes. After deleting all the contaminated states from the sample, the treatment group in the pre-intervention period consisted of 8

[7] Although, the sources of formal credit available to firms are from formal banks and specialist Banks like the Bank of Industry (BoI), at the time of this study only the BoI could be identified as an institution having credit schemes with subsidised interest rate for SMEs.
[8] See the 2016 Annual Report for the Bank of Industry in Nigeria for full details of states that participated in State/BOI matching funds.

states with 808 firms and a control group of 6 states with 680 firms. The treatment group for the post-intervention period consisted of 5 states with 709 firms and 4 states in the control group with 638 firms.

SAMPLE 3

This sample consisted of the states that participated in the MSMEDF without being exposed to other schemes and are common across both years. The control group is comprised of non-treated States without MSMEDF, other schemes and are also common[9] across both years. This represents our clean sample. In this case, we address the problem of non-overlap of states and contamination due to participation in similar credit schemes in samples 1 and 2 by constructing this reduced sample. There are 4 states in the treatment group with 462 and 480 firms in 2010 and 2014 respectively. The control groups consisted of 2 states with 225 and 251 firms in the two years respectively. Tables A2.2 and A2.3 in the Appendix to this chapter provide the lists of treated and control States for these three samples in 2010 and 2014.

The Enterprise Survey (ES) contains information that includes firm characteristics such as ownership, geographical location, industry sector, managerial traits and other industrial attributes. It also contains detailed information on formal sources of finance. We use information from the section on access to financing by companies from formal sources captured in the survey questionnaire to construct the main outcome variable used in this study. The construction of the outcome variable is conditioned upon the distinction between credit participation and access to credit. Although this concept has been used interchangeably, access to credit means a firm is eligible to borrow given the availability of credit but may choose not to participate in borrowing activities (Doan *et al*., 2010). On the other hand, credit participation means an eligible firm has decided to participate in borrowing activities and has already borrowed. While credit participation is more from the demand side, access to credit is more related to the supply side (Gama *et al*., 2017). For this study, we define access to credit as credit participation, where eligible firms choose to participate in borrowing activities and have received a loan from a formal financial institution. Since we do not know if the firms in MSMEDF States obtained credit from the

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[9] This sample compares like to like, and deals with the issue about representation of States from 2010 omitted in 2014 that may have been different from other States in 2010 due to security issues.

Fund, the dependent variable in this case is constructed based on whether firms gained access to credit, not necessarily from the Fund during the survey period.

The outcome variable is constructed from the question of whether firms received a loan or not in the last 12 months. Those firms that answered ‘yes’ to the question were then treated as those who have chosen to participate in borrowing activities and have received a loan. However, this did not reveal whether the participation was formal or informal. To identify whether the firms that chose to participate in borrowing activities borrowed from a formal financial institution, a further question was asked if the line of credit or loan was from a formal financial institution. For those firms that replied ‘yes’ to this question, there was a follow-up question asking for the type of financial institution that granted this loan. Those who reported having credit from private commercial banks, state-owned banks or government agencies, and microfinance banks, were recognised as having received a formal bank credit.

Given how the scheme is set up, it is designed to provide formal financial institutions (i.e., private, state and non-financial banks in Nigeria) with funding resources. The programme’s design informs the construction of the outcome variable, which includes all eligible financial institutions. In our view, this provides a useful measure based on our working definition of access to credit.

Table A2.4 of the Appendix to this chapter describes the variables included in the analysis. There is growing literature on the determinants of access to credit. The firm-level studies of access to credit attempt to identify observable characteristics of a firm and other factors that determine a firm’s access to credit (see Aga and Reilly (2011) for a detailed review on factors influencing access to credit). The choice of the control variables used is based on the determinants of access to credit found important in the existing literature.

Table 2.1 provides a summary of the key outcome variable, bank credit provided to enterprises before (i.e., 2010) and after (i.e., 2014) the financial intervention scheme for the three samples. We compared credit in States where the MSMEDF operated with those where it did not, and several points emerge when comparing the mean values of bank credit outcome rates for the two groups. In Sample 1, firms in the treatment group of the pre-intervention period have lower mean values for the outcome variable relative to the control group, with statistically significant differences in mean. With the introduction of the scheme, the treatment group recorded a sharp increase in mean values for the outcome variable relative to the control group. This indicates that the intervention increases access to credit for firms in the treated group. Looking at Samples 2 and 3, there was an increase in the mean values of the outcome variable in the treatment groups relative to the control groups prior to the intervention. The three samples maintained the same pattern after the scheme's introduction, with programme participation associated with higher mean values for the treatment groups, revealing a strong positive correlation between the treatment and the firm's access to credit.

The difference in the incidence of loan take-up between the treated and non-treated states, and between the treated states in both years, widened substantially after the intervention, suggesting that the scheme may have had the desired effect and may have benefited weaker firms that were initially credit constrained. Participation in the programme improved access to credit relative to what might have prevailed had they not participated. However, these are raw means, and we have not controlled for the role and influence of confounders. Self-selection of firms may bias efforts to estimate the impact of the programme from a simple comparison of post-treatment outcomes of the treatment and control groups. In order to overcome this, we will need to control for a variety of different characteristics (or confounders) in our analysis.

Table 2.1: Summary statistics of outcome variable (Bank Credit) for the treatment and control groups

Samples		Treatment	Control	Differences t-test
Sample 1	2010	0.137	0.176	-0.039***
		(0.009)	(0.010)	(0.013)
	2014	0.338	0.260	0.078***
		(0.015)	(0.012)	(0.018)
Sample Size				
2010		***1595***	***1337***	
2014		***1064***	***1432***	
Sample 2	2010	0.158	0.150	0.008
		(0.013)	(0.014)	(0.019)
	2014	0.339	0.169	0.170***
		(0.018)	(0.015)	(0.023)
Sample Size				
2010		***808***	***680***	
2014		***709***	***638***	
Sample 3	2010	0.152	0.111	0.040
		(0.017)	(0.021)	(0.028)
	2014	0.323	0.112	0.211***
		(0.021)	(0.020)	(0.033)
Sample Size				
2010		***462***	***225***	
2014		***480***	***251***	

Standard errors in parenthesis.
Difference defined as the means of treatment group minus means of the control group.
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations using WBES data.

Since large firms were excluded from participating in the MSMEDF, the loan rate of large firms is used to inform whether loan rate incidence may already have been higher in the MSMEDF States. Table A2.5 in the Appendix to this chapter reveals that the loan rates for larger firms in the treated states were not statistically different from those in the control states. This is suggestive that lending practices were unlikely to differ between States subject to the treatment and those not. This could be taken to suggest what is actually observed for the SMEs represents an effect of the MSMEDF intervention and not inherently different lending practices across states.

Table 2.2 provides selected summary statistics for the input variables used in the empirical analysis for Sample 1. The input variables capture the firm as well as managerial attributes. When comparing the mean values of the independent variables for the treatment and control groups, the pre-intervention characteristics suggest that enterprises with lower sales, those without an audited financial statement or not owned by a partnership, are more likely to be in the control than the treatment group. In contrast, older firms, firms owned by sole proprietorship, and having a female entrepreneur are more likely to be in the treatment group. The t-test indicates that some of these group differences are statistically significant at the 1% level. On the other hand, the post-intervention characteristics suggest that firms in the treatment group were on average owned by sole proprietorship, have some form of fixed assets that can be used as collateral in raising loans, and have more managers with vocational education. This result indicates that the firms differ in their characteristics. This highlights the dangers of comparing raw means between the treatment and control groups, as reported in Table 2.1 above.

Table 2.2: Summary statistics of explanatory variables for the treatment and control groups-Sample 1

	Pre Intervention – 2010			Post Intervention - 2014		
Variables	**Treatment Mean**	**Control Mean**	**Differences t-test**	**Treatment Mean**	**Control Mean**	**Differences t-test**
lnsales	16.048	16.154	-0.106***	13.870	14.270	-0.400***
	(0.034)	(0.038)	(0.051)	(0.092)	(0.081)	(0.092)
Age	13.465	12.620	0.845***	15.457	15.624	--0.168
	(0.240)	(0.264)	(0.357)	(0.362)	(0.288)	(0.457)
sole_prop	0.836	0.800	0.036***	0.848	0.811	0.036***
	(0.009)	(0.800)	(0.014)	(0.011)	(0.010)	(0.015)
partnership	0.033	0.047	-0.014*	0.040	0.040	0.001
	(0.004)	(0.006)	(0.007)	(0.006)	(0.005)	(0.008)
ltd_comp	0.129	0.150	-0.022	0.110	0.145	-0.035***
	(0.008)	(0.010)	(0.013)	(0.010)	(0.009)	(0.014)
retail	0.197	0.185	0.012	0.185	0.216	-0.031***
	(0.010)	(0.011)	(0.015)	(0.012)	(0.011)	(0.016)
fixasset	0.476	0.484	-0.007	0.484	0.411	0.073***
	(0.013)	(0.014)	(0.019)	(0.015)	(0.013)	(0.020)
voceduc	0.190	0.196	-0.006	0.169	0.124	0.046***
	(0.010)	(0.011)	(0.015)	(0.011)	(0.009)	(0.014)
unieduc	0.780	0.776	0.004	0.808	0.846	-0.037***
	(0.010)	(0.011)	(0.015)	(0.012)	(0.010)	(0.015)
exper	11.708	12.432	0.723***	12.139	12.565	-0.426
	(0.191)	(0.223)	(0.292)	(0.293)	(0.230)	(0.367)
Sample Size	**1,595**	**1,337**		**1,064**	**1,432**	

Standard errors in parenthesis
*** p<0.01, ** p<0.05, * p<0.1
Only summary statistics of statistically significant differences are reported.

Source: Author's calculations using WBES data.

2.6 Empirical Methodology

2.6.1 The Propensity Score Matching (PSM) Technique

Assessing the impact of the MSMEDF on firm access to bank credit requires making an inference about the outcome that would have prevailed if a firm had not participated in the programme. The treatment (MSMEDF) is represented by a binary random variable D_i. The outcome of interest (access to bank credit) is represented as Y_i, where Y_1 denotes the outcome conditional on participation and Y_0 for non-participation. The causal effect is measured by the difference between the two potential outcomes, thus known as the average treatment effect (ATE).

The ATE may not be completely useful for policy purposes, as it captures an average across the entire population, ignoring the assumption on the independence of costs from potential outcomes. The data are either for the firm that participated in the programme or those that did not, but not both concurrently. A firm either receives the treatment or does not receive the treatment, and because the firms are not observed with or without treatment at the same time, the causal effect for a single firm cannot be measured. This poses a serious problem and represents a missing counterfactual common in programme evaluation. Therefore, the most common evaluation measure is the Average Effect of Treatment on the Treated (ATT); this estimates the mean difference between the real and counterfactual outcomes. This represents the main impact of the programme among those that participated. The ATT is given as follows:

$$ATT = E[Y_1 | D=1] - E[Y_0 | D=1] \quad [2.1]$$

In an experimental approach where participation is randomly assigned, the difference in means between the treated and control group provides an unbiased estimate of the average treatment effect.

The idea of a quasi-experiment is to mimic the particular characteristics of a randomised experiment as closely as possible. The current study employs the Propensity Score Matching (PSM) technique, initially introduced by Rosenbaum and Rubin (1983), and combines it with a Difference-in-Difference (DID) technique to minimise the role of possible time-invariant unobservable confounders. This facilitates the estimation of effects that potentially attempt a causal interpretation. These techniques are used to eliminate potential biases from observed and unobserved variables, and to balance the two groups together with some matching criteria.

Rosenbaum and Rubin (1983) proposed PSM as a method to reduce bias in the estimation of treatment effects with observational data. The method exploits the probability of treatment assignment conditional on a set of observed covariates. Based on the balancing property, the distribution of observed baseline covariates will be similar between treated and control groups. The PSM matches firms in the treatment group with firms in the control group using firm-level data for the year 2010, before the introduction of the intervention, and then for the year 2014, after the intervention.

Given the absence of panel dimensions to these data on the SME sub-sector in Nigeria, the PSM is applied separately to a cross-section of firms before and after the introduction of the programme with the aim of matching treated firms to the non-treated firms with an identical set of observable characteristics. While the PSM deals with bias from observed variables, the DID is generally more effective in dealing with bias from unobserved variables (Heckman *et al*., 1998; Smith and Todd, 2005).

To solve the selection bias problem with the PSM using non-experimental data, one has to invoke some identifying assumptions. The treatment is required to satisfy some form of exogeneity. This assumption is known as the Conditional Independence Assumption (CIA). CIA requires that, conditional on the observable characteristics of possible participants, the decision for participation in the programme should be independent of the outcome measures. Any systematic differences in outcomes between the treated and control group with the same covariate values should be exclusively attributed to the treatment.

Second, the Stable Unit Treatment Value Assumption (SUTVA) assumes that the treatment received by one unit does not affect the outcome of another unit. This ensures that there are no general equilibrium or spillover effects associated with the treatment. In addition, the units are assumed to have received the same dose of the treatment with no unobserved treatments present through spill-overs. If these assumptions are satisfied, it is theoretically possible to obtain an unbiased estimate of the effect of the intervention on credit access of firms.

Using the PSM approach, we solve the counterfactual problem by computing the estimated effect of the ATT for the period before the introduction of the programme at the baseline, defined as ATT_0. Then, we do the same for the year after the intervention, defined as ATT_1. The difference between the pre-intervention ATT_0 and post-intervention ATT_1 (i.e., the DID) is taken to represent an attempt of the causal impact of the intervention programme on access to bank credit. Using the DID[10] method, the unobservable effects that are constant over time are differenced out. This allows for the control of these unobservable characteristics, and the identification of the treatment effect.

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[10] The difference-in-difference procedure is used primarily to remove state-level unobserved confounders. In the absence of panel data, the propensity score matching (PSM) technique just accounts for the role of observables, hence its description as 'selecting on observables'. Therefore, the ability to exploit a short two-year panel with PSM enables the elimination of both observable confounders and unobservable confounders that are assumed fixed over time.

2.6.2 The Logistic Treatment Assignment Equation

The first step in PSM is the estimation of the treatment assignment equation. A logistic regression model is used to estimate the treatment equation, which predicts the probability of a firm being in the treated state. The model will help generate precise predictions for the treatment assignment equation, which can then be used for matching. Since the propensity score model is assumed to satisfy the exogeneity assumption, the covariates included in the model are expected to influence the decision to participate and the outcome variable simultaneously. This implies that the explanatory variables can be correlated with the outcome variable, but are assumed to be unaffected and independent of the treatment to avoid any confounding effects. Even though the outcome of interest for the treated and control group might be correlated with the assignment, once the effects of the explanatory variable are netted out using the treatment assignment equation, the outcome is assumed to be independent of the treatment assignment (Heckman *et al*., 1998).

To ensure that only variables unaffected by the treatment are included in the model, the selected variables should either be fixed over time or measured before the intervention (Caliendo and Kopeinig, 2008). In this study, the selection is based on attributes the literature suggests might determine firm access to credit. These factors include observable characteristics such as the firm's age, sector, and business type. The managerial attributes include educational level, gender and experience, and other measures, such as international certification, audited financial statement, export zone, and value of the fixed asset. Excluded from the treatment assignment equation are observable variables measured after the intervention and those affected by the intervention, such as sales and employment size. Because of this, firms may decide to invest the fund in activities that may increase their sales, thereby increasing the firm's employment size.

The treatment assignment is then estimated using a logistic model with firm-level and other characteristics. The explanatory variables from the logistic model are then used to construct the propensity scores on which the firms from the two groups are subsequently matched. We may not be interested in how well the logistic model predicts whether or not a firm received treatment, because the logistic regression is exclusively used to create the propensity scores. In addition, this is mostly a statistical exercise without any economic interpretation for the estimates. However, precision in the estimated propensity score can be important. The consistency of propensity score estimates depend on the correct specification of the propensity score model, as propensity score methods can be sensitive to mis-specification; this may have implications for the quality of the estimated propensity scores, and therefore the matching. A well-determined treatment assignment equation is advantageous in obtaining a well-specified propensity scores.

2.6.3 The common support

Once the treatment assignment equation has been estimated, the next thing to investigate is a common support. This is a vital assumption designed to ensure that all observations have a positive but not perfect probability of being in either the treatment or the control group. The probability of participating in the programme for the treated and non-treated groups should be in the same domain; this implies that all propensity scores for the control overlap with those from the treatment group. Most commonly, the common support property is assessed by inspecting the minima and maxima of the propensity scores for both the treated and non-treated units. Those in the treatment group with a probability of participation outside the range are dropped from the empirical analysis. Therefore, the programme effect cannot be estimated for these units. If a substantial proportion of the sample is rejected, the treatment effect will be based on a small sub-sample that may not represent the population. A systematic difference in observed characteristics between the retained and the dropped sample may affect the interpretation of the treatment effect (Khandker *et al*., 2009). Therefore, it is useful to consider the characteristics of those dropped from the sample. Based on the propensity scores, we will then match the firms in the treatment group within the common support with the firms in the control group.

2.6.4 The matching technique

The matching technique pairs members of the treated group with those of the control group with similar observed characteristics. The treatment impact is then estimated by subtracting mean outcomes of a matched control group from the mean outcomes of a matched treatment group.

In the literature, several matching algorithms are discussed. However, Caliendo and Kopeinig (2008) identified four main methods of propensity score matching. The most commonly used algorithm is the nearest neighbour matching; this selects a unit from the treatment group and matches it to a unit in the control group with the closest propensity score. Matching can be done with or without replacement. Matching without replacement requires that once a unit in the control group is selected to be matched with a unit in the treated group, that selected unit in the control group is no longer available to be used as a match for another treated unit. As a result, each control unit is matched only once.

On the other hand, matching with replacement requires that an untreated unit can be selected more than once and included in more than one matched set. However, there is a trade-off between bias and variance when matching is undertaken with replacement. This approach decreases the bias and increases the covariate variance, given that the same untreated unit is used in more than one matched set (Mitra and Reiter, 2016). One of the issues in matching subjects with control is determining the distance for matched pairs, but the nearest neighbour matching technique does not minimise the total distance within matched pairs (Gu and Rosenbaum, 1993), which is one of the drawbacks of nearest neighbour matching.

Caliper matching is similar to the nearest neighbour technique. Caliper matching imposes a tolerance level where comparison units are matched with treated units within a certain width of the propensity score; this avoids the risk of bad matches due to a distance within the matched pairs. Lunt (2014) suggested a narrow caliper could improve the performance of propensity score matching by reducing bias and producing closer matching. Inversely, a tight caliper could lead to some unmatched units. Therefore, it is difficult to establish a tolerance level (Imai and Ratkovic, 2014) on which to produce good matches.

One of the matching algorithms used to estimate the propensity score is kernel matching. Kernel matching measures the distance between observations by putting a weight on comparison groups when computing the estimated treatment effect. Unlike other matching methods that use only a few observations from the comparison group, kernel matching uses weighted averages of all observations in the control group to construct the counterfactual outcome. This gives the advantage of a lower variance because more information is used. However, Caliendo and Kopeinig (2008) noted that using all observations could lead to bad matches. Therefore, there should be strict compliance with the common support condition.

For this study, we employ the Epanechnikov kernel density, which is bounded between -1 and +1 and is inverted U-shaped. The following formula gives the Epanechnikov probability density function:

$$k_{1(u)} = \frac{3}{4}(1-u^2)1(|u| \leq 1) \qquad [2.2]$$

Weights are then assigned to each treatment and control pair based on their distance. The closer the units in terms of the propensity score, the higher the weight assigned. Lower weight is assigned to more distant observations. The weights are then normalised to ensure they sum to one. The weighted average is computed for each treatment unit using all the control group units, and the difference between the weighted averages of each unit from the control group and the treatment group generates the ATT for the impact analysis of the programme on access to the bank by firms. The ATT is constructed as follows:

$$ATT_{KM} = \frac{1}{N^T} \sum_{i \in T}\left(Y_i^T - \sum_{j \in C} \widehat{\emptyset_J}\ Y_j^C\right) \qquad [2.3]$$

Where: $\widehat{\emptyset_J}$ is estimated standardised weight measuring distance between treatment and control.

T is treatment and C is control

$$\widehat{\emptyset_J} = K\left(\frac{p_i - p_j}{L_i}\right) / \sum_{i \varepsilon T}^{N^T} K\left(\frac{p_i - p_j}{L_{ij}}\right)$$

$$Where\ L_{ij}\ is\ the\ bandwidth.$$

The selection of the bandwidth is crucial in kernel density estimation as noted by Chu *et al*. (2017). We will use the Epanechnikov kernel density bandwidth of 0.06, which is the default bandwidth generally used in literature. However, we will vary the bandwidth between 0.04 and 0.08 to demonstrate whether the results are sensitive to different bandwidth.

2.6.5 The balancing properties

A key issue underlying the internal validity of PSM is the balancing property. The balancing property is examined to check if the matching procedure used above balances the distribution of the relevant covariates in both the control and treatment groups after matching. It is important to determine the quality of the matching procedure, and several procedures can be used to assess matching quality. One suitable method is the Standardized Bias (SB) measure suggested by Rosenbaum and Rubin (1983) defined as the difference of sample means between the treated and matched control groups as a percentage of the square root of the average of sample variances for both groups. The SB measures the distance in the marginal distributions of the covariates in the treated and the control group before and after matching. The SB before matching is given as:

$$SB_{Before} = \frac{\overline{X_T} - \overline{X_C}}{\sqrt{0.5(V_T\,(X) + V_C\,(X))}}\,.100 \qquad [2.4]$$

Where T denotes treatment group, C denotes control group, X_T and V_T are the mean and variance of the treatment and control groups before matching.

The SB after the matching is given as:

$$SB_{After} = \frac{\overline{X_{T,M}} - \overline{X_{C,M}}}{\sqrt{0.5(V_{T,M}\,(X) + V_{C,M}\,(X))}}\,.100 \qquad [2.5]$$

M denotes the fact that the values are from the matched samples. Most empirical studies generally expect that after successful matching, a bias reduction between say 3% and 5% is sufficient (Caliendo and Kopeinig, 2008) for a quality match.

Another check is using a t-test to conduct a test of differences in covariate means on the matched samples. The t-test is used to check for any significant difference in covariates means for the treatment and control group. Differences are expected before matching; after matching, however, there should be no significant differences in the mean values. Therefore, the covariates should be balanced in both groups in terms of this distributional moment. Also, an F-test is used to investigate any differences in the sampling variances for the set of continuous variables. It is expected there should be no differences in the variances for the continuous variables between the two groups after matching.

Other diagnostic tests include the pseudo R^2, the prob-value of the likelihood ratio test (LRT) and Rubin's B and R statistics. If the matching is good, there should be no variation in the distribution of covariates between the treatment and control groups after matching. Therefore, the pseudo R^2 from the logistic regression treatment assignment after matching should be very low, indeed close to zero. Also, using the matched data, the LRT for the significance of the estimates for these variables should be statistically insignificant. Following the work of Rubin (2001), the Rubin's B (which is the standardised percentage difference of the means of the linear index of the propensity score in the treated and non-treated matched group), is expected to be less than 25%. Likewise, Rubin's R (which is the ratio of the treated to the non-treated variances of the propensity score index for the matched data), is expected to lie between 0.5 and 2 to confirm that the quality of the match is good and that the match is balanced in terms of covariates. It is only after satisfying the matching quality diagnostics that we estimate the treatment effect and compute the standard errors for the ATT estimates.

2.6.6 The estimation of standard errors

Compared to the traditional regression methods, the PSM includes variances from the deviation of the propensity score, the common support, and in the order in which the treated units are matched. This adds variation beyond the normal sampling variation (Caliendo and Kopeinig, 2008; Khandker *et al*., 2009). Failure to account for these variations will cause the standard errors to be incorrectly estimated (Heckman *et al*., 1998). Therefore, an increasingly popular approach employed to compute standard errors is bootstrapping (Efron and Tibshirani, 1993).

Bootstrapping treats the original sample as the population and draws many independent random samples from the original sample of data to compute a sample statistic. This exercise is replicated a number of times; each time the estimation exercise is re-done, different estimates are yielded at each repetition. The number of bootstrap replications should be of adequate size to determine the sampling distribution. This study uses a bootstrapping method with 200 replications to estimate the standard error. Imbens (2004) argued that, since bootstrapping estimators are asymptotically linear, this is likely to lead to valid standard errors in PSM applications.

2.7 Empirical Results

This section discusses the results obtained from the implementation of the PSM technique using the World Bank Enterprise Survey datasets. The first part of this section presents estimated results from the logistic treatment assignment equation; this models the probability of selection into treatment (i.e., the Micro Small and Medium Enterprises Development Fund), and provides the propensity scores for matching firms in the control group with those in the treated group. We then evaluate the matching quality using post-estimation diagnostic tests. The balancing test is performed to determine that each firm in the treated and non-treated group has a similar distribution of characteristics. This is followed by the interpretation of the results of whether firms that participated in the programme differ significantly from non-participating firms regarding their access to bank credit. Finally, we discuss the empirical results and assess whether the estimated effects are interpretable as causal or not.

2.7.1 The logistic regression model

The treatment assignment equation is estimated using a logistic regression model. The treatment is receiving a loan. The logistic model includes all time invariant variables measured before the intervention. The sales and employment size variables are excluded from the treatment assignment equation model because they are assumed to be influenced by the treatment. However, a potential concern remains that the exclusion of these variables would lead to the mis-specification of the propensity score model. In order to demonstrate that the exclusion of these variables from the propensity score model will not pose a serious mis-specification problem, we first estimated different models including and then excluding the sales and employment size variables. The estimates contained in Table A2.6 in the Appendix to this chapter reveal that these variables are not highly correlated with the treatment to such a degree that they alter the relationship with the loan receipt.

We then estimate the treatment assignment equation model. All covariates included in the model are assumed to have satisfied the exogeneity assumption. However, we acknowledge that the inclusion of fixed asset and gender variables in the model could be a source of concern, given that the variables may likely be affected by the intervention. The summary statistics show that more than 50% of firms lack collateral in the form of fixed assets, implying that fixed assets may affect the participation of firms in the programme.

We justified the inclusion of fixed asset and gender dummy variables based on the design of the intervention programme and the eligibility criteria for accessing the fund. First, firms must have certain fixed assets as appropriate collateral before participating in formal financial institutions' borrowing activities. These collaterals are mainly real estate assets (such as land and buildings). In a developing market like Nigeria, creditors are usually reluctant to accept movable assets (such as machinery, accounts receivables, and inventory) as security for bank credit (Calomiris *et al*., 2017). Therefore, a firm must have demonstrable fixed assets to access bank loans. In this case, the fixed asset variable is assumed to have been determined before the intervention and cannot be affected by the treatment. In the case of the gender variable, although the programme is geared towards women, it will take longer for the firm to change the composition of its management to reflect the company's gender profile. However, given the short time period that has elapsed since the intervention, we believe this is highly unlikely.

Table 2.3 presents the treatment assignment equation. The dependent variable represents the treatment (i.e., whether the firm with access to bank loan is located in the state that participated in the MSMEDF or not) and is estimated using a logistic regression model. The results suggest that the age of the firm significantly determines the log odds ratio of firms being treated in both the pre-intervention and post-intervention periods. The fixed asset variable entered with the expected sign and statistically significant results for the post-intervention period. The result is consistent with Bester (1987), who reports that firms with collateral resources are more likely to gain access to credit. The positive effect may be due to the fact that banks request some form of tangible assets to be pledged as collateral, which can be liquidated if default occurs. Similarly, the age variable is significant in determining the log odds ratio of a firm in the treated states in the pre-intervention period.

Table 2.3: Pre- and Post-Intervention Logit Model Regression Result – Sample 1

VARIABLES	2010 Treated_States	2014 Treated_States
Age	0.046***	-0.016*
	(0.011)	(0.010)
age2	-0.001**	0.000
	(0.000)	(0.000)
Partnership	-0.412**	-0.049
	(0.195)	(0.211)
ltd_comp	-0.133	-0.618***
	(0.158)	(0.214)
Retail	0.204*	-0.128
	(0.118)	(0.111)
Intcert	0.288**	0.315**
	(0.143)	(0.157)
Fixasset	-0.006	0.322***
	(0.082)	(0.086)
Expzone	0.297*	-0.109
	(0.176)	(0.177)
Voceduc	-0.083	0.396***
	(0.116)	(0.118)
Exper	-0.079**	-0.071***
	(0.033)	(0.023)
ltd_exp		0.026*
		(0.015)
age_exp		0.001*
		(0.000)
Constant	0.310*	0.122
	(0.184)	(0.157)
Observations	**2,932**	**2,496**

a.Standard errors in parenthesis, *** p<0.01, ** p<0.05, * p<0.1

b. Treated_States are firms with access to credit in treated States.

c. Only statistically significant results are reported

Source: Author's calculations using WBES data.

As earlier stated, the statistical significance of the variables is not of primary concern here; this is because the logistic model is primarily designed to generate good predictions for the treatment assignment that can be used for matching purposes and not necessarily to study the factors that determine participation into the programme. Therefore, there is no economic theory that underlines the specifications used here, and the use of higher order and interactions is mainly for generating good tractions.

Figures A2.2 and A2.3 in the Appendix to this chapter present the two kernel density graphs; the plot of the propensity scores for the treated and control groups shows where the common support is. A visual inspection of the graph shows that there is a good overlap of the propensity scores of the two groups in the two periods, while Figures A2.4 and A2.5 in the Appendix to this chapter provides the histogram of propensity scores by treatment and control group for both years. Similarly, the histogram suggests common support is found, but there are some propensity scores in the control group at the tail end of the distribution that cannot be matched with those in the treatment group in either 2010 and 2014 data. We determine the common support property by looking at the minima and maxima of the propensity scores in both years. Table A2.7 shows that there are only eight and three propensity scores off the common support in 2010 and 2014 respectively, and there is just one treated firm off the common support in 2014. All these cases are excluded from further analysis.

Using the estimated propensity scores from the logistic regression model, we matched each treated firm with a weighted average of all non-treated firms in each year using a weighted kernel density function, the Epanechnikov density. Following precedent in the literature, we used the standardised bias (SB) measure suggested by Rosenbaum and Rubin (1983) to measure the distance in the marginal distributions of the covariates. The SB is computed for each covariate in the treatment assignment equation before and after matching (see Figures A2.6 and A2.7 in the Appendix to this chapter).

Tables 2.4 and 2.5 provide evidence that the pre- and post-intervention data meet the covariate balancing assumption. As shown in the data section, there exist significant differences in the means and variances of the covariates. However, after the match, the t-test of differences in means shows that there are no significant differences in means in the matched samples. The F-test yields similar results for the differences in sampling variances for the set of continuous variables, and the null hypothesis of common sample variance is upheld at the 5% level of significance with all the ratios lying between 0.91 and 1.10.

Table 2.4: Pre-intervention (2010) covariates balancing check by mean and variance differences

	Mean		t-test			V(T)/
Variable	Treated	Control	%bias	T	p>t	V(C)
Age	13.367	13.114	2.7	0.77	0.44	1.02
age2	265.11	256.93	2.0	0.6	0.549	1.05
Partnership	0.03331	0.03056	1.4	0.44	0.659	.
ltd_comp	0.12822	0.12375	1.3	0.38	0.704	.
retail	0.19673	0.20334	-1.7	-0.47	0.641	.
services	0.29038	0.28254	1.7	0.49	0.625	.
medium	0.33941	0.33473	1.0	0.28	0.78	.
intcert	0.09428	0.09287	0.5	0.14	0.815	.
audit	0.21999	0.21309	1.6	0.47	0.637	.
fixasset	0.4758	0.47167	0.8	0.23	0.815	.
expzone	0.06034	0.05515	2.3	0.63	0.53	.
gender	0.14456	0.13854	1.7	0.49	0.626	.
noeduc	0.03017	0.0295	0.4	0.11	0.912	.
voceduc	0.19045	0.18883	0.4	0.12	0.907	.
exper	11.713	11.9	-2.4	-0.69	0.491	0.98
exper2	195.27	200.71	-1.9	-0.57	0.567	0.95
exper3	4194.9	4366.1	-1.7	-0.51	0.61	0.94
ser_med	0.13953	0.13581	1.0	0.3	0.761	.

* if variance ratio outside [0.91; 1.10]

Source: Author's calculations using WBES data.

Table 2.5: Post-intervention (2014) covariates balancing check by mean and variance differences

	Mean		t-test			V(T)/
Variable	Treated	Control	%bias	T	p>t	V(C)
age	15.357	15.312	0.4	0.09	0.929	1.01
age2	368.91	365.69	0.5	0.11	0.913	1.09
Partnership	0.04049	0.0464	-3.0	-0.67	0.505	.
ltd_comp	0.11017	0.11964	-2.8	-0.68	0.494	.
retail	0.1855	0.19245	-1.7	-0.41	0.683	.
services	0.32109	0.31956	0.3	0.08	0.94	.
medium	0.40678	0.40482	0.4	0.09	0.927	.
intcert	0.0904	0.08907	0.5	0.11	0.915	.
audit	0.22316	0.22893	-1.4	-0.32	0.751	.
fixasset	0.48305	0.47843	0.9	0.21	0.831	.
expzone	0.05556	0.05913	-1.5	-0.35	0.723	.
gender	0.1177	0.11917	-0.5	-0.1	0.917	.
noeduc	0.00282	0.00257	0.5	0.11	0.909	.
voceduc	0.16949	0.16332	1.7	0.38	0.703	.
exper	12.051	12.031	0.2	0.05	0.96	1.03
exper2	232.49	229.23	0.9	0.2	0.84	1.04
exper3	6066.1	5885.7	1.1	0.25	0.802	0.89
ltd_exp	1.2717	1.3638	-2.0	-0.45	0.651	0.9
age_exp	241.18	242.28	-0.3	-0.07	0.944	1.07
ser_med	0.14313	0.14337	-0.1	-0.02	0.987	.

* if variance ratio outside [0.89; 1.13]

Source: Author's calculations using WBES data.

After re-running the treatment assignment equation using the matched data only, Table 2.6 reveals that the pseudo-R^2 is considerably lower at 0.001 for both pre-intervention and post-intervention data when compared to the 0.014 and 0.018 reported in the original sample for both years, respectively. Further, the LRT for the overall significance of the estimated relationship is statistically insignificant with 4.21 and 2.29 compared to 55.15 and 63.53 from the unmatched data for 2010 and 2014, respectively. In addition, the estimated Rubin criteria are met, with all the SB estimates less than 5% in absolute terms, with the Rubin B less than 25% and R lies between the recommended 0.5 and 2.

Table 2.6: Pre- and Post-intervention balancing property diagnostics test

Year	*Ps R2*	*LR chi2*	*p>chi2*	*Mean Bias*	*Med Bias*	*B*	*R*	*%Var*
2010:unmatched	0.014	57.15	0.000	5.0	5.5	28.1*	0.94	60
Matched	0.001	4.21	1.000	1.5	1.7	7.3	1.06	0
2014:unmatched	0.019	63.53	0.000	4.4	3.8	32.4*	1.24	100
Matched	0.001	2.29	1.000	1.0	0.7	6.6	0.95	0

* if B>25%, R outside [0.5; 2]

Source: Author's calculations using WBES data.

The implementation of the matching procedure appears to have been successful. The evidence suggests that the quality of the match is good. Having achieved effective balancing, we now compute the ATT estimates for the impact of the MSMEDF within a DID framework.

The results of the impact analysis (which uses the kernel density approach) are presented in Table 2.7 for both the pre-intervention and post-intervention periods. The standard errors for the ATT estimates are computed using the bootstrapping technique with 200 replications and the Epanechnikov kernel density bandwidth of 0.06.[11] The average treatment effect between firms assigned to the treatment and those assigned to the control group in the pre-intervention period shows a negative difference in means. The result suggests that the incidence of loan take-ups is lower for firms in the treated group than the untreated group. The difference is statistically significant. This implies that there is a significant difference in the firm's access to bank credit in the treated and non-treated states prior to the introduction of the programme.

[11] The results are sensitive to different specification of the bandwidth from 0.04 to 0.08.

Table 2.7: Average treatment effect of MSMEDF participation on a firm's access to bank credit (Sample 1)

	Bank Credit
2010(ATT_0)	-0.029***
	(0.014)
2014(ATT_1)	0.074***
	(0.018)
D-i-D_1(ATT_1-ATT_0)	0.103***
	(0.023)
Sample 2010:	
Treated	1,333
Control	1,591
Sample 2014:	
Treated	1,062
Control	1,430

Bootstrapped standard errors with 200 replications in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations using WBES data.

The post-intervention average treatment effect (ATT_1) reveals a significant increase in the incidence of loan take-up for those firms in the treated states compared to those in the non-treated states. On average, the impact of the intervention significantly increases the firm's access to bank credit by approximately 7.4 percentage points.

The estimated effect using the DID method suggests that the intervention increases access to bank loan credit by 10.3 percentage points. This is significant at a 1% level of statistical significance. However, this result may not be interpretable as a causal effect for a number of reasons. First, control and treatment groups may be contaminated with the effect of other lending activities. Second, the fact that the states are different in the two data sets. Therefore, the DID technique may not eliminate the influence of the unobservables in this particular case.

The analysis is re-done using the second sample, consisting of what we call a set of non-contaminated states.[12] The sample complements the first where all the states entered the analysis in respect of whether they participated in another programme or not. The estimation techniques follow the same methodology described above. Table A2.8 in the Appendix to this chapter presents the treatment assignment equation. The plot of the

[12] States that did not participate in other similar credit schemes, e.g., the BOI/State Matching Funds.

propensity scores for the treated and control groups reveals where the common support is in terms of the two kernel density graphs (see Figures A2.8 and A2.9 in the Appendix to this chapter). Figures A2.10 and A2.11 provide the histogram of propensity scores by treatment and control group for both years. Tables A2.9 and A2.8 provide evidence that the pre-intervention and post-intervention data meet the covariate balancing assumption. After the match, the t-test of differences in means reveals that there are no significant differences in means in the matched samples. The F-test yields similar results for the differences in sampling variances for the set of continuous variables, and the test for the balancing properties of the covariates reveals that an effective balancing is achieved (see Table A2.11).

We then used a restrictive sample to evaluate the effect of the intervention through the DID_2 as presented in Table 2.8. The result reveals that the coefficient of the average treatment effect is still positive and significant, yielding a slightly higher magnitude. In this case, the effect of the programme reveals an increase in bank loan take-up by 12.6 percentage points, still within the ‘ball park’ of estimates reported in Table 2.7 above. Therefore, the positive effect of the intervention on credit uptake appears robust to the exclusion of spill-over effects from other interventions. Nevertheless, we still have the problem that the DID approach did not remove any potential time-invariant unobservables.

Table 2.8: Average treatment effect of MSMEDF participation on a firm’s access to bank credit (Sample 2)

	Bank Credit
2010(ATT_0)	0.035***
	(0.018)
2014(ATT_1)	0.161***
	(0.025)
DID_2(ATT_1-ATT_0)	0.126***
	(0.030)
Sample 2010:	
Treated	667
Control	807
Sample 2014:	
Treated	705
Control	637

Bootstrapped standard errors with 200 replications in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author’s calculations using WBES data.

The problem identified earlier in the dataset having different states surveyed in the two years raises a potential issue of selection associated with the composition of the sample since a number of states surveyed in 2010 were not the same as those in 2014. In the absence of having a complete overlap of the states, the DID strategy may not effectively eliminate the effect of time-invariant unobservables. In order to address this problem, Sample 3 restricted the data to only those states that were surveyed in both years. This helps in cleaning out the effect of state specific unobservable confounders, because the states are the same. The estimates in Table 2.9 replicate the approach used in Tables 2.7 and 2.8 above. The relevant diagnostic checks from the logistic regression model and the balancing properties are presented in the Appendix to this chapter (see Tables A2.12 and A2.13). All the diagnostics tests confirm that the balancing property is satisfied for this reduced sample.

Table 2.9: Average treatment effect of MSMEDF participation on a firm's access to bank credit (Sample 3)

	Bank Credit
2010(ATT_0)	0.069***
	(0.026)
2014(ATT_1)	0.211***
	(0.036)
DID1(ATT_1-ATT_0)	0.142***
	(0.044)
Sample 2010:	
Treated	215
Control	455
Sample 2014:	
Treated	250
Control	469

Bootstrapped standard errors with 200 replications in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations using WBES data.

Table 2.9 reveals a broadly similar pattern of results. The estimated effect of the programme remains significant and exerts a positive effect on the firm's access to credit with an increase in loan take-up of about 14.2 percentage points; this is similar in magnitude to the estimates reported for Samples 1 and 2. Overall, the positive impact of the intervention on credit take-up appears robust to the three samples used in this analysis. This provides some reassurance that the intervention did have a positive effect on the outcome of interest here.

2.8 Discussion of Findings

The MSMEDF appears to have animated the interest of firms in participating in borrowing activities, and this is consistent with the claim that a reduction in interest rates will spur substantial borrowing activities. Before the introduction of the programme, the interest rate charged on loans was between 16.16% and 21.85% in 2012 (Central Bank of Nigeria, 2012). Therefore, the lower interest rate of 9% for the MSMEDF might well explain the increase in loan take-ups by firms.

One of the major reasons identified in the literature for the failure of similar credit programmes in Nigeria was the lack of awareness in the targeted group (Agusto and Co, 2015). However, another possible reason for the positive impact of this scheme might be the increase in awareness of the programme among firms in the states through the active involvement and participation of state governments. There are electoral gains from government-sponsored awareness among the firms in the states, and this could have increased the number of firms being aware of and participating in the programme.

An important issue of concern in evaluation studies is the violation of the stable unit treatment value assumption (SUTVA), which ensures ‘no spill-over’ effect. In this study, given that some untreated states share common boundaries with the treated states, the possibility that firms in the non-treated states took up loans from banks in the treated states could not be excluded. If this happened, the spill-over effect undermines the identification strategy. However, it is worth noting that given the one-year period governing our analysis, it is implausible to argue that non-treated firms could relocate to establish their businesses in the treated states. The stringent conditions in business start-ups in Nigeria mean that transition from one state to another might not be achieved seamlessly. This explanation is consistent with the World Bank report on the ease of doing business, with Nigeria ranking 147 out of 189 countries (World Bank, 2013). The ease of doing business indicators is measured by the procedures, time and costs incurred to complete a transaction given relevant regulations.

As is apparent from the design of the programme, state governments have a Special Purpose Vehicle (SPV) in place to monitor and ensure that the intervention fund is disbursed to the targeted group in the state. Moreover, most state governments are incentivised to use the success of the programme in their states to secure political advantage. In addition, the Central Bank of Nigeria has a robust supervisory framework with branch offices in each state of the Federation to monitor the development of the Bank's financial programmes. These measures suggest that SUTVA, that ensures there is no general equilibrium or 'spillover' effects associated with the treatment, may be satisfied in this case.

In an attempt to identify the causal effect of the MSMEDF, using our approach, we started with a full sample size (Sample 1). However, we encountered two problems. One was the problem of the control and treatment group that may have been contaminated with the effect of other lending activities competing concurrently with the MSMEDF. The second problem was that the states surveyed were not common across the two years. In this case, the difference-in-difference strategy cannot eliminate the effect of the unobservable confounders. To address these problems, we then moved to a reduced sample (Sample 2), where we cleaned out the control and treatment groups to have a group of non-contaminated states; however, the state specific unobservable confounders could not be eliminated using DID for the same reason as in Sample 1. Sample 3 was then constructed as our preferred sample, giving a clean sample that we were more confident satisfied the key requirements for this approach. Having the same states across the two years makes the two groups as similar as possible. The DID strategy was able to eliminate the effect of state specific unobservable confounders, since the states are common.

Although the DID technique may have eliminated the effect of state specific unobservable confounders, there is a potential problem of the unobservable at the level of the firms, since the firms are different. In this case, the DID may not have eliminated the effect of firm specific confounders. In a situation where the unobservable confounders at the firm level are highly correlated with the observable confounders, the effect of the firm unobservable must have been absorbed by these observables. In these circumstances, we would be more confident that the DID technique provides a causal effect. However, we do not have information if this is indeed the case here. Therefore, unobservable firm-level confounders could still be relevant here, weakening the case that the estimates are causal effects.

2.9 Conclusions

This research is the first study to our knowledge that has attempted to empirically examine the impact of the MSME credit scheme in Nigeria. Our approach exploits state variation in access to the MSMEDF to evaluate its impact on the incidence of firm loan take-up. The design of the intervention, which includes the participation of state governments and the fund's disbursement through participating financial institutions, provides a useful empirical test case. Although our study follows the methodology used in similar studies for other countries, our analysis differs regarding its research objectives. While other studies focus on the impact of credit schemes on the volume and availability of loans to firms and other performance measures, this study exploits short-medium term measures, the incidence of loan take-up opportunities as measured by participation in borrowing activities.

In order to investigate the impact of the intervention on access to credit, we used a cross-section of the World Bank Enterprise Survey data using the PSM technique combined with the DID method to minimise the influence of observable and unobservable confounders. Our results suggest that the introduction of the MSMEDF resulted in an increase in firm bank borrowing applications in the range of 10 to 14 percentage points. These estimates are robust to the exclusion of a variety of different firms from the sample, but the tests of statistical significance retain adequate power given all the estimated treatment effects are significant at the 1% level using two-tailed tests. The results, while more modest in magnitude, are in line with findings for the effect of SME schemes in other countries that have studied the effect of such financial interventions on the availability of credit and other performance measures. Most importantly, the empirical evidence contradicts the widespread perception in anecdotal accounts about the policy intervention reported in the print and broadcast media in Nigeria. At the policy level, the results suggest that effectively designed SME programmes might be sound strategies to increase SME participation in borrowing activities from formal financial institutions. As the first study to have attempted the evaluation of this scheme, the study provided insights into the areas where evaluation studies could be improved in Nigeria. There is the need to have a centralised repository of relevant data to encourage the evaluation of these intervention programmes in a systematic and coherent way, therefore building on the work undertaken in this chapter.

It is worth mentioning that, given the limitations of our dataset, the empirical strategies appear to have done an effective job in dealing with the effect of observable confounders in the analysis. Also, our preferred sample (Sample 3) presents us with the opportunity to eliminate state specific unobservable confounders, given that the states are common across the two states. However, because the firms are different over the two time periods, the DID could not eliminate the effect of unobservable confounders at the level of the firm. Therefore, unobservable firm-level confounders could still be relevant here. This highlights the need for the construction and maintenance of a nationally representative panel dataset of enterprises for Nigeria. Furthermore, given the limited number of states used in the analysis, our findings are only applicable to those states in our sample and cannot be generalised nationally across Nigeria. Therefore, the external validity of the empirical analysis undertaken is weak, although we have a greater degree of confidence in the internal validity of this exercise.

While this study revealed a positive effect of the scheme on access to credit by SMEs, little is known about the sectors driving this effect, and whether the programme attracted new firms into the credit market. More research is needed to unpack the effect of the programme across sectors and investigate the impact of new entrants in the market. A study to estimate the longer-term effect of the programme on other firm performance measures (e.g., sales) using more recent World Bank firm-level data provides an interesting topic for future research.

Appendix to Chapter 2

Table A2.1: Anecdotal Evidence from Print and Broadcast Media in Nigeria

Date	Newspaper and Author	Title	Reasons	Speakers
June 15, 2015	Vanguard: Providence Obuh	*Too many issues with N220bn MSMEDF*	75% collateral requirement for PFIs too high.	Acting Director, Central Bank of Nigeria.
October 25, 2015	The Guardian: Mathias Okwe	*Uncertainty over N220 CBN MSME Fund*	• Inability to provide seed capital by State governors. • The intervention fund frustrated by State governors.	• Anonymous sources within and outside CBN • Former Governor, Akwa-Ibom state
April 10, 2017	Thisday: Ndubuisi Frances	*Why CBN's N220bn MSMEs Devt Fund Recorded Low Patronage*	Stringent conditions attached to accessing the funds.	The Managing Director, Fortis Microfinance Bank
February 14, 2018	New Telegraph: Correspondents	*Low funds uptake hinders CBN's MSMEDF schemes*	• Interest rate too high. 5% interest rate ideal. • Low public awareness.	Vice president, Small and Medium Industries (SMI)
July 24, 2017	The Sun: Bimbola Oyesola	*How banks, politicians hijacked CBN's N220bn MSME intervention fund*	Banks are made to bear full credit risk for the facility.	The president, Nigerian Association of Small and Medium Enterprises.
August 5, 2018	Vanguard: Providence Emmanuel	*Why CBN's intervention funds are not making the desired impact*	Stringent criteria and bottlenecks.	Managing Director, Coastline microfinance bank
January 23, 2018	Premium Times: Micheal Eboh	*Investigation: How CBN's N220 billion fund for small businesses is shrouded in secrecy, 'malpractices.*	Politicians, State and bank officials subverting the programme and approval processes.	The Director-General, Lagos Chamber of Commerce and Industry.

Sources: Authors compilation from Nigerian Print Media

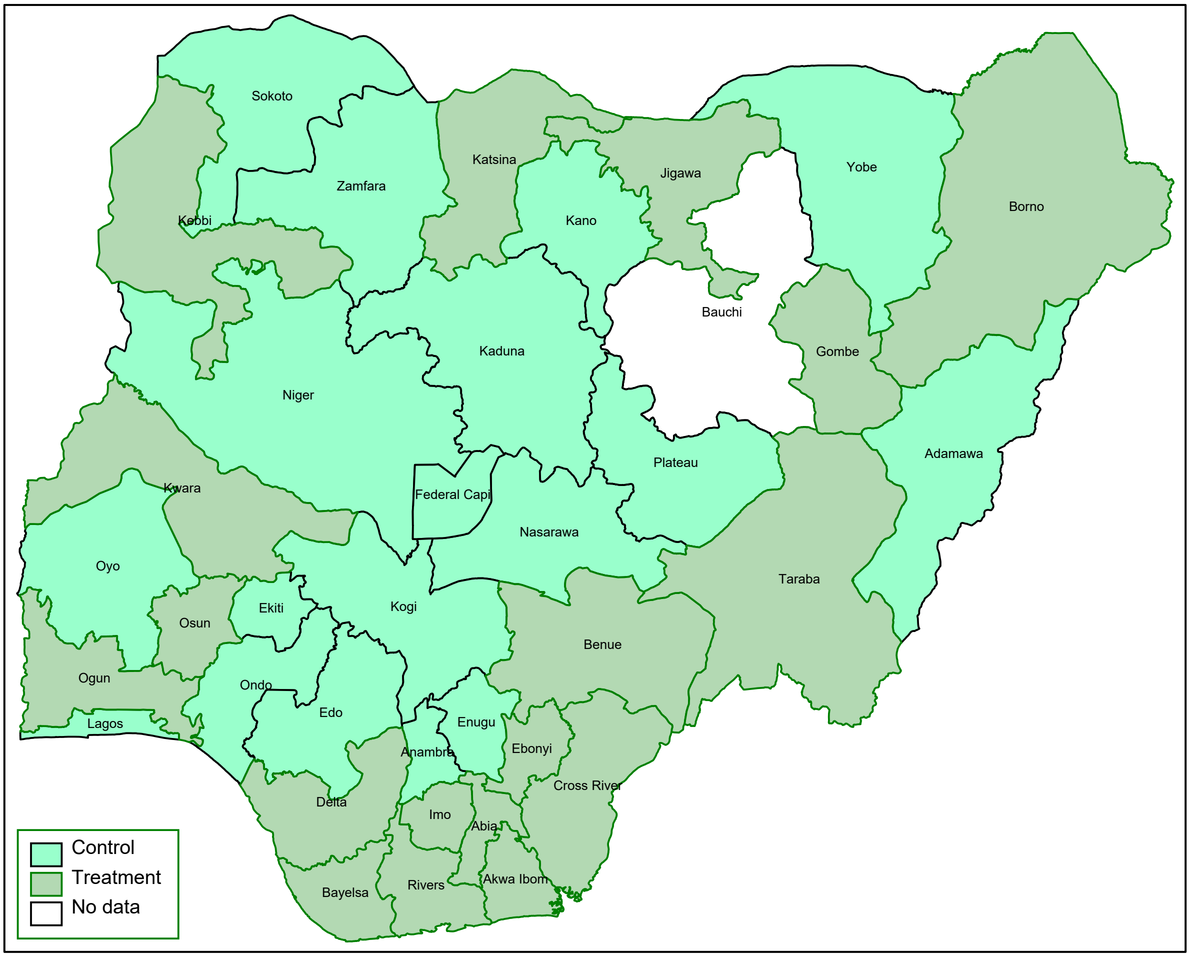

Figure A2.1: Programme Assignment by State

Note: The treatment consists of the treatment groups in the three samples (see Tables A2.2 and A2.3). The restrictive sample common across the two survey periods are Zamfara, Jigawa, Kebbi, Oyo, Nasarawa and Kastina. Bauchi State has no data across the two surveys.

Source: Authors calculation using MSMEDF and WBES data.

Table A2.2 States allocation to Treatment and Control Groups across the three samples (2010)

PRE- INTERVENTION PERIOD - 2010				
Sample 1	**Treatment**	**Control**	**MSMEDF States**	**States in other credit schemes**
Full sample of all the States that participated in MSMEDF without restriction to participation in other credit schemes.	Akwa Ibom Bayelsa Benue Borno Delta Gombe Jigawa Kebbi Kwara Ondo Osun Oyo Taraba Zamfara	Adamawa Eboyi Edo Ekiti Imo Kastina Kogi Nasarawa Niger Plateau Rivers Yobe	Akwa Ibom Bayelsa Benue Borno Delta Gombe Jigawa Kebbi Kwara Ondo Osun Oyo Taraba Zamfara	Niger Kogi Osun Edo Taraba Delta Kwara Ekiti Ondo Gombe Plateau Rivers
Sample 2	**Treatment**	**Control**		
Reduced sample of States restricting to participation in MSMEDF. Other Schemes eliminated.	Zamfara Jigawa Kebbi Akwa Ibom Bayelsa Benue Borno Oyo	Adamawa Ebonyi Imo Kastina Nasarawa Yobe		
Sample 3	**Treatment**	**Control**		
Restrictive sample of States that participated in MSMEDF and are common across the two survey periods.	Zamfara Jigawa Kebbi Oyo	Nasarawa Kastina		

Source: Author's compilation from CBN and BoI

Table A2.3 States allocation to Treatment and Control Groups across the three samples (2014)

POST- INTERVENTION PERIOD - 2014				
Sample 1	**Treatment**	**Control**	**MSMEDF States**	**States in other credit schemes**
Full sample of all the States that participated in MSMEDF without restriction to participation in other credit schemes.	Abia Cross River Enugu Oyo Gombe Jigawa Kebbi Kwara Sokoto Zamfara	Abuja Anambra Kaduna Kano Lagos Kastina Nasarawa Ogun Niger	Abia Cross River Enugu Bauchi Oyo Gombe Jigawa Kebbi Kwara Sokoto Zamfara	Anambra Niger Kaduna Kano Kwara Gombe Enugu Cross River
Sample 2	**Treatment**	**Control**		
Reduced sample of States restricting to participation in MSMEDF. Other Schemes eliminated.	Zamfara Jigawa Kebbi Sokoto Oyo	Nasarawa Kastina Abuja Lagos		
Sample 3	**Treatment**	**Control**		
Restrictive sample of onlt States that participated in MSMEDF and are common across the two survey periods.	Zamfara Jigawa Kebbi Oyo	Nasarawa Kastina		

Source: Author's compilation from CBN and BoI

Table A2.4: Description of Variables

Variable Name	Variable Description
bank_credit	A dummy variable that equals 1 if the firm secured credit from private commercial banks, state banks and microfinance banks, 0 otherwise
treat_state	A dummy variable equals 1 if the firm is located in the states that participated in the MSMED Funds, and zero otherwise
empsize	The total number of individuals working (includes temporary and permanent workers)
lnsales	Logarithm of total sales
age	The age of the firm, given by the current year, less the year in which the firm started to operate for the first time
sole_prop	A dummy variable that equals 1 if the firm is a sole proprietorship, and 0 otherwise
partnership	A dummy variable that equals 1 if the firm is a partnership company, and 0 otherwise
ltd_comp	A dummy variable that equals 1 if the firm is a limited company, and 0 otherwise
manufactur~g	A dummy variable that equals 1 if the firm is in the manufacturing sector, and 0 otherwise
retail	A dummy variable that equals 1 if the firm is in a retail sector, and 0 otherwise
services	A dummy variable that equals 1 if the firm is in the services sector, and 0 otherwise
intcert	A dummy variable that equals 1 if the firm has an international certification, and 0 otherwise
small	A dummy variable that equals 1 if the firm is a small firm (<=19), and 0 otherwise
medium	A dummy variable that equals 1 if the firm is a medium firm (>=20 and <=99), and 0 otherwise
large	A dummy variable that equals 1 if the firm is a medium firm (>=100), and 0 otherwise
fixasset	A dummy if the firm has a fixed asset that may be accepted as collateral for a bank loan, 0 otherwise
audit	A dummy variable that equals 1 if the firm has its financial statement audited by an external auditor, and 0 otherwise
expzone	A dummy variable that equals 1 if the firm is located in an export processing zone, and 0 otherwise
gender	A dummy variable that equals 1 if the top manager is a male, and 0 otherwise
noeduc	A dummy variable that equals 1 if the top manager has no education, and 0 otherwise
voceduc	A dummy variable that equals 1 if the top manager has a vocational education, and 0 otherwise
unieduc	A dummy variable that equals 1 if the top manager has a university education, and 0 otherwise
exper	The total number of years of experience of the top manager

Source: Author’s definition using WBES data.

Table A2.5: Summary statistics of outcome variable (Bank Credit) in treatment and control groups for large firms

Samples	Treatment	Control	Differences t-test
Sample 1			
2010	0.5714	0. 7895	0.2180
	(0.1373)	(0.0670)	(0.1380)
Sample Size	**14**	**38**	
2014	0.5152	0.4324	-0.0827
	(0.0883)	(0.0826)	(0.1209)
Sample Size	**33**	**37**	
Sample 2			
2010	0.7576	0. 6875	0.7347
	(0.0758)	(0.1197)	(0.0637)
Sample Size	**14**	**38**	
2014	0.3793	0.5591	0.1797
	(0.0917)	(0.0442)	(0.1024)
Sample Size	29	37	
Sample 3			
2010	0.7826	0. 5000	0.7241
	(0.0879)	(0.2236)	(0.0844)
Sample Size	**23**	**6**	
2014	0.3793	0.2778	-0.0827
	(0.0917)	(0.1086)	(0.1209)
Sample Size	**29**	**37**	

Source: Author’s calculations using WBES data.

Table A2.6: Logit Estimation – Access to loan model

Variables	1 Impact Effect	2 Marginal/ Impact effect	3 Marginal/Impact effect
Sample 1			
treatment dummy	0.072***	0.072***	0.080***
	(0.019)	(0.019)	(0.020)
employment size		0.000	0.000
		(0.000)	(0.000)
sales(log)			0.008***
			(0.003)
other variables	YES	YES	YES
Pseudo R	0.049	0.049	0.053
Observations	**2,496**	**2,496**	**2,496**
Sample 2			
treatment dummy	0.159***	0.158***	0.176***
	(0.023)	(0.023)	(0.026)
employment size		-0.000	-0.000
		(0.001)	(0.001)
sales(log)			0.010***
			(0.005)
other variables	YES	YES	YES
Pseudo R	0.091	0.091	0.098
Observations	**1,347**	**1,347**	**1,347**
Sample 3			
treatment dummy	0.198***	0.199***	0.237***
	(0.029)	(0.029)	(0.031)
employment size		-0.001	-0.001
		(0.001)	(0.001)
sales(log)			0.013***
			(0.007)
other variables	YES	YES	YES
Pseudo R	0.145	0.146	0.172
Observations	**731**	**731**	**731**

*** p<0.01, ** p<0.05, * p<0.1. Dependent variable is the access to bank credit variable (impact effect). Employment size (marginal effect); sales (marginal effect). Other control variables include: age; partnership; limited liability companies; retail trade; services sector; firms with international certification; medium firms; fix asset; audited financial statement; export zone; educational level, gender and experience of the manager.

Source: Author's calculations using WBES data.

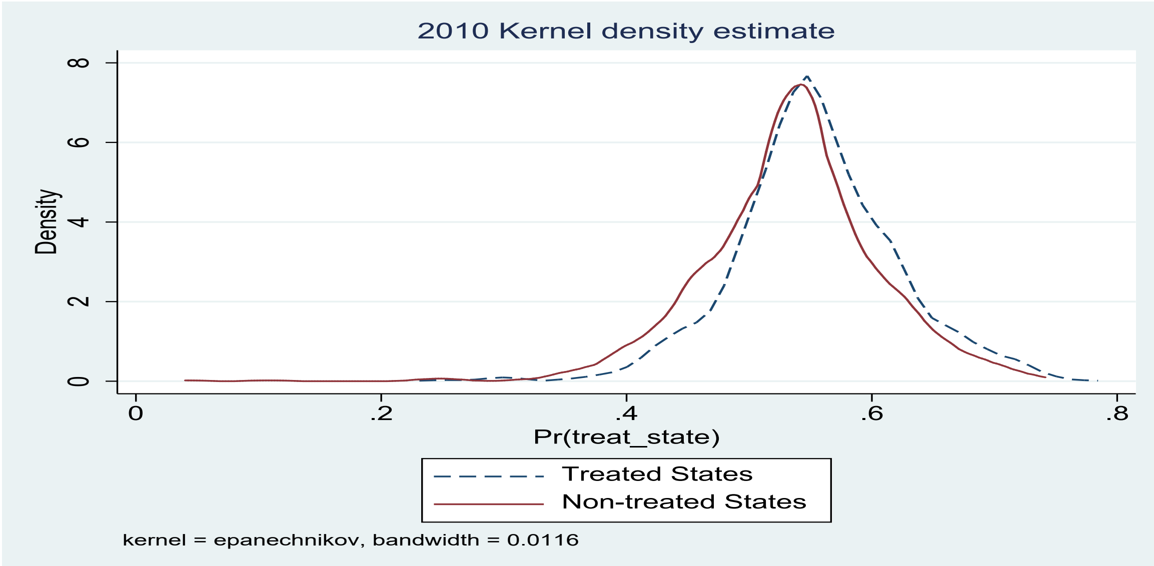

Figure A2.2: Kernel Density of Propensity Scores for the pre intervention years (Sample 1)

Source: Author's calculations using WBES data.

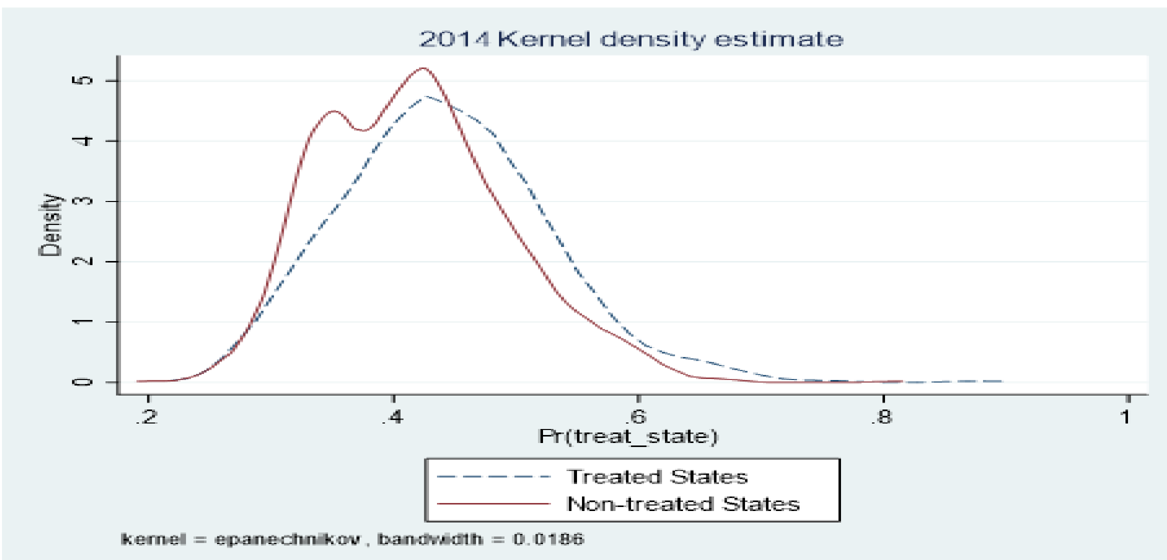

Figure A2.3: Kernel Density of Propensity Scores for the post intervention years (Sample 1)

Source: Author's calculations using WBES data.

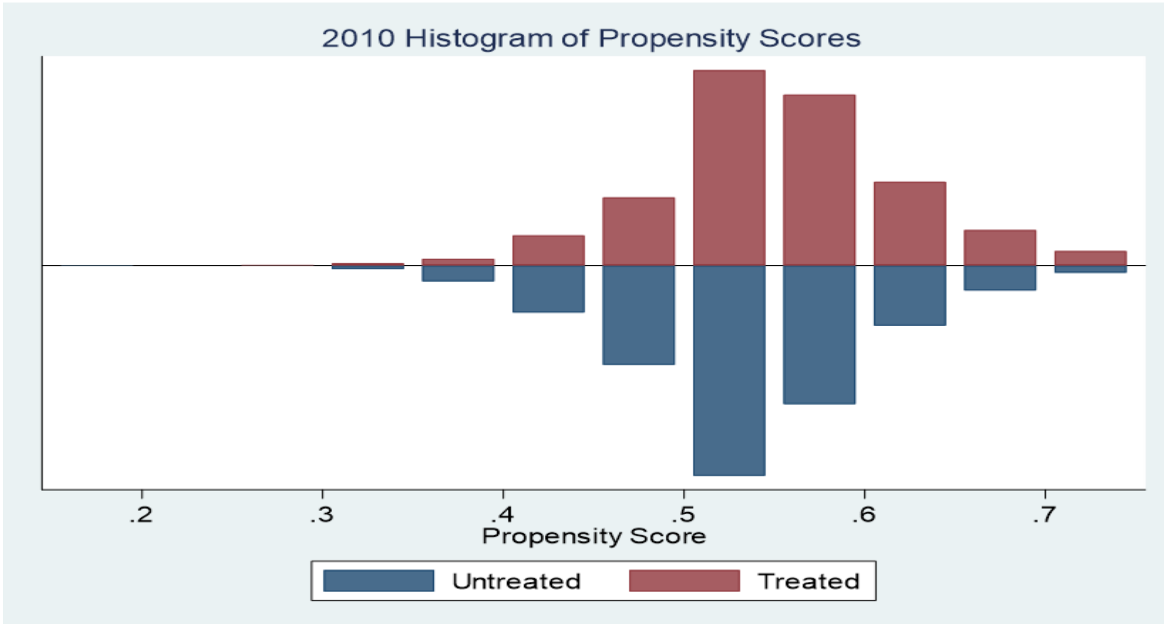

Figure A2.4: Histogram of Propensity Scores by Treatment and Control for pre intervention periods (Sample 1)

Source: Author's calculations using WBES data.

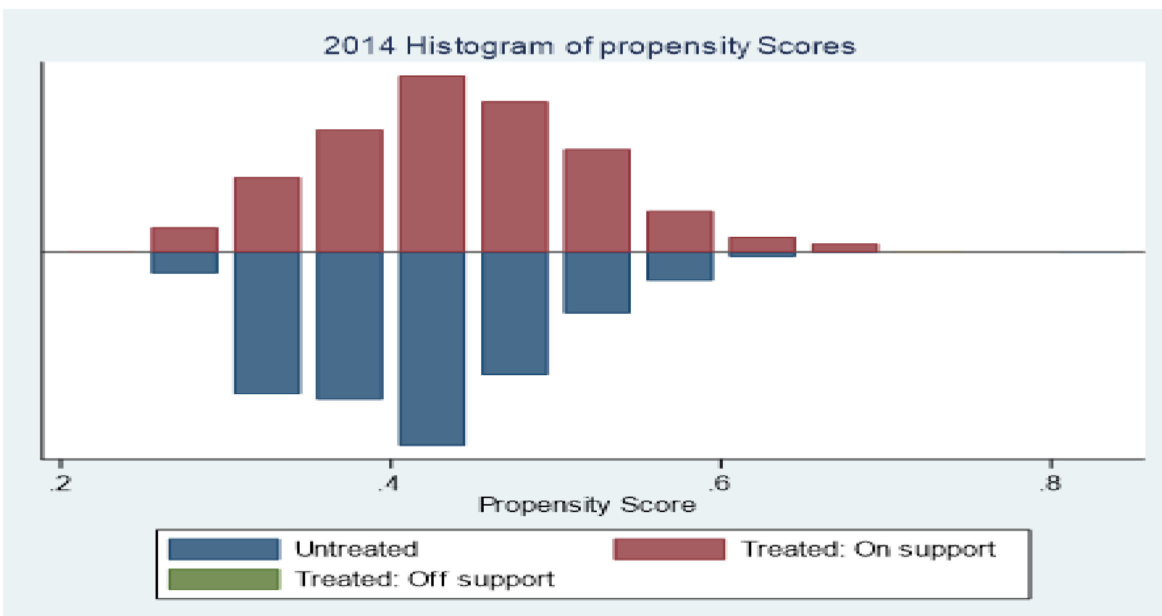

Figure A2.5: Histogram of Propensity Scores by Treatment and Control for post intervention periods (Sample 1)

Source: Author's calculations using WBES data.

Table A2.7: Common Support of Propensity Scores in Pre and Post Intervention period.

Common Support	Sample 1	Sample 2	Sample 3
2010			
0	8	14	16
1	2924	1474	671
2014			
0	3	5	12
1	2493	1342	719
Sample Size:			
2010	2932	1488	687
2014	2496	1347	731

Source: Author's calculations using WBES data.

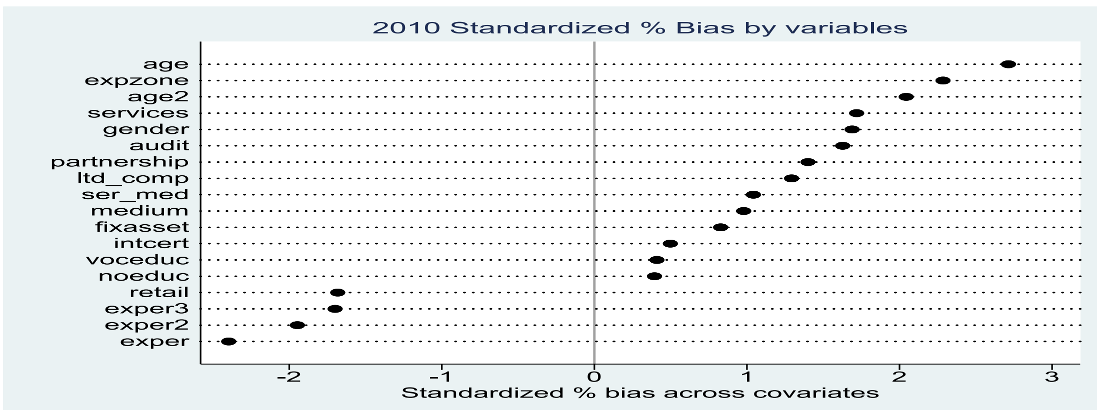

Figure A2.6: Pre-intervention Standardized % Bias (After Matching) by Variables (Sample 1)

Source: Author's calculations using WBES data.

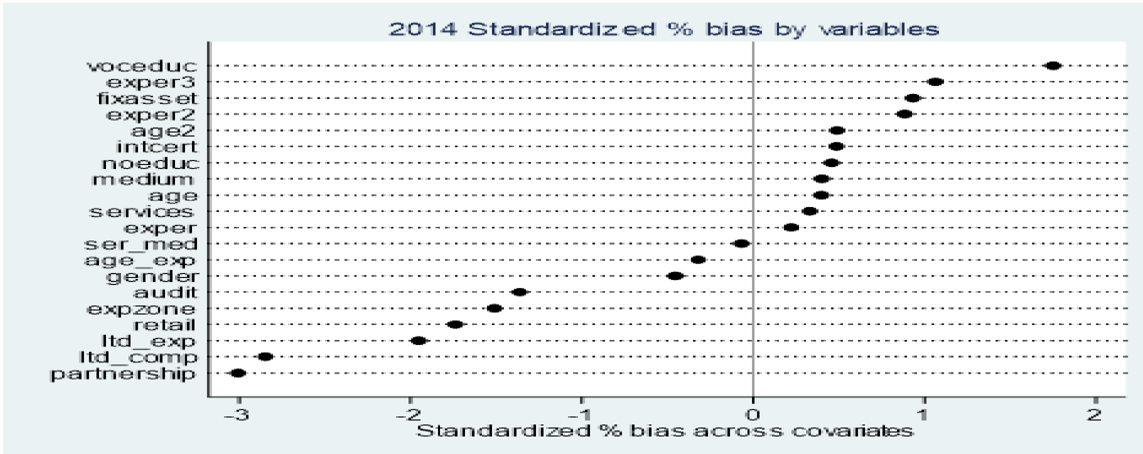

Figure A2.7: Post-Intervention Standardized % Bias (After Matching) by Variables (Sample 1)

Source: Author's calculations using WBES data.

Table A2.8: Pre- and Post-Intervention Logit Model Regression Result – Sample 2

Variables	2010 Treated_States	2014 Treated_States
Retail	0.290*	0.087
	(0.169)	(0.156)
Intcert	-0.303	0.591**
	(0.281)	(0.238)
Fixasset	0.190	0.453***
	(0.118)	(0.119)
Expzone	-0.919**	0.280
	(0.407)	(0.272)
Gender	-0.448***	0.023
	(0.166)	(0.184)
Voceduc	0.186	0.379**
	(0.166)	(0.164)
Exper	0.048**	-0.044**
	(0.024)	(0.019)
exper2	-0.002**	0.001**
	(0.001)	(0.000)
med_exp	-0.034**	-0.015
	(0.016)	(0.012)
Constant	-0.136	0.191
	(0.204)	(0.189)
Observations	1,488	1,347

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

Source: Author's calculations using WBES data.

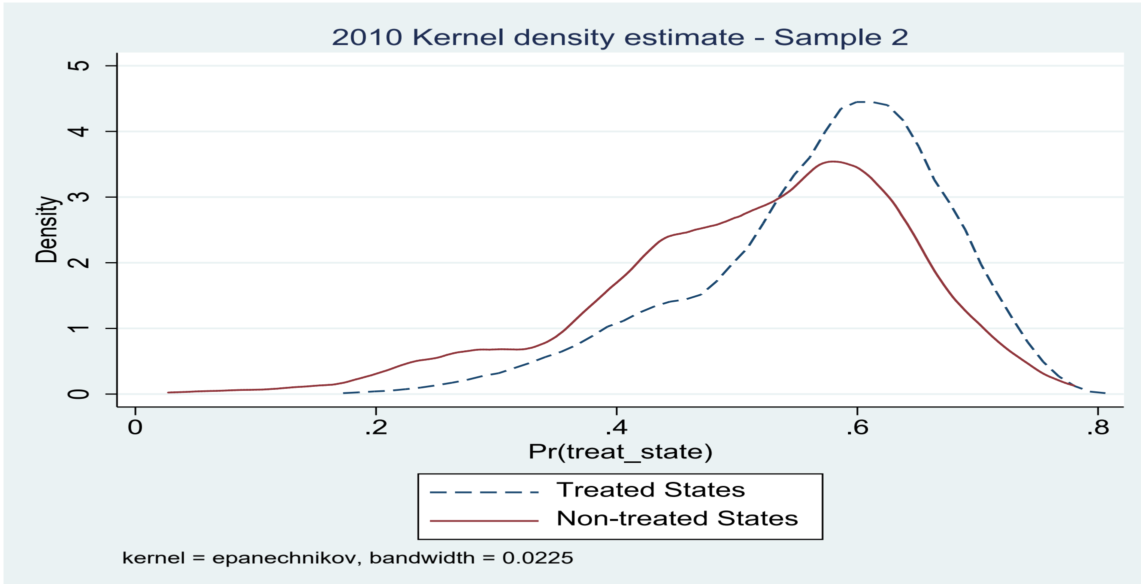

Figure A2.8: Kernel Density of Propensity Scores for the pre intervention years (Sample 2)

Source: Author’s calculations using WBES data.

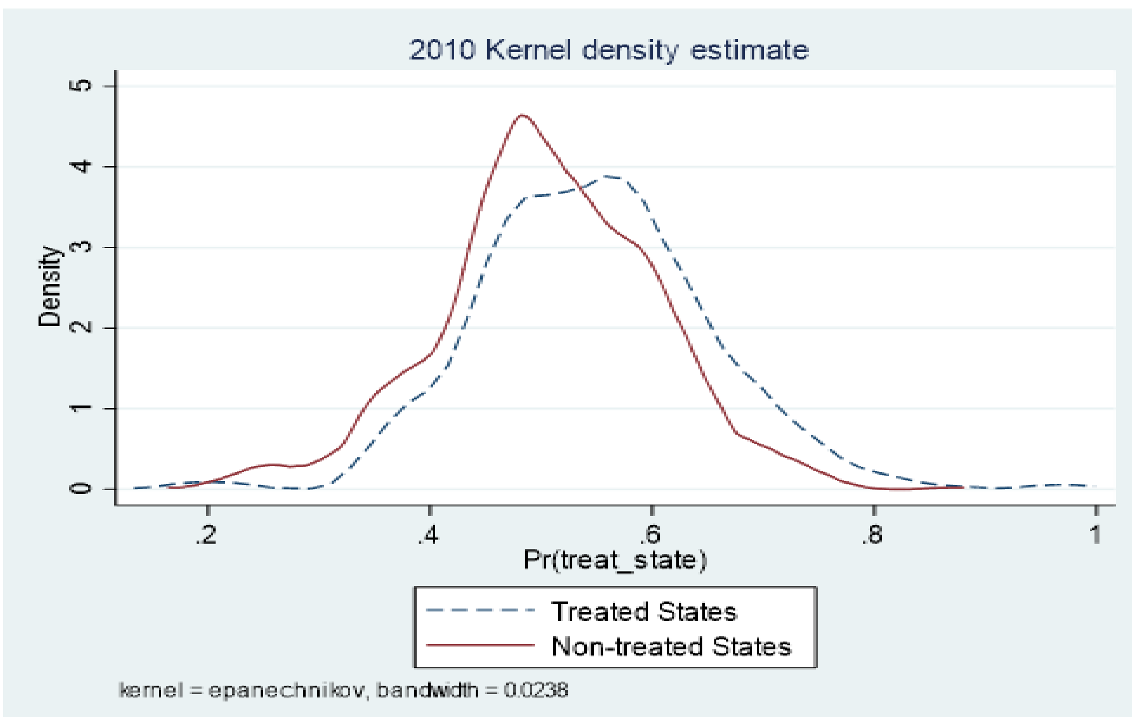

Figure A2.9: Kernel Density of Propensity Scores for the post intervention years (Sample 2)

Source: Author’s calculations using WBES data.

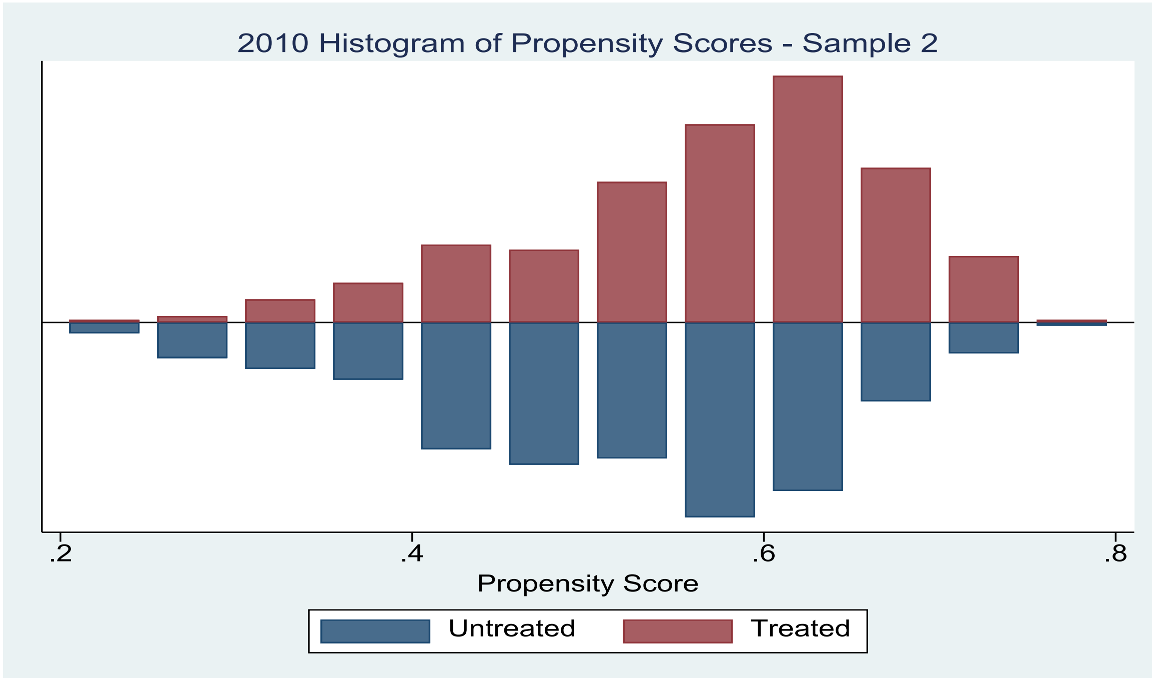

Figure A2.10: Histogram of Propensity Scores by Treatment and Control for pre intervention periods (Sample 2)

Source: Author's calculations using WBES data.

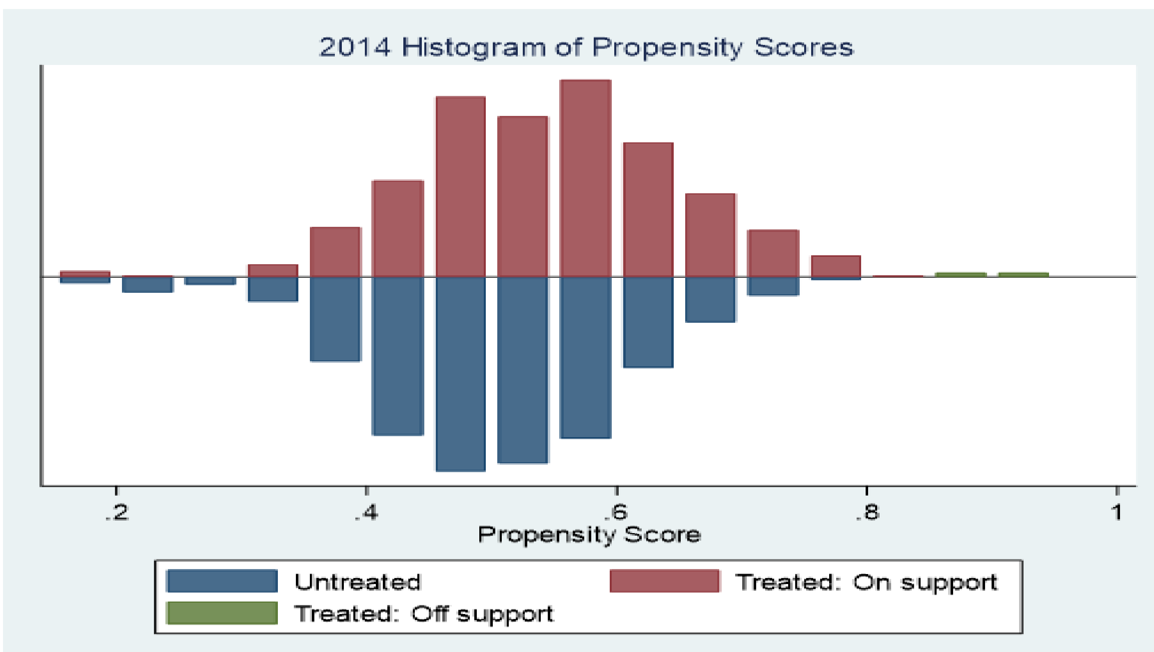

Figure A2.11: Histogram of Propensity Scores by Treatment and Control for post intervention periods (Sample 2)

Source: Author's calculations using WBES data.

Table A2.9: 2010 covariates balancing check by mean and variance differences-Sample 2

Variable	Mean Treated	Control	t-test %bias	t	p>t	V(T)/ V(C)
age	13.073	12.776	3.2	0.66	0.511	1.05
age2	255.32	243.38	3.0	0.63	0.528	0.91
partnership	0.03717	0.03657	0.3	0.06	0.948	.
ltd_comp	0.09046	0.08978	0.2	0.05	0.962	.
retail	0.2119	0.21016	0.4	0.09	0.932	.
services	0.28872	0.29294	-0.9	-0.19	0.852	.
medium	0.22429	0.23027	-1.3	-0.29	0.775	.
intcert	0.05328	0.05102	1.0	0.2	0.838	.
audit	0.1487	0.15111	-0.6	-0.14	0.892	.
fixasset	0.43494	0.44177	-1.4	-0.28	0.782	.
expzone	0.01735	0.01679	0.3	0.09	0.931	.
noeduc	0.04585	0.03819	3.9	0.77	0.443	.
voceduc	0.22057	0.21597	1.2	0.22	0.823	.
gender	0.13135	0.12555	1.7	0.35	0.728	.
exper	11.668	11.649	0.3	0.05	0.959	0.98
exper2	187.19	187.96	-0.3	-0.06	0.949	0.92
aud_part	0.00248	0.00286	-0.7	-0.15	0.881	.
ltd_int	0.01859	0.01764	0.7	0.14	0.887	.
expz_ltd	0.00496	0.00401	1.1	0.29	0.775	.
gen_ltd	0.0062	0.00661	-0.4	-0.1	0.918	.
med_exp	2.4919	2.5834	-1.4	-0.32	0.752	1.02

* if variance ratio outside [0.87; 1.15]

Source: Author’s calculations using WBES data.

Table A2.10: 2014 covariates balancing check by mean and variance differences

Variable	Mean Treated	Control	%bias	t-test t	p>t	V(T)/ V(C)
age	14.804	14.777	0.3	0.05	0.961	1.01
age2	328.57	327.09	0.3	0.05	0.957	1.01
partnership	0.03004	0.03311	-1.7	-0.33	0.743	.
ltd_comp	0.12732	0.142	-4.2	-0.8	0.422	.
retail	0.17883	0.19088	-3.1	-0.58	0.562	.
services	0.33619	0.32321	2.8	0.52	0.606	.
medium	0.402	0.412	-2.0	-0.38	0.704	.
intcert	0.09156	0.0868	1.7	0.31	0.755	.
audit	0.21602	0.22539	-2.2	-0.42	0.673	.
fixasset	0.46495	0.4601	1.0	0.18	0.856	.
expzone	0.06724	0.06	3.0	0.55	0.579	.
noeduc	0.00286	0.00129	2.0	0.64	0.52	.
voceduc	0.16595	0.16533	0.2	0.03	0.975	.
gender	0.10873	0.11194	-1.0	-0.19	0.848	.
exper	12.311	12.266	0.5	0.09	0.928	1.01
exper2	238.75	236.63	0.6	0.12	0.908	1.04
aud_part	0.01144	0.01211	-0.6	-0.11	0.909	.
ltd_int	0.00858	0.00953	-0.7	-0.19	0.852	.
expz_ltd	0.01288	0.01326	-0.3	-0.06	0.949	.
med_exp	5.201	5.4085	-2.3	-0.42	0.672	0.92

* if variance ratio outside [0.86; 1.16]

Source: Author's calculations using WBES data.

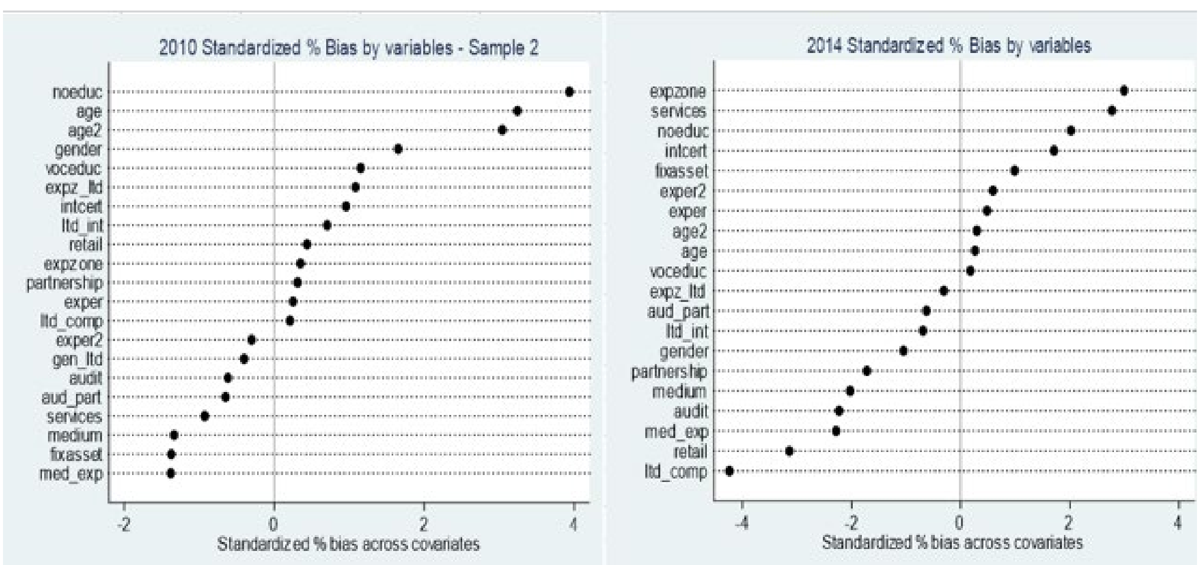

Figure A2.12: Standardized % Bias (After Matching) by Variables (Sample 2)

Source: Author's calculations using WBES data.

Table A2.11: Pre- and Post-intervention balancing property diagnostics

Year	Ps R2	LR chi2	p>chi2	Mean Bias	Med Bias	B	R	%Var
2010:unmatched	0.035	71.26	0.000	8.3	6.3	44.6*	0.79	80
Matched	0.001	2.37	1.000	1.2	0.9	7.7	1.28	0
2014:unmatched	0.034	63.44	0.000	6.6	5.5	43.7*	1.21	80
Matched	0.001	2.72	1.000	1.5	1.4	8.8	1.37	0

* if B>25%, R outside [0.5; 2]

Source: Author's calculations using WBES data (2010 and 2014).

Table A2.12: Pre- and Post-Intervention Logit Model Regression Result – Sample 3

	2010	2014
VARIABLES	treated_State	treated_State
ltd_comp	-1.431***	0.088
	(0.551)	(0.240)
Retail	0.113	0.519**
	(0.301)	(0.234)
Medium	-0.261	-0.401**
	(0.386)	(0.170)
Intcert	0.606	1.411***
	(0.411)	(0.397)
Audit	-1.322***	0.132
	(0.477)	(0.227)
Fixasset	-0.097	0.632***
	(0.208)	(0.175)
Exper	0.002	-0.084***
	(0.040)	(0.031)
exper2	-0.002	0.002**
	(0.001)	(0.001)
age_med	-0.077***	
	(0.025)	
ltd_ser	1.646**	
	(0.648)	
ltd_med	2.366***	
	(0.640)	
Constant	0.988***	0.920***
	(0.345)	(0.277)
Observations	687	731

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

Source: Author's calculations using WBES data (2010 and 2014).

Table A2.13: Pre- and Post-intervention balancing property diagnostics (The same states)

Year	*Ps R2*	*LR chi2*	*p>chi2*	*Mean Bias*	*Med Bias*	*B*	*R*	*%Var*
2010:								
unmatched	0.106	91.29	0.00	10.4	7.3	78.9*	0.49*	20
Matched	0.002	8.28	0.99	2.8	2.5	19.0	1.54	0
2014:								
unmatched	0.065	59.73	0.00	8.8	5.9	62.3*	1.32	14
Matched	0.002	15.88	0.67	4.0	3.3	9.3	1.15	0

* if B>25%, R outside [0.5; 2]

Source: Author's calculations using WBES data.

Chapter Three - Is there a Gender Dimension to Small and Medium Enterprise Credit Market Participation and Loan Success in Nigeria? (Essay 2)

3.1 Introduction

Finance is a crucial element in the growth and survival of enterprises, particularly small and young firms (Beck *et al*., 2006). In contrast to large enterprises with access to credit through capital markets, bank lending is the primary source of external finance for Small- and Medium-Sized Enterprises (SMEs) (Berger and Udell, 2002). Stiglitz and Weiss (1981) demonstrate that asymmetric information generates agency problems of adverse selection and moral hazard, leading to credit rationing in the credit market. The imperfection of information between lenders and borrowers forces banks to rely on the observable attributes of firms when screening borrowers for their creditworthiness. Among the attributes of a firm that lenders may use to inform creditworthiness or risk is gender. A growing literature reveals that the gender of the owner plays a crucial role in securing (or otherwise) access to credit, with female-owned firms argued to be more credit constrained than male-owned firms (Muravyev *et al*., 2009; de Mel *et al.*, 2009). Alternative explanations have been proposed in the literature to explain the observed gaps in the credit market.

Gender discrimination may result from taste-based discrimination (Becker, 2010). This is related to the lender's preferences and cultural beliefs about the demographic characteristics of the borrower. On the other hand, unequal treatment may result from statistical discrimination (Arrow, 1973) due to information asymmetry. Lenders may not have sufficient information on female-owned firms' quality, solvency, and creditworthiness, and they may therefore be perceived as riskier than their male counterparts (Aristei and Gallo, 2016), even when they have similar attributes. However, there is mixed evidence of gender differences in access to finance (see Klapper and Parker (2011) for a detailed review).

The relative finance gap in MSMEs worldwide is a major concern for policy-makers. The International Finance Corporation (IFC) estimated that there are approximately 9.34 million formal female-owned MSMEs globally, with 24% of such MSMEs located in Sub-Saharan

Africa (International Finance Corporation, 2014). They defined the total MSME finance gap for women as the difference between current supply and potential demand, which financial institutions can potentially address (International Finance Corporation, 2017). The MSME finance gap is estimated to be approximately US$1.7 trillion globally. In a sample of micro-enterprises' finance gap, estimates ranged from US$103 billion (37%) in East Asia to US$16 billion (29%) in the Middle East and North Africa for female-owned businesses. For SMEs, the comparable gaps were US$1.2 trillion (59%) in East Asia, and US$23 billion (8%) in South Asia. Sub-Saharan Africans have the second-highest proportion for the female SME finance gap of US$42 billion, representing 17% of the estimated financial gender gap. In order to place these numbers in context, there are 23 million women entrepreneurs, accounting for 41% of business owners in the MSME sub-sector in Nigeria (PWC, 2020a). The SME finance gap in Nigeria is estimated at US$92 billion, with the female-owned SME finance gap estimated at US$18 billion (International Finance Corporation, 2018).

Government interventions in the credit market potentially provide an effective policy intervention strategy to address the gap. In Nigeria, SME financing has been at the forefront of the country's development agenda. Two categories of financial institutions actively involved in credit delivery in Nigeria are private sector-led and public sector-led institutions (Mordi *et al*., 2014). The private sector-led institutions comprise the Deposit Money Banks (DMB) in conjunction with initiatives such as the Enterprises Equity Investment Scheme (SMEEIS), the Access Bank/IFC gender empowerment programme, microfinance institutions, and private entities such as the Dangote Foundation/BoI fund. Public sector-led institutions include various government intervention schemes and institutions supporting SMEs' credit delivery. Currently, there is an array of government programmes aimed at closing the financial gap in the MSME sub-sector. The primary objectives of these schemes are similar (i.e., to increase credit availability to SMEs), although not all schemes, both present and past, had a specific objective of financing female-owned firms in Nigeria. On this basis, the gender finance gap in Nigeria merits further research attention, particularly in the context of supply-side factors related to potentially discriminatory lending practices, and demand-side factors based on women not having the skills for a loan application that may lead to rejection.

One particularly interesting and under-researched area is the study of access to credit, conditional on participation in the credit market. Most existing research studies explore credit access as an independent and distinct concept from participation. While access to finance is vital for facilitating new business start-ups, funding business investments, and ensuring firm growth, not all enterprises in the credit market secure access to credit. Access to credit is defined as meaning that a firm is eligible to borrow given the availability of credit and has been successful in their borrowing activity. On the other hand, credit participation is defined here to mean an eligible firm has decided to participate in borrowing activities and has applied for a line of credit. Therefore, this definition is invariant to whether or not the loan is approved or rejected. In this study, access to credit is dependent on participation. The first phase is whether or not firms participate in a formal credit market, and the second phase is whether or not they are successful conditional on their initial participation. We then exploit the distinction between credit participation and access to credit to identify firms that applied for credit (i.e., loan application) and were successful in their loan application (i.e., loan success) to examine the gender dimension.

Although there is a considerable empirical literature that considers many aspects of access to credit, there is much less focus and evidence on the impact of gender on access to credit conditional on participation. The current study contributes to this gap in the literature by empirically investigating whether gender differences exist in (i) loan applications and (ii) successfully securing credit in Nigeria. One area of policy intervention necessary to boost female-owned firms' access to credit is understanding the factors that influence a firm's participation in credit markets, the factors that lenders use in screening borrowers, and whether a gender differential exists in either of these processes. The insights provided by this research may help guide an effective policy intervention designed to address gender disadvantage in access to credit.

This study contributes to the empirical analysis of unequal treatment in credit markets for SMEs in Nigeria. Adopting financial services' indicators, we investigate gender differences in credit market participation and loan success, where access to credit (i.e., loan success) determines whether a firm successfully acquired credit. The definition of access to credit in our study differs from other studies that have distinguished between participation and access to credit, as some of these studies define access to credit based on rejection or loan denial: we define access to credit based on approval or success. In contrast to a previous study for Nigeria (Nwosu *et al.*, 2015), our study benefited from more recent data, the 2014 World Bank Enterprises Survey. This survey provides detailed information on a firm's gender structure based on the critical role women play in both ownership and management. We consider alternative gender definitions, identifying firms with 100% female ownership, majority ownership, minority ownership and female managed firms.

Furthermore, motivated by our findings in an earlier empirical chapter, we include a crucial policy variable, MSMEDF, in the analysis. This can be used by decision-makers to direct efforts towards improving loan applications and the success of female entrepreneurs in government credit schemes. We also contribute methodologically to the existing literature by proposing a bivariate probit model to deal with the potential problem of selection bias when the demand for credit may jointly determine the probability of borrowing from a formal financial institution. To our knowledge, this study is the first to adopt a bivariate probit model with partial observability in investigating a gender effect in credit market participation and loan success for SMEs in Nigeria.

The remainder of the paper is organised as follows. Section 3.2 discusses the contextual framework of government interventions in the credit market in Nigeria, with a focus on their key lending and outcome objectives. Section 3.3 provides an account of the literature on the finance gender gap, different definitions of credit access, and gender compositions. Section 3.4 details the dataset and variables used, particularly regarding gender and access to credit variables. Section 3.5 discusses the econometric methodology. Section 3.6 provides the econometric results and discusses the regression analysis findings with their potential policy implications. Section 3.7 presents a summary of the key findings, policy implications, and some conclusions.

3.2 Context

3.2.1 Small and Medium Enterprise Interventions in the Credit Market in Nigeria

In order to facilitate the flow of funds to SMEs, the Nigerian government implemented various strategies, including specific interventions and bilateral arrangements, and developed various institutions and programmes.

Government intervention in the credit market in Nigeria began with the establishment of the Nigerian Industrial Development Bank (NIDB) in 1964. Although the bank’s primary aim was to provide medium-term to long-term loans to large industries, it also had a particular unit focused on SME finance. The NIDB was merged with the Nigerian Bank for Commerce and Industry and Family Economic Advancement Programme (FEAP) in 2001 to form the Bank of Industry (BoI). The bank has been restructured to manage some specialised development funds of private individuals, the Central Bank of Nigeria (CBN) and the Federal Government of Nigeria, and targeted selected economic sectors and groups. Of all the loans disbursed by the bank, 96% went to SMEs (Mordi *et al.*, 2014).

In 1971, the Federal Military Government established the Small Industries Development Programme to provide technical and financial support for SMEs, and this led to the Small Industries Credit Scheme (SSICS). The scheme's main objective was to provide loans on liberal terms to SMEs. However, the scheme's success was hampered by a lack of management workforce to supervise and monitor projects. Many unviable projects were therefore funded, leading to significant repayment defaults. In 1984 and 1988, the Federal government expanded its credit allocation to SMEs through two loan schemes known as SME I and SME II. The main objective of the SME I loan scheme was to train the workforce and develop an efficient institutional structure capable of providing technical services and credit to the SME sector.

The Small and Medium Enterprises Equity Investment Scheme (SMEEIS) was launched in 2001 to address the long-term capital needs of SMEs. It required banks to set aside a portion of their profit for equity investment for SMEs.

The programme's main objective was to complement the Federal government's efforts to stimulate economic growth, develop local technology, and generate employment. In 2010, the CBN instituted the N200 billion Small and Medium Scale Enterprises Guarantee Scheme (SMECGS). The programme's objective was to fast-track the growth of the country's manufacturing and industrial sector by providing guarantees for loans to SMEs.

To achieve this objective, the plan aimed to: (i) stimulate industrialisation through the expansion of access to credit and the introduction of value-added tax; (ii) increase output and employment through the diversification of the government's revenue base and sustainable input into the industrial sector; and (iii) improve the living standards of the people.

Although the Nigerian government established several schemes and programmes targeted at promoting access to credit in the economy, Enhancing Financial Innovation and Access (EFInA) reported in 2010 that 39.2 million Nigerian adults, representing about 46.3% of the Nigerian adult population, did not have access to formal financial services (EFInA, 2010). In particular, financial exclusion stands at 36% for women and 24% for men. Against this backdrop, the National Financial Inclusion Strategy (NFIS) was developed in 2012 to boost financial inclusion to 80% of the adult population and reduce financial exclusion to 20% by 2020.

The CBN set up primary tools for driving the strategy to achieve the financial inclusion target. Among the tools were credit enhancement programmes, such as the Micro, Small and Medium Enterprises Development Fund (MSMEDF) discussed in the previous chapter, established in 2013. The scheme's main objective was to enhance MSMEs' access to financial services, increase productivity and output levels of micro-enterprises, create jobs, and engender inclusive growth.

There is an additional point worth noting regarding this scheme. The scheme allocated 60% of the fund to female entrepreneurs, distinguishing the objective of this scheme from others. The primary implication is that the allocation of 60% of the fund to women was anticipated to increase the availability of credit for female entrepreneurs in Nigeria to address the significant problem of gender in access to credit. This scheme became the first intervention scheme in Nigeria to allocate a certain percentage of its funds to female entrepreneurs.

3.3 Literature Review

There have been many empirical studies on the various factors that determine the level of access to credit. These include the firm's attributes and owners, profitability, and the number of employees. In the case of the firm's attributes, the age and size of the borrowing firm, among other factors, play an essential role in gaining access to credit. Beck *et al*. (2006) find some support for the firm's size, age, and ownership to predict financing constraints. Coluzzi *et al*. (2015) investigate the determinants of financing obstacles in five European Union countries, and report that age, size, sector and level of sales (as a proxy measure of firm profitability) are significant determinants.

Another strand of literature emphasises the owners of the firms as a significant driver of access to credit. Specifically, an entrepreneur's characteristics play a statistically significant role in accessing finance within a weak institutional environment (Wellalage and Locke, 2017). Various studies reveal that the experience/skills of the manager/owner (Quartey *et al.*, 2017) and the gender and educational level of the entrepreneur (Nguyen *et al.*, 2019; Mascia and Rossi, 2017) play an important role in accessing credit. Among the determinants of access to credit, gender has received attention, with contrasting findings on which groups (i.e., female or male entrepreneurs) are more financially constrained. On the one hand, evidence shows that female-owned SMEs face more significant constraints in access to financial services than their male counterparts. Brana (2013) compared access to micro-credit for men and women in France from 2000 to 2003. The author reports that women face stringent price and non-price terms for their loan contracts compared to men, and the interest rate charged is generally higher for women, even when they operate in the same business venture. Asiedu *et al.* (2013) examine the role of gender in a firm's access to finance in developing countries, with a focus on Sub-Saharan Africa. The findings reveal that female-owned firms in Sub-Saharan Africa are more financially constrained than male-owned firms. However, the results for other regions suggest no evidence of gender differences.

The second strand of literature finds that men are more credit constrained than women in certain circumstances. Using data from almost 1,000 small-scale enterprises in Ethiopia, Aga and Reilly (2011) find that male-owned firms are more credit constrained than their female-owned counterparts. This finding suggests that Microfinance Institutions usually target lending to female-owned enterprises. Similarly, using data on Sub-Saharan African SMEs, Hansen and Rand (2014a), and Wellalage and Locke (2016) find that the credit constraint gap is linked to favouritism towards smaller enterprises with female ownership. It is often argued that the seemingly conflicting empirical evidence on gender differences could be attributed to country-specific markets and institutional factors (Brown *et al*., 2011). For example, in balancing gender discrimination in socio-economic activities, microfinance policy has been emphasised over the years to address poverty and promote the empowerment of women (Salgado and Aires, 2018). As a result, millions of women in emerging markets and developing economies have been the target of microfinance programmes to help their access to micro-credit services and bridge the gender gap in participation in productive activities (see Zhang and Posso, 2017; Bezboruah and Pillai, 2017). Increased credit accessibility for women positively contributes to their well-being, as it improves their ability to exercise their choice and freedom to choose (see Ganle *et al*., 2015). The potential of individuals is better explored, and their desires satisfied, when they can effectively control their choices given their current endowments (Mahmud *et al*., 2012; Tahir *et al*., 2018). From this perspective, the studies have argued that more funds are channelled to female-owned enterprises, giving them undue advantages, ultimately mitigating the extent of gender disparities.

The contrasting empirical findings may be related to other explanations. Hansen and Rand (2014a) pointed out that the differences may be due to how the key credit measure is defined in the various studies. The study classified existing studies into three categories based on how credit is measured: perception-based studies, credit access studies, and direct credit constraint measures. The author argued that using comparable data from the World Bank enterprise and investment dataset, the different credit definitions used in three different studies (viz., Aterido *et al*., 2013; Asiedu *et al.*, 2013; Hansen and Rand, 2014a) all reached different conclusions regarding gender differences. In addition, Piras *et al*. (2013) argued that the different ways female-owned firms are defined partially explain the heterogeneity in empirical studies of gender discrimination in access to credit.

3.3.1 Credit measures

The methods for constructing a credit constraint variable have varied somewhat across this research area. The empirical studies of credit constraints have traditionally placed the concept into three broad categories: perception-based, credit access, and direct credit constraint measures (Hansen and Rand, 2014a). The perception-based approach is based on the degree of credit constraint firms face using ratings measures. Most datasets (for example, World Bank Enterprise Survey data commonly used in literature) ask the degree to which access to finance is an obstacle to the current operations of the establishment and, given categorised choices, rated from no constraints to severe constraints. The credit constraint variable is then constructed from these responses with this variant of the measure used by Beck, *et al. (2006)* and Asiedu *et al.* (2013). On the other hand, the direct credit constraint measure is constructed based on whether the firm applied for any loans or a line of credit and the reasons stated for not applying (see, for example, Bigsten *et al*., 2003; Hansen and Rand, 2014a; Nwosu *et al.*, 2015; Wellalage and Locke, 2017). Credit access is constructed based on formal financial services, such as overdraft facilities and traditional bank loans (see, for example, Muravyev *et al.*, 2009; Reilly and Aga, 2011; Aterido *et al.*, 2013; and Chaudhuri *et al*. (2020).

Following the above classification, we constructed our credit indicators based on credit access to capture a loan application, which will be used to model credit demand. We also constructed the indicator for access to credit and loan success based on the direct credit access measure used to model credit supply. These two definitions will enable us to investigate how the factors affecting demand for credit differ from those affecting credit supply.

3.3.2 Gender measures

The literature on the gender gap in access to credit has highlighted the use of ownership and management structure of women in the firm to measure gender roles (Piras *et al.*, 2013). Different authors have defined the gender of the owner by introducing a certain threshold level of ownership to capture the different degrees or share of female responsibility in the firm (Presbitero *et al*., 2014). A simple way to define gender of a sole owner is to limit the sample to sole proprietorship firms. One major drawback of doing this is the potential loss of data points, and the findings may be somewhat limited by the restrictive definition of the gender structure. The definition of gender based on female participation in the firm’s ownership structure is limited by data and detailed survey questions that captured the percentage of female participation in the firm. In most of the existing literature, these ownership structures are categorised based on what percentage of the firm is owned by women, while others adopt the definition of at least one female owner. The other most common definition used is the gender composition of the management structure within the firm, which includes firms or enterprises fully managed by women.

3.3.3 Discrimination in the credit market

Discrimination is present in a credit market when personal characteristics, such as the gender of ownership of the borrowing firms, influence a lender’s decision on loan applications. However, the observed gaps in access to credit can stem from both demand-side and supply-side factors (Cavalluzzo *et al*., 2002; Blanchard *et al*., 2008). In the case of a supply-side factor, lenders treat borrowers with broadly similar characteristics and creditworthiness differently (Muravyev *et al.*, 2009). On the other hand, gender-based discrimination in firms that stem from a demand-side factor is related to a perception of the approval probability, leading to female self-restraint in asking for credit due to fear of rejection (Stefani and Vacca, 2015). Differences in characteristics and preferences between male and female-owned firms in financing needs and credit use could also affect loan application behaviour (Nguyen *et al.*, 2019; Ongena and Popov, 2016) (International Finance Corporation, 2017).

Arising from these arguments, the taste-based economics of discrimination theory proposed by Becker (2010) frames finance discrimination within a market context to analyse and identify reasons for the observed gender differential in access to credit. According to the theory, lenders may treat borrowers differently because of preferences or cultural beliefs about gender. The author argued that discrimination has costs for both victims and discriminators. For instance, if discrimination depresses the income or wealth of women relative to men due to institutional credit scores, a discriminator with a high marginal propensity to lend to men or who is gender-biased will have to bear the cost of dealing with the borrowing attitude of less-efficient men than prospective higher performing women. Therefore, discriminating actors indirectly subsidise discrimination and pay a higher price in equilibrium. This action produces two costs: women face credit constraints, and discriminating lenders lose potential gains from limiting market size. The overall cost to society will be to reduce the contribution of women to economic activities and growth. Therefore, Becker's economic approach highlights one likely incentive for non-discriminating actors to make loans to women: financial institutions could increase their profits by lending more to women.

Becker's (2010) theory suggests better competition would help mitigate gender-based access to credit discrimination, implying that low levels of competition make discrimination less expensive. When a lender earns abnormal profits, the marginal cost of lending to a less efficient client in a "preferred" group is minimal, rather than an equally productive and prospective agent from a "discriminated" group. However, when competition improves, the entry of new results-oriented lenders in the market with less taste for discrimination can increase discrimination costs. As Levine *et al*. (2008) emphasised, finance or access to credit fits comfortably within Becker's taste-based theory of discrimination. Therefore, reforms to the financial sector are needed to motivate lenders, such as banks, to finance the consumption expenditures of households and investment spending of the most productive businesses to intensify competition among economic units. The increased competition in the financial industry will increase the economic opportunities of disadvantaged groups and, consequently, decrease the entry barriers of new businesses in the real economy. It will also encourage the efficient allocation of financial resources within the economy. The central theoretical proposition is that a developed financial sector that emerges via the elimination of credit constraints imposed on the disadvantaged group, will drive growth, improve the efficiency of capital allocation, and ultimately decrease poverty and gender inequality (e.g., see Aghion and Bolton, 1997; Galor and Zeira, 1993; Galor and Moav, 2004).

An alternative discrimination model, information-based discrimination (Arrow, 1973), arises from asymmetric information in a market context. Some argue that, given their lower diffusion, information about female-owned businesses in the economy is limited and less reliable. This lack of information makes access to credit at fair prices more difficult for creditworthy female borrowers. Lenders find it rational to deny women credit if they believe that borrowers' demographic characteristics negatively correlate with creditworthiness based on group-level performance. This theory is also known as statistical discrimination (Phelps, 1972).

Some empirical studies (see Aristei *et al.*, 2016; Lee *et al*., 2015) report discrimination in access to credit against female-owned firms, while controlling for firm heterogeneous characteristics such as age, educational level, sector and size. However, the literature emphasises that it is generally difficult to control all relevant firm-level characteristics; this may affect access to credit since most empirical testing of discrimination in the credit market is typically modelled using a univariate probability regression model framework. A univariate probit or logit model of the dependent dummy variable (where there is either access to credit/loans or not) is generally regressed on a set of explanatory variables. These comprise a vector of borrower characteristics, including a dummy variable for gender or the owner or manager. The evidence for discrimination is found if the coefficient on the gender variable yields a negative and statistically significant effect after controlling for various other firm-level characteristics.

The regression-based approach, the most widely used methodology in discrimination research, has some drawbacks. One of the major issues is that a direct regression-based approach may produce a biased estimate of the discriminatory effect due to the role of endogeneity. Wellalage and Locke (2017) examined the gender balance in credit markets for SMEs in South Asia using World Bank Enterprise Survey data. They revealed that SME owner's/top manager's gender might be endogenous to credit constraint due to reverse causality, omitted variables and other unobservable factors. The authors tested for endogeneity using the Smith and Blundell test, and found that the relevant female variables were endogenous regarding credit constraints. They argue that in a case of omitted variable bias, it is difficult to control for factors exogenous to the borrowers, such as the tastes and preferences of the lending officer making the loan decision. Bellucci *et al*. (2010) demonstrated this conclusion in their studies by investigating the role of gender in bank-firm relationships. They found that gender-based discrimination in credit markets is partly driven by the loan officer's tastes and preferences once individual effects are controlled for. Most studies find that once firm or individual observed characteristics are taken into account, the gender effect disappears (see Bardasi *et al*., 2007; Bruhn, 2009; Corsi and De Angelis, 2017; Moro *et al.*, 2017; Pham and Talavera, 2018), therefore suggesting no evidence of gender discrimination against female entrepreneurs. Collinearity is another potential problem that stems from the correlation between independent variables in the regression. This problem leads to inefficiency rather than inconsistency in the estimates, making it difficult to detect effects. Agier and Szafarz (2013) acknowledge the problem of multicollinearity in their study. The authors investigated whether men and women benefit from the same credit condition using 34,000 loan applications from a Brazilian microfinance institution. The regressors used in their analysis include the characteristics of the borrowers and the amount requested. Since the requested amount is dependent on the applicant's characteristics, including a loan request variable among the regressors may correlate with other variables. The paper addresses the issue by adopting a partial least square (PLS) regression method; this involves a two-step process of separating the impacts of demand-side and supply-side factors on loan size. They found no gender bias in loan denial, but found a differential gender treatment with regards to credit conditions.

Another problem with the single equation approach is sample selection issues that exclude variables not observed for firms in a random sample, such as loan denials, collateral requirements and interest rate variables. For example, Robb and Wolken (2002) report that excluding a sample of firms that did not apply for loans because of a fear of rejection – referred to in the literature as 'discouraged borrowers' – creates an apparent potential selection bias problem.

Therefore, to identify the underlying causes of gender differences in access to credit, some researchers have moved away from intercept shift differences towards the decomposition of inter-group differences attributable to differentials in observable characteristics (endowments) and differentials in treatment (i.e., coefficients) between gender groups. This approach provides a more substantive way of understanding the disparities in the access to credit market, but requires clean separation of samples by gender and sufficient data points in each sub-sample to ensure the common support property is satisfied. The decomposition technique used is commonly attributed to Blinder (1973) and Oaxaca (1973). Using the Blinder-Oaxaca decomposition technique, Hansen and Rand (2014b) examined credit constraint differentials between male and female manufacturing entrepreneurs using firm-level data from 16 Sub-Saharan African countries. The study finds that small enterprises owned by female entrepreneurs are less likely to be credit constrained compared to their male counterparts, while the result is reversed for medium-sized enterprises. The decomposition results reveal that the credit gap is mainly due to differences in the unexplained or treatment component (i.e., a pure gender effect).

Aristei and Gallo (2016) also investigate gender differences in credit rationing probabilities, using firm-level data on 28 transitional European countries. The authors report that gender differences do not explain credit denial probabilities in terms of the observed firm characteristics, but rather unexplained or treatment differences do. The decomposition analysis reported in these studies revealed that gender differences in coefficients (i.e., treatment differences) are more important than gender differences in endowments in explaining the gender differential in access to credit. Fortin *et al*. (2011) argue that such index number decompositions will yield an average treatment effect on the treated (ATT) only when a set of general assumptions relating to ‘weak ignorability’ and the use of a simple counterfactual (entailing no spill-over effects) are satisfied.

Using the Fairlie (1999) decomposition technique, Aterido *et al.* (2013) analysed data from the World Bank Enterprise Survey (WBES); these data allow them to estimate gender differences in access to credit by enterprises and use of formal and informal financial services by individuals in Sub-Saharan Africa. They find an unconditional gender gap, but they note that gender has no significant effect on the probability of firms accessing credit once firm characteristics are controlled for. Nwosu *et al.* (2015) also used a similar dataset and methodology to Aterido *et al.* (2013) to investigate whether female entrepreneurs experience discrimination in formal credit markets in Nigeria. In accordance with Aterido *et al.* (2013), they find no evidence of discrimination against women in formal credit markets, again after controlling for various firm-specific characteristics.

The study by Nwosu *et al.* (2015) is among the first, and most recent, to have adopted a more rigorous approach to examining the gender differential on access to credit in Nigeria. However, the analysis undertaken in our study differs in many distinct ways. Using earlier data (i.e., the 2010 WBES) to that used for our analysis, their study fails to consider the dynamic aspect of gender compositions by limiting their sample to sole proprietorship, capturing firms with female owners only. This restriction reduces the number of useable observations and presents a limited portrait of the gender-relevant dimension. Our study explicitly measured gender using alternative definitions from the 2014 WBES, by drawing from responses to the questions of ‘what percentage of the firm is owned by female?’ and ‘is the top manager female?’. The use of responses to these questions, in our view, captures the different roles played by women in ownership and management positions, allowing us to examine the gender effect along a different dimension than heretofore in the literature.

Another gap in the literature that this study seeks to bridge is the potential problem of selection bias. In determining the effect of gender in the literature, most researchers adopt the method of conditioning on observable characteristics. Such studies use gender dummies and interaction terms in a simple regression model to estimate the effect of interest, while ignoring the problem of selection bias. However, Nwosu *et al.* (2015) attempt to mitigate the potential problem of selection bias in their analysis by modifying the approach used in Hansen and Rand (2014a), which is an extension of the study by Bigsten *et al.* (2003) and Byiers *et al*. (2010), to construct the credit constraint variable. They argue the way the credit constraint variable is constructed can mitigate the selection bias problem. Therefore, conditioning on the credit demand of firms, they identified credit constrained firms as those who applied for a line of credit and were denied. They also identified firms that did not apply for credit due to reasons such as “application procedures too complex”, “collateral requirements unattainable”, or “possible loan size and maturity insufficient”. The authors excluded from their analysis responses of “interest rates too high” or “did not believe it would be approved” and “insufficient profitability” as reasons for not applying. The study based its argument on the fact that for those who did not believe their loans would be approved, this constituted an internal self-selected group that did not possess the attributes (income, collateral, viable business plan, etc.) required by lenders and cannot therefore be classified as entrepreneurs. The paper argues that such internal self-selection explains the behaviour of many women, small farmers, micro-entrepreneurs and poor people who rely heavily on informal financial sources (Baydas *et al*., 1994).

However, there is evidence in the literature that suggests a firm’s lack of loan application may be due to past discrimination in the credit market (Cavalluzzo and Cavalluzzo, 1998), or that female firms are more likely to avoid credit applications due to credit market concentration related to mergers in the banking industry (Cavalluzzo *et al.*, 2002). Stefani and Vacca (2015) find significant evidence in gender differences when the non-application is due to fear of rejection, particularly in regard to bank loans.

It could be argued that those respondents who believe their loan application may not be approved have unobservable characteristics, not necessarily because of the lack of attributes required by lenders, that render them less likely to have applied in the first place even though they are credit constrained. Omitting this critical question and ignoring the fact that female-owned firms exhibit different demand patterns from male-owned firms, even when they have the required documentation, poses a potential sample selection problem. Their study failed to address this potential issue explicitly using an appropriate modelling approach.

Therefore, our analysis is taken one step further by using a bivariate probit model to mitigate the potential selection bias problem arising from the selection of the sample of those whose loan application was successful conditional on applying for a loan. In addition, decomposition analysis will be undertaken to break down the total gender gap into treatment and endowment effects because there may be differences in the effect of the covariates on participation and loan success along a gender dimension that an intercept shift may not capture alone. The decomposition approach adopted will be determined by the empirical evidence detected in regard to selection bias.

Table A3.1 in the Appendix to this chapter summarises the main features of the empirical literature reviewed in this area. The empirical studies found different results for participation and credit access across countries. The summary revealed that seven of the eleven studies recorded negative gender effects in loan success/approval, with two positive effects and three zero or null effects. The review identified two major assumptions on participation in credit markets and access to credit in the literature. The first view assumes that borrowing from a formal financial institution is solely determined by the bank's decision on access, and borrowers have a positive demand for formal credit. In this case, the concepts of credit access and credit participation are interchangeably used, and the analysis is based on a univariate probit model. The second view differentiates between credit participation and access. Credit participation is related to a borrower's decision on the demand side, while credit access is related to the lender's decision on the supply side.

This view assumes that the probability of borrowing from a formal financial institution is jointly determined by credit demand and a lender’s decision on credit. The sub-sample is assumed to be a self-selected sample that requires an econometric technique that deals with the potential problem of selection bias. Eight of the eleven studies investigated credit access using univariate analysis, while only three papers reported in this table assumed that credit supply is jointly determined with credit demand.

Another important point is that female involvement in firm ownership and management is crucial in dictating gender differences in credit market outcomes. Presbitero *et al.* (2014) found that firms with a predominant presence of female owners have a higher probability of being credit rationed than their male counterparts. Their result is consistent with other studies (e.g., Muravyev *et al.*, 2009) that use the restrictive definition of female ownership in developing countries. Therefore, the share of gender in a firm determines the outcome of the estimated gender differences in the probability of credit access and participation. Therefore, the dominance of any of the two gender groups makes a substantial difference.

Finally, the positive effects recorded in these studies are partly explained by the preferential treatment given to female entrepreneurs, especially in developing countries where government credit schemes are targeted towards female-led firms (Wellalage and Locke, 2017) as they are less likely to get financing from formal financial institutions (Klapper and Parker, 2011). Hansen and Rand (2014b) argued that female favouritism in credit is explained by self-selection into entrepreneurship.

3.4 Data Section

The Nigerian World Bank Enterprise Survey (WBES) data collected in 2014 contain a wide range of information on access to finance, corruption, political, infrastructure, crime, competition, labour market and legal obstacles. The survey also contains information on a firm’s ownership, a top manager’s employment experience, and other firm-level information. One of the key strengths of the survey that makes it suitable for the current research question is its coverage of small and medium enterprises (SMEs), the finance module, and gender information.

The WBES sample comprises 2,676 firms. Using a stratified sampling procedure, the surveys were administered ‘face-to-face’ with business owners and their top firm managers. The surveys are stratified according to three criteria: sector of activity, firm size, and geographical location. Stratification by sector of activity covers three main sectors: manufacturing industry, services sector and retail trade. Firm size divides the population of firms across three strata: small firms (5-19 employees), medium firms (20-99 employees), and large firms (100 or more employees). The geographical distribution is defined to reflect the distribution of non-agricultural economic activity in 19 states of the Federation. The firms are randomly chosen within each stratum, and the sample is representative at the national level.

Given the objective of the research, the sample of firms is restricted to formal small and medium scale enterprises. The motivation for this focus in the current study is to explore gender participation and access to credit with respect to SMEs. In particular, we want to examine unequal treatment with respect to gender in access to credit for SMEs[13] given women entrepreneurs are better represented in this sector than in larger enterprises. After excluding large firms and those firms for which there are missing values for all variables of interest, we were left with an overall sample of 2,304 firms that constitute the set of usable data for our empirical analysis. The data include all the necessary information to construct the firm's financial indicators, the productivity measures and the firm-level control variables required for our regression analysis; they also include key information relating to the firm ownership and management along the gender dimension.

3.4.1 Definition of variables and summary statistics

The motivation for this study relates to the hypothesis that the probability of borrowing from a formal financial institution is jointly determined by the demand for credit and the bank's decision on access to credit. The data include information on whether SMEs applied for a loan from a formal financial institution and, if so, whether or not the application was successful. These data thus provide the basis for constructing two binary outcome variables. The study employs a relatively broad definition of formal financial institutions to encompass all loans taken from Nigeria's private and public financial institutions. As discussed in the contextual framework, a review of SME financing in Nigeria has shown that credit to SMEs is derived from two types of institutions: private sector-led institutions and public sector-based institutions. Private sector-led institutions constitute deposit

[13] We exclude large firms from the analysis because of the small number with female ownership. Indeed, only eight of the large firms in the sample are actually female-owned. Thus, the number of large firms that are female-owned is vanishingly small and could not support any kind of meaningful empirical analysis.

money banks and microfinance banks, while public sector-based institutions are the specialised banks established by the government to provide credit facilities to SMEs.

As noted earlier, from the access to credit module contained in the WBES, two key variables for our analysis can be constructed. These are whether a firm applied for credit and, conditional on having applied, whether the application was successful. The application and success variables are constructed from a survey question asking firms whether they had applied for a loan or line of credit in the last 12 months. For those firms that replied ‘yes’ to this question, the application variable takes a value of 1, and 0 otherwise. A follow-up question was asked, whether the loan applied for was approved or rejected. The firm is deemed successful if the loan application is approved and the ‘success’ variable takes a value of 1, and a value of 0 otherwise.

To define the key explanatory variables germane to the research question, the analysis considers definitions of gender composition based on ownership structure and managerial position in the firm using a survey question that asked respondents what percentage of the firm is owned by females. Those firms that replied ‘0%’ are classified as 100% male-dominated firms (‘male100’); this takes a value of 1 if 100% male-dominated, and 0 otherwise. Those that replied ‘100%’ are classified as 100% female firms (‘fem100’); this takes the value of 1 if 100% female firms, and 0 otherwise. Those that fall between ‘50-99%’ are classified as 50% female majority firms (‘femmaj’); this takes the value of 1 if 50% female-owned, and 0 otherwise. Lastly, those that fell between ‘1-49%’ are classified as female minority firms (‘femmin’); this takes the value of 1 if female minority, and 0 otherwise. Our second measure identifies female-managed firms if the top manager is a woman (‘fem_mgt’); this equals 1 if this is the case, and 0 otherwise. Therefore, with respect to most of the existing literature, the construction of our gender variables allows us to capture the role women play at different levels in the firm without limiting the sample size only to sole proprietorship to female-only firms.

The WBES dataset includes information about firm-level characteristics, managerial attributes and performance measures. Firm-level characteristics include firm size, age, legal status and the production sector within which the firm operates. Managerial attributes are captured using gender, managerial, educational, and experience levels dummies. A variable capturing financial transparency and quality is constructed based on a dummy variable for whether or not the firm has financial statements certified by external auditors (i.e., 'Audit'). This variable adopts a value of 1 if this is the case and 0 otherwise.

Additional measures of credit riskiness relate to a firm's export activity (a dummy variable for whether the firm exports) and the possession of fixed assets that can be used as collateral (defined here by a dummy variable). The model also includes a regressor capturing firm performance, measured as a log of sales. Finally, we included a policy dummy, MSMEDF, that captures participation in government financial support intervention funds. The use of this variable as a control in this case is strongly motivated by the findings in our previous chapter that participation in the credit development scheme MSMEDF increases a firm's access to credit, on average and *ceteris paribus*. In addition, the empirical findings in the existing literature suggest that most government intervention schemes usually target lending to female-owned firms, giving them an undue advantage in the credit market. Therefore, we anticipate that a financial support programme variable may play an important role in facilitating access to credit for women. The MSMEDF variable is constructed to take a value of 1 if a firm is located in the state with access to the MSMEDF and 0 otherwise. Table A3.2 in the Appendix provides a complete set of variable definitions.

The summary statistics reported in Table 3.1 revealed that 52.8% of firms applied for a line of credit or loan; 30.5%[14] of firms reported their loan applications were successful at the time of this study. Regarding our key explanatory variable, the gender variables reveal that 69.7% of the firms are male-dominated, 18.3% are 100% owned by women, 9% of the sample are firms with 50-99% female ownership, 3% are female firms with 1-49% ownership and 11.9% are run by female managers. This profile aligns with previous research on gender participation in the credit market. The percentage of female-owned and female-managed firms is lower than that of male-owned firms; this could suggest that male owned firms are more likely to participate in the credit market than female owned firms. However, whether gender gap represents an unequal treatment requires econometric analysis. The analysis takes this characterisation as a starting point to gauge whether gender bias exists in the credit market for bank loans. An extensive literature treats the issue of bank discrimination against female-owned firms as mainly a loan

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[14] This is comparable to the scale of bank financing in other developing countries, such as India, Indonesia, Brazil, Bangladesh and China (Allen *et al*., 2005; Ayyagari *et al*., 2010).

supply-side problem, with little consideration that the borrowing behaviour of female-owned firms contributes to the lack of loan demand by female firms (Baydas *et al*., 1994). Considering this fact, therefore, the current research links unequal treatment in the credit market to loan demand and supply, which has important policy implications.

In addition to our key variables, we control for other explanatory variables expected to capture both firm and owner characteristics. The firm's characteristics include the age of the company, measured as the difference between the current year and the year the company started operations. In the literature on the capital structure of small businesses, Berger and Udell (1998) argued that a particular phase of a business's life cycle determines its financial needs. Studies have classified firms into new and old firms along the business life cycle. It is anticipated that the financial needs of new firms will differ from older firms, because younger firms will have smaller earnings than older firms due to a lack of experience or exposure in the market. New firms are defined as firms less than 15 years of age, and old firms are above 15 years of age (La Rocca *et al*., 2011). We then constructed two dummy variables: *age14* that takes a value of 1 if the firm's age is below 15 years, and 0 otherwise, and *age15* that takes the value of 1 if the firm's age is above or equal to 15 years. About a third of the enterprises are less than 15 years of age. The variable *lnsales* is a measure of firm size, constructed as a logarithm of total sales at the end of the financial year.

A complementary measure of firm sizes based on employment reveals that small firms represent about 58% of the sample, while 42% are medium-sized firms. The firms in the sample are classified into three main activity sectors: services, retail and manufacturing. As can be seen from the summary statistics, most firms (49%) are in manufacturing, 21% are in the retail sector and 31% are in the services sector. Maintaining an audited financial statement is an important determinant of access to credit, as banks use it to measure the firm's transparency and quality. About one-quarter of the firms have their financial statements audited in the current financial year.

Another important determinant of access to credit is the possession of a fixed asset, which could provide collateral for a loan application. The variable *fixasset* takes the value of 1 if the firm has land or a building that a financial institution could use as collateral, and 0 otherwise. However, only about 18% of the firms reported having such fixed assets even though more than half of the sample applied for loans and about 30% of firms were successful. In comparing the number of successful firms with those with fixed assets, the result suggests that either collateral was not required or is provided in other ways. This is more common in developing economies with underdeveloped financial systems. Financial institutions in these economies may resort to using alternative methods for collateral requirements such as third-party guarantees where new or small firms without fixed assets could have another individual or firm act as their guarantors. In some cases, lenders could use a firm's track record of financial dealings with the bank or their social reputation to infer a borrower's loan repayment capacity. There is anecdotal evidence that this is indeed the case in Nigeria. For instance, Sterling Bank has a lending 'app' known as Specta and Social Lender, which uses a user's social media profile to check their creditworthiness. Through this service, they can offer loans to individuals and businesses based on their social reputation (Jackson, 2016).

The firm's export orientation indicates whether or not a firm is engaged in external trading: approximately 11.7% are exporting firms. In terms of businesses type, the variable *sole_trade* takes the value of 1 if the firm is a sole trader, and 0 otherwise. Over three-quarters of the firms are sole traders.

Apart from this attribute of the firm, the set of explanatory variables also includes owner(s) attributes. In order to control for the top manager's experience, we use the number of years the manager has worked in the firm, denoted as *experience.* The average top manager has approximately 12 years of working experience in the firm. Finally, we control for the educational level of the top manager. The educational level of the manager is classified into three categories (high school or less, vocational and university) depending on their educational attainment. About 39% of the top managers have attended at least high school, 12% vocational training, and 49% have a university degree.

When we divided the sample into male and female sub-groups, the summary statistics reveal that female-owned firms are more likely to have applied for a loan than male-owned firms. Among those whose loans were successful, women were as likely as men to have had their loans approved.

Table 3.1: Summary Statistics for Full Sample and Sub-sample by Gender

	Overall Sample	Male Sample	Female Sample
application	0.528	0.519	0.554
	(0.499)	(0.500)	(0.497)
success	0.305	0.311	0.290
	(0.461)	(0.463)	(0.454)
male100	0.697	ƥ	ƥ
	(0.459)		
fem100	0.184	ƥ	ƥ
	(0.387)		
femmaj	0.090	ƥ	ƥ
	(0.287)		
femmin	0.029	ƥ	ƥ
	(0.168)		
fem_mgt	0.119	ƥ	ƥ
	(0.324)		
MSMEDF	0.536	0.526	0.562
	(0.499)	(0.499)	(0.497)
age14	0.574	0.578	0.562
	(0.494)	(0.494)	(0.497)
age15	0.426	0.422	0.438
	(0.495)	(0.494)	(0.496)
lnsales	14.103	14.104	14.100
	(2.928)	(2.949)	(2.872)
small	0.576	0.578	0.568
	(0.494)	(0.494)	(0.496)
medium	0.424	0.422	0.432
	(0.494)	(0.494)	(0.496)
manufacturing	0.487	0.479	0.506
	(0.450)	(0.500)	(0.500)
retail	0.208	0.210	0.202
	(0.406)	(0.408)	(0.402)
services	0.305	0.311	0.292
	(0.461)	(0.463)	(0.455)
audit	0.234	0.223	0.261
	(0.423)	(0.417)	(0.440)
fixasset	0.178	0.173	0.195
	(0.383)	(0.378)	(0.397)
exporter	0.117	0.066	0.065
	(0.322)	(0.248)	(0.247)
sole_trade	0.779	0.768	0.811
	(0.415)	(0.422)	(0.392)
exper	12.368	12.380	12.424
	(8.718)	(8.776)	(8.833)
highsch_less	0.391	0.409	0.344
	(0.488)	(0.492)	(0.476)
vocational	0.116	0.117	0.113
	(0.320)	(0.322)	(0.316)
university	0.493	0.474	0.543
	(0.500)	(0.499)	(0.499)
No. Obs	**2,304**	**1,674**	**630**

a. Standard deviation in parenthesis. b. * Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. c. ƥ denotes not applicable in estimation. d. The gender distinction is based on only majority (i.e., 100% + 50%) female-owned and male-owned firms.

Source: Author's calculations using 2014 WBES data.

Table 3.2 presents the summary statistics and mean differences between the outcome variables. The statistics suggest some important differences.

Moving from the overall dataset to the sub-sample of firms that applied for a loan, 1,217 firms applied for a loan from formal financial institutions, and 703 applications were successful, representing 58% of the sample. This finding is similar to the results obtained from the SME survey undertaken in 2015 by the Central Bank of Nigeria (CBN), in partnership with the International Finance Corporation (IFC), a member of the World Bank Group. Their study reported that 31%[15] of SMEs surveyed had obtained a loan from a formal financial institution in Nigeria, and most loan applications were successful (87%); this is higher than the 58% reported for this study. One possible explanation for the high success rate of loan applications in their study may be attributed to the surveyed states. The study was conducted in six economically active states in Nigeria (viz., Lagos, Rivers, Anambra, Abuja, Kano, and Bauchi states). Smaller companies may not have met the lending criteria and so did not apply for bank loans, leading to higher success rates observed in their study than in the current research (Central bank of Nigeria and IFC, 2017). Overall, the findings reported here appear more plausible.

The summary statistics for the sub-sample reveals that not all firms that applied for credit were successful as firms could be rejected based on a lender's screening mechanism, such as the observable characteristics of the firm. In addition, the result reveals that being successful is conditional on application, as successful firms can only be observed if they apply for a line of credit. In such a case, the sample constitutes a self-selected sub-sample, which has an implication for the econometric technique used. The issue of selection bias will be examined in more detail in the empirical section of this chapter.

The sub-sample of applicants is characterised by a more significant proportion of firms in states with MSMEDF funding than the sub-sample of non-applicants. This number is entirely logical since the scheme's main objective is to increase credit availability to firms. The summary statistics reveal that some differences exist between the application and success sub-samples. The differences in firm-level characteristics will be examined in more detail using an alternative econometric strategy in subsequent analysis. These simple descriptions of the application and success outcomes do not reveal the nature of

[15] Comparable to 31% for our study (see Table 3.1).

the differences in characteristics between gender groups, which is what attention now turns.

Table 3.2: Test of Differences in Means in Application and Success

	Apply=0 Mean	Apply=1 Mean	Diff. t-test	Success=0 Mean	Success=1 Mean	Diff. t-test
fem100	0.166	0.199	-0.0334	0.232	0.175	0.057***
	(0.011)	(0.0110)	(0.016)	(0.019)	(0.014)	(0.006)
femmaj	0.093	0.088	-0.005	0.091	0.085	0.0113
	(0.009)	(0.008)	(0.012)	(0.013)	(0.011)	(0.008)
femmin	0.026	0.032	-0.006	0.025	0.037	-0.012
	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)	(0.010)
fem_mgt	0.113	0.125	-0.012	0.130	0.121	0.009
	(0.010)	(0.009)	(0.013)	(0.015)	(0.012)	(0.019)
MSMEDF	0.447	0.615	-0.168***	0.597	0.627	-0.030
	(0.015)	(0.014)	(0.021)	(0.022)	(0.018)	(0.028)
age14	0.609	0.542	0.067***	0.564	0.526	0.038
	(0.015)	(0.014)	(0.021)	(0.022)	(0.019)	(0.029)
age15	0.391	0.458	-0.067***	0.436	0.474	-0.038
	(0.015)	(0.014)	(0.020)	(0.022)	(0.019)	(0.029)
lnsales	13.786	14.386	-0.600***	14.339	14.421	-0.082
	(0.095)	(0.077)	(0.122)	(0.118)	(0.102)	(0.157)
small	0.590	0.563	0.027	0.576	0.553	0.023
	(0.015)	(0.014)	(0.020)	(0.022)	(0.019)	(0.029)
medium	0.410	0.437	-0.027	0.424	0.447	-0.023
	(0.015)	(0.014)	(0.020)	(0.022)	(0.019)	(0.029)
manufacturing	0.511	0.464	0.047***	0.514	0.428	0.084
	(0.015)	(0.014)	(0.020)	(0.022)	(0.019)	(0.029)***
retail	0.206	0.210	-0.003	0.196	0.219	-0.023
	(0.012)	(0.012)	(0.017)	(0.018)	(0.016)	(0.024)
services	0.282	0.326	-0.044***	0.290	0.353	-0.063
	(0.014)	(0.013)	(0.019)	(0.020)	(0.018)	(0.027)***
audit	0.205	0.260	-0.055***	0.228	0.283	-0.055
	(0.012)	(0.013)	(0.018)	(0.019)	(0.017)	(0.025)***
fixasset	0.135	0.218	-0.083***	0.165	0.256	-0.091***
	(0.010)	(0.012)	(0.0160)	(0.016)	(0.016)	(0.024)
exporter	0.076	0.056	0.020**	0.095	0.027	0.068***
	(0.008)	(0.007)	(0.010)	(0.013)	(0.006)	(0.013)
sole_trade	0.748	0.808	-0.060***	0.817	0.801	0.016
	(0.013)	(0.009)	(0.017)	(0.017)	(0.015)	(0.023)
exper	12.269	12.502	-0.234	12.538	12.477	0.061
	(0.264)	(0.254)	(0.0367)	(0.394)	(0.332)	(0.514)
highsch_less	0.430	0.357	0.073***	0.407	0.320	0.087***
	(0.015)	(0.014)	(0.020)	(0.022)	(0.018)	(0.028)
vocational	0.124	0.108	0.016	0.123	0.098	0.024
	(0.010)	(0.009)	(0.013)	(0.014)	(0.011)	(0.018)
university	0.446	0.535	-0.089***	0.471	0.582	-0.111***
	(0.015)	(0.014)	(0.020)	(0.022)	(0.019)	(0.029)
No. Obs	**1,087**	**1,217**		**514**	**703**	

Notes:
a. Difference calculated as mean (0) minus mean (1)
b. Standard errors in parenthesis.
c. * Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level.

Source: Author’s calculations using 2014 WBES data.

Table 3.3 reports sample statistics for the difference in means of the dependent (or outcome) variables divided into the different gender classifications of male-led and female-led firms as outlined earlier. The sample sizes for success is conditional on loan application. The first row presents the summary statistics of 100% male owned firms versus 100% female owned firms. The second row represents 100% male owned and majority female owned firms. The third row represents 100% male owned and female minority firms. The fourth row presents an alternative sample of majority male firms and majority female owned and managed firms.

It is evident from the table that considerable differences exist between male-led and female-led firms in terms of raw averages. Female-led firms with 100% ownership have higher demand for credit and lower access to credit when compared to their male counterparts. The results reveal that when the focus is on two heterogeneous female-dominated and male-dominated groups, it can dictate important differences in lending outcomes. The results suggest that female entrepreneurs are not discouraged from applying for loans but are treated differentially when they do apply. The result for the other gender groups revealed insignificant differences between male and female ownership types. In some cases, where female ownership participation may include cases where a woman is one of a number of owners, her ownership share is irrelevant (Aterido *et al*., 2013); this conceals the magnitude of the gender effect.

However, these results are based on raw estimates and so any inferences on unequal treatment or discrimination cannot be provided here. In order to effectively evaluate gender differences, we will need to control for other factors related, *inter alia*, to a firm's characteristics and manager's/owners' individual-level attributes.

Table 3.3: Summary statistics of gender groups by application and success rates

	Male firm	100% Female firm	Differences
Application	0.5159 (0.0124)	0.5735 (0.0241)	-0.0576*** (0.0271)
Sample size	**1,607**	**422**	
Success	0.5959 (0.0171)	0.5082 (0.0322)	0.0877*** (0.0365)
Sample Size	**829**	**242**	
	Male firm	**Female majority firm**	**Differences**
Application	0.5159 (0.0124)	0.5144 (0.0347)	0.0015 (0.0368)
Sample size	**1,607**	**208**	
Success	0.5959 (0.0171)	0.5607 (0.0482)	0.0352 (0.0115)
Sample Size	**829**	**107**	
	Male firm	**Female minority firm**	**Differences**
Application	0.5159 (0.0124)	0.5821 (0.0607)	-0.0662 (0.0620)
Sample size	**1,607**	**67**	
Success	0.5959 (0.0171)	0.6667 (0.0764)	-0.0807 (0.0783)
Sample Size	**829**	**39**	
	Male managed firm	**Female managed firm**	**Differences**
Application	0.5169 (0.0125)	0.5535 (0.0187)	0.0366 (0.0225)
Sample size	**1,594**	**710**	
Success	0.5958 (0.0171)	0.5394 (0.0252)	0.0564** (0.0303)
Sample Size	**824**	**393**	

Notes:
a. Difference calculated as mean of Male firms minus mean of female gender group.
b. Standard errors in parenthesis.
c. * Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level.

Source: Author’s calculations using 2014 WBES data.

3.5 Econometric Methodology

This study uses a number of probit models in estimation, given the binary nature of the dependent variables. The dependent variables in this application assume either a value of 1 or 0 depending on whether or not a firm applied for credit and whether their loan application was successful.

However, given the structure of the response sequence presented in our analysis, the econometric framework laid out in the literature suggests two equations that can be estimated jointly; this is because it is possible that the factors determining the likelihood of a loan application also influence the rate of loan approval. Since successful firms can only be observed if they have applied for a loan, firms that are more likely to have an application approved may also be more likely to apply for a line of credit. In this case, the probability of access to credit may be jointly determined by the firm's decision to borrow and the bank's decision on approval. Therefore, successful firms may constitute a self-selected sub-sample, posing potential selection bias problems following the more conventional selectivity bias problem originally emphasised within a linear regression framework by Heckman (1979). One econometric strategy suggested in the literature of sample selection to deal with this non-random selectivity issue is the bivariate probit model; this provides a framework for the joint estimation of the loan application and loan success models.

Given the above consideration, in order to provide empirical evidence on the impact of female-owned firms on loan application and success, we conducted the empirical analysis in four stages:

Stage one: We initially explored the use of a bivariate probit model to assess the potential problem of selection bias, given that the decision of lenders on loan approval is likely dependent on a borrower's decision to participate in making a loan application.

Stage two: If no evidence of selectivity bias was found in our data, we used a univariate probit model to investigate the contribution of our key policy explanatory variables, female-owned and female managed firms, to the probability of a firm applying for a line of credit and being successful, and whether unequal treatment exists in the credit market for women. We also explored the determinants of loan application and success by including a wide range of independent variables expected to affect firm behaviour.

Stage Three: Since the use of pooled univariate and bivariate probit models alone does not deal adequately with differences in the effects of covariates across gender groups, we subsequently employed an Oaxaca-Blinder decomposition technique using linear probability models as a means of analysing the differences in outcomes between groups, which comprise male-owned firms and female-owned firms in our case. This allows the total gender gap in loan application rates and success rates between the two gender groups to be decomposed into a part attributable to differences in characteristics (i.e., the endowment component) and a part attributable to differences in coefficients (i.e., the treatment component). In the univariate and bivariate probit models, the effects of covariates are assumed to be the same across male and female groups with the gender effect restricted to simply the gender dummy alone (i.e., just the differences in the model intercepts). However, in undertaking the decomposition analysis we allow the entire process determining application and success to be different between gender groups across all covariates in addition to the gender intercept term.

3.5.1 Bivariate Probit Models

Poirier (1980) was one of the first econometricians to use the bivariate probit model to investigate a random utility model that reflected two decision-makers' joint unobserved binary choices. The model that arises from an observed binary outcome is not a univariate probit model, but a bivariate probit model in which only one of the possible outcomes of the joint choice is observable. This model became known as a bivariate probit with partial observability. Poirier (1980) noted that, in a selectivity context, it is important to determine whether inclusion in the sample is the result of a single binary decision, or whether its inclusion depends on a single binary variable arising from more than one binary decision. Since the early contributions of this methodology, economists have investigated joint probability outcomes using a variety of different bivariate probit models. Building on the original work of Poirier (1980), Meng and Schmidt (1985) outline different degrees of observability in bivariate probit models. These are classified as bivariate probit models with full observability, partial observability and censored probit (i.e., partial partial observability). Partial observability occurs when we can observe a positive outcome for only one of the dependent variables when the other is also positive.

This type of censored probit has been used in several studies in the literature. For instance, an early study by Farber (1983) examined the demand for trade unionism in the United States, Boyes *et al*. (1989) model loan default in the United States, and Litchfield and Reilly (2009) exploit this approach in the study of migration and the role of gender in Albania. A more recent study by Aristei and Gallo (2016) used this model to investigate gender differences in firm-level access to finance for a group of transitional European countries.

However, whether it is necessary to use a bivariate probit model is an empirical question. A bivariate probit model is only relevant when the error terms influencing demand for credit and those influencing success in securing credit are correlated, yielding the selectivity bias problem. If there is no correlation between the unobservables in these two equations, and therefore no evidence of selectivity bias, the complete model can be estimated using two separate unrelated standard univariate probit models. In this context, however, a key econometric challenge is empirically identifying the correlation coefficient in the bivariate probit models, which provides the basis for testing for the selection bias. This topic will be examined in more detail in the empirical section. It requires the use of exclusion restrictions justified on the basis of both a plausible narrative and empirical tests.

Therefore, our bivariate probit model contains two equations. The first deals with the decision of a firm to apply for a loan, and the second captures whether the loan application was successful. The dependent variables assume, respectively, a value of 1 or 0 depending on whether or not a firm applied for a loan, and whether the loan application was successful. Since the second model is based on a censored (or selected) sub-sample, separate estimation of the equation may be problematic if the error terms in the two models are correlated. In this case, the correct choice of model will not be a univariate probit since the unobservables determining the application may be correlated with the unobservables determining credit success. As a result, a simple probit estimation of the application and success models will potentially produce biased and inconsistent estimates of the effects on access to credit. Given this scenario, the study uses the bivariate probit with partial observability, allowing for the two error terms to be correlated and exploiting a framework within which selection bias can be empirically tested. If the error terms are correlated, the estimates of the bivariate model will be appropriate for our analysis. On the other hand, if the error terms are not correlated, we can then use separate univariate probit models for the analysis.

The formal structure of the bivariate model comprises two linear latent dependent variable equations:

$$y_{1_i}^* = \boldsymbol{X}_i' \beta + u_i \qquad [3.1]$$

$$y_{2_i}^* = \boldsymbol{Z}_i' \gamma + \varepsilon_i \qquad [3.2]$$

where $\boldsymbol{X}_i$ is the vector of variables determining the application for the loan and $\boldsymbol{Z}_i$ are the explanatory variables determining loan approval. Both vectors $\mathbf{X}_i$ and $\mathbf{Z}_i$ contain a gender dummy for ownership as defined earlier. However, $y_{1_i}^*$ and $y_{2_i}^*$ are not observed, but what is observed is the following set of binary outcomes:

y_{1_i} = 1 if the firm applied for a loan ($y_{1_i}^* > 0$)

$y_{1_i} = 0$ *if not* ($y_{1_i}^* \leq 0$)

And y_{2_i} = 1 if the application was successful ($y_{2_i}^* > 0$)

$y_{2_i} = 0$ *if not* ($y_{2_i}^* \leq 0$)

where y_1 and y_2 can be jointly determined, and u_i and ε_i have a bivariate normal distribution with a correlation coefficient ρ. The estimate $\hat{\rho}$ provides the basis for an empirical test for selectivity bias. If H_o: ρ = 0 is found to be satisfied, the outcomes are independent and there is no evidence of selectivity bias. In such circumstances, the two equations may be modelled separately as univariate probit models. However, if the null hypothesis is rejected, the presence of selectivity bias cannot be rejected and the two outcomes are correlated. In other words, the probability of one outcome depends on the probability of the other. Therefore, it is more efficient to estimate the two equations jointly.

The estimation approach for the bivariate probit exploits full information maximum likelihood (FIML) estimation techniques (Poirier, 1980; Maddala, 1986; Meng and Schmidt, 1985). In this case, the maximum likelihood estimation (MLE) technique can be used to consistently estimate β and γ.

The log-likelihood function is given as:

$$L(\beta, \gamma, \rho; X_{i,} Z_i) = \prod_{y_1=1, y_2=1} F(\boldsymbol{X}_i', \beta, \boldsymbol{Z}_i', \gamma; \rho) \prod_{y_1=1, y_2=1} F(\boldsymbol{X}_i', \beta, -\boldsymbol{Z}_i'; -\rho)$$

$$\prod_{y_1=0} [1 - \Phi(\boldsymbol{X}_i' \beta)] \qquad [3.3]$$

where:

$\Phi(.)$ = univariate standard normal cumulative distribution function.

$F(.,.;\rho)$ = bivariate standard normal cumulative distribution function.

ρ = the correlation coefficient between the unobservable errors u_i and ε_i.

If ρ = 0, no evidence of selectivity bias is present, and no efficiency loss is encountered in the separate estimation of either equation [3.1] or [3.2], the study can then estimate a univariate probit model.

3.5.2 Univariate Probit Models

In a univariate probit model, the model assumes that formal financial institutions determine whether the firms have access to its loans or not when they applied for a loan. This implies that all firms have a positive demand or supply for formal credit. The probability of borrowing is then described by a probability model where the decision to take a loan is dependent on some exogenous predictors including different firm-level characteristics. This is only possible when there is no evidence of selection bias between the two equations.

As with the bivariate probit model above, the univariate probit model is also generally motivated by a reference to a latent (unobservable) continuous dependent variable (y_i^*) and it is linked to an observable binary variable y_i that adopts a value of either 1 (if the event occurs) or 0 otherwise. As above, the latent dependent variable model can be expressed linearly as a function of a set of explanatory variables as follows:

$$y_i^* = \mathrm{x}_i^* \boldsymbol{\beta} + u_i$$

where $u_i \sim N(0, \sigma^2)$ and $y_i^* \sim N(\mathrm{x}_i^* \boldsymbol{\beta}, \sigma^2)$

The error term is assumed normally distributed with a mean of zero and a constant variance σ^2. The latent variable is an index of the unobserved propensity for the event to occur, and in this case reflects the underlying propensity of a firm to apply for loan or for the loan to be approved. If y_i^* > 0 then y_i = 1 (i.e., the event occurs), and if y_i^* ≤ 0 then y_i = 0 (i.e., the event does not occur). This approach links a discrete observable dependent variable to a continuous unobservable variable. The probability of the event occurring can be linked to the latent dependent variable as follows:

$$prop[y_i = 1] = prop[y_i^* > 0] = \Phi\left(\frac{\mathrm{x}_i^*\boldsymbol{\beta}}{\sigma}\right) = \Phi(\mathrm{x}_i^*\boldsymbol{\beta})$$

where y_i is the dichotomous realisation of the latent dependent variable (in this study, is either loan application or loan success), $\Phi(.)$ represents the cumulative distribution function for a standard normal random variable. Since we cannot identify the $\boldsymbol{\beta}$ vector separately from the ancillary σ parameter, it is conventional to normalise $\sigma = 1$ for identification purposes.

The likelihood function for the probit model is then expressed as:

$$L = \sum_{i=1}^{n} y_i \times log_e\,[\Phi(\mathrm{x}_i^*\boldsymbol{\beta})] + \sum_{\boldsymbol{i=1}}^{\boldsymbol{n}}(\mathbf{1} - y_i) \times log_e[1 - \Phi(\mathrm{x}_i^*\boldsymbol{\beta})] \qquad [3.4]$$

3.5.3 Diagnostics tests

Unlike ordinary least square (OLS), maximum likelihood estimators may be inconsistent when some econometric assumptions are violated (Hurd, 1979; Maddala and Nelson, 1975). Since the violation of a correct specification of normal distribution and homoscedasticity can potentially generate inconsistent mean estimates, these assumptions need to be tested rigorously in order to have confidence in the mean regression estimates. In order to investigate model adequacy, Chesher and Irish (1987) suggested the use of a residual based testing method known as an efficient score test. The test uses the normalised residual[16] to compute the Lagrange multiplier (LM) or score test. This variant of the LM testing principle can be used to test normality and homoscedasticity in addition to the functional form of the mean specification.

The score test statistics are based on the following expression:

$$LM = \mathbf{i}'\boldsymbol{R}(\boldsymbol{R}'\boldsymbol{R})^{-\mathbf{1}}\,\boldsymbol{R}'\mathbf{i} \qquad \sim \chi_p^2 \qquad [3.5]$$

which is distributed as chi-squared with p degrees of freedom. In this case, $\mathbf{i}'$ is an n×1 vector of 1s, and **R** is an n×q matrix of score contribution. **i′R** is a 1×q row vector of the sum of all the individual score contributions. **R′i** is the corresponding q×1 column vector. **(R′R)$^{-1}$** is the inverse of the variance-covariance matrix for the score contributions.

The resultant LM statistics test for the correct functional form through using the higher order terms (i.e., to a cubic term) of the standardised probit index. The test for homoscedasticity uses the original explanatory variables to provide a general heteroscedastic alternative, and the test for normality tests for departures from skewness and excess kurtosis in the generalised residuals.

3.5.4 Probit marginal and impact effects

The estimated probit coefficients can be interpreted in terms of their standardised probit index. It is generally more convenient to translate a probit regressor's coefficient into their

[16] The generalised residuals are also called the pseudo-residuals in the literature. These are obtained as the first order derivative of the log-likelihood function with respect to the probit model's constant term. The pseudo-residual is the difference between the actual outcome and the probit's predicted outcome. This difference is normalised, and then weighted by the probability that the index value actually occurs.

marginal and impact effects. The interpretation of the probit coefficients for continuous variables requires the calculation of marginal effects; these take the following form:

$$\frac{\partial \text{prob}(y_i=1)}{\partial x_k} = \frac{\partial \Phi}{\partial x_k} = \phi(x_i'\beta) \times \beta_k \qquad [3.6]$$

where $\Phi(.)$ denotes the cumulative distribution function and $\phi(.)$ is the corresponding probability density function for the standard normal distribution. The probability density function (pdf) translates the coefficient into probability points; this enables a probability point interpretation for a marginal effect in this case. In addition, the impact effects for the dummy variable are computed as:

$$\Delta = \Phi(x_i'\beta + \gamma) - \Phi(x_i'\beta) \qquad [3.7]$$

where γ is the corresponding parameter for the dummy variable. The computation of the impact effect within a probit assumes important relevance in the current application given the gender policy variables are all expressed in discrete binary form and are not continuous.

3.5.5 The Linear Oaxaca-Blinder Decomposition Analysis

Finally, in order to investigate the characteristics of firms that contributed to explaining the gender differentials in the credit market, we also adopted an Oaxaca-Blinder (OB) decomposition technique using a Linear Probability Model (LPM) rather than a probit model. The OB decomposition technique allows the mean differentials of an outcome variable between two groups to be decomposed into a part explained by observed characteristics (i.e., the endowment effect), and a part explained by differences in coefficients (i.e., the treatment effect); this may be due to discrimination or to any other unobserved differences between the groups. This decomposition technique has been applied mainly in the context of linear regression models. Fortin *et al.* (2010) argue that if a simple counterfactual is undertaken, the common support satisfied, and a weak ‘ignorability’ assumption made, the treatment and endowment components are identifiable. In the current case we use the OB decomposition with a LPM where the estimates are measured on a probability scale for ease of interpretation.

Using the linear decomposition approach (Blinder, 1973; Oaxaca, 1973), the model can be expressed as follows:

$$\bar{Y}_m = \bar{X}_{m'}\hat{\beta}_m \qquad [3.8]$$

$$\bar{Y}_f = \bar{X}_{f'}\hat{\beta}_f \qquad [3.9]$$

$$\bar{Y}_m - \bar{Y}_f = \underbrace{\left[\bar{X}_m - \bar{X}_f\right]'\hat{\beta}_m}_{endowment\ effect} + \underbrace{\bar{X}_{f'}\left[\hat{\beta}_m - \hat{\beta}_f\right]}_{treatment\ effect} \qquad [3.10]$$

where Y_j denotes application and success rates constructed as a dummy variable for the jth gender group. The overbar represents mean values, the circumflex denotes LPM coefficient estimates, and the subscripts “m” and “f” represent the male and female groups. This allows the overall average differential in loan application and success between the two gender-owned groups of enterprises to be decomposed into a part attributable to differences in characteristics (the endowment effect) and a part attributable to differences in coefficient (the treatment effect), which is taken to reflect the degree of unequal treatment or discrimination in the credit market. These two components have been referred to as the ‘explained’ and ‘unexplained’ components.

This approach requires the estimation of separate male and female equations. If we assume a male application or success rate structure in the absence of unequal treatment, the male group’s coefficient structure would prevail and the female coefficients would prevail in the absence of unequal treatment under a female structure. This highlights the “index number” problem as an identification problem relevant to this approach, where the estimates of the decompositions are sensitive to the coefficients (or weights) assigned to the gender-specific baskets of loan application and success determining characteristics. In response to this problem, Litchfield and Reilly (2011) suggested a desirable approach of presenting both estimates and assessing the level of sensitivity of these estimates. In this case, we believe that the male group will provide a more ‘trusted’ estimate in the absence of unequal treatment given that they are less likely to be affected by discrimination in the credit market. In addition, the 100% male group accounts for about 70% of the total sample, so it is reasonable to assume the male coefficient structure is the relevant benchmark in the absence of unequal treatment.

3.6 Empirical Results

Before proceeding to our econometric analysis, as a prelude we determine the correlation between the two outcome variables using a two-way contingency table. This exercise allows us to check whether there is any relationship between loan application and loan success in the absence of covariates. The two-way contingency coefficient value indicates that loan application is significantly correlated with the rate of loan success (Pearson chi 2(1) = 903.62, prob-value = 0.000). This finding offers tentative support[17] for the claim that the probability of borrowing from a formal financial institution is correlated with the firm's demand for loans and the bank's decision on access to credit. Therefore, the null hypothesis of independence of these two processes is decisively rejected by the data. However, the result only shows the correlation between the two outcome variables, without controlling for other factors. Given the potential for a correlation between the two models, we now use a bivariate probit model for the joint estimation of the application and success regression models after considering other factors that affect both loan application and loan success.

3.6.1 Bivariate probit model estimation

Table 3.5 presents the results for the bivariate model with partial observability initially used to jointly model loan application and success. Maximum likelihood estimation of the bivariate specification is straightforward, although there is an identification issue. As previously discussed in the methodology section, identifying the model's parameters is crucial, because poor identification can lead to misleading conclusions regarding the presence of selectivity bias. Poirier (1980) suggests that as long as there is one explanatory variable included in equation [3.1] and not in equation [3.2] and vice-versa, the key selection parameter is identified.

This inevitably means that identification is somewhat *ad hoc* in nature. In order to select the identifying variables, the relationship between the explanatory variables and the outcome variables is determined by standard tests of statistical significance. The purpose of the tests is to determine if the selected variables exerted an influence on either or both of the two outcome variables. The tests enabled the determination of variables in equation [3.1] that shifted the probability of application but not of success, and variables in equation [3.2] that shifted the probability of success but not of application. We then impose the

[17] It is important to note that the results of the contingency table only reveal the correlation between the two outcome variables, without controlling for confounding factors. The observed differences may be due to the importance of these confounding variables in explaining these outcomes, which emphasises the importance of undertaking econometric modelling in this case in order to control for the influence of such confounders.

relevant exclusion restrictions on the vectors $\boldsymbol{X}_i$ and $\boldsymbol{Z}_i$ in estimation in the light of these findings.

For the purpose of identification, statistically insignificant variables have been omitted from either the loan application equation or the success equation. The variables included in equation [3.1] (the loan application model) but not in equation [3.2] (the loan success model) are the policy variable (MSMEDF), lnsales variable, audit variable and sole trade variable. These variables are viewed as important determinants of application, but not loan success. The MSMEDF variable exerts a significant influence on loan application for firms located in states that participated in the programme but is not likely to affect the success rate of firms and therefore is omitted from the success equation (as it was not statistically significant). Lenders have no reason to consider whether or not a firm is located in states that participated in MSMEDF when making their decisions on loans. The sales variable influences loan application but is seen not to influence success rate and is omitted from the success equation as a consequence. A possible explanation for the insignificant effect of sales on success rather than application may be attributed to the fact that businesses do not report actual sales value for tax purposes. Therefore, banks are not inclined to use reported sales values as a determining factor for loan approval.

Another variable omitted from the success equation is the audit variable. The insignificant effect of the audit variable on success is not completely surprising because in Nigeria small business are exempted by law from having their financial statement statutorily audited. However, they are encouraged to engage an independent auditor to provide assurance that its financial reports are of high quality. Another reason may be that in developing economies, especially among small firms, businesses keep multiple books, which can result in a poor level of audit reporting. This practice may undermine the credibility of the reports and may result in financial institutions not using the reports for lending activities.

The sole trade variable is included in the loan application equation and omitted from the success equation. A possible explanation for the influence of sole trade in application and not success may reflect the correlation between audited account and business type. As sole proprietorship is exempted from auditing, banks may favour limited liability companies that have audited accounts over those that do not. It is acknowledged that the choice of identifiers here is somewhat *ad hoc* but the variables do appear to be fit for the purpose of identifying the selection effect in this bivariate probit application. In the current application, this bivariate probit approach provides the most obvious method to adopt here.

The excluded variables (exporter, retail and highsch_less) in equation [3.1] into a univariate probit model for equation [3.2] are important determinants of success and not application. The validity of the identifying restrictions is empirically explored by inserting the excluded variables in a univariate probit model for equation 3.2. This process confirmed the exclusion restriction criterion. On the assumption that this process of variable selection (although *ad hoc*) is valid, identification is achieved. A likelihood ratio test (LRT) was then used to determine whether or not the estimated correlation coefficient (rho) is statistically different from 0.

The likelihood ratio test fails to reject the null hypothesis of zero correlation (rho=0: chi2 (1) = 1.5359, prob-value = 0.2152. Therefore, the unobserved factors affecting the probability of loan application and success are uncorrelated in this case. As such, the bivariate probit model is not required for modelling these decisions. The two processes can then be modelled using separate standard univariate probit models.

Table 3.4: FIML Estimates for Loan Application and Success

	Application	Success
fem100	-0.1022	-0.0612
	(0.1071)	(0.3913)
Femmaj	0.1231	-0.6557
	(0.1778)	(0.4859)
Femmin	0.1081	-0.2035
	(0.0592)	(0.1163)
fem_mgt	-0.0148	0.2514
	(0.1309)	(0.4882)
MSMEDF	0.2792***	†
	(0.0217)	
Lnsales	0.0110	†
	(0.0107)	
age15	0.0762	0.2984
	(0.0902)	(0.3603)
Fixasset	0.2625***	0.8491
	(0.0970)	(0.5347)
Medium	0.0328	-0.2311
	(0.0830)	(0.2905)
Exporter	†	-0.0174***
		(0.0065)
sole_trade	0.2384***	†
	(0.0836)	
Audit	0.1763***	†
	(0.0745)	
Retail	†	0.5276
		(0.3997)
Services	0.1650*	0.1071
	(0.0903)	(0.3416)
Exper	-0.0371***	0.1781***
	(0.0155)	(0.0716)
exper2	0.0007***	-0.0003***
	(0.0003)	(0.0012)
highsch_less	†	-0.0032
		(0.0024)
Vocational	-0.0031***	0.0240
	(0.0012)	(0.0211)
Sample size	2,304	
Rho	-0.4512	
	(0.4858)	
LR test	1.5359	
Prob>chi2	0.2152	

Notes:

(a) Standard errors for the maximum likelihood estimated coefficients are reported in parentheses.

(b) * Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level

(c) † denotes omitted in estimation.

Source: Author's calculations using 2014 WBES data.

3.6.2 Univariate Probit Model Estimates

Table 3.5 provides the probit estimation results for both application and success models, and the estimates reported are the probit marginal/impact effects. Both models satisfy the key econometric assumptions inherent in the probit model at the 5% level of statistical significance; this provides confidence that the regression model mean estimates are consistent and efficient. Column 1 provides the estimates for the marginal and impact effects of the loan application model, while column 2 reports the estimates for the marginal and impact effects of the loan success models.

In order to obtain a clean estimate of the gender effect, the other explanatory variables allow us to control for the factors that determine the ability of firms to participate in making credit applications and be successful in loan applications. As expected, the sign and significance of the coefficients are consistent with other studies. The firm's age was found to be an important factor in loan application and success as the probability of older firms, aged 15 years and above, is found to be 6 percentage points higher for loan applications, and 4 percentage points more likely to be successful than younger firms (i.e., those aged 14 years and less). This may be due to the fact that younger firms lack collateral and loan track records, limiting their ability to access credit from a formal financial institution.

Audited firms are found to be associated with exerting a significant positive influence on both application and success, after controlling for other factors. The result indicates that having an audited financial account significantly increases a firm's probability of applying for a loan by 6 percentage points, and loan success by 5 percentage points. Banks are more inclined to favour firms that have audited accounts, because it informs on their quality and transparency. The result shows that an audited account is still relevant for SMEs in Nigeria, even though sole trade companies, which account for 78% of our sample (see Table 3.1), are exempt from the statutory auditing of their financial statements.

The results also suggest that the acquisition of human capital of a firm's owners, as reflected in a higher educational level, affects both the loan application and success rate. A manager with a high school education is less likely to apply and be successful for a loan than managers with a university education. This finding is in comport with that reported in previous studies, suggesting that tertiary education or professional training provides managers with broad skills and capabilities that are instrumental in making a convincing loan application (Kasseeah and Thoplan, 2012), and providing loan-financed project documentation that meets banking requirements. Highly educated firm owners are better positioned to secure bank loans than less educated owners. Bank lenders use education as a positive signal, enhancing firm success in securing credit if owners are highly educated (Coleman, 2004).

Even though the estimated coefficients for the MSMEDF variables are statistically significant in loan application and success, an interaction term between the four female variables and the programme variable for firms located in the states that participated in the MSMEDF credit scheme yield no gender differences[18] for female-led firms in MSMEDF participating states for loan success. This appears to suggest that one of the key objectives of this particular intervention, which was to enable and empower female entrepreneurs, was not met by the programme.

The empirical results reveal that a firm with fixed assets that can be used for collateral purposes is 12 percentage points more likely to apply for a loan and 14 percentage points more likely to have their loan approved than a firm that does not possess such an asset. Usually, banks require a fixed asset as security for the loan, reducing the bank's potential losses and discouraging moral hazard behaviour (Berger and Udell, 1998). A business without pledged collateral has limited leverage with a formal lender. As expected, collateral assets appear to have an influence on loan application and success for SMEs in Nigeria. However, the data here indicate an interesting result. The summary statistics revealed that only 17% of firms pledged collateral in the form of a fixed asset, but more than 30% (see Table 3.1) of firms that applied for loans were successful. This suggests that fixed assets may not be the only collateral required by lenders. Therefore, collateral may be provided in other ways (e.g., third-party guarantees and relationship lending).

Interestingly, while firms with high sales volume have higher loan application rate, success rate is not influenced by sales. A firm with a high sales turnover is more likely to apply for a line of credit. This result may be attributed to firms under reporting their sales values for the purpose of taxation. Therefore, financial institutions may not rely on sales figures when considering loan applications.

Surprisingly, firms in the retail and services sector are more likely to have better access to credit than manufacturing firms. A possible explanation for this could be that

[18] For brevity, the result is not reported in the analysis.

manufacturing firms may require a fixed investment in machinery and equipment and this requirement increases their need for long-term financing, which is considered riskier than short-term loans. In contrast, services-based businesses and retail firms are not capital intensive and may simply require short-term loans to finance their daily activities.

A somewhat puzzling finding is that exporting firms exhibit a negative and statistically significant result for success and not application. This result is not consistent with what is found in the existing literature. However, one possible explanation could be that, since we are dealing mainly with small and medium enterprises, banks perceive exporting SMEs as high risk. An exporting firm that is either a small or medium firm may not absorb an adverse negative shock associated with export activities (e.g., exchange rate volatility). Caggese and Cuñat (2013) find that new and small firms have a higher probability of default after a negative shock, since firms that start exporting only after incurring a fixed trading cost. In the event of credit constraints, this fixed cost drains reserves and increases the risk of bankruptcy.

We now turn to the key gender variables of interest. After controlling for a set of firm characteristics and other managerial attributes, no statistically significant differences emerge for our three measures of female-owned and female-managed firms compared to male-owned firms regarding loan application and success. The female dummy variables used did not enter significantly in any regression models, even when interacted with the policy variable MSMEDF.

Overall, the results of the univariate analysis revealed that there is no evidence of gender differences in credit market outcomes. Barsky *et al*. (2002) argued that statistically insignificant results of discrimination studies could result from non-overlap in group characteristics, under which standard regression analysis may yield uninformative results. However, there appears to be no evidence of such non-overlap in the current context.

Table 3.5: Estimates of Univariate Probit Model for Application and Success Rates

	Marginal/Impact Effects **Application**	Marginal/Impact Effects **Success**
fem100	0.0273 (0.0314)	-0.0429 (0.0273)
Femmaj	0.0154 (0.0388)	-0.0142 (0.0352)
Femmin	0.0990 (0.0657)	0.0995 (0.0679)
fem_mgt	-0.0007 (0.0219)	0.0091 (0.0335)
MSMEDF	0.1420*** (0.0217)	0.0886*** (0.0199)
Lnsales	0.0088*** (0.0038)	0.0035 (0.0036)
age15	0.0555*** (0.0240)	0.0447*** (0.0220)
Fixasset	0.1166*** (0.0276)	0.1396*** (0.0274)
Medium	-0.0070 (0.0219)	-0.0049 (0.0200)
Exporter	-0.0647 (0.0432)	-0.1886*** (0.0254)
sole_trade	0.1207*** (0.0294)	0.0840*** (0.0254)
Audit	0.0554*** (0.0252)	0.0486*** (0.0255)
Retail	0.0388 (0.0280)	0.0647*** (0.0270)
Services	0.0417* (0.0252)	0.0596*** (0.0237)
Exper	0.0020 (0.0033)	0.0025 (0.0031)
exper2	-0.0001 (0.0001)	-0.0001 (0.0001)
Highsch_less	-0.0502*** (0.0243)	-0.0748*** (0.0217)
Vocational	-0.0532 (0.0353)	-0.0748*** (0.0217)
N	2,304	1,217
Pseudo – R^2	0.0442	0.0442
Normality	$\chi^2_{(2)} = 5.291[0.071]$ [d]	$\chi^2_{(2)} = 4.812[0.090]$ [d]
Functional form	$\chi^2_{(3)} = 6.498[0.090]$ [e]	$\chi^2_{(3)} = 4.960[0.175]$ [e]
Homoscedasticity	$\chi^2_{(18)} = 20.148[0.325]$[f]	$\chi^2_{(18)} = 26.825[0.086]$[f]

Notes:
a. Standard errors in parentheses. b. (*) dx/dy is for discrete change of dummy variable from 0 to 1.
c * Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. d. p-value under the null of normality. e. p-value under the null of appropriate functional form. f. p-value under the null of homoscedasticity.

Source: Author’s calculations using 2014 WBES data.

3.6.3 The Oaxaca-Blinder Decomposition results for the LPM

Panel A of Table 3.6 reports the estimates for the gender decomposition in loan application and success rates. The estimated effects for the regressions for male and female groups are generally signed as anticipated and have plausible magnitudes. The estimates are not discussed in detail, although some points are worth noting about the regression estimates. The estimated coefficients are relatively well determined in the application equation for the male group. The results reveal that the MSMEDF policy raises the probability of exclusively male-owned firms increasing loan application rates by 16 percentage points greater in magnitude compared to the female-owned firms. However, the differential in point estimates across these two gender groups is not statistically significant at a conventional level, again suggesting no gender differential in loan take-up.[19] The experience variable suggests an interesting pattern, revealing an inverted U-shaped relationship. The estimated linear effect of experience on the application for female-owned firms is positive, and the quadratic term is negative, suggesting an inverted U-shaped relationship between loan application and experience, with the turning point around 18 years of the age of the firm for female-owned firms.

The foregoing results reveal that when the sample is split between male-owned and female-owned firms, some interesting findings emerge. In the pooled sample (see Table 3.5), fixed assets yield a significant determinant of both application and success. The split sample reveals that the male-owned firms are more likely to be successful if they have collateral in the form of a fixed asset. On the other hand, both male and female exporters are less likely to be successful.

[19] The estimated t-ratio associated with the test of this proposition is 1.20 in absolute terms.

Table 3.6: Decomposition of Gender Differences in Loan Application and Success Rates

	Application		Success	
	Male	**Female**	**Male**	**Female**
Panel A: Parameter estimates of Linear Oaxaca Decomposition				
MSMEDF	0.1569***	0.0909*	0.0243	-0.0068
	(0.0256)	(0.0505)	(0.0354)	(0.0694)
Lnsales	0.0092***	-0.0001	-0.0032	-0.0095
	(0.0044)	(0.0079)	(0.0064)	(0.0104)
age15	0.0510***	0.0112	0.0035	-0.0084
	(0.0278)	(0.0537)	(0.0373)	(0.0733)
Fixasset	0.0732***	0.1621***	0.1887***	-0.0770
	(0.0334)	(0.0615)	(0.0396)	(0.0790)
Medium	0.0027	-0.1117	-0.0406	0.1806***
	(0.0254)	(0.0493)	(0.0343)	(0.0668)
Exporter	-0.0615	0.0392	-0.3623***	-0.3770***
	(0.0495)	(0.1022)	(0.0709)	(0.1195)
sole_trade	0.1267***	0.0940	0.0074	0.0107
	(0.1500)	(0.1421)	(0.0457)	(0.1974)
Audit	0.0488	0.1164**	0.0289	0.0617
	(0.0320)	(0.0600)	(0.0435)	(0.0749)
Retail	0.0202	0.0496	0.0800*	0.0112
	(0.0324)	(0.0620)	(0.0448)	(0.0830)
Services	0.0245	0.0682	0.0235	-0.0101
	(0.0255)	(0.0593)	(0.0402)	(0.0783)
Exper	-0.0018	0.0106*	0.0071	0.0012
	(0.0040)	(0.0061)	(0.0058)	(0.0118)
exper2	0.0001	-0.0003***	-0.0002	-0.0001
	(0.0001)	(0.0001)	(0.0002)	(0.0003)
highsch_less	-0.0438	-0.0476	-0.1169***	-0.0797
	(0.0282)	(0.0557)	(0.0401)	(0.0765)
Vocational	-0.0714*	-0.0644	-0.1022*	-0.2158**
	(0.0406)	(0.0852)	(0.0574)	(0.1121)
Sample Size	**1,607**	**422**	**829**	**242**
Panel B: Linear Oaxaca Decomposition Estimates		**Application**		**Success**
Estimated gap		0.0576***		0.0876***
		(0.0278)		(0.0381)
Explained part (endowment effect)		0.0240		-0.0201
		(0.0262)		(0.0392)
Unexplained part (treatment effect)		0.0336		0.1078***
		(0.0369)		(0.0392)

Notes:
(a) Male and female estimates refer to 100% owned firms only.
(b) Estimates of linear OB decomposition assumes male coefficient structure in panel B.
(b) Standard errors in parentheses.
(c) * Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level.

Source: Author’s calculations using 2014 WBES data.

Panel B in Table 3.6 supports our empirical findings' robustness on gender gaps, as measured by a restrictive sample of 100% male-owned and female-owned firms. Column 1 reports the decomposition estimates for the application model. In raw terms, the estimated gap between the male and female group suggests that male-owned firms have a 5.8 percentage point higher probability of loan application than their female counterparts. However, this raw gap could be misleading, as the disaggregated effect into explained and unexplained parts shows there is no statistically significant effect when we assume a male set of coefficients in the absence of unequal treatment.[20] This result suggests no differences between male and female-owned firms in loan application once we control for all the characteristics and confounding variables. The result is consistent with the findings in the regression analysis for the bivariate and univariate probit models.

Moving on to the decomposition estimates for the success model reported in column 2 of panel B, the point estimates suggest male-owned firms have an 11 percentage points higher probability of obtaining a loan than female-owned firms. This gap is not explained by differences in observed characteristics and is entirely due to unequal treatment.

The key finding from the OB analysis is that there is a statistically significant treatment effect suggesting a female disadvantage with respect loan success. This was not detected in the earlier pooled analysis and emphasises the importance of allowing the estimated effects to differ across the gender ownership status of the SMEs.

Table 3.7 reports the results of the linear Oaxaca Decomposition estimations of having considered a number of variations in the construction of the dependent variables used. Related to female firms, the definition uses the sample of majority female owned firms, these include only 100% female owned, 50% female owned firms and female managed firms. Similar to what is found when using the 100% female and male sample in Table 3.6, most of these differences are attributed to a treatment effect for the success model. The gender discrimination effect reduces in magnitude when the restrictive sample size is relaxed to include 50%-owned and female managed firms. The results for the linear Oaxaca-Blinder decomposition using these alternative female firm definitions remain unchanged. Therefore, the result is not sensitive to redefining the key set of dependent

[20] We also decomposed the loan application and success gaps under the assumption that the female coefficients prevailed in the absence of unequal treatment. The results suggest relatively modest evidence of an "index number" problem. However, we assume the male coefficient structure as the relevant benchmark in the absence of unequal treatment.

variables. The result confirms the key finding that firms with majority female ownership exhibit a gender gap due to unequal treatment.

Table 3.7: Linear Oaxaca Decomposition Estimates for Majority Female Owned Firms & Female Managed versus Majority Male Owned & Male Managed Firms

	Application		Success	
	Male	**Female**	**Male**	**Female**
Panel A: Parameter estimates of Linear Oaxaca Decomposition.				
MSMEDF	0.1512***	0.1143***	0.0207	0.0046
	(0.0258)	(0.0381)	(0.0358)	(0.0539)
lnsales	0.0093***	-0.0078	-0.0005	-0.0110
	(0.0044)	(0.0062)	(0.0064)	(0.0091)
age15	0.0459**	0.0670**	0.0273	0.0267
	(0.0282)	(0.0402)	(0.0381)	(0.0549)
fixasset	0.0780***	0.1885***	0.1834***	0.0163
	(0.0332)	(0.0452)	(0.0397)	(0.0605)
medium	0.0208	-0.0716**	-0.0324	0.0603
	(0.0256)	(0.0373)	(0.0345)	(0.0524)
exporter	-0.0655	-0.0309	-0.3387***	-0.3053***
	(0.0495)	(0.0726)	(0.0710)	(0.1045)
sole_trade	0.1106***	0.0934***	0.0118	0.0288
	(0.0309)	(0.0444)	(0.0443)	(0.0674)
audit	0.0426	0.0599	0.0233	0.0527
	(0.0322)	(0.0431)	(0.0440)	(0.0579)
retail	0.0277	0.0472	0.0918***	0.0644
	(0.0326)	(0.0490)	(0.0450)	(0.0689)
services	0.0252	0.0848	0.0418	-0.1001**
	(0.0294)	(0.0422)	(0.0405)	(0.0584)
exper	-0.0019	0.0085*	0.0082	-0.0091
	(0.0041)	(0.0053)	(0.0060)	(0.0089)
exper2	0.0001	-0.0002**	-0.0002	0.0002
	(0.0001)	(0.0001)	(0.0002)	(0.0002)
highsch_less	-0.0355	-0.0851***	-0.1064***	-0.0623
	(0.0285)	(0.0423)	(0.0404)	(0.0620)
vocational	-0.0672*	-0.0152	-0.0780	-0.1048
	(0.0410)	(0.0619)	(0.0575)	(0.0866)
Sample Size	**1,594**	**710**	**829**	**242**

	Application	**Success**
<u>Panel B</u> Linear Oaxaca Decomposition estimates:		
Estimated gap	0.0366 (0.0228)	0.0564** (0.0313)
Explain part (endowment effect)	0.0198 (0.0184)	-0.0101 (0.0123)
Unexplained part (treatment effect)	0.0168 (0.0227)	0.0666*** (0.0317)

Notes:
(a) Combined sample of majority female firms include 100% female, 50% female and female managed firms.
(b) Standard errors in parentheses.
(c) * Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level.

Source: Author’s calculations using 2014 WBES data.

Building on the empirical work in Chapter 2, the impact of the MSMEDF policy is further investigated by the assignment of the gender differential into two components using the decomposition technique.

Table 3.8 reports the result of the treatment and endowment effects using either the male or female sample. As earlier stated, one of the objectives of the policy was supposed to make 60% of the fund available to female-owned firms. This analysis reveals that, contrary to the expectation of the programme, the MSMEDF policy did not increase access to credit for female-owned firms. Neither observed characteristics and treatment effects are important in explaining the observed gender differential in our outcomes of interest.

Table 3.8: Decomposition of Gender Differential in MSMEDF Participating States

	Application		**Success**	
	Male	**Female**	**Male**	**Female**
Explained part (endowment effect) of which MSMEDF:	0.0017 (0.0022)	-0.0038 (0.0028)	0.0035 (0.0032)	-0.0061 (0.0043)
Unexplained part (treatment effect) of which MSMEDF	-0.0289 (0.0275)	0.0310 (0.0295)	-0.0345 (0.0296)	0.0371 (0.0318)
Sample Size	**1,607**	**422**	**829**	**242**

Notes:
(a) Male and female estimates refer to 100% owned firms only.
(b) Linear OB decomposition estimates using male and female coefficients.
(b) Standard errors in parentheses.

Source: Author’s calculations using 2014 WBES data.

3.7 Summary, Policy Implications and Conclusions

This study focuses on gender differences in loan application and success. Given that bank loan success depends on the application, which constitutes a self-selected sub-sample of successful firms, this poses a potential problem of selection bias in the econometric modelling. In order to investigate this econometric issue, we used a bivariate probit with partial observability to control for selectivity bias and, jointly, model loan application and loan success. We then estimated the two equations jointly. The bivariate probit estimates reveal no correlation between the error terms in the two equations. Therefore, there is no evidence of selectivity bias, supporting the assumption of independence in the unobservables between the two processes and separate estimation of the two equations by two univariate probit models is permissible in this case. The results of the bivariate and univariate probit analysis show no evidence of unequal treatment in loan application and success. The results reveal that female entrepreneurs do not face unequal treatment in the loan applications and, when they apply, lenders do not appear to discriminate against them.

However, these estimates are based on pooled regression models that rely on a gender intercept term to inform for the presence or not of unequal treatment. We argue that the separation of the sample by gender group is necessary to discern whether important differences in lending outcomes actually exist. For this reason, we assume that the use of 100% female-owned and 100% male-owned firms is conceptually more persuasive to inform on unequal treatment. If present, the former group of firms will exhibit the strongest evidence for gender disparity. The next step was using an appropriate technique that will allow variation in effects across gender other than in terms of intercepts.

Given the univariate and bivariate probit analysis does not allow for differences in estimated effects across gender, the Oaxaca-Blinder decomposition technique using linear probability models was then employed as a means of analysing the differences in outcomes between groups, male owned firms and female owned firms in our case. The decomposition analysis is important because it allows the processes determining loan application and success to be different across the gender ownership status of the SMEs.

The OB decomposition analysis reveals that male-owned firms have about a 10 percentage point higher probability of obtaining a loan than female-owned firms. The result supports unequal treatment in bank loan success in the credit market. The gender gap in bank loan success is not explained by differences in the observed characteristics of firms but can be interpreted as gender-based discrimination in the credit market. The negative effect of gender differences in loan success is consistent with most findings in the literature survey (see Table A3.1 in the appendix to this chapter).

The estimates of the OB also revealed that the MSMEDF policy was not effective in increasing women's participation in credit markets as against the objective of making 60% of the fund available to female-owned firms in order to increase their access to credit. The analysis did not reveal any significant result of the MSMEDF policy when the gender differences were decomposed into its treatment and endowment effects. This result may be taken to suggest that, although gender development has been emphasised in the policy document, because of the existing unequal treatment in the market, the policy could not make much difference in loan success for female-owned firms. Therefore, the programme achieved its objective of increasing loan take-up by firms (see Chapter 2) but was not able to achieve its objective of increasing access to credit for female-owned firms.

Our preferred set of estimates is based on the OB decomposition as it permits separation of sub-samples. Overall, the results based on these procedures have some important policy implications. The key findings based on the OB suggest that unequal treatment is not an issue of credit market participation, since we find the unequal treatment is determined not at the application stage but at the loan approval stage. The empirical analysis therefore indicates the gap is more a supply-side problem than a demand-side phenomenon. Since we found differences in the estimated coefficients to be important for success, it may be the case that loan officers' prejudices affect decisions on loan approvals. The theory of taste-based discrimination suggests that financial provider preferences and cultural beliefs about gender may therefore hinder access to credit for female entrepreneurs (e.g., see Muravyev *et al.*, 2009). Therefore, the coefficient differences could be attributable to the behaviour of lending officers, the overwhelming majority of whom are male. In acknowledging this problem in 2014, the CBN directed that 40% and 30% of top management and board position in banks be reserved for women respectively. However, 7 years after the directive, only 7 women are managing directors of the 24 banks in the country, and only 21% of women occupy board or management positions in the banks (Ogwu, 2021).

A reform likely to increase a favourable outcome of loan applications is one that entails a sizeable increase in the number of female loan officers. This would help ensure that female borrowers are not affected by loan officers' prejudice in a male dominated sector. Therefore, there is a need for the CBN to increase implementation of the gender equity threshold in banks. Although the CBN Act did not give the authority to enforce gender parity on bank boards, moral suasion can still be utilised to make commercial banks do the CBN's bidding. In addition, loan officers need to be much more mindful and sensitive to gender issues and their potential to be discriminatory.

Appendix to Chapter 3

Table A3.1: Empirical Studies on Gender Discrimination in Participation and Credit Access

YEAR	AUTHOR	COUNTRY	DATA	METHODOLOGY	GENDER	FINDING	
Presence of gender discrimination							
						Participation	Credit Access
2016	Aristei, & Gallo	Europe	Business Environment and Enterprise Performance Survey (BEEPS), 2012.	Bivariate Probit and Oaxaca–Blinder decomposition.	At least one female owner. Female manager Manager/female owner 50% female owned Key role in management and ownership 0/1.	Negative effect	Negative effect
2013	Asiedu *et al.**	Sub-Saharan Africa, Latin America and Caribbean, East Asia and Pacific, and East and Central Europe	World Bank Enterprise Survey, 2006-2011. World Development Indicators, 2006-2011.	Ordered probit, probit model, OLS, conditional logit and iteratively reweighted least squares (IRLS).	At least one female owner 0/1	Not studied	Negative effect
2013	Aterido *et al.*	Sub Saharan Africa	World Bank Enterprise Survey,2005-2009.	Probit and Oaxaca–Blinder decomposition	At least one female owner. 100% owned. female manager 0/1.	Not studied	Negative effect
2011	Bardasi *et al.*	Eastern Europe, Central Asia, Latin America and Sub-Saharan Africa.	Business Environment and Enterprise Performance Survey (BEEPS), 2005. World Bank Enterprise Survey, 2006-2007.	OLS and Multinomial Logit.	At least one female owner 0/1.	Not studied	No effect

Continued on next page

Table A3.1: Empirical Studies on Gender Discrimination in Participation and Credit Access (continued from previous page)

YEAR	AUTHOR	COUNTRY	DATA	METHODOLOGY	GENDER	FINDING	
2020	Chaudhuri *et al.*	India	Census data provided by the Ministry of Small-Scale Industries, Government of India.	Logit model and nonlinear decomposition technique.	100% female-owned Female manager Female owner/manager	Not studied	Negative effect
2014	Hansen, & Rand*	Sub-Saharan Africa,	Investment Climate Assessment (ICA) surveys, 2006-2007.	Logit and generalised Oaxaca–Blinder decomposition	At least one female owner 0/1	Not studied	Positive effect
2017	Moro *et al.*	Europe	Survey of Access to Finance of Enterprise (SAFE). Bank Lending Survey (BLS).	Logit and Heckman sample selection.	Female Manager 0/1.	Negative effect	No effect
2009	Muravyev *et al.*	Eastern Europe and Central Asia.	Business Environment and Enterprise Performance Survey (BEEPS), 2005.	Probit, OLS and Heckman selection.	Majority female owner 0/1.	Not studied	Negative effect
2015	Nwosu *et al.**	Nigeria	World Bank Enterprise Survey, 2010.	Probit, PSM and Oaxaca–Blinder decomposition.	Female sole prop. Maj. shareholder 0/1	Not studied	No effect
2018	Pham and Talavera	Vietnam	NU-WIDER survey of Micro, small and medium enterprise.	Probit model with Heckman selection	Female owner	No effect	Positive effect
2014	Piresbitero *et al.*	Barbados, Jamaica, and Trinidad and Tobago.	Finance and Gender Issues in the Caribbean Survey (FINGEN), 2012.	Probit and Blinder-Oaxaca Decomposition.	WOB[a] WMB[b] WO&MB[c] WLB[d]	Positive effect	Negative effect
2017	Wellalage and Locke*	South Asia	World Bank Enterprise Survey, 2014.	IV-probit model and Fairlie Nonlinear decomposition	At least one female owner. Female Manager 0/1.	Not studied	Positive effect

a. A firm has all or predominantly women among owners.
b. A firm has all or predominantly women among managers.
c. A firm has all or predominantly women among managers and owners.
d. The firm largest shareholder/owner is a woman and she make major strategic and financial decisions.

Source: Author's compilation of empirical literature.

Table A3.2: Description of Variables

Variable Name	Variable Description
application	A dummy variable that equals 1 if the firm applied for credit from a formal financial institution, equals 0 otherwise
success	A dummy variable that equals 1 if the firm secured credit from a formal financial institution, equals 0 otherwise
MSMEDF	A dummy variable equals 1 if the firm is located in the states that participated in the MSMED Funds, and equals 0 otherwise
age14	A dummy variable equals 1 if age<15 years, and zero otherwise.
age15	A dummy variable equals 1 if age>=15 years, and zero otherwise.
lnsales	Log of total sales
exper	The total number of years of experience of the top firm manager
sole_trade	A dummy variable that equals 1 if the firm is a sole proprietorship, and equals 0 otherwise
manufacturing	A dummy variable that equals 1 if the firm is in the manufacturing sector, and equals 0 otherwise
retail	A dummy variable that equals 1 if the firm is in a retail sector, and equals 0 otherwise
services	A dummy variable that equals 1 if the firm is in the services sector, and equals 0 otherwise
Small	A dummy variable that equals 1 if the firm is a small firm (<=19), and equals 0 otherwise
medium	A dummy variable that equals 1 if the firm is a medium firm (>=20 and <=99), and equals 0 otherwise
fixasset	A dummy if the firm has a fixed asset that may be accepted as collateral for a bank loan, equals 0 otherwise
audit	A dummy variable that equals 1 if the firm has its financial statement audited by an external auditor, and equals 0 otherwise
exporter	A dummy variable that equals 1 if the firm is an exporting firm, and equals 0 otherwise
male100	A dummy variable that equals 1 if female ownership is 0%, and equals 0 otherwise.
fem100	A dummy variable that equals 1 if female ownership is 100%, and 0 equals otherwise
femmaj	A dummy variable that equals 1 if women ownership is between 50-99%, and equals 0 otherwise
femmin	A dummy variable that equals 1 if women ownership is between 1-49%, and equal 0 otherwise.
fem_mgt	A dummy variable that equals 1 if the top manager is a woman, and equals 0 otherwise
highsch_less	A dummy variable that equals 1 if the top manager has high school education, and equals 0 otherwise
vocational	A dummy variable that equals 1 if the top manager has a vocational education, and equals 0 otherwise
university	A dummy variable that equals 1 if the top manager has a university education, and equals 0 otherwise

Source: Described by author based on WBES data (2014).

Chapter Four - The Impact of Unanticipated Shocks on Household Welfare in Nigeria (Essay 3)

4.1 Introduction

Providing an insight into the impact of shocks on household welfare has been the subject of much empirical research. Shocks are unanticipated events that occur in an economy that potentially have sizeable impacts on the affected individual or larger population, causing significant and sudden welfare loss. Shocks are categorised into two broad categories, idiosyncratic and covariate shocks (Krueger *et al.*, 2016). Idiosyncratic shocks are personal to households and individuals and largely affect a particular household through death, disability, and illness, or the unemployment of household members. In contrast, covariate shocks are location specific and affect many people concurrently in the same location or community. These shocks include, for example, floods, drought, erosion, conflict, or diseases that affect livestock and crops. Some types of covariate shocks, such as macro-economic shocks, are also spatial in nature. The effects of such shocks are not limited to a particular geographical location but can spread contagion to many countries across the world, plunging economies into macro-economic crises; examples include the Great Depression of the 1930s or the 2008 financial crisis known as the Great Recession. When a macro-economic crisis occurs, households incur its effects through several channels. These can be traced back to the various indicators of well-being and the prices to which a household is exposed when purchasing goods and services. An important impact on households is via relative price changes (Ferreira *et al.*,1999). Shocks such as declines in Gross Domestic Product (GDP), exchange rate depreciation, commodity price changes, and generally high inflation rates, all affect relative prices. Many developing countries have experienced such shocks and hence such crises over the last two decades or so.

Between 2014 and 2016, Nigeria witnessed its first major recession in 25 years, which was driven by sharp decline in global oil prices. The period was characterised by a sharp decline in GDP and major fluctuations in macro-economic indicators with substantial increases in food prices. Food price inflation rose from 9.2% in 2014 to 19.4% in 2018; this was partly due to the removal of subsidy by the Nigerian government. The causes of the food price hike and volatility are still the subject of debate among researchers. However, its potential welfare impact on households has attracted attention among researchers and policy-makers alike, especially in a country where food expenditure represents a substantial share of total household expenditure.

The purpose of the current study is to examine the impact of food price shocks on an array of household welfare measures. In order to obtain a deeper understanding of the welfare effect of food price shocks, it is also important to examine their effects in combination with other shocks. This is because households are prone to various forms of environmental, personal and economic shocks that affect their welfare and increase their vulnerability to poverty. In order to investigate this theme, the study exploits the Nigerian 2018/2019 General Household Survey (GHS) data that contain detailed information on types of economic shocks experienced by households. Therefore, the analysis uses household-level responses to questions from the survey's economic shock module to construct three types of shock: personal, financial and local. In contrast to the existing literature, the study measures the intensity of shocks, constructed as a count variable.

The empirical findings reveal that among the three types of shocks, financial shocks exert the most negative influence on household welfare measures. A financial shock is the most persistent felt shock across the different metrics (food expenditure, non-food expenditure, household assets and the household savings rate) and across the unconditional distribution of the metrics used. This result provides some empirical insight into the household-level effects of the sharp increase in inflation rates recorded between 2016 and 2019, which is within the reporting period for the shock data used in this analysis.

This study is of particular importance for a number of reasons. First, although a significant number of studies have investigated the impact of shocks on household welfare measures, most studies often assume an homogenous relationship between shocks and welfare measures, using standard OLS mean regression techniques. Although there are existing studies on the distributional welfare analysis of the impact of shocks in other countries, this has scarcely been undertaken for Nigeria. The current study focuses on the distributional impact of shocks on welfare measures using unconditional quantile regression techniques. Households in developing countries like Nigeria are confronted with a variety of unanticipated shocks, which may have significant effects on their welfare. The insights obtained on the distributional effect of shocks on household welfare are essential for the design of mitigating policies.

Second, this paper contributes to the ongoing debate over the impact of shocks on household welfare by focussing on a novel angle: the distinction between food and non-food expenditure. Most studies that have analysed the impact of shocks on consumption expenditure have examined aggregate household expenditure. However, household spending is significantly different along the dimensions of food and non-food items, with the consumption pattern of most developing countries skewed towards the food item. For example, households in a developing country like Nigeria in 2019 spent 57% of their total household expenditure on food with the remainder spent on non-food items (National Bureau of Statistics, 2020). It is also possible that spending on food and non-food items will have differential effects across household types, as low-income households spend a larger part of their income on food. In any event, given that food expenditure occurs more frequently than non-food expenditure, an aggregated analysis may tend to distort overall welfare impacts. The ability to separate these two effects may be important for the design of policy responses.

Furthermore, most studies on the welfare impact of food price shocks confine their analysis to substitution effects among food groups (e.g., grains, vegetables, fruits, meats) without taking into account non-food consumption groups (i.e., consumer durables, health and education). As pointed out by Avalos (2016), households substitute between food and non-food consumption to minimise the negative welfare effects of an increase in food prices, as food prices increase not only at different rates among the food groups, but also in proportion to many non-food groups. Therefore, it is necessary to separate the total consumption expenditure into food and non-food expenditure components to determine whether there exists any disparity between the two. A disaggregated analysis can also offer better insights into understanding the welfare impact of shocks on both rich and poor households, whose consumption pattern tends to differ.

This study goes a step further to examine not only the impact of food price hikes on food and non-food expenditure, but also on household assets and savings. A considerable body of the literature on savings explores the concept of precautionary savings. It is commonly believed that households save because they provide resources that can be used to protect against shocks. These resources provide a buffer to manage unforeseen and unusual expenses that current income cannot support. On the other hand, households may decide to consume more, drawing down on their savings during periods of economic shocks. Our study provides an insight into which shocks trigger a household’s response in drawing from savings when faced with an unanticipated shock. In addition, we also explore the role of household assets as shock absorbers and the extent to which they provide some resilience to households in the event of a shock impacting a household.

The question of whether, and to what extent, heterogeneity in shocks (personal, financial and local) impacts food and non-food expenditure, household asset depletion, household savings, and food poverty, comprise important and under-researched questions that this chapter attempts to address.

The structure of the chapter is now outlined. The next section provides the contextualisation for the empirical analysis. This is followed by a literature review of existing studies on shocks. This is then followed by a section discussing the data, and an empirical methodology section. A penultimate section reports the empirical results and this is followed by a section containing a discussion of the results and the policy implications. A final section offers some concluding remarks.

4.2 Context

Like most developing economies, Nigeria faces diverse shocks that render the livelihoods of individuals, households, and communities within the country vulnerable. These shocks range from slow-onset local shocks, such as droughts, floods, and pest infestation that affect large numbers of households and individuals, to smaller-scale personal shocks that directly affect fewer individuals and livelihoods, such as job loss, business failure or death in the household. There are also socio-political shocks, such as conflict, civil unrest, and insurgency, that are invariably triggered by a single cause or combination of underlying economic factors. In addition, there are country-level shocks caused by macro-economic or exchange rate fluctuations in the economy that lead to price instability. Indeed, changes in the global oil and food prices have generally been viewed as the primary source of macro-economic fluctuations. These two price changes significantly impacted the Nigerian economy because of its core dependence on agriculture and oil production. Nigeria has witnessed several macro-economic shocks at each stage of its development, usually characterised under sub-periods representing an era before the introduction of the structural adjustment programme (SAP), during the SAP period, and then over the post-SAP period.

The administrative structure of Nigeria consists of 36 states, including Abuja (Federal Capital Territory), and is divided into six main geo-political zones: North-Central, North-East, North-West, South-East, South-South and South-West. Before discovering crude oil in commercial quantities in 1956, the country was known for its agrarian economy, exporting commodities such as cocoa, palm oil, rubber, and groundnuts. The dominance of agriculture characterised this earlier period of the economy. The 1962-1968 development plan, the first national plan, emphasised the introduction of specialised agriculture development schemes to improve food production and export-led growth. The country was then delineated into three regions, the Western, Northern and Eastern regions, to enable the specialisation in the production of commodities in which these areas had a comparative advantage. The Western Region specialises in the production of cocoa, groundnut pyramids in the Northern Region and palm oil production in the Eastern Region. These cash crops became the primary source of foreign exchange earnings for the economy. During this period, Nigeria became the world's largest exporter of groundnut, the second-largest exporter of cocoa and palm produce, and a major exporter of rubber and cotton. The country was self-sufficient in food production, and the agriculture sector contributed about 65% to GDP and over 70% of total exports. This period witnessed a relatively stable exchange rate, low inflation and unemployment rates.

The contribution of agriculture began a downward trend in the oil boom period as the economy diverted its attention away from agriculture. The production of food for local consumption and cash crops for export declined, and the importation of food began to increase. Available data reveal that the share of agricultural products in total exports decreased to less than 2% in the 1990s from over 60% in the 1970s (Olajide *et al*., 2012). The contribution of petroleum to GDP rose from 0.6% in the 1960s to over 50% in the 1970s. By 1974, Nigeria became a net importer of essential foods as the government spent a significant portion of its foreign exchange earnings on food importation. The enormous transfer of wealth from the oil boom led to increased public expenditure, which fuelled inflation. The inflation rate rose to about 40% in 1975 with an overvalued currency that encouraged imports, making the economy heavily dependent on this source. The private sector was underdeveloped due to a lack of investment in capital projects. The sharp increase in public expenditure created a serious structural problem that widened inequality and imbalances within the country. Rural-urban drift increased as the labour force migrated to cities in search of employment opportunities (Fenske and Zurimendi, 2017).

After the oil boom, the oil glut of the mid-1980s emerged and this led to a near-complete economic collapse in Nigeria. Given the sharp decrease in world oil prices, Nigeria's foreign reserves depleted, and the shortfall in revenue made it difficult for the government to implement its development plan. The government resorted to external loans to cover the fiscal deficits. Over this period, the economy was faced with rising imports, a persistent balance of payments deficit, economic depression, soaring inflation and high unemployment rates. In addressing these fundamental economic problems, the government introduced the Structural Adjustment Programme (SAP) in 1986, to restructure and diversify the economy's productive base in order to reduce dependence on the oil sector and imports. However, the drawbacks of this policy option have been seen to outweigh its benefits (Adeoye, 1991).

After the SAP era, the Nigerian government adopted several short- to long-term economic management instruments (national rolling plans) to develop a strong economy that could absorb both internally and externally generated shocks. For example, the Vision 20:2020 development plan was introduced in 2010 to cover the period 2010-2020 to reduce poverty, create jobs, improve living standards and build the foundation for inclusive growth. This initiative ran parallel with the Nigerian version of the United Nations Millennium Development Goals (MDGs) of eradicating extreme poverty and hunger.

Despite these robust development plans, an over-dependence on oil made the Nigerian economy vulnerable to global oil price shocks. The global oil price represents the primary source of revenue upon which the Nigerian government budget is benchmarked. A negative oil price shock will affect the economy as a whole. For example, the recent decline in the global oil price led to a steady decline in GDP, with a sharp reduction in the external reserves of the country, deepening household economic hardship.

Figures 4.1 to 4.3 plot some key macro-economic indicators for the period between 2010 and 2019, within which many market equilibrium-disturbing events occurred. In 2016, Nigeria witnessed its first recession in 25 years following the oil price collapse of 2014. The economy contracted with a negative growth rate of 1.5% against a positive rate of 2.8% in 2015, highlighting the depth of the economic crisis (see Figure 4.1). The country had fewer buffers and policy instruments to cushion the adverse effects of the economic crisis as the country's Excess Crude Account was depleted, and external reserves were heavily reliant on short-term flows. In addition to declining revenues in the oil sector, oil price shocks also spread to non-oil sectors through foreign exchange channels. The Nigerian currency came under sustained pressure due to falling foreign exchange reserves. The value of the Naira fell from N158.55 to the US dollar in 2014 to N253.49 to the US dollar in 2016 (see Figure 4.2), representing an almost 60% decline.

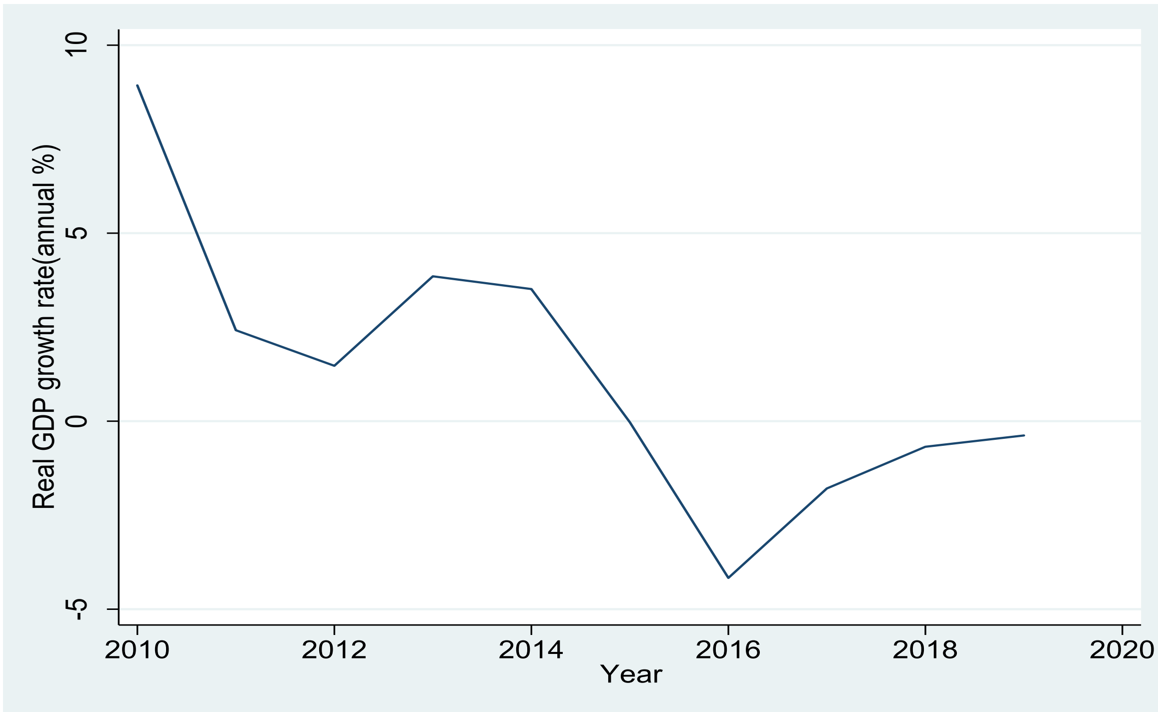

Figure 4.1: Real GDP Per capita Growth (annual %) 2010-2019

Source: Author's calculations using data based on Central Bank of Nigeria (2019)

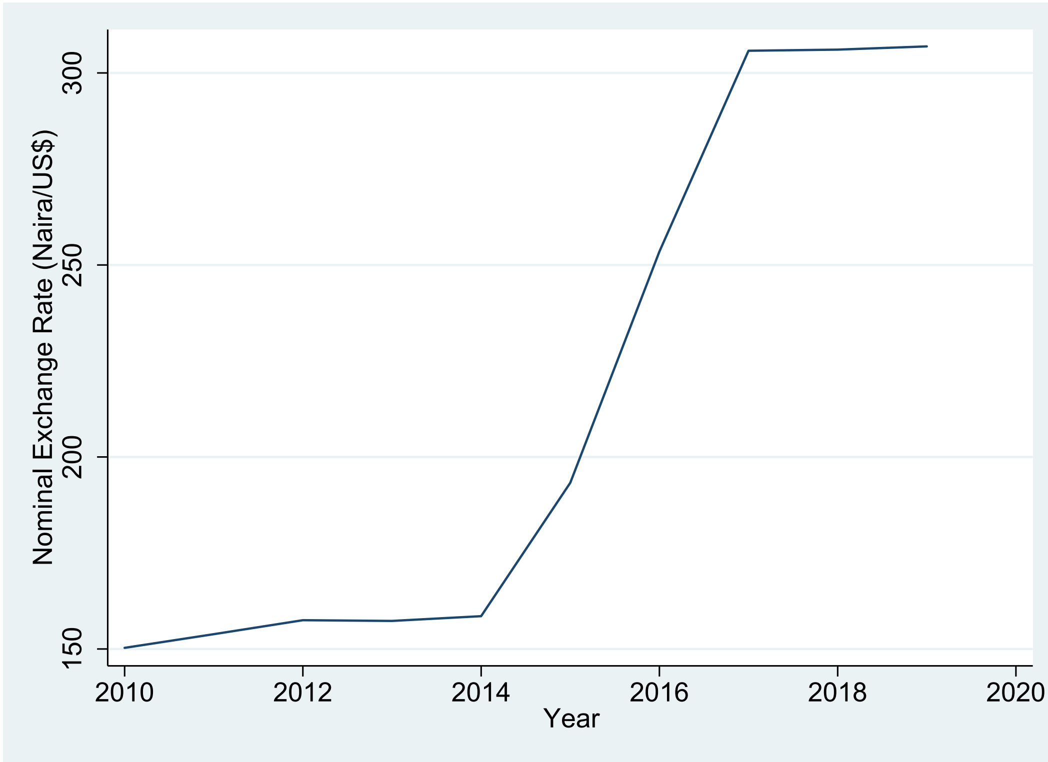

Figure 4.2: Nominal Exchange Rate (Naira: US$) 2010-2019

Source: Author's calculations using data based on Central Bank of Nigeria (2019)

The devaluation of the currency exerts an inflationary impact because of the country's over-dependence on the import of consumer goods. The period also recorded a sustained rise in both food and core inflation rates (see Figure 4.3).

The inflationary pressures emerging in the economy forced the prices of many commodities to spiral upwards, with the inflation rate estimated to have risen from 8% in 2014 to about 16% in 2016. Food prices, especially staples, increased, and available data from the National Bureau of Statistics (2017b) reported that food inflation rose from 9% in 2015 to about 19% in 2016. Figure 4.3 presents the food and core inflation rate, less farm produce, and reveals a sharp spike in the food inflation rate between 2016 and 2018, coinciding with the timing[21] of the shocks that will be examined in this study. The magnitude of the effect of the food inflation is subject to debate but is likely to be sizeable

[21] The data for this analysis are a cross-section for 2018/2019 and cover shocks that occurred in the previous three years.

compared to other shocks in the economy. It is intended that the research in this study will provide some empirical insights into the potential magnitude of these effects.

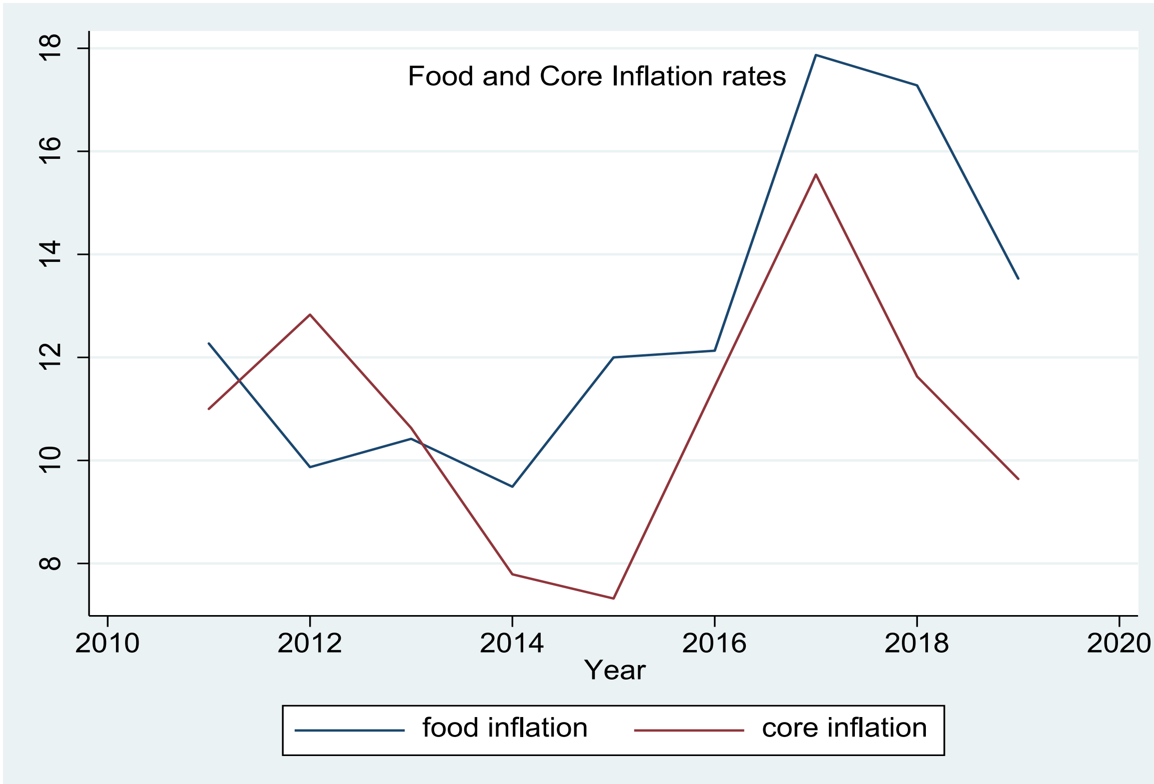

Figure 4.3: Food and Core Inflation Rates 2010-2019

Source: Author's calculations using data based on Central Bank of Nigeria (2019)

Another type of shock exhibited by the Nigerian economy relates to political shocks. In 2015, Nigeria's postponed presidential election sent an adverse signal to investors regarding stability, and most companies postponed investment decisions until after the election (Karadima, 2015). In the build-up to the general election in 2014, the stock exchange market recorded a 4.27% loss, reflecting the degree of uncertainty ahead of the election. Similarly, in 2018 the stock exchange depreciated by almost 20% due to uncertainties associated with the general election (Vanguard, 2019). Nigeria experienced a major dramatic shift in its political landscape in 2015, when an incumbent president lost the election for the first time in the history of the country since independence. The period was followed by a protracted delay of about six months with everything put on hold as the President announced his cabinet and the economic roadmap for government. Political shocks are often accompanied by increased policy uncertainty (Aaberge *et al*., 2017). The possibility of changes in policy by the incoming government potentially affected investor confidence and thus some of the macro-economic indices.

In addition to the unstable economic and political environment, Nigeria experienced an increase in crime, some of which was related to the activities of the terrorist group Boko Haram in the Northern part of the country. The Boko Haram insurgency is estimated to have killed at least 36,000 people and displaced 2 million in the North-Eastern regions of Nigeria (BBC, 2021). The group intensified attacks on security and government establishments, schools, places of worship and public places. Their activities displaced communities and disrupted both farming activities and other economic activities in the affected areas. Repeated clashes between nomadic herders and farming communities over access to natural resources in the northern and central regions also resulted in increased casualties and migration in the region.

Nigeria was threatened by food scarcity as traders from the Northern part of the country experienced difficulties transporting their commodities to other parts of the country. The food scarcity was exacerbated by the migration of farmers away from their farms through the fear of attack by the Boko Haram sect. Food supplies continued to tighten over insecurity, placing upward pressure on food prices. The country resorted to importing food items and this further exposed the economy to global food price fluctuations. There was also disruption of crude oil production due to a local conflict in the Niger Delta.

In conclusion, a review of the macro-economic fluctuations that have impacted Nigeria reveals that even though the oil price shocks started in mid-2014, the effect on the economy was not actually felt until 2016 and thereafter. The objective of the current study is to investigate the effect of these recent shocks (that occurred up to three years before the year of the survey) on an array of household welfare measures.

4.3 Literature Review

This section provides an overview of the literature that has examined the impact of a variety of economic shocks on household assets, expenditure patterns, savings and poverty. In addition, the review covers the micro-economic level analyses of various shocks to provide a broader picture of the impact of personal, locality-specific and financial shocks on the relevant household welfare indicators.

In developing countries, individuals and households are prone to experiencing several shocks that can cause significant and sudden welfare loss. The literature classifies these shocks into two broad categories, covariate and idiosyncratic shocks (Krueger *et al.*, 2016; Dercon *et al.*, 2005; Calvo and Dercon, 2005). As noted earlier, covariate shocks are location specific and affect many households in the same community, while idiosyncratic shocks are specific to households and individuals with effects that are largely restricted to particular households. However, these two broad definitions of shocks are further re-classified into different categories based on the nature or origin of the shock. These classifications include shocks related to climate, economic or financial status, crime, health, among others (Dercon and Clarke, 2009). For example, climate or weather shocks relate to changes in weather, such as floods, drought, erosion, and diseases affecting livestock and crops. Health shocks affect households in the form of death, disability, and illness. Crime shocks affect households through conflicts, theft, robbery and the intentional destruction of assets. In contrast, macro-economic fluctuations in the economy induce financial shocks leading to changes in input and output prices, invariably resulting from the increased prices of essential food items. This generally leads to severe adverse income shocks.

A financial shock is reasonably classified as either an idiosyncratic or a covariate shock, depending on the sources of the shock. For example, a financial shock that comes from an increase in the prices of food items, or through changes in the prices of inputs and outputs can be classified as a covariate shock since the effect is felt across the whole community. On the other hand, a financial shock due to job loss is classified as an idiosyncratic shock, given the effect is on a specific household. These distinctions are important because evidence suggests that different shocks imply different effects on household welfare, and households are unlikely to exhibit similar responses in coping with the impact of these shocks (Ansah *et al.*, 2021). Therefore, shock types and their nature turn out to be among the most critical determinants of household shock responses.

Generally, in developing countries, households respond to idiosyncratic shocks using informal mechanisms (Pradhan and Mukherjee, 2018). Informal mechanisms tend to be more diverse in contrast to formal approaches because households can quickly secure assistance from neighbours, reallocate labour and liquidate assets locally when a whole community or significant parts of it are not affected at the same time. However, households react to covariate shocks differently by relying on external transfers from outside the community, including migrant remittances and government support to smooth their consumption (Townsend, 1994). Apart from the nature and severity of shocks, a household's perception of a shock may also influence its reactions and responses (Josephson and Shively, 2021).

The economic impact of covariate shocks, especially natural disasters, on household welfare has attracted considerable attention over the past decades. The exogeneity of natural disaster shocks is generally seen as useful in an econometric sense as it enables the estimation of an unbiased effect of the shock on the outcomes of interest. In particular, researchers have relied on quasi-experimental methods to examine the impacts on household welfare of various natural disasters, such as earthquakes and typhoon cyclones (Luo and Kinugasa, 2020; Seriño *et al*., 2021), floods and drought (Salvucci and Santos, 2020; Arceo-Gómez *et al*., 2020). The effect of covariate shocks on household economic welfare resulting from flooding is discussed by Oskorouchi and Sousa-Poza (2021) for Afghanistan. The evaluation examines the long-term impact of floods on food security as measured by the micronutrient consumption of calories using the national Risk and Vulnerability survey. The estimated outcome suggests a reduction in calorie consumption and a 27 percentage point change in the probability of deficiency in vitamin C for a household with at least 12 months of exposure to flooding.

Further analysis in this study predicts a marginal change to the food security impact in the wake of price and income shocks. It also informs on the impact of natural disasters on household income levels and poverty status. Households exposed to the flooding witnessed a 3% reduction in their per capita yearly income and were 3 percentage points more likely to be in poverty. The research provides an understanding of the direct impact of flooding and the mechanisms through which the exogenous shock is transmitted to households. The study clearly articulates the synergies at work with natural disasters as the impact of location-specific shocks are associated with other shocks and can lead to a high degree of vulnerability in household welfare indicators.

A household's vulnerability to natural shocks in the short run can also have potential long-term impacts on household welfare measures and poverty levels. Building on the literature relating to the impact of climate change on household well-being, Arceo-Gomez *et al.* (2020) estimate the effect of the 2011 droughts on household per capita income, poverty status and children's school attendance for Mexican households engaged in agriculture.

The results indicate a significant negative impact of the drought on household income that was associated with a 5 percentage point increase in household poverty. However, households with experience of water scarcity had more resilience and the drought exhibited less of an impact compared to those households with a relatively low experience of water scarcity.

Salvucci and Santos (2020) provide an empirical analysis of the effect of the 2015 flood in Mozambique on household consumption and poverty in the short term. Using a difference-in-difference (DID) econometric framework, the authors report a significant reduction in household consumption with magnitudes ranging between 11%-17% for different household types, with stronger adverse findings for households located in the more rural areas. On the other hand, households in the rural areas witnessed an increase in poverty levels of 6 percentage points. The relevance of the above findings can be situated in the management of disasters to prevent further vulnerability for those economies prone to flooding with similar characteristics, especially those within the African context.

The literature on personal shocks is centred around an understanding of the role of idiosyncratic shocks and their effect on poverty and the economic welfare of households. The study by Atake (2018) exploits health shocks in its investigation of the factors that lead to welfare loss and vulnerability to poverty in Sub-Saharan Africa. The study was carried out using household-level surveys in three African countries: Niger, Burkina Faso and Togo. The authors found that households in all three countries are vulnerable to poverty. They concluded that poverty is the leading cause of welfare loss from health shocks as poor households reduce food and non-food expenditure when faced with health shocks. Hangoma *et al*. (2018) shows that disabling health shocks experienced within a household lowers consumption and reduces income earned in Zambia. Using repeated cross-sectional household survey data to estimate a seemingly unrelated regression model, the authors find that health shocks (injury) represent one of the most significant risks to economic wellbeing. The incidence of injury reduced earned income and increased medical expenses. This shock exposed households to consumption fluctuations that may have a broader impact on poverty, malnutrition, and the household's overall wellbeing.

A financial shock is generally defined in the literature using changes in food and non-food prices. Examples of studies investigating the impact of food prices on household welfare include Alem and Söderbom (2012), who examine the effect of food price shocks on household-level variables in Ethiopia and find that a food price shock adversely affects households with low levels of assets. Yousif and Al-Kahtani (2014) investigate the impact of high food prices on Saudi consumers and find that high food prices reduce the consumption of major food commodities, increases expenditure, and thus lead to the erosion of household savings. Rufai *et al*. (2021) found that lower prices for major foods consumed in households increase the income available for farmer health spending. At the same time, the increase in input prices has a significant negative impact on health expenditure. Chiripanhura and Niño-Zarazúa (2016) evaluate the impact of food, fuel and the 2014 financial crisis on households below the poverty line. The analysis was undertaken for Lagos and Kano states using household-level surveys to capture the induced price shocks on household welfare indicators for those in poverty. The results predict a reduction in both consumption and the probability of children being sent to school, and an increased probability of the household resorting to the use of child labour. In addition, the short-term coping strategies of poor households indicate increased susceptibility to remain within the poverty trap. When households are faced with a substantial adverse effect of food prices, reducing the food budget and adjusting other non-food expenditure is often the most direct response to coping with the shock (Adekunle *et al*., 2020).

Vu and Glewwe (2011) argued that the impact of prices on welfare is sensitive to whether the household is a food producer or a food consumer. The impact of prices can lead to welfare gains or losses. For example, as food prices rise, net sellers tend to enjoy welfare improvements, while net buyers tend to exhibit welfare losses (Mbegalo, 2016). For a food-producing household, a fall in prices or an increase in the demand for commodities sold will increase farmer income. On the other hand, an increase in the price of food items consumed, especially in rural areas where poor and disadvantaged households spend more than 80% of their income on food (Elijah, 2010), the burden of rising food prices leads to a welfare loss.

However, the analysis of financial shocks should not be undertaken in isolation of other shocks as there is often an intimate connection between their occurrence. Most of the empirical literature has examined the impact of different types of shocks in isolation, even though many households face multiple shocks simultaneously (Komarek *et al*., 2020; Béné *et al*., 2017). The literature on the Nigerian economy has provided some insights on the impact of various shocks on household welfare indicators. Ajefu (2017) analysed the impact of shocks on household income, consumption expenditure and informal insurance for Nigeria using the Nigerian Household Panel Survey for 2010/2012. The empirical estimation uses the fixed effects and probit model estimation strategy to evaluate covariate shocks (i.e., a rainfall shock), and the variation in self-reported shocks on household income and consumption expenditure. The paper also explored the use of risk-coping strategies to smooth consumption over time. Their results predict a 14.3% reduction in consumption expenditure due to a one-unit increase in an agricultural shock. There is no indication that the effect of health and economic shocks or idiosyncratic shocks on household consumption expenditure varies with household characteristics. Assets represent a measure of wealth and a precautionary well-being indicator (Carroll *et al*., 2019). Therefore, an understanding of how household tangible saleable assets, such as live-stock, jewellery, vehicles, and non-tangibles or financial assets (savings), are affected by exogenous shocks is pertinent for households in developing countries with daily concerns for poverty reduction.

Quisumbing *et al*. (2018) highlight the variation of shock impacts on individuals within a household using cross-country analysis for Bangladesh and Uganda. Using household panel data from the International Food Research Institute for Bangladesh and Uganda, the authors estimate the impact of a multitude of shocks, including food and non-food (fuel) prices, personal shocks (health issues) on assets ownership with a specific emphasis on gender. The study found a negative impact of personal shocks (illness and death of husbands) on Bangladesh women's asset ownership (landholding). In Uganda, fuel prices and drought shocks reduced a wife's asset holdings compared to that of her husband's. The approach here provides an understanding of the vulnerability that arises from shocks. Assets and savings are a strategic mechanism to cope with adverse events in developing countries. The role of gender ownership can also provide a heterogeneous view of households faced with the challenges of natural disaster shocks.

There is a growing literature on the effect of shocks on household savings. Household savings are an essential indicator of enhanced welfare for either precautionary or investment purposes. Therefore, shocks derived from a potential reduction in a household's propensity to save, either through consumption or redistribution through other mitigating welfare consequences, are vital to explore. Savings are a medium through which households can escape the poverty trap. Luo and Kinugasa (2020) estimate the impact of the 2008 Sichuan earthquake on household savings using a synthetic control method. The utilisation of the earthquake as a natural experiment in generating a comparative event study for both the short-term and long-term impacts indicated a decline in household savings from 24% to 7% for rural areas and 23% to 21% for urban areas. The research found no long-term impact on household savings as the estimates revert to the baseline values within a year.

The interplay between the various vulnerability indicators (consumption expenditure, assets and savings) may lead to inequality within a country or household. Economic inequality is a threat to growth, development and social cohesion within a country. Recent literature has demonstrated that economic shocks can widen inequality. Amare *et al*. (2021) evaluate consumption inequality for households engaged in agricultural activity in Nigeria and Uganda affected by rainfall shocks. Rainfall shocks can cause variability in agricultural productivity. The research found that a 10% increase in rainfall shocks is associated with a 38% and 52% reduction in household consumption for Nigeria and Uganda respectively. The estimated impact of the rainfall shock on household consumption inequality for Nigeria and Uganda was 25% and 48%, on average.

A study in Australia by Botha *et al*. (2021), using novel data collected from 2,078 Australian residents during the COVID-19 pandemic, estimated a set of unconditional quantile regression models. The empirical results reveal that a labour market shock is associated with a 29% lower level of perceived financial wellbeing on average. The unconditional quantile regression results indicate that lower levels of financial wellbeing are present across almost the entire welfare distribution with the exception of the very top. These findings contrast with the view that pandemics tend to lead to more equal societies in economic terms (Scheidel, 2018). The “Great Leveler” argument identified in this study as representing the “Four Horsemen” of mass mobilisation warfare, transformative revolutions, state collapse, and catastrophic plagues, can all, in different ways, reduce the fortunes of the rich, thereby reducing inequality.

However, van Bavel and Scheffer (2021) argued that the exception to Scheidel’s view has occurred in situations where the poor leverage organisations or institutions, such as guilds, fraternities, trades unions, cooperatives, and political movements, to shape a response to shocks. Their view is interesting from a policy perspective as the rise and decline of inequality might indicate the absence of a policy response by governments or the existence of an appropriate policy to deal with a particular shock.

It is worth noting the double (or dual) impact of idiosyncratic and covariate shocks when these occur contemporaneously during an aggregate macro-economic shock. Considering that households are already stressed by the existence of macro-economic shocks, the occurrence of any other shock could compound or exacerbate the effect of shocks on household welfare. Aggregate shocks are mostly unpredictable and tend to affect almost all the macro-economic aggregates of the economy.

Using idiosyncratic shocks and household savings relationships, Krueger *et al*. (2016) investigate the changes in household income, wealth and household preferences before and after a macro-economic shock. Using panel data for the United States, the authors investigate households at different points of the wealth distribution and explore how expenditure patterns differ before and after the 2007-2009 financial recession. The evidence predicts an amplification of the macro-economic shock effect from wealth inequality, especially if households with little net worth experienced a sharp reduction in their savings propensity. Therefore, precautionary savings can delay consumption and directly worsen the macro-economic shock effect at the aggregate level. On the other hand, when consumption is higher than income, households need to raise consumption by contracting debts and selling assets to smooth their consumption (Rakib and Matz, 2016).

Josephson and Shively (2021) found similar results for the impact of different types of shocks on household labour allocation during a macro-economic shock. Using Zimbabwe's hyperinflation and currency collapse, they argued that shocks negatively affect household labour allocation, and compound existing stress. Focusing on the effect of an aggregate shock on economic outcomes and well-being in rural Uganda, Mahmud and Riley (2021) found negative effects of the COVID-19 lockdown on household non-farm income. The study provides evidence of a decrease in well-being with a 40% decrease in food expenditure as households also depleted their savings by nearly 50%.

Despite the recent interest in the impact of shocks on different household outcomes, there is still relatively little evidence on the distributional impact of shocks on household welfare measures. A few studies have considered the distributional effect of shocks and welfare consequences (Wagstaff and Lindelow, 2014; Heltberg and Lund, 2009; Hoddinott, 2006). Nevertheless, most existing studies assume an homogeneous relationship between shocks and household welfare across the distribution, using standard linear regression techniques, such as the mean-based ordinary least squares (OLS) method. Such a mean regression approach summarises the average relationship between shocks and household welfare indicators based on the conditional mean of the welfare distribution (Koenker and Hallock, 2001). However, this approach gives only a partial view of the effect as it assumes an homogeneous/mean effect of shocks on welfare; this may neglect the detection of some valuable heterogeneity that is informative to researchers and policy-makers.

This paper aims to contribute to the growing literature on the impact of shocks on household welfare indicators by using multiple shocks to empirically examine the effect of idiosyncratic and covariate shocks on savings, food and non-food expenditure, and household asset levels in Nigeria. For a developing country like Nigeria, the consumption pattern is biased towards food, with households consuming more food than non-food items. In most developed countries, the situation is the reverse with consumption patterns driven more by non-food items (National Bureau of Statistics, 2020). This study focuses on the impact of an increase in food item prices, although other shocks are also considered. The emphasis is motivated by the fact that consumer prices appear more relevant in this context since Nigerian households spent about 57% of their total expenditure on food in 2019 (National Bureau of Statistics, 2020). The country is among those exhibiting the highest volatility of domestic prices in staple foods. Therefore, for policy-making, one component of anti-inflationary policy may be to exercise some control over food prices (Zaman and Khan, 2018) as most urban and rural poor households are net food buyers and thus vulnerable to food price hikes (Avalos, 2016).

This study primarily focuses on examining the impact of a financial shock, as mediated through an increase in the prices of food on welfare measures in Nigeria, with the role of other shocks also examined as a sub-theme. Recent empirical studies have confirmed that oil price shocks affect the economy through changes in spending by domestic households as discretionary income changes (Edelstein and Kilian, 2009; Baumeister and Kilian, 2016; Baumeister *et al*., 2018). Therefore, by looking at the impact of food price increases, it is hoped to capture the effect of the global food price increase and its ultimate effect on household welfare. In addition, the study will provide an essential insight into the impact of shocks on consumption inequality and asset depletion.

4.4 Data Section

The cross-sectional household data used for the current analysis is obtained from the latest wave of the 2018/2019 Nigerian General Household Survey (GHS). The GHS was initially implemented as a larger cross-sectional survey of 22,000 households, which was last conducted in 2010. Through a collaboration between various agencies, such as the Federal Ministry of Agriculture and Rural Development, the National Food Reserve Agency, the Bill and Melinda Gates Foundation, the World Bank, and the National Bureau of Statistics (NBS), the survey was updated to include a panel component (GHS-Panel). The GHS-Panel is conducted every two years, covering 5,000 households, carried out in two visits (post-planting visit in July - September 2018 and postharvest visit in January - February 2019).

The fourth wave was conducted in 2018/2019, and the households were selected using a stratified two-stage cluster design. The sampling frame includes all 36 states of the Federation and Federal Capital Territory (FCT), Abuja. The two-stage cluster sample selection process involved the selection of 500 enumeration areas (EAs) in each state and FCT, and ten households in each of the EAs. The sample is representative at the national level, as well as at the regional and urban/rural levels. The number of households interviewed for this latest survey comprises just under 5,000, who comprise those households within the panel outlined above.

The present analysis relies on the cross-section of the 2018/2019 Nigerian GHS survey from this panel. However, some important individual-specific time-invariant variables were found to be changing over time. There was also significant variation in the nature of the question asked about shocks across waves of the panel. The question in the first and second waves captured if shocks affected households in the past five years, while the question was revised in the third and fourth waves to capture shocks only in the past three years. Therefore, we decided not to use the panel for our analysis but just focus on the most recent year within the panel

After excluding outliers and missing values, we are left with an overall sample of 4,970 households that constitute the set of usable data points for our empirical analysis.

The GHS has detailed modules on self-reported shocks, household expenditure on food and non-food consumption, assets and savings, thereby allowing us to explore links between economic shocks and household welfare measures. The survey collects a wide range of information at both the individual and household level. The individual level variables include gender, age, marital status, educational level, and employment status of the head of household. The set of household level variables that can be constructed from the data are household assets, household size, household consumption on food, and non-food expenditure in per capita terms. Furthermore, we use the household expenditure data to determine whether a household is above or below the relevant national poverty line for Nigeria. The GHS also enables us to create a variable for the savings of households and the geographical location and settlement type within which the households reside (i.e., the six geo-political regions and urban/rural). Finally, the economic shock module is used to construct three key variables for our analysis. The survey provides information on variable shocks that affected a household in the last three years. The relevant shocks are classified under three broad groups: personal, financial and local shocks.

4.4.1 Definition of variables and summary statistics

The dataset contains information on relevant household expenditure that permits the construction of three welfare monetary metrics based on expenditure (food and non-food) and asset status. The dependent variables are constructed as a logarithm of consumption expenditure (i.e., food and non-food), the logarithm of household assets, and a savings variable constructed as a discrete 0/1 dummy variable, and the food poverty line is constructed as a binary 0/1 variable to reflect household poverty level. Since consumer consumption expenditure includes food and non-food components, the former makes up a large part of the spending of low-income households in Nigeria, as in most other poorer countries (Ozughalu and Ogwumike, 2015). Therefore, it is necessary to disaggregate consumption expenditure into its food and non-food expenditure components to confirm whether any heterogeneity exists in shock effects between the two.

This study used consumption expenditure as a measure of household welfare because, conceptually, consumer spending better reflects the extent to which a household reaches a certain level of welfare (or “utility”), while income represents the opportunity to achieve a certain level of well-being (National Bureau of Statistics, 2020). More importantly, households rarely report their income with an acceptable level of precision, perhaps due to tax or confidentiality concerns; this makes income a less preferred measure of household welfare.

Expenditure is measured in thousands of Naira and represents the amount spent on the purchase of food and non-food items. The food consumption module was administered with a recall period covering the previous seven days. It includes various food categories such as grains; vegetables, tubers and legumes; beverages; meat, poultry and fish; dairy products, eggs as well as fats; and fruit. It also includes questions about the source of the food, such as gifts, and own production, as well as the expenses involved in consuming it. Therefore, food consumption expenditure represents the annual value of per capita food consumption.

Non-food items have varying reference periods, ranging from 7 days, 1 month, 6 months, and 12 months, which are related to the frequency of purchases. The expenditures for these items are then annualised to include expenditure on education, housing rent, and other non-food goods and services, like clothing, small appliances, fuel, recreation, household items and repairs etc. The consumption expenditure variable is constructed as a logarithm of food and non-food per capita expenditure. We then use the household expenditure together with the estimated food poverty lines in the local currency to assess households as either being below or above these poverty lines.[22] A dummy variable is created for food poverty using the relevant poverty lines as benchmarks. Food poverty takes the value of 1 if household per capita food expenditure is below or equals the poverty line and 0 if not. For the purpose of this study, we focused only on the food poverty line as computed by the NBS using the cost of basic needs approach. Although there are no published spatial deflators developed or available at state level for Nigeria, the Statistics Office use a spatial and seasonal price deflator in the construction of the food poverty line, which is the one used in this study.

Our measure for household assets includes data on household ownership of livestock and more than 33 consumer durables (including questions on furniture, radio, television, refrigerator, car, mobile phones, and other household appliances). Households were asked if they own any of the items and if they wanted to sell them, how much would they receive in Naira. We compute household assets as the total value of all productive assets (livestock) and consumer durables. The value of land is not included in the asset measure

[22] The national poverty line was 137,430 Naira and the food poverty line was 81,767 Naira per person annually as of 2019.

because the land market is fairly thin, and it is difficult to get a good sense of the value of land.

The asset variable was constructed as the logarithm of the real value of the consumer durables. The asset variable was not interpreted as providing a measure of the current welfare of the household: it was intended as a measure of stock of wealth of the household. In order to account for inflation, all expenditure and asset data were adjusted to the common base year of 2010 by using the relevant consumer price index. All the analyses therefore used real values of consumption expenditure and assets.

Since there are no data for the value of savings, savings is constructed as a binary indicator that takes the value 1 if a household has savings of any type and 0 otherwise. The binary indicator capture saving behaviour of households through whether or not they engage in saving behaviour.

Our main independent variables of interest, the shock variables, were based on responses to the questionnaire in the household shocks module. This asked about the number of times households had experienced a particular shock in the last three years before the year of the survey, indicating the frequency and the severity of such shocks. Following the literature, the 22 shocks listed were categorised under three broad groupings: personal, local and financial. The personal shocks include death and illness of a family member, loss of employment, non-farm business failure and the destruction of harvest by fire. Local shocks include floods, drought, pest invasion and violence/conflict.

The questions on the financial shocks in the module include increases in the price of inputs, a fall in the price of output and increases in the price of major food items consumed. Our key financial shock variable was constructed based on an increase in the food prices facing consumers, with the aim of separating producer prices (input and output prices) from consumer prices (prices of food items consumed). Vu and Glewwe (2011) argued that the welfare effect of food price shocks may be ambiguous if there are no clear distinction in price changes between production and consumer prices. Therefore, a food price shock can affect a household differently based on whether they are net buyers, net sellers or self-sufficient. The lack of data on households affected by input price shocks motivated the focus on food items consumed, which are most affected by food price spikes, which reflect the huge food inflation rate shown in Figure 4.3. Therefore, our analysis is based only on food price increases as they affect households in Nigeria.

The shock variables are then constructed as a discrete ordinal variable based on the frequency of times households experienced a given shock in the three years preceding the survey year. The construction of the shock variables is in contrast to studies that construct binary variables as a measure of shocks. discrete ordinal measure was adopted as the objective is not to investigate shock impact effects, but more of the scale and the intensity of the shocks on households. The use of a binary variable approach has its limitations in samples due to the varying distribution of certain rare events. For instance, if a rare event such as location specific shocks is present, a dummy variable may be less likely to detect an effect of interest. Instead, we use the count variable to estimate the contribution of shocks to the welfare of households as this potentially captures the persistence of the shock effects. Table A4.1 in the Appendix to this chapter describes all the variables used in our analysis.

There are always issues in regard to the accuracy of household expenditure data. There is clear evidence of 'winzorization' being used by the Central Statistics Office at the top end of the food distribution. However, measurement errors in these measures only impact the efficiency of the OLS estimates and not their consistency. However, the various 'shock' measures are self-reported and may be subject to error. This potentially has implications for the consistency of the econometric estimates. However, we take the view that the reporting errors may average to zero across the sample and thus may not prove consequential for our analysis. It also worthy to note that shocks are recorded using 3-year recall periods. This reference period needs to be taken into consideration when interpreting our findings. The study used a measure of self-reported emergency saving behaviour constructed as a binary indicator. Since the data on the amount of savings was not available, which could have captured the intensity of savings, the binary indicator of emergency saving was considered a good measure of household's saving habit and could be used to identify households' behaviour of setting aside money for emergencies. Similar to savings, the study relies on self-reported value of household assets.

Table 4.1 presents the summary statistics of unanticipated shocks experienced by a household over the last three years. Personal or idiosyncratic shocks, including the death of a family member, accounted for about 13% of the retrospectively reported household shocks. The frequency of experiencing personal shocks within the last three years range from 0-9 occurrences. A household that self-reported experiencing a financial shock accounted for 34.9% of the sample, with a possible five consecutive occurrences.

Similarly, approximately 8.6% of the households reported having suffered a local shock (e.g., adverse weather shock), with one household indicating an occurrence of as many as nine such shocks in the last three years.

Table 4.1: Summary Statistics of Household Unanticipated Shocks

Variable	Sample	Mean	Min	max
Personal shocks	4,970	0.130 (0.444)	0	9
Financial shocks	4,970	0.349 (0.583)	0	5
Local shocks	4,970	0.086 (0.349)	0	9

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

Focusing on household characteristics of different groups in the sample, Table 4.2 reveals that the proportion of farm households that were affected by all three shocks in the last three years is lower than the proportion of non-farm households. While the proportion of rural households affected by personal and local shocks is more than the proportion of urban households. In regard to financial shocks, the proportion of rural household who are predominantly farmers is less than the proportion of urban households. Meanwhile, the proportion of female-headed households affected by personal and financial shocks is higher than their male counterparts.

Table 4.2: Time-invariant Characteristics of Households by Shocks

Variables	Farm	Non-farm	Urban	Rural	Male	Female
Personal shock	0.1216 (0.4202)	0.1395 (0.4723)	0.0675 (0.3095)	0.1586 (0.4922)	0.1215 (0.4246)	0.1629 (0.5163)
Financial shock	0.3124 (0.5726)	0.3782 (0.5904)	0.3861 (0.5422)	0.3315 (0.6009)	0.3230 (0.5579)	0.4557 (0.6682)
Local shock	0.0708 (0.2935)	0.1056 (0.4062)	0.0341 (0.1981)	0.1108 (0.3978)	0.0878 (0.3494)	0.0804 (0.3456)
Sample size	**2,755**	**2,215**	**1,585**	**3,385**	**4000**	**970**

Standard deviations in parenthesis.

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

As a preliminary insight prior to our econometric analysis, the summary statistics offer some understanding of the distribution of key variables within the data. Table 4.3 presents the summary statistics of household welfare indicators (food expenditure, non-food expenditure and assets), savings and poverty measures. The mean of the welfare measures is broadly comparable with the median, indicating that the mean is a reasonable representative central location value of the distribution of all the welfare indicators. The overall estimate of household savings and poverty rates stood at 0.524 and 0.522, respectively.

Table 4.3: Mean, Median and Standard Deviation of Welfare Indicators

Variable	Mean	Median	Minimum	Maximum
Log_e (Food expenditure)	6.6145 (1.7800)	6.8916	2.4849	11.6053
Log_e (Non-food expenditure)	9.4412 (1.2287)	9.5060	5.1240	14.4775
Log_e (Asset value)	11.8060 (1.5593)	11.8859	5.4806	17.4089
Savings	0.5243 (0.4995)	‡	‡	‡
Poverty	0.5223 (0.4996)	‡	‡	‡

Notes: Standard deviations are reported in parentheses. The sample size is 4,970.

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

Table 4.4 presents the descriptive statistics for the welfare indicators in actual Naira values. The statistics indicate a significant difference in the mean per capita food and non-food expenditure, household assets and the level of savings between farm and non-farm, and poor and non-poor households. The results indicate that non-farm and non-poor households are relatively better-off than farm and poor households. However, we could not find any relationship between agricultural households and poor households. The results may suggest that among the poor households there could be some that grow food and have an additional forms of savings, given that assets are often a form of savings, especially for agricultural households. Table A4.2 in the Appendix to this chapter presents the summary statistics of explanatory variables used in this analysis.

Table 4.4: Mean of Welfare Indicators in actual Naira value

Variable	**Farm**	**Non-farm**	**Poor**	**Non-poor**
Food expenditure	15,393 (2911)	23,917 (4539)	4,185 (2020)	37,001 (5108)
Non-food expenditure	8,060 (1968)	16091 (3464)	2,652 (2552)	22,961 (3923)
Asset value	2,652 (5722)	5,392 (1297)	321,226 (7194)	518,686 (1299)
Savings	0.4556 (0.4981)	0.5768 (0.4942)	0.4748 (0.4994)	0.5736 (0.4946)
Sample Size	**2,215**	**2,755**	**2,557**	**2,413**

Notes: Standard deviations are reported in parentheses.

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

Figure 4.4 reports the distribution of household food expenditure in 2018. The plots reveal that there is a small rightward shift in the distribution of food expenditure patterns of households. When the food expenditure is disaggregated into farm and non-farm households, Figure 4.5, representing the kernel densities for farm and non-farm households, reveals that the farm density is clearly to the right of the non-farm density, implying that non-farm households tend to have higher food consumption expenditure. On average, these households spend about N 23,917 (see Table 4.4) a week on household food consumption compared to farm households, which spend about N 15,393. The difference between farm and non-farm households is slightly greater in the right tail of the density with non-farm households slightly better off than farm households.

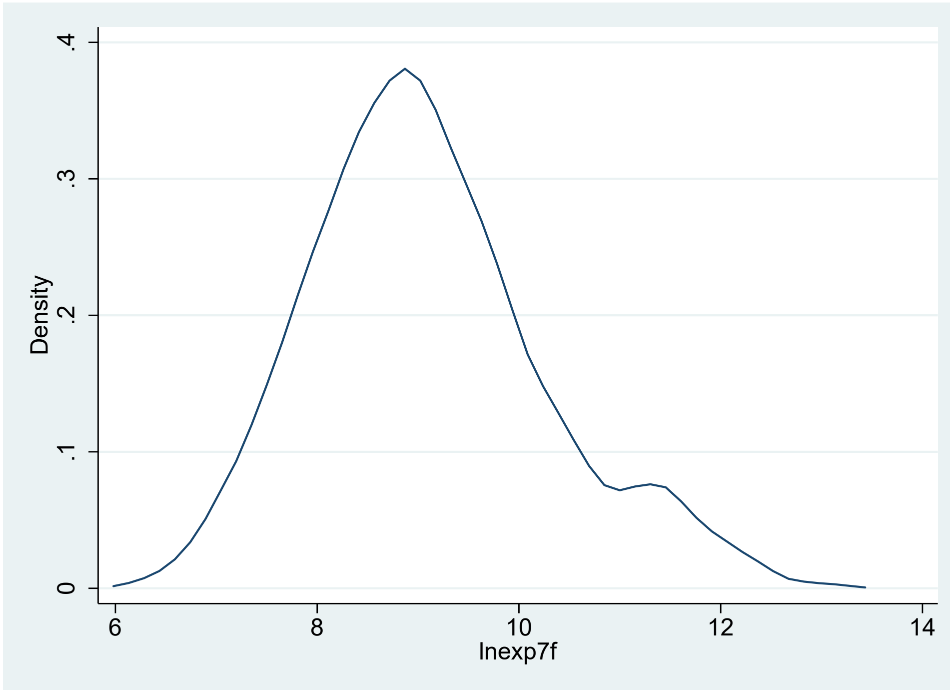

Figure 4.4: Kernel Density of the Distribution of Household Food Expenditure

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

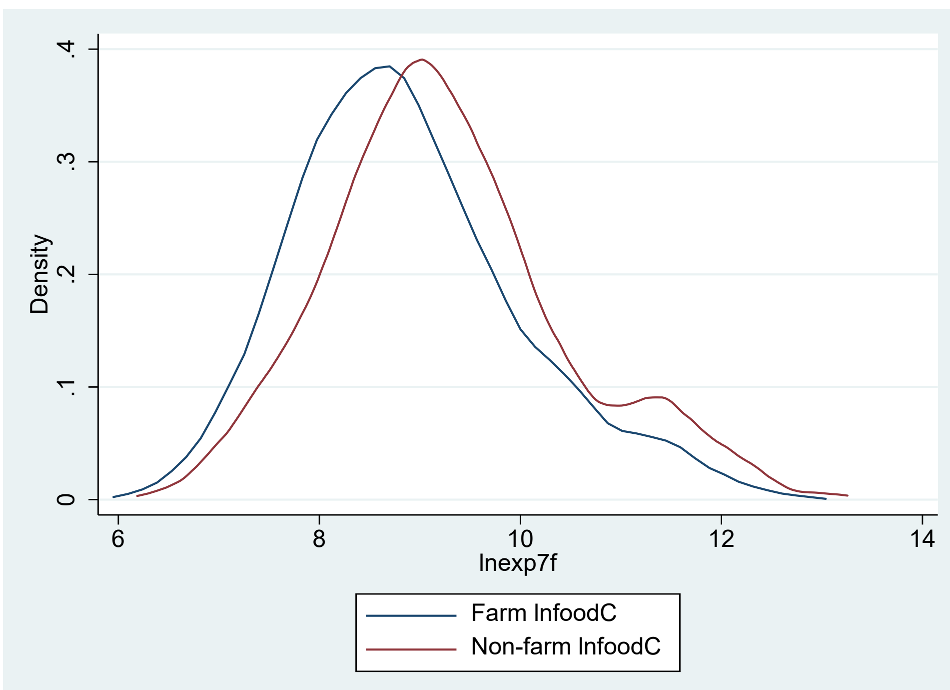

Figure 4.5: Kernel Density of Farm and Non-farm Household Food Expenditure

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

The density plot for the distribution of non-food expenditure (see Figure A4.1) indicates a more normal spread across the distribution. Figure A4.2 reflects the distribution of household asset values and exhibits a peak in the middle with more households at the left-hand side of the distribution.

Nigeria, like other developing countries, experiences food poverty. The household expenditure pattern in Nigeria reveals that 56.65% of total household expenditure in 2018 was spent on food (National Bureau of Statistics, 2020). About 40% of the Nigerian population in 2018 was rated to be living below the poverty line (National Bureau Statistics, 2019). In this study, the proportion of people living below the food poverty line was 52% (see Table 4.3). The poverty level is illustrated in Figure 4.6 of kernel density of food expenditure with the position of the food poverty line for the log of food expenditure super-imposed. Eigbiremolen and Ogbuabor (2018) found similar results in a related study using the General Household Survey. The authors found that about half of the population was food-poor in 2013, with 50.6% and 39.5% of the population for rural and urban households respectively. In an earlier study, Ozughalu and Ogwumike (2015) used the 2004 National Bureau of Statistics Nigeria Living Standard Survey to examine the incidence, depth and severity of food poverty in Nigeria. Their results reveal that food poverty incidence stood at about 50%. Therefore, our food poverty rate of 52% is consistent with previous findings in the literature for Nigeria.

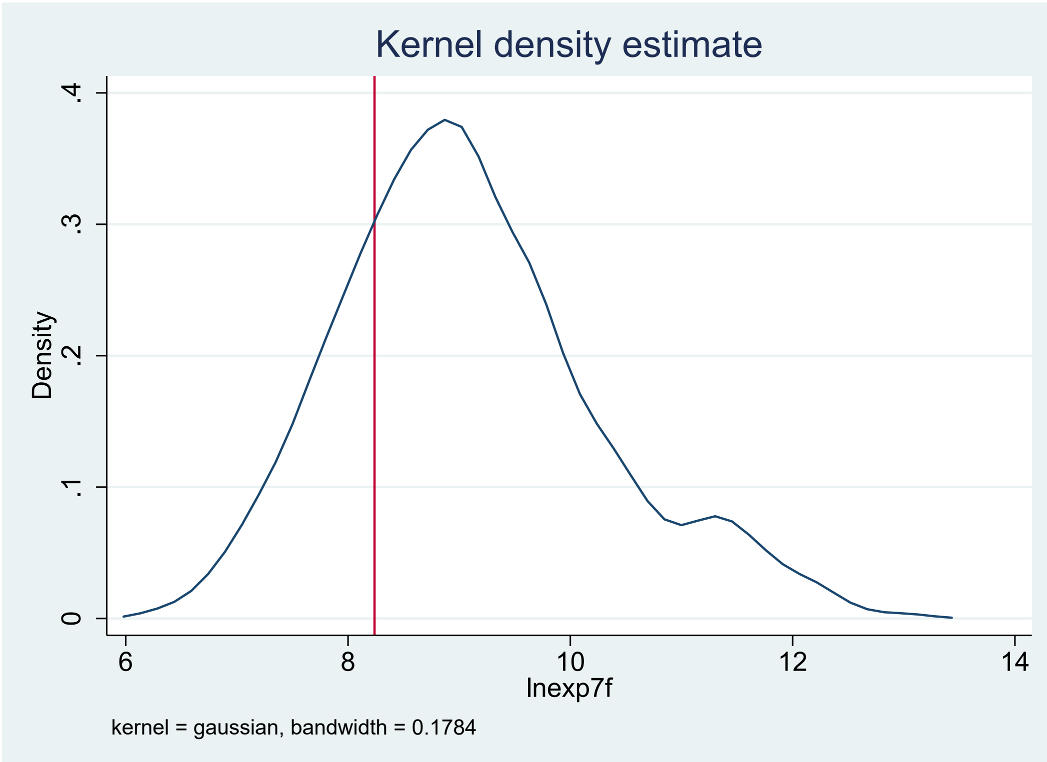

Figure 4.6: Log of Food Expenditure by Poverty Status

Source: Author's calculations using WBES data and NBS website.

Attention now turns to Table A4.3 in the Appendix to this chapter that provides summary statistics of the means and the distributional differences of food expenditure data at different quantiles (10th, 25th, 50th, 75th and 90th quantiles). On average, the distribution of food expenditure for households that experienced the event of a financial shock is not significantly different from those not experiencing any type of shock. On average, however, there is a significant difference in the distribution of food expenditure for households that experienced personal and local shocks from those with no shocks. However, at the different quantiles, the result reveals some variation across the three different types of shocks. Households at the bottom end of the food expenditure distribution exhibit a larger impact for financial shocks.

Table A4.4 in the Appendix to this chapter reports the summary statistics for the non-food expenditure category. The households not affected by any form of shocks have higher non-food expenditure levels for most of the distribution across the three types of shocks. This result suggests that households not experiencing shocks were marginally better off in terms of their non-food expenditure levels.

The story is similar for household assets in Table A4.5. Households not affected by shocks exhibit higher asset levels than households affected by the different types of shocks, revealing significant differences between the two groups, on average and across the asset distribution. These findings reveal that shocks exhibit a heterogeneous impact on household food, non-food expenditure and assets across selected percentiles of the unconditional log expenditure and asset distributions. Overall, the results reveal less evidence that households are adjusting their food or non-food expenditures. However, the summary statistics tentatively suggest that assets appear to be used as a way of off-setting shocks, with suggestive evidence that households are depleting their assets in response to shocks.

The analysis is extended to incorporate other indicators in savings and food poverty rates presented in Table A4.6. The household level of savings reveal that those not affected by shocks enjoy higher saving rates than those affected by personal and financial shocks. This results tentatively suggest that households affected by personal and financial shock may have used their savings as a financial cushion or shock absorber. The proportion of households below the food poverty line does not register any significant difference between financial shock and non-shock affected households. However, there is a significant difference between households affected by personal and local shocks with those not affected by any shocks.

It may seem surprising that there are some proportions of households that recorded no financial shocks, because food price inflation generally affects all households. However, this is not always the case, as food price shocks may affect households differently depending on whether they are net buyers, net sellers or self-sufficient.

Although the increase in food prices has led to a general increase in the prices of various food products, it's not always possible for every household to have the same impact. For instance, households that were not affected by the food price hike may have experienced an increase in their income, thus, no significant impact on household expenditures.

Therefore, the impact of food price inflation may not be homogeneous across households. However, we acknowledge that among households with no financial shock, there may be households affected by other types of financial shock, such as an increase in input prices for agricultural households. Unfortunately, we do not have sufficient data to conclude that this is the case in this study. Table A4.7 report the summary statistics of the characteristics of households that reported no shocks and those households reporting shocks.

The data also provide useful household and individual characteristics to be used below in our empirical methodology. Table A4.5 in the appendix to this chapter provides summary statistics for the explanatory variables used as covariates in the econometric models determining the impact of unanticipated household shocks on household welfare. The characteristics include household demographics, such as household size, dependency ratio, settlement type and regional locations. In addition, the survey provides information on household head characteristics, including age, marital status, educational attainment and gender. The average household size is reported to be six, with 32% of households located in urban settlements. The sample is representative across the regional locations with each containing a similar proportion of households. The average age of a household head is 49 years, with 80% male headed. The sample reveals 75% of household heads are married with 23% having no formal education. The proportion of unemployed heads accounted for 9.7%, with approximately 45% in agricultural employment. Household heads in self-employment or business activities accounted for 39% of the sample.

In conclusion, the raw data confirm a negative association between shocks and the various household welfare indicators, both at the mean and at the different quantiles. Given that the objective of this study is to examine the distributional impact of shocks on household welfare, the empirical framework used to inform the effect of shocks on the unconditional log distribution of expenditure and assets is discussed in the next section.

4.5 Empirical Methodology

4.5.1 Introduction

In order to estimate the distributional effect of shocks on household welfare measures, the study adopted the unconditional quantile regression based on the concept of Recentred Influence Function (RIF). A probit model was employed in measuring the impact of shocks on savings and food poverty rate.

4.5.2 The Unconditional Quantile Regression Model

Under certain assumptions (i.e., linearity, homoscedasticity, independence, or normality), OLS provides consistent estimates of the effect of a regressor on the unconditional mean of the outcome variable. OLS also provides an informative numerical value but nothing more than an average point estimate that captures the relationship between the explanatory and outcome variables. The approach assumes an homogeneous effect across the outcome distribution; this could potentially conceal valuable policy-relevant information if the effect varied across the entire distribution of an outcome variable (Borah and Basu, 2013).

An alternative to mean-based regression is the quantile regression (QR) model that has two characterisations in the literature: the conditional and unconditional quantile regression models, defined here as CQR and UQR. Unlike the mean-based regression model, the QR is less sensitive to outliers and heteroscedasticity (Deaton, 1997). The QR offers a more heterogeneous view of the relationship between outcome and input variables and provides a means of modelling the level of changes in the outcome variable at various distribution points, conditional on other characteristics. Koenker and Bassett (1978) developed the conditional quantile regression (CQR) model to provide a framework within which the assumption of homogeneity across the conditional distribution of the outcome variable can be relaxed. In the case of a conditional quantile regression, the inclusion of covariates in the regression model has the effect of re-defining quantiles (Killewald and Bearak, 2014), with observations defined to be at the median or other quantiles of the distribution. Given the linear formulation of the regression model in terms of covariates, the quantile coefficient estimate is solved using a linear programming approach with either simplex algorithms or barrier methods (Koenker, 2017). Since the law of iterated expectations does not hold in a CQR model, the estimate of the conditional quantile of the dependent variable cannot be generalised to the level of the population. This constraint of the CQR makes the conditional quantile results difficult to interpret and less secure from a policy perspective (Cobb-Clark *et al*., 2016). This shortcoming has led researchers to develop an alternative, known as the unconditional quantile regression (UQR) model.

For the current study, the impact of shocks at different quantiles of the household welfare distribution was evaluated using unconditional quantile regression models based on Recentred Influence Functions (RIFs), which were originally developed by Firpo *et al*. (2009). The technique estimates marginal effects at various quantiles of the outcome variable and is widely believed to yield more policy-relevant information than conditional quantile regression models (Tran and Van Vu, 2020; Khanal *et al*., 2018). This then allows an explicit distributional analysis for the effect of covariates. The Firpo *et al.* (2009)

approach addresses policy issues that depend on the unconditional statistical properties of the outcome variable. The distinct advantage of the unconditional quantile regression over the other methods is that it is based on the use of statistical functionals. A statistical functional is any function of the outcome variable's (i.e., food expenditure, non-food expenditure and household assets in this study) distribution function defined as F (·), and sometimes expressed as v(F). For example, the functional of interest may be the mean, variance, or selected quantiles. The RIF unconditional quantile is based on the Influence Function (IF). It provides the framework to calculate the effect of adding or deleting an individual observation (or data contamination) on a specific quantile statistic without the need to recalculate the statistic. Assume IF (y; v, F) is the influence function corresponding to an observed outcome variable y (e.g., the log of food expenditure, non-food expenditure and household assets, and other measures of welfare in this case) and the distributional statistic is defined as v (F_y). Assume the RIF corresponding to this case is defined as RIF (y; v) where:

$$RIF\ (y; v) = v(F_y) + IF\ (y; v, F) \qquad [4.1]$$

The influence function (IF) of a quantile value q_τ for a random variable y is given by:

$$IF\ (y; q_\tau) = \frac{\tau - I(y \leq q_\tau)}{f_y(q_\tau)} \qquad [4.2]$$

where:

τ = the quantile of interest (e.g., the 10th percentile);

I(.) = an indicator function that adopts a value of 1 if $y \leq q_\tau$, and a value of 0 otherwise.

q_τ = the population quantile of the τ^{th} quantile of the unconditional distribution of y.

$f_y(q_\tau)$ = the probability density value of the outcome variable corresponding to the quantile value q_τ

The distributional statistic of interest can be expressed as the average of the conditional expectation of the RIF given the covariates (i.e., the mean of the RIF is the quantile of interest).

The RIF can be obtained by adding back the quantile statistic of interest to the original IF (i.e., q_τ):

$$\mathrm{RIF}\ (y;\ q_\tau) = q_\tau + \frac{\tau - I(y \leq q_\tau)}{f_y(q_\tau)}$$

After some manipulation, this can be re-arranged as follows:

$$\mathrm{RIF}\ (y;\ q_\tau) = q_\tau + \frac{I(y > q_\tau)}{f_y(q_\tau)} - \frac{1 - \tau}{f_y(q_\tau)} \qquad [4.3]$$

The RIF is a dichotomous variable that can take either one of two values:

$q_\tau + \frac{\tau - I}{f_y(q_\tau)}$ when the random variable is below (or equal to) the quantile value q_τ. or $q_\tau + \frac{\tau}{f_y(q_\tau)}$ when the random variable is above the quantile value.

The conditional expectation E [RIF (y; q_τ) | **X** = x] is a linear function of the probability that the random variable (y) is above the quantile. In turn, this is a linear function of a set of covariates x contained in **X** that can be estimated using a simple linear probability model (LPM).

Following Firpo *et al.* (2009), the conditional expectation RIF regression is then expressed as follows:

$$E\ [\mathrm{RIF}\ (y;\ q_\tau)\ |\ \mathbf{X}] = \mathbf{X}'\boldsymbol{\beta} \qquad [4.4]$$

Equation [4.4] can be estimated by OLS, regressing the conditional expectation of the RIF on the explanatory variables **X**. Firpo *et al*. (2009) demonstrate that such an OLS regression provides estimates for the **β** coefficients that represent the marginal effect on the unconditional τ^{th} quantile of a small change in the distribution of a (continuous) explanatory variable and impact effects (for the dummy variables) for the **X** covariates, holding everything else constant, and these are expressed in probability points. Assuming standard assumptions, the OLS estimates are known to be consistent.

In order to implement the unconditional quantile regression method, and to estimate Equation [4.4], the RIF expression [4.3] needs to be computed given that it is unobserved in practice. The RIF expression requires computing the sample quantile value q_τ and then estimating the density value at this point $f_y(\hat{q}_\tau)$ using non-parametric kernel density methods. Using a 'plug-in' method, the RIF estimate for each observation can be obtained by plugging the density estimates into Equation [4.3]. In order to get the quantile value of interest, the probability is then multiplied by the inverse of the density; this changes the outcome variable at each quantile from a probability effect to a quantile effect, which is what is required here. The mean of the transformed variable now corresponds to the selected quantile of interest.

The conditional expectation RIF regression model is then expressed for the current application as follows:

$$E[RIF(y; q_\tau) \mid \mathbf{X}] = \mathbf{X}'\boldsymbol{\beta} + \gamma_1 Per\_Shocks + \gamma_2 Fin\_Shocks + \gamma_3 Loc\_Shocks \qquad [4.5]$$

where the **X** matrix contains the array of demographic and other household-level characteristics discussed in the data section, with the three shock variables included based on the number of times households experienced a particular shock over the prior three-year reference period.

In order to estimate the unconditional quantile regression by OLS using the proposed welfare models in Equation [4.5] to find the associated distributional effects of the shocks on welfare indicators, the empirical approach proceeds as outlined in Firpo *et al.* (2009). First, the RIF-OLS regression method involves the estimation of a linear probability model for being above the quantile of interest (q_τ). The estimates of the $\boldsymbol{\beta}$ coefficients in the models yield marginal/impact effects expressed in terms of probability points. Second, the resultant marginal/impact effect is divided by the kernel (probability) density evaluation at the quantile of interest; this locally inverts the (unconditional) probability effects into (unconditional) quantile effects using an estimated scaling factor given by $\frac{1}{\hat{f}_y(\hat{q}_\tau)}$. The estimator for the density at different quantiles of the outcome variable uses a non-parametric kernel density estimator. The kernel density estimator is defined for quantile q_τ as:

$$\hat{f}_y(\hat{q}_\tau) = \frac{1}{N \times b_y} \sum_{i=1}^{N} K_y\left(\frac{y_i - \hat{q}_\tau}{b_y}\right) \qquad [4.6]$$

where $K_y(z)$ is defined as a kernel function from a choice of different kernel densities (Epanechnikov in this case), the bandwidth (b_y) is a positive scalar (also known as the smoothing or bandwidth parameter) set in advance (e.g., a value of 0.1). The sampling variances for the RIF regression estimates are computed using bootstrapping techniques. Given the research focus, a set of RIF equations for selected quantiles at the 10th, 25th, 50th, 75th and 90th percentiles (i.e., τ= 0.1, 0.25, 0.5, 0.75 and 0.90) are reported in the tables, but relevant plots are used to display the effects at all quantiles across the unconditional distribution using relevant point estimates and their corresponding confidence intervals.

There is also a conceptual difference between conditional and unconditional quantile regressions regarding inequality. While the conditional quantile regression captures the within-group inequality effect controlling for other observed covariates, the unconditional quantile regression captures the total inequality effect of both between-group and within-group effects (Fournier and Koske, 2013). Thus, the latter provides a measure of inequality based on the entire welfare distribution.

As noted earlier, the unconditional quantile regression model has a number of advantages. The RIF-based unconditional quantile regression is estimated by OLS rather than by linear programming techniques. Most importantly, in the case of this study, the RIF-based procedure is extendable beyond quantiles to other inequality-based sample statistics (e.g., the Gini, the variance, the standard error, the inter-quantile-range, and Atkinson's inequality index).

4.5.3 Probit model

Given households can cope with the effect of a shock not only by smoothing consumption and through the sale of assets but also through savings, this study also examines whether households that have savings are more resilient to shocks and if shocks move households below the poverty line. In order to ascertain whether households deplete their savings to cope with the effect of shocks, and if the probability of households are propelled into poverty by such shocks, the study also estimates both a savings and food poverty equation using a non-linear probability model (i.e., the probit model).

The probit model assumes that while we only observe the value of 0 and 1 for an outcome variable, there is an underlying latent (unobserved) variable (y_i^*) that determines the value of an observable binary variable y_i; this adopts a value of either 1 (if the event occurs) or 0 otherwise. The latent dependent variable model can be expressed linearly as:

$$y_i^* = x_i'\boldsymbol{\beta} + \mu_i \qquad i = 1,\ldots,n \qquad [4.7]$$

In the above expression, x_i is a k × 1 vector of characteristics or explanatory variables and β is a k × 1 vector of coefficients. It is assumed that, in practice, the response variable is unobservable, and instead what is observed is a dummy variable defined by:

$$y_i = \begin{cases} 1 & \text{if } y_i^* > 0 \\ 0 & \text{otherwise} \end{cases} \qquad [4.8]$$

The dummy variable determines which of two possible outcomes is observed. The probability of the event occurring can be linked to the latent dependent variable as follows:

$$P[y^* > 0] = P[y_i = 1] = \Phi(z_i) \qquad [4.9]$$

where y_i is the dichotomous realisation of the latent dependent variable (i.e., savings or poverty in this study), $\Phi(\,\cdot\,)$ denotes the cumulative distribution function operator for the standard normal, and the standardised probit index is given by:

$$z_i = \mathbf{X}_i'\boldsymbol{\beta} + \alpha_1 \text{per\_shocks}_i + \alpha_2 \text{fin\_shocks}_i + \alpha_3 \text{loc\_shocks}_i$$

For identification purposes, a unitary assumption is made for σ, which allows the scale of y_i to be fixed. The log-likelihood function is then defined as:

$$\mathrm{L} = \sum_{i=1}^{n} y_i \ln\big(\Phi(z_i)\big) + (1 - y_i)\ln\big(1 - \Phi(z_i)\big) \qquad [4.10]$$

The parameters are estimated using a conventional non-linear optimisation algorithm.

4.6 Empirical Results

The empirical results are now presented, starting with the impact of shocks on the household welfare measures (food expenditure, non-food expenditure and household asset). This is then followed by a discussion of the impact of shocks on savings and food poverty rates of households.

4.6.1 The distributional effect of shocks on household welfare measures

The estimates for the impact of shocks on household food expenditure using unconditional quantile regressions are reported in Table 4.5 For comparison, the OLS (mean) estimates are also reported in column 1, which provides the average effect of shocks on food expenditure. The unconditional quantile regression estimates are reported in the other five columns showing the heterogeneity of the estimated shock effects. The distributional analysis provides empirical insights into the impact of shocks and where along the unconditional household expenditure and assets the shocks exert their effects are strongest. For simplicity, the estimates of the unconditional quantile regressions are reported for the 10th, 25th, 50th, 75th and 90th percentiles respectively. The standard errors reported in parentheses are based on bootstrapping with 500 replications. The remaining control variables included in the specifications are as defined in Table A4.2 in the Appendix.

The OLS coefficient suggests that personal, financial and local shocks exert a negative and significant association with food expenditure even after controlling for important individual and household-level characteristics. The finding reveals that households who in the past three years experienced unanticipated shocks, currently have lower level of food expenditure. On average, the result suggests that households with a larger number of unanticipated shocks exhibit a negative relationship with food consumption. Attention now turns to the unconditional quantile regression estimates. These reveal that heterogeneity exists, especially, in the impact of financial shocks across the distribution when compared to the mean effect of the OLS estimate. From the OLS regression, the estimate of the financial shock is 12%. Yet, these estimates vary significantly from 10% at the 25th quantiles, 14% at the 50th, 18% at the 75th and 23% at the 90th percentiles. It is clear from the result that the negative effect of financial shock from the mean estimates is not constant across the food expenditure distribution. Households located at the 10th percentile do not report significant changes in their food expenditure. The insignificant effect at the 10th percentile could be attributed to households who are producing their own food.

Table 4.5: Food Expenditure OLS and RIF Quantile Estimates

	Mean Estimates	**RIF-Quantile Estimates**				
VARIABLES	**OLS**	**RIF_10**	**RIF_25**	**RIF_50**	**RIF_75**	**RIF_90**
per_cshocks	-0.0789***	-0.0296	-0.0874**	-0.0999***	-0.0784**	-0.0684
	(0.0220)	(0.0455)	(0.0390)	(0.0319)	(0.0398)	(0.0901)
fin_cshocks	-0.1140***	-0.0167	-0.0975***	-0.1420***	-0.1760***	-0.2270***
	(0.0154)	(0.0328)	(0.0251)	(0.0260)	(0.0362)	(0.0687)
loc_cshocks	-0.0618**	-0.0851	-0.0331	-0.103**	0.00568	-0.0787
	(0.0263)	(0.0667)	(0.0517)	(0.0434)	(0.0577)	(0.100)
R-squared	0.732	0.352	0.474	0.480	0.418	0.351
Observations	4,970	4,970	4,970	4,970	4,970	4,970
Households & other controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1. The regression estimations control variables are household characteristics like head's age, household size, dependency ratio, gender, education, employment, and marital status. Additionally, the control variables include location and regions.

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

In order to provide a sharper and clearer insight on the shock gradient across the different percentiles, Figure 4.7 plots the points estimates for the financial shock variable from 5th to the 95th percentile for every second percentile, using confidence intervals. For brevity, the focus will be on the plot for the financial shocks since it exhibits the largest effect on food expenditure.

Turning our attention to the financial shock plot for the food expenditure, the curve reveals common effects between the 74th and the 84th percentiles. The explanation for this relates back to the kernel density plot for food expenditure. It should be remembered that to estimate the unconditional quantile regression model by OLS, the RIF variables need to be computed. This requires exploiting scaling weights from a smoothed kernel density function reflecting the height of the curve at the relevant quantiles. The estimated weights corresponding to the height of the curve are then used to translate the estimated probability effects for the covariates from a linear probability regression model of being above the actual quantile into quantile effects. This is done through scaling the linear probability model estimates by the reciprocal of the kernel density estimate. In our case, however, the kernel density function for the log of food expenditure is not smooth at the right tail, as shown in Figure 4.4, but somewhat flat over this region. This means the height of the curve is the same across the flat segment of the kernel density, meaning the estimated quantile effects are also the same.

The key finding from the analysis is that the impact of financial shocks on food expenditure is considerably more negative at the top end of the distribution than other parts of the distribution.

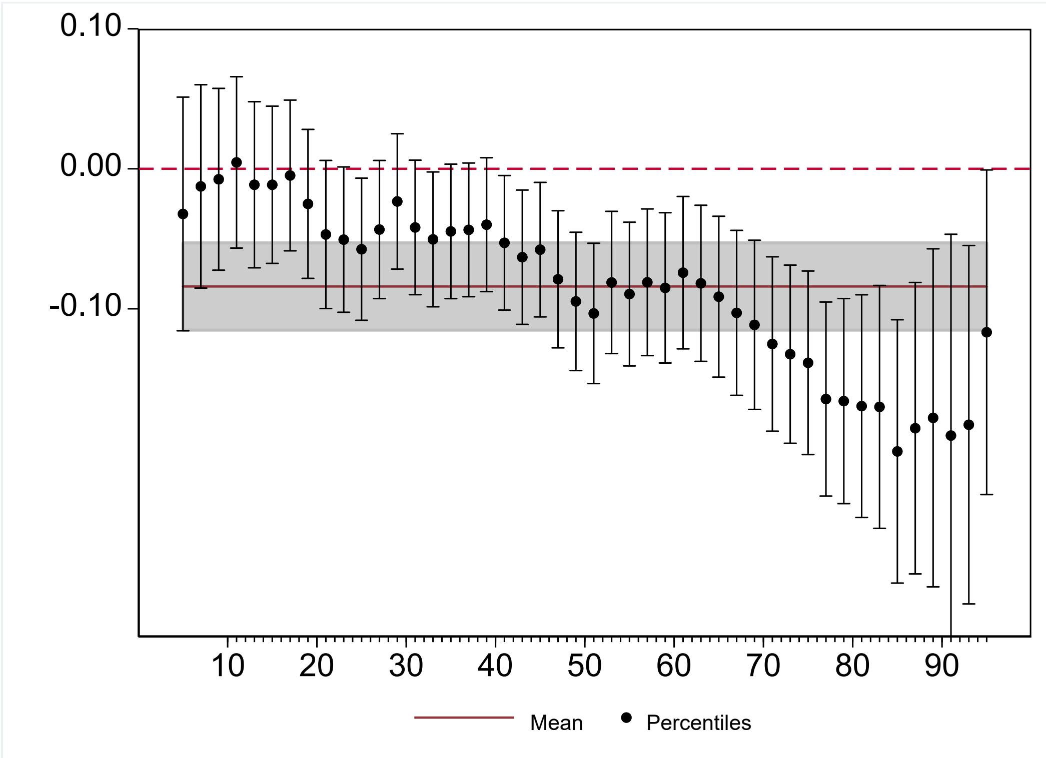

Figure 4.7: Log of Food Expenditure Confidence Interval for Financial Shock

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

We now turn to examine the impact of shocks on non-food expenditure. Table 4.6 presents the results for the OLS and unconditional quantile regression models. The results reveal that households that experienced an increase in food prices in the past three years are those with decreased levels of non-food expenditure in the current period. Each additional financial shock in the past three years leads to a 12% decrease in the level of non-food expenditure, suggesting that households have substantially adjusted their non-food expenditure level in the current period based on shocks in the past.

Again, the last five columns of Table 4.6 contain the estimates for the unconditional quantile regression models. The different unconditional quantile regression estimates reveal that the impact of a financial shock is significant and differs across quantiles. The effects are stronger and negative as we move up the household unconditional non-food expenditure distribution. This negative impact is of greater magnitude for households located at the top percentiles of the unconditional non-food expenditure distribution, with a 19% reduction in non-food expenditure detected for households affected by an increase in food prices at the 90th quantile of the distribution. However, households located at the bottom quantiles of the distribution do not report significant changes in their expenditure patterns in response to a financial shock. The effect of a financial shock on non-food expenditure is likely explained by substitution effects, as most households cut back their spending on non-essentials when affected by a food price hike. This finding supports those reported in Avalos (2016), who found that households substitute between food consumption and non-food consumption in order to mitigate the adverse effects of food price increases.

This narrative differs for households affected by personal and local shocks in this study. The OLS regression and the RIF quantile estimates do not reveal significant impacts for either type of shock on non-food expenditure.

Table 4.6: Non-food Expenditure OLS and RIF Quantile Estimates

	Mean Estimates	**RIF-Quantile Estimates**				
VARIABLES	**OLS**	**RIF_10**	**RIF_25**	**RIF_50**	**RIF_75**	**RIF_90**
per_cshocks	-0.0182	-0.0362	0.0219	-0.0113	-0.0150	-0.0586
	(0.0199)	(0.0435)	(0.0309)	(0.0332)	(0.0379)	(0.0358)
fin_cshocks	-0.117***	-0.0326	-0.0639***	-0.103***	-0.178***	-0.193***
	(0.0168)	(0.0294)	(0.0241)	(0.0237)	(0.0276)	(0.0313)
loc_cshocks	0.00445	-0.0142	-0.00658	0.00203	-0.0351	0.0428
	(0.0374)	(0.0871)	(0.0737)	(0.0490)	(0.0517)	(0.0698)
R-squared	0.508	0.162	0.299	0.369	0.307	0.201
Observations	4,970	4,970	4,970	4,970	4,970	4,970
Households & other controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**

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

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

The plots for the point estimates and confidence interval in Figure 4.8 mirror the distributional effect shown at different quantiles across the non-food expenditure distribution for financial shock. From around the 15th percentile onwards there is a statistically significant decrease along the unconditional log non-food expenditure distribution. The reduction in the level of non-food expenditure widens and becomes more statistically significant with progress along the distribution, suggesting an adjustment in current spending is being made by the households affected by financial shock in the past three years.

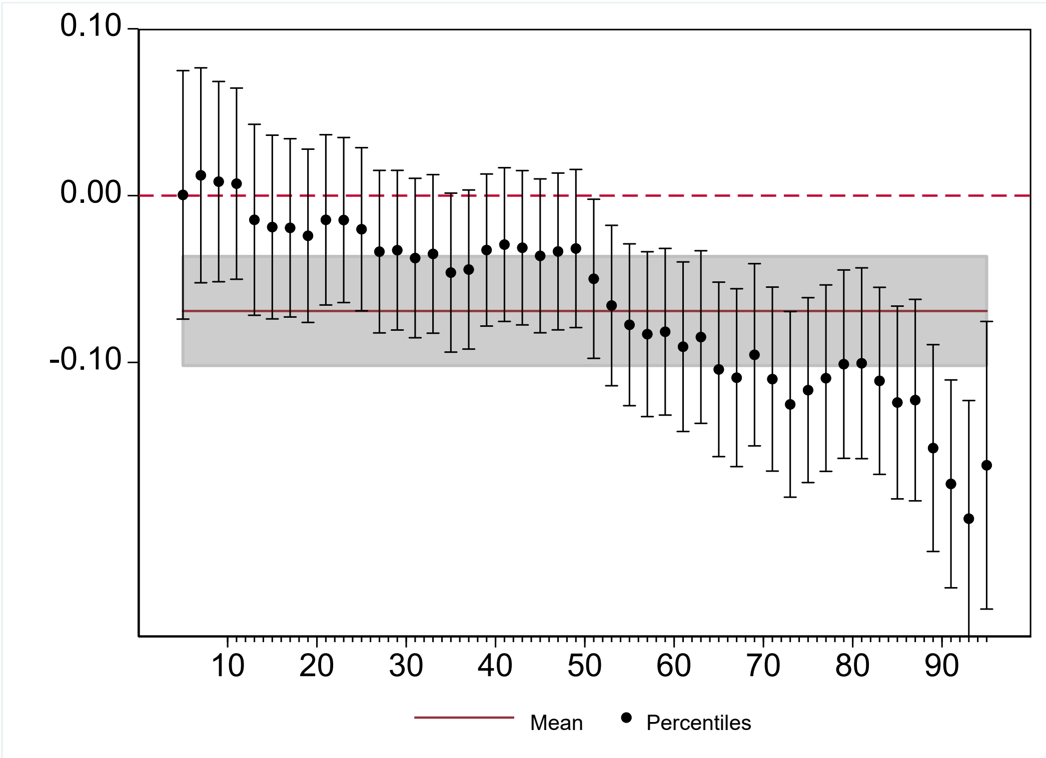

Figure 4.8: Log of non-food expenditure confidence interval plots for financial shock

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

Table 4.7 presents the estimated impact of shocks on household assets, our third welfare measure based on the household asset values. The result shows that the impact of financial shock is uniformly positive across the household asset distribution, indicating that households that were affected by financial shocks in the last three years are associated with a decrease in asset value. The OLS results reveal that, on average, both financial shocks and local shocks reduce household asset values. A significant finding from the unconditional quantile regression analysis reveals that the effect of financial shocks and local shocks varies significantly across the distribution of household asset values, indicating an heterogeneous effect of these shocks. The OLS estimates suggest that a 32% and 23% reduction in household assets can be attributed to the impact of financial shocks and local shocks respectively. The estimates for the unconditional quantile regression reveal that the impact of financial and local shocks varies across quantiles of the asset distribution, except at the 90th percentile of the household asset distribution. The estimates for the median regression are broadly comparable to those obtained for the mean regression. The estimated impact of financial shock on household asset ranges from 29%-48%, while the range for local shocks is between 16%-44%.

However, the effect of personal shocks on household assets is statistically insignificant for both the OLS regression and at the quantiles, except at the 75th percentile that recorded a negative impact of 16%. The findings here provide interesting insights, highlighting the responsiveness of asset holdings to adverse external shocks. The significant negative impact of financial and local shocks on household assets in general might indicate that assets can easily be converted into cash or any form of buffer for consumption during a severe covariate shock. It is also possible that households that were severely affected by financial and local shocks would prefer depleting their assets to cope with the adverse effect of shocks rather than reducing food and non-food expenditure. This makes intuitive sense, since a reduction in food and non-food expenditure may entail compromising the nutrition and the quality of life of households.

Table 4.7: Household Assets - OLS and RIF Quantile Estimates

	Mean estimates	**RIF-Quantile estimates**				
Variables	**OLS**	**RIF_10**	**RIF_25**	**RIF_50**	**RIF_75**	**RIF_90**
per_cshocks	-0.0601	-0.101	-0.0461	0.0342	-0.155***	-0.158
	(0.0448)	(0.105)	(0.0662)	(0.0529)	(0.0549)	(0.103)
fin_cshocks	-0.318***	-0.306***	-0.290***	-0.319***	-0.291***	-0.475***
	(0.0344)	(0.0766)	(0.0534)	(0.0471)	(0.0418)	(0.0831)
loc_cshocks	-0.227***	-0.435***	-0.253***	-0.218***	-0.161**	-0.156
	(0.0524)	(0.146)	(0.0897)	(0.0724)	(0.0640)	(0.102)
R-squared	0.318	0.107	0.184	0.212	0.217	0.182
Observations	4,970	4,970	4,970	4,970	4,970	4,970
Households & other controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**

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

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

Further illustration of the distributional effect of financial shock on household assets can be seen in Figure 4.9, which plots the points estimates for the financial shock variable from 5th to the 95th percentile for every second percentile, using confidence intervals. The curve reveals a common negative effect along the household asset distribution.

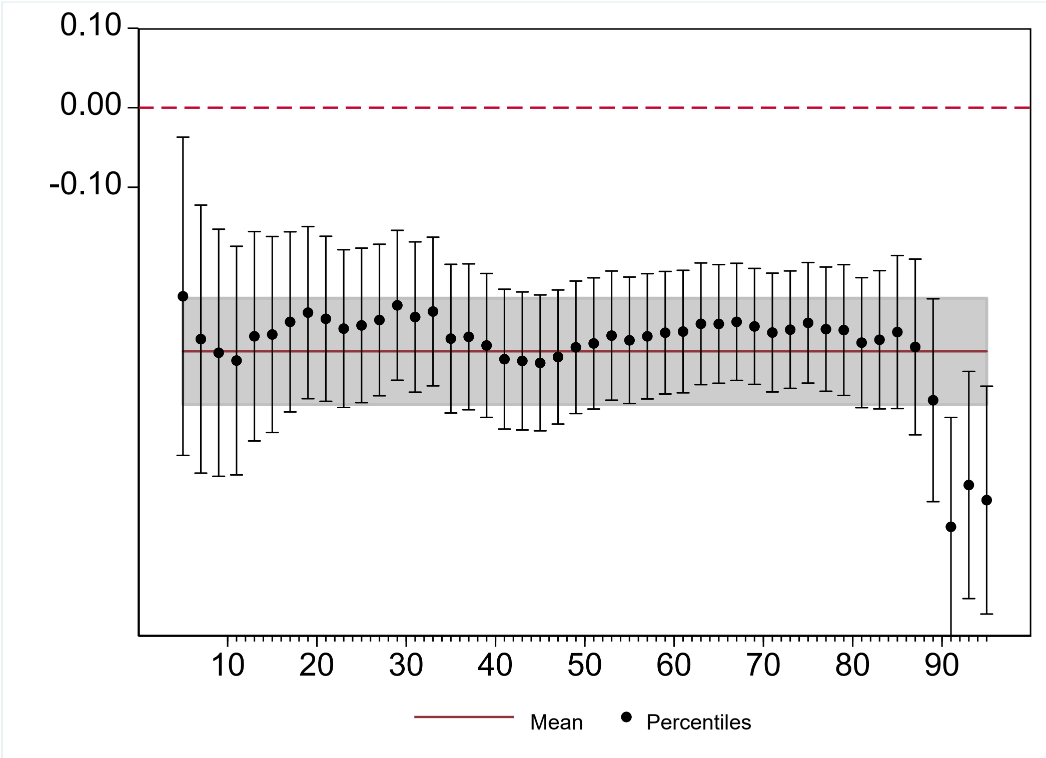

Figure 4.9: Log of household asset confidence interval plots for financial shock

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

The disaggregation of our sample into farm and non-farm households yields an interesting result. The estimates of the farm and non-farm food per capita expenditure presented in Table 4.8 revealed that, on average, there is negative association between farm and non-farm households that were affected by food price shock in the last three years and their level of current food expenditure. However, a more complete and more interesting picture of the relationship emerged when looking at the results estimated from the unconditional quantile regression models. These results show that the effect of financial shock on food consumption was not significant at the 10th, 25th and 50th percentiles of the food expenditure distribution. The significantly heterogeneous effect is only seen at the top end of the distribution. The effect was the largest for households at the top end (the 75th and 90th percentiles) of the food expenditure distribution. Specifically, the effect of financial shock reduces the level of current food expenditure by 19% and 26% for those in the 75th and 90th percentiles. However, we found that non-farm households who in the previous years were affected by financial shock, currently had lower levels of food expenditure at all the quantiles, except in the 10^{th}.

This result suggests that it is plausible that farm households may be able to break even when the prices of staple food products go up, as these households are not affected by the rising prices of other products (De Janvry and Sadoulet, 2011). If so, this would affect the impact of a food price increase on food consumption, this may suggest that those poorer households at the bottom of the distribution, that are not affected by food price shocks, may be producing their own food and are self-sufficient. On the other hand, net buyers are more vulnerable to the effects of food-price shocks, because they produce less than they need and cover the shortfall with purchases from the market. While net sellers sell their surplus and gain during food price shocks, they may be affected mostly by the increase in input prices. What is less clear is whether farm households would also have faced higher input prices. Unfortunately, there is no adequate information available to assess whether farm households are affected by both food price and input price increases.

Table 4.8: Farm and Non-Farm OLS and RIF Quantile Regression Estimates for Food Expenditure

FARM HOUSEHOLD FOOD PER CAPITA EXPENDITURE						
Variables	**OLS**	**RIF_10**	**RIF_25**	**RIF_50**	**RIF_75**	**RIF_90**
per_cshocks	-0.0593*	-0.0197	-0.0745	-0.107**	-0.0607	-0.0356
	(0.0312)	(0.0609)	(0.0455)	(0.0483)	(0.0664)	(0.108)
fin_cshocks	-0.115***	-0.0444	-0.0216	-0.0591	-0.192***	-0.258***
	(0.0255)	(0.0441)	(0.0377)	(0.0393)	(0.0579)	(0.0952)
loc_cshocks	-0.0617*	-0.127	-0.00179	-0.0653	-0.0471	-0.158
	(0.0327)	(0.0842)	(0.0579)	(0.0548)	(0.0808)	(0.102)
Observations	2,215	2,215	2,215	2,215	2,215	2,215
R-squared	0.710	0.323	0.457	0.466	0.410	0.323
Households & other controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
NON-FARM FOOD PER CAPITA EXPENDITURE						
Variables	**OLS**	**RIF_10**	**RIF_25**	**RIF_50**	**RIF_75**	**RIF_90**
per_cshocks	-0.0957***	-0.0469	-0.117**	-0.131***	-0.0760	-0.0703
	(0.0289)	(0.0768)	(0.0529)	(0.0446)	(0.0568)	(0.112)
fin_cshocks	-0.1180***	-0.0741	-0.124***	-0.103***	-0.178***	-0.190**
	(0.0194)	(0.0537)	(0.0337)	(0.0339)	(0.0439)	(0.0926)
loc_cshocks	-0.0730*	-0.0763	-0.1370*	-0.1130*	0.1320	-0.1420
	(0.0426)	(0.116)	(0.0753)	(0.0636)	(0.0869)	(0.178)
Observations	2,755	2,755	2,755	2,755	2,755	2,755
R-squared	0.739	0.383	0.485	0.469	0.419	0.365
Households & other controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**

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

Source: Author's calculations using the General Household Survey-GHS data (2018/2019)

In Table 4.9, based on the sample of farm and non-farm households, the result reveals that the effect of financial shocks on non-food expenditure is similar for both farm and non-farm households. The results suggest a significant negative association between financial shock and non-food expenditure among farm and non-farm households. The effect along the unconditional distribution of non-food expenditure is heterogeneous. No significant association is found for both personal and local shocks with the level of non-food expenditure.

Table 4.9: Farm and Non-farm OLS and RIF Quantile Regression Estimates for Non-food Expenditure

FARM HOUSEHOLD NON-FOOD PER CAPITA EXPENDITURE						
Variables	**OLS**	**RIF_10**	**RIF_25**	**RIF_50**	**RIF_75**	**RIF_90**
per_cshocks	0.00594	0.0278	0.0429	-0.00838	0.00266	0.0253
	(0.0249)	(0.0516)	(0.0406)	(0.0408)	(0.0496)	(0.0641)
fin_cshocks	-0.0987***	-0.0702	-0.0860**	-0.0879**	-0.134***	-0.151***
	(0.0232)	(0.0459)	(0.0353)	(0.0358)	(0.0417)	(0.0561)
loc_cshocks	0.00939	0.00625	-0.0412	0.0192	-0.0144	-0.0699
	(0.0326)	(0.0585)	(0.0517)	(0.0508)	(0.0550)	(0.0584)
Observations	2,215	2,215	2,215	2,215	2,215	2,215
R-squared	0.422	0.112	0.232	0.314	0.266	0.181
Households & other controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
NON-FARM HOUSEHOLD NON-FOOD PER CAPITA EXPENDITURE						
Variables	**OLS**	**RIF_10**	**RIF_25**	**RIF_50**	**RIF_75**	**RIF_90**
per_cshocks	-0.0443	-0.0784	0.0148	-0.0627	-0.0231	-0.0669
	(0.0320)	(0.0704)	(0.0433)	(0.0464)	(0.0463)	(0.0517)
fin_cshocks	-0.132***	-0.0116	-0.121***	-0.173***	-0.159***	-0.205***
	(0.0213)	(0.0422)	(0.0340)	(0.0330)	(0.0363)	(0.0425)
loc_cshocks	-0.0192	-0.0170	-0.0729	-0.0426	0.0281	0.0461
	(0.0408)	(0.114)	(0.0650)	(0.0574)	(0.0646)	(0.0802)
Observations	2,755	2,755	2,755	2,755	2,755	2,755
R-squared	0.489	0.203	0.336	0.360	0.273	0.178
Households & other controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**

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

Source: Author's calculations using the General Household Survey-GHS data (2018/2019)

Table 4.10 reports the estimates of the OLS and the UQR models for household assets between the farm and non-farm household. The results reveal a significant negative association between financial shocks and asset accumulation among farm and non-farm households, on average and *ceteris paribus*. The results indicate significant variation in the effects at various quantiles of the asset distribution both in farm and non-farm households. Being affected by local shocks in the last three years is also negatively associated with current asset levels. Notably, the result shows a significant variation in the 10th, 25th and

50th percentiles for farm households, but not at the top end of the asset distribution. In contrast, non-farm households exhibit significant variation in the 50th, 75th and 90th percentiles and not at the bottom end of the distribution. The result suggests that the heterogeneous response may be related to how farm and non-farm households deplete their asset holdings when faced with location-specific shocks.

Table 4.10: Farm and Non-farm OLS and RIF Quantile Regression Estimates for Household Assets

FARM HOUSEHOLD ASSET						
Variables	**OLS**	**RIF_10**	**RIF_25**	**RIF_50**	**RIF_75**	**RIF_90**
per_cshocks	-0.0321	-0.156	0.107	0.0594	-0.101	-0.155
	(0.0666)	(0.139)	(0.0909)	(0.0839)	(0.0783)	(0.123)
fin_cshocks	-0.222***	-0.192*	-0.245***	-0.217***	-0.195***	-0.208***
	(0.0501)	(0.115)	(0.0748)	(0.0649)	(0.0576)	(0.0680)
loc_cshocks	-0.257***	-0.518**	-0.347***	-0.216**	-0.0755	-0.0629
	(0.0705)	(0.205)	(0.119)	(0.101)	(0.0703)	(0.105)
Observations	2,215	2,215	2,215	2,215	2,215	2,215
R-squared	0.251	0.086	0.141	0.168	0.186	0.159
Households & other controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
NON-FARM HOUSEHOLD ASSET						
Variables	**OLS**	**RIF_10**	**RIF_25**	**RIF_50**	**RIF_75**	**RIF_90**
per_cshocks	-0.0847	-0.0349	-0.0869	-0.0702	-0.198***	-0.150
	(0.0598)	(0.153)	(0.0970)	(0.0744)	(0.0759)	(0.125)
fin_cshocks	-0.415***	-0.402***	-0.351***	-0.403***	-0.416***	-0.583***
	(0.0474)	(0.106)	(0.0725)	(0.0587)	(0.0613)	(0.0961)
loc_cshocks	-0.188**	-0.189	-0.126	-0.262**	-0.364***	-0.204
	(0.0857)	(0.199)	(0.149)	(0.120)	(0.0998)	(0.151)
Observations	2,755	2,755	2,755	2,755	2,755	2,755
R-squared	0.325	0.136	0.198	0.208	0.209	0.165
Households & other controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**

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

Source: Author's calculations using the General Household Survey-GHS data (2018/2019)

Overall, the results suggest that the mean regression approach used to study the effects of shocks on household welfare may conceal some heterogeneity that exists in the impact of shocks on the welfare distribution of households. We also find some interesting results in the disaggregated models of farm and non-farm households.

4.6.2 The impact of shocks on household savings rate and food poverty level

Table 4.11 presents the results of the probit estimates of savings and household poverty level. The table reports the probit impact effects on household savings and poverty levels.[23]

The estimate in the first column suggests that households that were affected by financial shocks in the last three years have lower probability of savings by 5 percentage points, on average and *ceteris paribus*, and the effect is statistically significant at the 1% level using a two-tailed test. In contrast, households affected by previous local shocks have higher probabilities of savings in the current period. On the face of it, this appears on the face of it to be somewhat counter-intuitive. However, it is generally believed that households use savings to smooth lifetime consumption, and individuals tend to be more conservative and save more in times of increased uncertainty, which is consistent with a precautionary savings motive hypothesis. Recall that the local shocks component includes natural disasters and conflict, which constitute events of great uncertainty. This finding is broadly in line with historical experience and previous empirical evidence (Giavazzi and McMahon, 2012; Luo and Kinugasa, 2020) on the impact of shocks on saving behaviour of households. This indicates that the motive behind household savings determines how households hold and use savings during and then after a crisis. It is possible that households that lost assets during the natural disaster will have to spend their savings to recover them. This finding suggests that savings are used as a cushion and a mode of financial resilience for the households. However, it needs to be stressed that the foregoing is largely descriptive in nature and the most effective way of identifying the impact of shocks on savings would require panel data before and after the shocks. However, such data are not available to us in this study.

[23] The estimates for the probit models provide the impact of the relevant shocks on the probability of a household either being in food poverty or undertaken savings at the average

We now discuss the impact of shocks on the household food poverty incidence. The probit estimates are reported in Table 4.11 (see the second column).

Table 4.11: Probit Estimates of Savings and Food Poverty Rates

Variables	Savings	Food Poverty
per_cshocks	-0.0221 (0.0155)	0.0389*** (0.0118)
fin_cshocks	-0.0493*** (0.0108)	0.0521*** (0.00956)
loc_cshocks	0.0441** (0.0197)	0.0326** (0.0158)
Sample size	**4,970**	**4,970**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
The regression estimations control variables are household characteristics like head's age, household size, dependency ratio, gender, education, employment, and marital status. Additionally, the control variables include location and regions.

Source: Author's calculations using the General Household Survey-GHS data (2018/2019)

The estimates suggest that all three measures of shocks have a statistically significant effect on the food poverty rate. As expected, our results support the argument that with a negative impact of shocks on household welfare, food poverty rates increase. The rising cost of food and other natural calamities such as floods, and personal shocks such as death and illness, can have a significant impact on the most vulnerable households in the country. These factors can lead to an increase in poverty and the failure of these families to access adequate food which can lead to detrimental effects even in the short-term.

4.7 Policy Implications

The food price hike represents an additional economic shock for the Nigerian economy during a recession. Therefore, its effect on household welfare is potentially significant. After the 2014 recession, the economy became highly vulnerable, particularly due to the pronounced decline in oil prices globally. The gloomy macro-economic conditions, combined with the civil unrest and insurgency, hampered the production of food, especially in the northern part of the country, and this may have led to an increase in food inflation. The study exploited the sharp increase in the food inflation rate between 2016 and 2019 to examine the impact of unanticipated shocks, with a special focus on financial shocks (as measured by the increase of food price shocks), on household welfare measures.

Our research shows that financial shocks exert the most influence on welfare measures, and households deplete their assets and savings to cushion the effect of such shocks. The use of household assets and savings as shock absorbers to mitigate the effect of shocks may be a recommended measure for coping with adverse effects of shocks if it is a one-off event. However, if financial shocks are allowed to persist over a long period because of poor economic management, then households will become extremely vulnerable to a point where they may not have these resilient tools to mitigate the negative effects of shocks in the long run. The management of financial shocks are important because if they are driven by inflation, the Central Bank has potential control and policy instruments that can be leveraged to mitigate the short-run effects of food price hikes. Therefore, it is crucial that the Central Bank gets the management of food inflation right; otherwise, it can have adverse effects on food and non-food expenditure, leading to the depletion of assets and savings. The government needs to put in place a robust economic management policy to enable the economy to rebound and allow households to restock and replenish their savings after using them as shock absorbers in the short run. This measure will help reduce households' vulnerability and enhance their resilience to economic shocks in the long run.

The significant regional effects of location-specific shocks re-emphasise the need for targeted and differentiated regional based policies. In line with our findings, the national climate change policy document for Nigeria identified the coastal regions, erosion and desertification-prone areas in the south-eastern and northern parts of Nigeria as the most vulnerable regions to climate change shocks. While the Central Bank has direct control and policy instruments that can mitigate the effect of financial shocks, regional and state governments are left with little or no power to mitigate regional specific local shocks that are associated with climate change. Climate change interventions are controlled at the federal level; therefore, regional interventions will require a complete restructuring of regional powers, so that the regions can have direct control over regional measures.

4.8 Conclusions

This study examined the impact of shocks on welfare measures using data drawn from the 2018/2019 Nigerian General Household Survey. The analysis further explored the impact of shocks on savings and food poverty rates. The study contributes to a sparse literature on the distributional impact of shocks on selected household welfare indicators and the important shock absorbers used by households to cushion the adverse effect of financial shocks in Nigeria.

Our analysis differs in terms of empirical methodology and research objectives from earlier studies undertaken in this area. This study analysed the heterogeneous effect of shocks, focusing on their distributional impact rather than on the common homogenous effect of shocks that have featured more prominently in this research area. The primary research objective explores a specific shock event, the sharp spike in food inflation rate due to increase in prices of food items, in addition to personal and local shocks. The study provided some empirical insights on the potential magnitude of the effect of the food price hike in Nigeria between 2016 and 2019.

The key research finding is that financial shocks, among other shocks, exert the most influence on household welfare measures, and the use of assets and savings as shock absorbers provide the cushion and financial resilience for most affected households.

Another finding of interest is that the study revealed a stronger and larger effect of financial shocks on household assets, and a significant effect on savings and poverty rates. The study finds that households experienced a sharp decrease in their asset values by 32%, on average, in response to a financial shock. The financial shock is also found to have depleted household savings by 5 percentage points, while food poverty rate increased by 5 percentage points over the same period. The magnitude of the financial shock estimates provides empirical insight into the sharp spike witnessed in the food inflation rate between the period 2016 and 2019 (see Figure 4.3). The result demonstrates the importance and the use of household assets and savings as shock absorbers in cushioning the adverse effects of shocks over this period. The results reveal that these mechanisms are successful in mitigating the effect of shocks, but an important question we need to ask is, what if these shocks continue. Households cannot replenish their assets and savings rendering them more vulnerable to future shocks, which will have significant implications for the welfare of households.

A key policy implication is for the Central Bank to take food inflation management seriously. If food price increases are allowed to persist over time because of the poor economic management of policy by the government, households will use up their assets and savings, and will become extremely vulnerable to increased levels of poverty in the future. This impact is seen in the significant effect recorded for the household food poverty rate. The study also identified those households that are most affected by shocks, showing a regional and location dimension to the effect of shocks. Unlike in the case of a financial shock, the Central Bank has some policy instruments to control and mitigate the effect of inflationary shock on the economy; local shocks may require a complete restructuring of regional powers to be able to mitigate the effects of local shocks.

Appendix for Chapter 4

Table A4.1: Description of Variables Used in the Analysis

Variable	Variable Description
Log of total asset	The log of total household asset.
Log of food expenditure	The log of weekly household food per capita expenditure.
Log of non-food expenditure	The log of monthly household non-food per capita expenditure.
Savings	=1 if the household has savings; = 0 otherwise.
Food poverty rate	=1 if the household is below food poverty line; = 0 otherwise
Personal shocks	Frequency of times a household is affected by personal shocks
Financial shocks	Frequency of times a household is affected by financial shocks
Local shocks	Frequency of times a household is affected by local shocks
North Central	=1 if the household is in North Central Region; = 0 otherwise
North West	=1 if the household is in North West Region; = 0 otherwise
North east	=1 if the household is in North East Region; = 0 otherwise
South-South	=1 if the household is in South-South Region; = 0 otherwise
South East	=1 if the household is in South East Region; = 0 otherwise
South West	=1 if the household is in South West Region; = 0 otherwise
Dependency ratio	Ratio of dependents to the working age
Urban	=1 if the household is in the urban area; = 0 otherwise.
Gender	=1 if the head of household is male; = 0 otherwise.
Age	The age of the head of household.
Age2	The squared of the age of the head of household.
Household size	The total number of individuals in the household.
Married	=1 if the individual is married; = 0 otherwise.
Divorced	=1 if the individual is divorced; = 0 otherwise.
Widowed	=1 if the individual is widowed; = 0 otherwise.
Single	=1 if the individual is single; = 0 otherwise.
No education	=1 if the individual has no education; = 0 otherwise.
Informal education	=1 if the individual achieved informal education; = 0 otherwise.
Primary education	=1 if the individual achieved primary level; = 0 otherwise.
Secondary education	=1 if the individual achieved secondary level; = 0 otherwise.
Tertiary education	=1 if the individual achieved tertiary level; = 0 otherwise.
Public employment	=1 if the individual is in public employment; = 0 otherwise.
Private employment	=1 if the individual is in private employment; = 0 otherwise.
Farm employment	=1 if the individual is in farm employment; = 0 otherwise.
Business employment	=1 if the individual is in private business; = 0 otherwise.
Unemployed	=1 if the individual is unemployed; = 0 otherwise

Source: Author’s description based on General Household Survey-GHS 2018/2019)

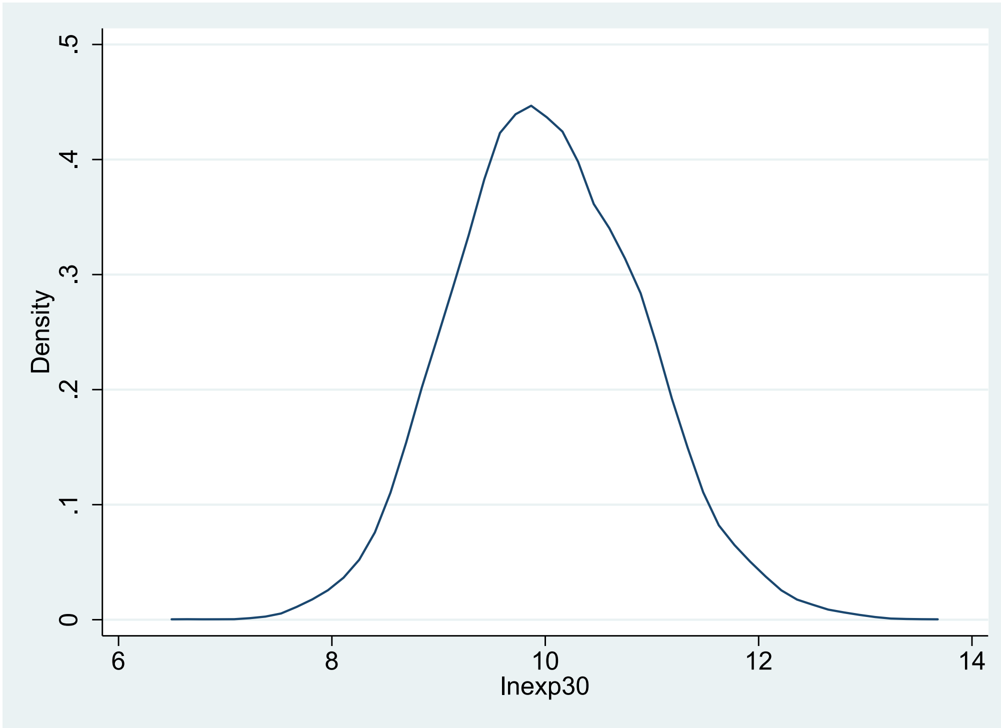

Figure A4.1: Kernel Density of the Distribution of Household Non-Food Expenditure

Source Author's calculations using the General Household Survey-GHS data (2018/2019).

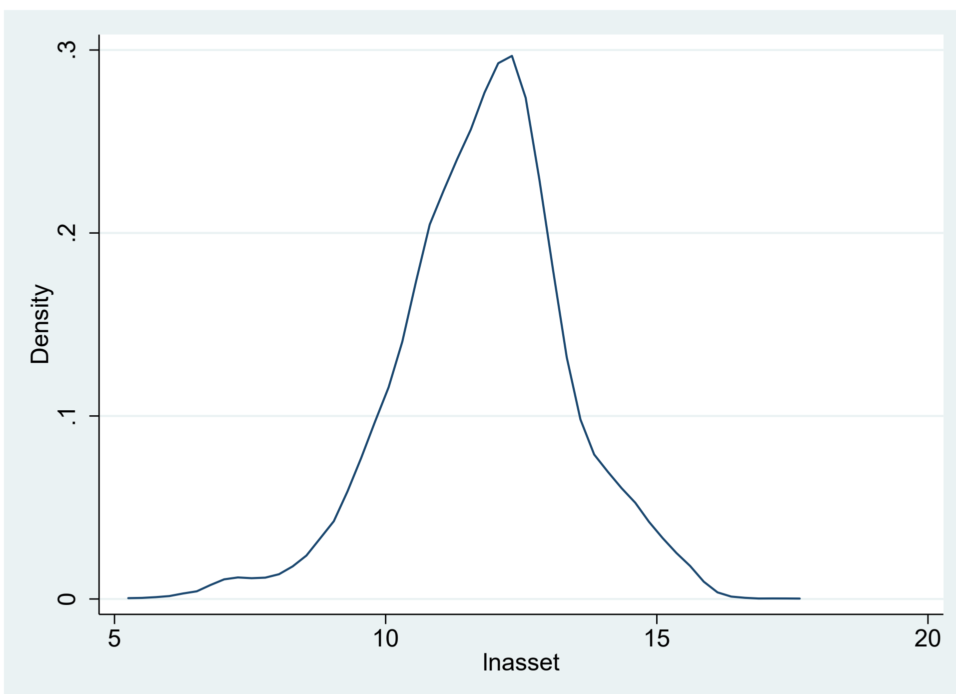

Figure A4.2: Kernel Density of the Distribution of Household Asset Value

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

Table A4.2: Summary Statistics of Explanatory Variables

VARIABLES	(1) N	(2) mean	(3) Sd
Household Demographics:			
Household size	4,970	6.017	3.659
Urban	4,970	0.319	0.466
Head's Demographics:			
Gender	4,970	0.805	0.396
Age	4,970	49.75	15.377
Age2	4,970	2711.2	1653.5
Head's marital status			
Married	4,970	0.753	0.432
Divorced	4,970	0.0357	0.185
Widowed	4,970	0.159	0.366
Single	4,970	0.053	0.223
Head's education			
No education	4,970	0.230	0.421
Primary education	4,970	0.243	0.429
Secondary education	4,970	0.264	0.441
Tertiary education	4,970	0.201	0.392
Informal education	4,970	0.073	0.260
Head's employment			
Unemployed	4,970	0.097	0.296
Public employment	4,970	0.029	0.169
Private employment	4,970	0.038	0.191
Business employment	4,970	0.390	0.488
Farm employment	4,970	0.446	0.497
Location			
South East	4,970	0.165	0.371
North East	4,970	0.166	0.372
South-South	4,970	0.164	0.371
North Central	4,970	0.144	0.351
South West	4,970	0.166	0.371
North West	4,970	0.170	0.376

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

Table A4.3: Difference in Log of Food Expenditure between Shocks and no shocks

Food Expenditure	No Shocks	Personal shocks	Financial shocks	Local shocks
Mean	9.1343	8.9219	9.1311	8.8075
	(0.0229)	(0.0534)	(0.0287)	(0.0607)
Differentials		-0.2124***	-0.0032	-0.3268***
		(0.0581)	(0.0566)	(0.0649)
Percentiles				
10th	7.6879	7.5080	7.8221	7.3360
	(0.0305)	(0.0798)	(0.0357)	(0.1029)
Differentials		-0.1799***	0.1342***	-0.3516***
		(0.0854)	(0.0470)	(0.0115)
25th	8.3015	8.0859	8.3753	7.9883
	(0.0268)	(0.0667)	(0.0347)	(0.0847)
Differentials		-0.2156***	0.0738*	-0.3132
		(0.0719)	(0.0438)	(0.2981)
50th	9.0182	8.7993	8.9974	8.6207
	(0.0264)	(0.0604)	(0.0353)	(0.0694)
Differentials		-0.2189***	-0.0208	-0.3975***
		(0.0659)	(0.0440)	(0.0743)
75th	9.8120	9.5968	9.7196	9.5488
	(0.0347)	(0.0743)	(0.0451)	(0.0857)
Differentials		-0.2152***	0.0924*	-0.2632***
		(0.0820)	(0.0569)	(0.0925)
90th	10.8291	10.5763	10.6722	10.3788
	(0.0648)	(0.1343)	(0.0814)	(0.1418)
Differentials		-0.2528***	-0.1569	-0.4503***
		(0.0222)	(0.1040)	(0.1559)
Sample size	**2,723**	**513**	**1,522**	**370**

Notes: a. Differentials denotes the difference between households with shocks and households with no shocks; the standard errors for each selected quantile value are obtained using an unconditional quantile regression for each sub-sample regressing the relevant RIF on only a constant; the standard error reported in parentheses are robust.
b. Personal shock, financial shock and local shock are dummy variables defined as whether you had any shock.
c. The difference in the sample size is due to households being affected by more than one shock.
d. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

Table A4.4: Difference in Log of Non-food Expenditure between Shocks and No shocks

Non-food Expenditure	No Shocks	Personal shock	Financial shocks	Local shocks
Mean	10.0718	9.8989	10.0458	9.2323
	(0.0178)	(0.0364)	(0.0204)	(0.0573)
Differentials		-0.1729***	-0.0260	-0.8395***
		(0.0405)	(0.0271)	(0.0600)
10th	8.9152	8.8130	9.0225	8.7789
	(0.0257)	(0.0648)	(0.0304)	(0.0783)
Differentials		-0.1022	-0.1073***	-0.1363*
		(0.0697)	(0.0398)	(0.0824)
25th	9.4144	9.3630	9.5082	9.2073
	(0.0220)	(0.0520)	(0.0279)	(0.0648)
Diff		-0.0514	-0.0938***	-0.2071***
		(0.0565)	(0.0355)	(0.0684)
50th	10.0344	9.8440	10.0478	9.7151
	(0.0219)	(0.0502)	(0.0293)	(0.0578)
Differentials		-0.1904***	0.0134	-0.3193***
		(0.0548)	(0.0366)	(0.0618)
75th	10.7096	10.4765	10.5995	10.3341
	(0.0257)	(0.0529)	(0.0328)	(0.0559)
Differentials		-0.2331***	-0.1101***	-0.3755***
		(0.0588)	(0.0417)	(0.0615)
90th	11.2661	10.9832	11.0615	10.9992
	(0.0309)	(0.0536)	(0.0344)	(0.0646)
Differentials		-0.2829***	-0.2046***	-0.2669***
		(0.0619)	(0.0462)	(0.0716)
Sample size	**2,723**	**513**	**1,522**	**370**

Notes: see notes above in Table 4.4.

Source: Author’s calculations using the General Household Survey-GHS data (2018/2019).

Table A4.5: Difference in Log of Asset between Shocks and No shocks

Asset Value	No shocks	Personal shocks	Financial shocks	Local shocks
Mean	12.0610	11.4916	11.4932	11.3359
	(0.0298)	(0.0618)	(0.0389)	(0.0796)
Differentials		-0.5694***	-0.5828***	-0.7038***
		(0.0689)	(0.0489)	(0.0850)
10th	10.1322	9.6952	9.5589	9.3021
	(0.0492)	(0.1403)	(0.0854)	(0.1869)
Differentials		-0.4370***	-0.5733***	-0.8301***
		(0.1487)	(0.0985)	(0.0373)
25th	11.0779	10.6819	10.5883	10.4599
	(0.0368)	(0.0953)	(0.0563)	(0.1170)
Differentials		-0.3960***	-0.4896***	-0.6180***
		(0.1022)	(0.0672)	(0.1216)
50th	12.1000	11.7629	11.5957	11.4496
	(0.0341)	(0.0789)	(0.0453)	(0.0904)
Differentials		-0.3371***	-0.5043***	-0.6504***
		(0.0070)	(0.0578)	(0.0972)
75th	12.9909	12.2556	12.4702	12.3094
	(0.0358)	(0.0602)	(0.0401)	(0.0738)
Differentials		-0.7353***	-0.5207***	-0.6815***
		(0.0700)	(0.0537)	(0.1017)
90th	14.1723	12.9174	13.2562	13.0257
	(0.0769)	(0.0889)	(0.0708)	(0.1189)
Differentials		-1.2549***	-0.9161***	-1.1466***
		(0.2912)	(0.2862)	(0.3099)
Sample Size	**2,723**	**513**	**1,522**	**370**

Notes: see notes above in Table 4.4.

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

Table A4.6: Differences in Savings Rate and Food Poverty Rate between Shocks and No shocks

	No Shocks	Personal shock	Financial shock	Local shock
Savings	0.5409	0.4659	0.5039	0.5405
	(0.0096)	(0.0220)	(0.0128)	(0.0259)
Differentials		-0.0750***	-0.0370***	0.0004
		(0.0240)	(0.0160)	(0.0276)
Food poverty rate	0.4950	0.5789	0.5013	0.6297
	(0.0096)	(0.0251)	(0.0128)	(0.0251)
Differentials		0.0839***	0.0063	0.1347***
		(0.0269)	(0.0160)	(0.0269)

Notes: a. Differentials denotes the difference between households with shocks and households with no shocks; robust standard error in parenthesis.
b. Personal shock, financial shock and local shock are dummy variables defined as whether you had any shock.
c. The difference in the sample size is due to households being affected by more than one shock.
d. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Source: Author's calculations using the General Household Survey-GHS data (2018/2019).

Table A4.7: Household Characteristics by Types of Shocks and No shocks

Variables	No shock	Personal shock	Financial shock	Local shock
NC	0.1888	0.0312	0.1025	0.0784
	(0.3914)	(0.1740)	(0.3034)	(0.2691)
NE	0.1484	0.2885	0.1261	0.3216
	(0.3555)	(0.4535)	(0.3321)	(0.4677)
NW	0.1998	0.1520	0.1110	0.1432
	(0.4000)	(0.3594)	(0.3143)	(0.3508)
SE	0.1172	0.3080	0.2096	0.2378
	(0.3217)	(0.4621)	(0.4072)	(0.4263)
SS	0.1172	0.2066	0.2497	0.1648
	(0.3217)	(0.4053)	(0.4330)	(0.3715)
SW	0.1788	0.0097	0.2050	0.0514
	(0.3833)	(0.0983)	(0.4038)	(0.2210)
urban	0.3316	0.1657	0.3784	0.1324
	(0.4708)	(0.3722)	(0.4852)	(0.3394)
gender	0.8278	0.7739	0.7536	0.8216
	(0.3777)	(0.4187)	(0.4310)	(0.3833)
age	49.5310	49.9961	50.5703	48.7162
	(15.4055)	(15.4079)	(15.5648)	(14.3620)
hhsize	6.1069	6.1150	5.6419	6.5459
	(3.8265)	(3.6132)	(3.2502)	(3.7553)
DepRat	1.1680	1.2056	1.1822	1.2360
	(1.1832)	(1.2258)	(1.2609)	(1.2237)
divorced	0.0279	0.0390	0.0486	0.0378
	(0.1647)	(0.1938)	(0.2151)	(0.1911)
widowed	0.1418	0.1813	0.2011	0.1459
	(0.3489)	(0.3856)	(0.4009)	(0.3535)
single	0.0525	0.0585	0.0553	0.0405
	(0.2231)	(0.2349)	(0.2232)	(0.1974)
married	0.7778	0.7212	0.6977	0.7757
	(0.4158)	(0.4488)	(0.4594)	(0.4177)
noedu	0.2483	0.2105	0.1987	0.2459
	(0.4321)	(0.4081)	(0.3999)	(0.4312)
Info_edu	0.0793	0.0643	0.0499	0.0973
	(0.2703)	(0.2456)	(0.2179)	(0.2968)
primary	0.2093	0.3216	0.2720	0.3108
	(0.4069)	(0.4676)	(0.4451)	(0.4634)
secondary	0.2475	0.2632	0.3009	0.2541
	(0.4317)	(0.4408)	(0.4588)	(0.4359)
tertiary	0.2156	0.1404	0.1773	0.0919
	(0.4113)	(0.3477)	(0.3821)	(0.2893)
Farm_emp	0.4565	0.4854	0.3922	0.5378
	(0.4982)	(0.5003)	(0.4885)	(0.4993)
Pub_emp	0.0360	0.0097	0.0269	0.0108
	(0.1863)	(0.0983)	(0.1620)	(0.1035)
Priv_emp	0.0393	0.0214	0.0486	0.0108
	(0.1943)	(0.1450)	(0.2151)	(0.1036)
Biz_emp	0.3882	0.3977	0.3909	0.3784
	(0.4874)	(0.4899)	(0.4881)	(0.4856)
unemployed	0.0801	0.0858	0.1413	0.0676
	(0.2714)	(0.4902)	(0.3484)	(0.2513)
Sample size	**2,723**	**513**	**1,522**	**370**

Standard deviations in parenthesis.

Source: Author’s calculations using the General Household Survey-GHS data (2018/2019).

Chapter Five - Conclusions and Agenda for Future Research

This thesis explored the concept of financial constraints and financial resilience using firm-level as well as household-level data. The first two essays explored in turn the impact of the CBN intervention fund on MSME's access to bank loans, and the gender dimension to SME credit market participation and loan success in Nigeria using the World Bank Enterprise Survey data. The third essay investigated the impact of unanticipated shocks on household welfare using the General Household Survey data for Nigeria for a recent year.

The first study explored a unique credit intervention scheme, the Micro, Small and Medium Enterprises Development Fund (MSMEDF), to investigate whether the programme had any significant impact on firm incidence of loan take-up. In order to measure the impact of the programme (MSMEDF), we exploited the variability in compliance in the programme, given that not all states satisfied the CBN eligibility requirement and were thus unable to participate in the programme. The study employed a quasi-experimental approach using observational data with a treatment group that participated in the MSME programme and a control group that did not. The World Bank firm-level data and the evaluation methods used provided a unique opportunity to address the problem of selectivity bias from observed and unobserved firm characteristics, and effective identification of an appropriate control group and treatment group void of non-overlapping states and any contamination due to participation in similar credit schemes.

Using a propensity score matching technique combined with a difference-in-difference approach, the study found evidence that the programme exerted a positive effect on firm access to bank credit. The programme is estimated to have increased firm incidence of loan take-up by approximately 10 to 14 percentage points.

However, given the limitation of our dataset, it is important to reiterate some potential limitations of this essay. Our preferred sample, comprising non-overlapped states and no contamination due to participation in similar credit schemes, presented us with the opportunity to eliminate state specific unobservable confounders. However, because the firms are different, the difference-in-difference technique could not eliminate the effect of unobservable confounders at the level of the firm: unobservable firm-level confounders could still be relevant and lurking beneath the surface. This weakens the case that the estimates are causal in nature. In addition, the only dataset available at the time of this research was the 2014 WBES that contained a time frame that is likely to be too short to examine the long-term effects of the programme; this may be more important for the evaluation of the programme. These limitations are emphasised here to outline a potential research direction for future work.

It is worth emphasising that the first essay provided insights into the areas where evaluation of intervention programmes could be improved in Nigeria. Currently, there is no centralised repository of beneficiary data for most of the development fund interventions in Nigeria, despite the government's commitments to establishing such programmes. Without a dedicated database, it is difficult to evaluate the impact of these programmes in a systematic way. For instance, it would be useful to identify the same firms that participated over time in order to eliminate firm-level confounders that could affect estimates of the programme impacts of interest. In addition, it might be useful if the Central Bank of Nigeria, in collaboration with the National Bureau of Statistics, developed such a systematic dataset covering all the different SME programmes in Nigeria.

Since the effect of development fund intervention programmes may take longer to impact on firm performance measures, it is worth exploring, as part of an agenda for future research, the longer-term effects of the programme on other firm performance measures (e.g., sales, employment and firm productivity) using more recent World Bank firm-level data. In addition, more research is needed to unpack the various effects of the programme across sectors and investigate the impact of new entrants in the market as a result of this type of programme.

The second essay provided an empirical study of the gender dimension of SME credit market participation and loan success in Nigeria, using data drawn from the World Bank Enterprise data. The analysis used responses to questions from the access to credit module of this survey. In particular, respondents were asked whether they applied for credit, and if so, was the application successful. The sequence of questions, and the fact that only a sub-sample of respondents provided information on whether their application was successful, warrant caution in the econometric treatment of these responses. An exclusive focus on those firms whose application was successful neglects the fact that they potentially constitute a self-selected sub-sample, and this posed a selectivity bias problem requiring a more sophisticated econometric treatment.

In order to deal with the econometric issues generated by this response sequence, we used a bivariate probit model with partial observability that controls for selectivity bias. The study further used an OB decomposition technique as a means of analysing the differences in outcomes between groups. This made it possible to discern important differences in lending outcomes between ownership types that are exclusively male and those that are exclusively female. Using our preferred estimates based on the OB decomposition, the findings reveal evidence of discrimination in loan success in 100% female-owned firms. Female-owned firms are not constrained in applying for loans, but they are less likely to be successful in their loan application compared to their male counterparts. Building on the empirical work in the first essay, the study revealed that the MSMEDF did not achieve its objective of increasing access to credit for female-owned firms.

It is important to highlight the limitations of this empirical work that could be addressed in future research. The empirical analysis indicates that the gap is more of a supply side than a demand side problem. However, the dependent variables are only representative of the view of firms, not of banks, as the dataset does not contain information on the views of the lenders or loan officers. Future work can improve on this through the use of a detailed dataset that captures both the views of the borrowers and the lenders in regard to loan application outcomes. Furthermore, since loan officers' personal prejudices can affect loan approval decisions, financial providers need to be more mindful and sensitive to gender issues and their potential vulnerability to be discriminatory. The need for training for those engaged in lending decisions to eliminate a potential for gender and other biases merits consideration here. In the absence of such initiatives, women could consider having men on the board of their company to mitigate the negative consequences of discrimination. On the other hand, since financial provider preferences and cultural beliefs about gender may hinder access to credit for female entrepreneurs (e.g., see Muravyev *et al.*, 2009), applications should be blind, and loan officers should not know the status or ownership of the borrowing firm. For example, the study and findings of Goldin and Rouse (2000), which ensured those judging performance were blinded to the gender status of applicants, has some relevance here, although policy implementation may be a difficult issue. However, an alternative approach to this problem might be to increase the number of female loan officers.

The third and final paper examined the impact of unanticipated shocks on household welfare, inequality, household savings, and the food poverty rate in Nigeria. The study provided an empirical insight into the impact of the major food price hike within the period of this study. It focused on examining the impact of a financial shock, as mediated through an increase in the prices of food consumed, in combination with an array of other shocks on welfare indicators. Using the General Household Survey data and unconditional quantile regressions based on RIF methods to examine the distributional effect of shocks, this essay revealed that financial shocks, among other shocks, exerted the most influence on household welfare measures. Our findings reveal that a financial (food price) shock in Nigeria accounted for reduced household food and non-food expenditure, asset, savings, and increases food poverty rate. In particular, we show that the use of assets and savings as shock absorbers provide financial resilience to most affected households to enable this outcome. Nevertheless, this essay is not without its limitations.

By acknowledging that the impact of prices on welfare is sensitive to whether the household is a food producer or a food consumer, and that change in the price of food items can lead to welfare gains or losses for food and non-food producing households (Vu and Glewwe, 2011), we attempted to separate this effect by estimating the impact of food prices on farm and non-farm households. Although our results suggested that the impact of the financial shock, which is consumer prices, is the same for both farm and non-farm households, our findings could not unpack if this was the case for producer prices. An important future direction for research, which is not attempted in this essay due to a lack of data, would be to estimate the impact of financial shocks on farming households using producer rather than consumer prices and thus separating these two channels. This is clearly an issue that requires further investigation, but currently lies beyond the scope of this thesis.

Finally, there are a few observations that can be made about this empirical work from the perspective of a Central Banker. The first and third essays are most relevant to the policies of the Central Bank of Nigeria. The first essay reveals the importance of programme implementation in the evaluation process. The findings from this natural experiment reveal that those who did not participate in the programme were those who did not meet the eligibility criteria, which provides a very crude kind of randomisation. One would have preferred a systematic implementation of a pilot programme in the first instance, where participation was completely randomised and the process was more akin to that of a randomised experiment.

In the third paper, the findings reveal the importance of the use of household assets and savings as shock absorbers in cushioning the adverse effects of a financial shock. The results suggest that these mechanisms are successful in mitigating the effect of shocks in the short term, but an important question to pose is what if these shocks persist? Households cannot replenish their assets and savings, and that makes them more vulnerable to future shocks; this will have implications for the welfare of households. A policy implication that logically follows from this view is that the CBN should take food price inflation management seriously in order to avoid the persistence of such price shocks. If food price increases persist over time, and the CBN does not respond to these shocks in a sensible way due to poor economic management, this then exacerbates the immediate impact of the shock, and household resilience diminishes. Therefore, it is unclear what type of resilient procedure households use in the long run if these shocks are allowed to persist over time. This highlights the importance of the Central Bank using appropriate levers and implementing appropriate macro-economic policies to ensure such shocks are mitigated. This is easier said than done, but resilience for those households at the bottom end of the distribution, and those most vulnerable to poverty, is weak. Therefore, ensuring that the Central Bank gets macro-level polices right assumes a particular significance for those that are the least resilient and the most vulnerable.

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