African Review of Economics and Finance Vol 13(1) 2021

African Review of Economics and Finance | ISSN 2042-1478 | Vol 13 | Issue 1 | June 2021

The impact of formal financial services uptake on asset holdings in Kenya: Causal evidence from a propensity score-matching approach

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Abstract

Literature on the impact of financial services on economic wellbeing has largely relied on findings from randomised control trials. Given the scarcity of such trials, this has led to gaps in the sector's understanding of financial inclusion as a development tool, hence a lack of consensus on whether financial inclusion as a strategy indeed leads to improved individual outcomes. To close this gap, this study employs the propensity score-matching technique on the 2016 FinAccess Kenya Household Survey dataset to estimate the average treatment effect of taking up financial services. Our findings suggest that individual take-up of savings, credit and insurance have positive effects on household economic welfare.

Keywords: Financial inclusion; development; asset ownership; propensity score matching; multiple correspondence analysis.

Article history:
Received: 10 March 2019
Accepted: 11 December 2019

1. Introduction

Financial inclusion has emerged as a global policy priority in addressing development outcomes, such as poverty, inequality and unemployment, particularly in developing countries (Alliance for Financial Inclusion, 2017). Klapper et al (2016) argue that financial inclusion is a key enabler of 11 of the 17 United Nations Sustainable Development Goals.[1] The elimination of extreme poverty is among the goals that financial inclusion is believed to support.

To design effective policies that truly improve people's economic wellbeing, it is critical to first determine whether financial inclusion actually has the positive impact that it is believed to have. In recent years, there has been a growing demand for rigorous evidence of the causal relationship between the use of financial services and the expected outcomes (Mckenzie, 2009). However, the evidence has been mixed. This lack of consensus about financial inclusion as an effective development tool poses challenges for policymakers, donors and international development organisations alike, particularly when faced with uncertainty when making crucial decisions about how best to achieve the ultimate goal of improving people's lives.

One of the possible reasons that the evidence is scant and seen as mixed is the reliance in the field of financial inclusion on randomised control trials (RCTs) as the primary means for establishing credible evidence of impact.[2] While RCTs are for good reason considered the gold standard of impact evaluation, they also have limitations in both implementation and findings, leaving certain gaps in the literature. Most notably, RCTs are project and context specific and therefore do not translate easily into policy recommendations (Duflo *et al*., 2008; Khandker *et al*., 2010).

There has been limited application of alternative statistical methods that can help to fill the gap that RCTs leave with regard to the rigorous evaluation of the impact in the financial inclusion space.[3] It is against this backdrop that we seek to contribute to the evidence base by exploring the impact of financial services on people's wellbeing. This paper emphasises the potential of using quasi-

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[1] The Sustainable Development Goals are the set of goals adopted by the United Nations General Assembly in 2015 in support of the 2030 Agenda for Sustainable Development. For the complete list of 17 goals, refer to http://www.un.org/sustainabledevelopment/sustainable-development-goals/.

[2] See Duflo *et al.* (2008) for an exposition of randomised control trials, with an emphasis on its rationale and applicability.

[3] For an overview of both experimental and non-experimental techniques as well as their application in development economics, see Gertler *et al*. (2011) and Khandker *et al.* (2010).

experimental methods as credible impact evaluation tools to complement the results of experimental studies. Further, we take advantage of the increasing availability of datasets that enable the investigation of the impact of financial services at a national level.[4] To this end, we apply a propensity score matching (PSM) approach on the 2016 FinAccess Kenya household survey dataset to investigate the impact of taking up formal financial services on household asset ownership.

2. Impact of financial services on economic welfare

2.1. Defining financial inclusion and welfare

According to the World Bank[5], financial inclusion means that:

> "individuals and businesses have access to useful and affordable financial products and services that meet their needs – transactions, payments, savings, credit and insurance – delivered in a responsible and sustainable way."

The idea is that financial services facilitate day-to-day living and help individuals and businesses to set long-term goals and plan for unexpected emergencies. Financial services allow people to expand their businesses, invest in education or health, manage risks and weather financial shocks, which can help to improve the overall quality of their lives.

In terms of the anticipated economic welfare outcomes of financial inclusion, savings allow individuals to smooth consumption over time and to finance productive investments in human and business capital (Karlan *et al.*, 2014), which should lead to greater accumulation of wealth and increases in income. Credit allows people to invest, acquire productive assets and build their businesses (Van Rooyen *et al.*, 2012). Insurance allows households to protect their assets from loss and in general overcome unexpected economic shocks of all types without having to sell off assets as a coping strategy (Akotey and Adjasi, 2014; De Bock and Ontiveros, 2013).

For the purposes of this study, we focus on take-up of formal financial services, specifically savings, credit and insurance, as the indicator of financial inclusion. Formal financial services are those that are provided by entities that are registered and regulated by appropriate authorities in a country. We acknowledge that individuals have to meaningfully and responsibly use the financial services they have taken up to derive the benefits thereof, else they render no impact.

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[4] Examples of demand-side surveys include, but are not limited to, Global Findex, FinScope, FinAccess, Financial Inclusion Tracker and Financial diaries.

[5] http://www.worldbank.org/en/topic/financialinclusion/overview

In this study, we attempt to measure the impact of financial inclusion on economic welfare. We use an asset ownership indicator as a measure of economic welfare. According to Akotey and Adjasi (2014):

> "… using household assets instead of income or expenditure to measure welfare levels is more accurate and reliable. The measurement of income of households in the informal sector is hindered by seasonality, recall bias and households' reluctance to divulge sensitive information concerning their income and expenditure levels."

Therefore, for this study, asset ownership is a proxy indicator for economic welfare more broadly.

2.2. Literature review of causal relationship between financial inclusion and economic welfare

Rigorously testing the impact – or lack of impact – of financial services on individual welfare largely relies on RCT evaluations of specific financial inclusion interventions. While there is some evidence of positive impact, the ever-growing body of literature on the impact of financial inclusion clearly points to mixed results, as studies often point to no significant impact, and in some cases negative impact (Banerjee *et al.*, 2015b; Karlan *et al.*, 2017; Van Rooyen *et al.*, 2012).

Some studies have shown a positive causal relationship between savings and economic welfare, particularly asset ownership. Brune *et al*. (2016) found that provision of commitment savings for Malawian farmers led to increased agricultural inputs, crop sale proceeds and household expenditures. In Nepal, Prina (2015) found that women's access to savings accounts increased monetary and non-monetary assets.

Based on a review of randomised evaluations across six countries conducted between 2003 and 2012, Banerjee *et al.* (2015b) found evidence that microcredit has consistent modest effects on development outcomes of the average borrower. Cintina and Love (2017) investigated the impact of microfinance borrowers on various financial and non-financial indicators in Hyderabad and found significant positive effects on durable-goods purchases, health expenditure and "temptation goods"[6]. Attanasio *et al*. (2015), Augsburg *et al*. (2015), and Crépon *et al.* (2015) also consistently found increases in self-employment options, increased assets and inventory for self-employment activities, but there were no significant effects on household income.

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[6] Described as "eating out, alcohol/tobacco and gambling."

On the other hand, in an impact evaluation of village savings and loan associations in Ghana, Malawi and Uganda, Karlan *et al.* (2017) found no significant impact on typical welfare indicators such as income, consumption, food security or asset ownership. Dupas and Robinson (2013) found that the randomised provision of formal savings accounts to non-farm microenterprises in rural Kenya increased both productive investment and private expenditure for female market vendors. Van Rooyen *et al*. (2012) conducted a systematic review of impact evaluation studies of micro-credit and micro-savings in sub-Saharan Africa. They found that micro-credit has both positive and negative effects on the income of poor people, and micro-savings has some positive effects, but largely no effect on economic welfare outcomes. They also found that micro-credit may have increasingly negative effects over time, with recurring clients' businesses becoming less successful. Nanor (2008) explores the impact of micro-credit directly on household income. The results were mixed, with clients' household income significantly higher than that of non-clients within two of the four districts examined, but significantly lower in the other two. Using an RCT to evaluate a microcredit programme in India, (Banerjee *et al*., 2015a) found no evidence of increased income from credit and mixed results in terms of stock of durable goods.

There have been fewer impact studies on insurance. Additionally, insurance refers to a broad host of products, which makes it even more challenging to draw conclusions about insurance in general based on these studies that focus on specific contexts and insurance products. Nonetheless, the results are generally positive. Using various quasi-experimental methods, Akotey and Adjasi (2014) investigated the welfare-enhancing benefits of taking up microinsurance, and they found positive and significant effects on Ghanaian households' asset holdings. A number of studies have found evidence that when faced with an adverse event, those that were insured against the event were less likely to use their savings and/or sell their assets as a coping strategy (Aggarwal, 2010; Janzen and Carter, 2013; Levine *et al*., 2016).

2.3. Methodological issues on financial inclusion impact measurement

To substantiate the impact of take-up of financial services, one has to prove, by means of rigorous evaluation, that observed outcomes are due solely to the take-up of these services and would not have occurred in their absence. Therefore, a key challenge in using observational data to estimate differences in outcomes between treatment and control groups that were not randomly assigned is that those that voluntarily opted into the treatment (i.e. opened a bank account or took out a loan) are arguably systematically different from those that did not. Resultantly, the two groups are likely to have different outcomes, regardless of the treatment. The strength of RCTs is that they attempt to eliminate the self-selection bias and create a credible counterfactual by assigning the treatment randomly.

While RCTs are a powerful tool in the impact evaluation toolkit, there are certain limitations that underline the importance of complementing the findings from RCTs with other methodological approaches to build a credible body of evidence for or against the efficacy of financial inclusion, or any other development intervention. We highlight four potential limitations below that particularly affect the financial inclusion body of evidence.

Firstly, in practice, RCTs can be challenging to implement as they are deliberately designed to deny treatment to some people. Researchers need to continually assess the feasibility to mitigate this challenge, for example by using measures such as phased-in rollouts to ensure that everyone receives the treatment eventually, with consideration to the RCT budget constraint. Nevertheless, in some contexts RCTs may simply not be feasible. For example, a common issue is ethical concerns about assigning people into treatment and control groups (Khandker *et al*., 2010). RCTs also often suffer from practical difficulties in implementing the randomisation in the field, which results in findings that are compromised by project parameters (Barrett *et al*., 2010; Khandker, 2010).

Secondly, by design, RCTs measure the effect of the intention to treat rather than the effect of the treatment itself. Researchers can offer participants a financial service as a treatment, but in reality not everyone in a random treatment group will take up the service. In fact, take-up is usually modest (Banerjee et al., 2015b). Therefore, the precision of results of the RCTs is compromised when trying to draw a conclusion about the effect of the actual take-up of financial services (Banerjee *et al.*, 2015b; Cintina and Love, 2017; Khandker *et al.*, 2010).

Thirdly, the RCT approach lends itself to testing treatments to which the target population is not yet exposed. The treatment and control groups therefore need to be drawn from a population of "marginal" clients, i.e. first-time borrowers and unbanked populations. The body of RCT literature does not address the inframarginal clients (Banerjee *et al*., 2015b), which may have a fundamentally different response to the treatment from the marginal clients.

Finally, RCTs are well placed to answer questions about the impact of a specific intervention in a specific context, which is valuable for the implementers and funders of the intervention. However, this leaves a gap for policymakers, international donors and other institutions that need to make decisions across entire economies and who set strategic priorities and policies at national and global levels. The usual approach to increasing the external validity of the findings from an RCT study is to perform a systematic review of multiple RCTs, for example the one conducted by Van Rooyen *et al*. (2012); but still, the results are not easily generalisable and therefore do not readily translate into policy (Khandker *et al*., 2010).

Using nationally representative observational data would help to overcome some of these challenges. However, it is essential to address any potential self-selection bias when estimating impact. Seminal works of Rosenbaum and Rubin (1985, 1983) proposed that observable characteristics can be used to reduce or eliminate the selection bias in estimated treatment effects. One such approach is PSM. PSM is an impact evaluation technique that matches each participant with a statistically identical nonparticipant and then measures the average difference in the outcome variable between the participants and the nonparticipants (Khandker *et al*., 2010). We can create statistically identical treatment and control groups by estimating propensity scores – representing the likelihood of treatment based on observed covariates – and applying a matching algorithm (Garrido *et al*., 2014; Guo and Fraser, 2015; Li, 2013). Using this technique selection bias can be reduced or eliminated (Austin, 2011; Rosenbaum and Rubin, 1985).

While PSM is widely used in other fields such as public health, where random assignment is not always possible, there are few examples of its application in the financial inclusion field. The applications that we are aware of are Cintina and Love (2017), and Swain and Floro (2012). Using a dataset that was collected as part of an RCT study, Cintina and Love use PSM to measure the impact of microcredit in India. Swain and Floro use PSM to measure the impact of participation in bank-connected self-help groups also in India. The increasing availability of large nationally representative household survey datasets that are focused on financial inclusion presents new possibilities to apply quasi-experimental techniques such as PSM on these cross-sectional data sets[7] and contribute to the debate on the impact of financial inclusion.

[7] Instrumental variables approach is another example of a quasi-experimental technique that utilises cross-sectional data. Akotey and Adjasi (2014) use this approach to estimate the impact of microinsurance on asset accumulation in Ghana.

3. Data and methods

3.1. Data

This study was undertaken using the 2016 FinAccess Kenya Household Survey dataset, which employed a stratified multi-stage cluster sampling design to ensure representativeness at the national, provincial and urban/rural levels (Central Bank of Kenya *et al*., 2016).[8] The sample consists of 8,665 households, within which individuals were surveyed between August and October 2015. Detailed information was collected on households' and individuals' demographic profiles and income patterns, household characteristics and asset holdings, risk exposures and coping mechanisms, financial behaviours, attitudes and perceptions.

3.2. Methods

We use the Principal Score Matching (PSM) to estimate the effects of take-up of financial services on households' asset ownership, as a proxy indicator of the household's economic wellbeing. Take-up of savings[9], credit[10] and insurance[11] in Kenya are the respective treatments under investigation.

Based on observed covariates unaffected by the treatment, the PSM constructs a statistical counterfactual (comparison) group which is derived from a model of the probability of participating in the treatment. Participants are matched with observationally similar non-participants based on the participation probability (propensity score). The average treatment effect is then derived from computing the mean difference in the outcomes between the two groups (Khandker *et al*., 2010).

To implement this, we first estimate the propensity to take up these financial services, using a probit regression model for each respective treatment on potential explanatory variables as follows:

$$Pr(Treatment_i = 1 \mid X_i) = \Phi(\beta X_i + v_i) \tag{1}$$

where $Treatment_i$ represents take-up of a financial service by household i, and takes on the value 1 if the household has taken up the product, and 0 otherwise.

[8] The goal of this study is to estimate the effect estimates for the sample itself; an investigation of population-level inferences is beyond our scope.

[9] Savings account at SACCO, MFI, Mshwari or KCB M-Pesa, Bank, or Postbank.

[10] Credit includes personal, mobile banking, and app-based loans; loans from MFIs, SACCOs, or the government; bank overdraft or credit cards; mortgage loans from banks, building societies, or the government.

[11] Insurance includes car insurance, house or building insurance, crop insurance, livestock insurance, private medical insurance, life insurance, education policy, NSSF, or retirement annuity.

The vector X_i represents explanatory variables, such as respondent demographics and household characteristics, which affect the probability of take-up. $\Phi(.)$ represents the normal cumulative distribution, and vector β represents the estimated coefficients.

Next, we create our outcome indicator. Drawing on Booysen *et al*. (2008) and Filmer and Scott (2012), our outcome of interest is an asset-based wealth index which is theorised to represent long-term household socio-economic status. The idea of using the asset index as a measure of socio-economic position or status stems from the fact that monetary measures such as income or consumption expenditure are generally difficult to measure and also suffer for recall bias and reluctance to divulge information (Howe *et al.,* 2008). Many researchers argue that using the asset index as a welfare measure is more accurate than using income or consumption expenditure (Akotey and Adjasi, 2014; Booysen *et al.*,2008). The asset index approach to measuring social economic status has been commonly applied on demographic health studies (DHS) and the variables commonly used include ownership of a range of durable assets (e.g. car, refrigerator, television), housing characteristics (e.g. material of dwelling floor and roof, toilet facilities), and access to basic services (e.g. electricity supply, source of drinking water) (Howe *et al*., 2008). Among these variables, those which do not carry any discriminant information are usually dropped (Chamboko *et al.*, 2017).

We use multiple correspondence analysis (MCA) to estimate the asset index based on binary indicators of household assets, and categorical indicators that describe household characteristics and access to public service infrastructure (Asselin, 2002; Howe *et al*., 2008; Johnston and Abreu, 2013). The choice to use the MCA over the competing commonly used method – the principal component analysis was because the MCA is more suited to discrete or categorical variables and makes few assumptions about the distribution of the indicator variables. This differs from the PCA which was essentially developed for continuous variables (Booysen *et al.*, 2008).

After computing the asset index, we then match households based on their propensity to take up financial services. To achieve this, we use the most commonly using matching method – nearest-neighbour matching. Using this approach, every treatment unit is matched with a counterfactual non-treatment unit with the closest propensity score. Since the difference in the propensity scores for matched individuals can actually be large, this may result in poor matches. As part of robustness checks as well as to remedy the possibility of large differences between matches, we imposed a threshold on the maximum allowed distance between propensity scores (caliper). We used as maximum distance of 0.02 to ensure reasonable proximity of the matched based on their propensity scores.

Finally, using households matched based on their propensity to take up a financial product, we estimate the sample average treatment effect on the treated group (ATT):

$$ATT = E(Assets(_{1,i}) \mid Treatment_i = 1) - E(\text{Assets}_{0,i} \mid Treatment_i = 0) \qquad (2)$$

where $E(\cdot)$ represents the expected outcome, while $Assets_{1,i}$ and $Assets_{0,i}$ are the outcomes observed for households that actually take up financial services and their matched counterfactuals, respectively.

4. Results

4.1. Estimation of the propensity to take up savings, credit and insurance

The first step is to determine a propensity score model by using a set of explanatory variables for each treatment model that minimise the standardised percentage bias between treated and untreated groups (Rosenbaum and Rubin, 1985). Rosenbaum and Rubin define the percentage bias as "the mean difference as a percentage of the average standard deviation". This is to say that there may be sets of covariates that allow for a stronger predictive model, but would not be used in the propensity model because the covariates did not balance well across the treatment and control groups. We tested covariates that, based on the literature, are known predictors of an individual that uses formal financial services. For each of the three treatment models (savings, credit and insurance), we selected the set of covariates that both predicted take-up of the specific financial service and also minimised the percentage bias between the treatment and control groups.

In Table 1, we report estimates for the treatment models described in Equation 1. Notably, the education level and monthly income of the respondent have significant positive effects on the probability of take-up across the three product types. Gender is significant but works in opposite directions depending on the financial service. Women are more likely to take up formal credit than their male counterparts, while men are more likely to take up saving accounts and insurance products.

Based on the selected covariates, each respondent is assigned a score that indicates his or her individual propensity to take up savings, credit and insurance, respectively.

Table 1: Propensity to take up Savings, Credit and Insurance

	(1) Savings	(2) Credit	(3) Insurance
ln(income)	0.0856*** (0.0036)	0.0504*** (0.0031)	0.0607*** (0.0030)
Female	-0.0337*** (0.0089)	0.0437*** (0.0118)	-0.0446*** (0.0066)
Married	0.0265** (0.0093)	0.0837*** (0.0109)	
Primary completed	0.1399*** (0.0102)	0.0615*** (0.0092)	0.0286** (0.0093)
Secondary completed	0.2891*** (0.0101)	0.1472*** (0.0090)	0.1219*** (0.0089)
Hhsize	-0.0132*** (0.0018)		
Married*female		-0.0881*** (0.0143)	
Cellphone		0.1151*** (0.0130)	0.0265* (0.0138)
Mobile money			0.0770*** (0.0122)
Less than 30 min to bank			0.0391*** (0.0074)
N	8518	8518	8335

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

Notes: The covariates for each respective treatment were selected to minimise the standardised percentage bias between treated and untreated groups; hence only relevant estimates for each model are displayed here.

4.2. Asset-based wealth index using multiple correspondence analysis

The next step was to create an asset-based wealth index by using MCA on a set of 14 assets that the survey respondents were asked whether or not their household has. It is standard practice that the components that explain the greater part of the variation should be considered (OECD, 2008; Chamboko *et al*., 2017). In this case, the first component of the MCA explains 89.46% of the variance in the selected variables and is used to compute the asset index. The weights generated from the MCA are presented in Table 2. Consistently and as theoretically expected, Table 2 shows that access to all the stated assets was associated with higher factor scores, indicating a higher asset holding.

Table 2: Wealth Index Frequencies and Factor Scores from Multiple Correspondence Analysis

Asset	Response	N	%	Dimension
Radio	Yes	4890	56.43	0.053
	No	3775	43.57	-0.069
TV	Yes	2568	29.64	1.88
	No	6097	70.36	-0.792
VCR	Yes	1877	21.66	2.473
	No	6788	78.34	-0.684
Music system	Yes	833	9.61	3.532
	No	7832	90.39	-0.376
Computer	Yes	481	5.55	4.621
	No	8184	94.45	-0.272
Refrigerator	Yes	478	5.52	5.182
	No	8187	94.48	-0.303
Microwave	Yes	216	2.49	6.591
	No	8449	97.51	-0.169
Stove	Yes	173	2	5.93
	No	8492	98	-0.121
Bicycle	Yes	1508	17.4	0.572
	No	7157	82.6	-0.12
Motorcycle	Yes	703	8.11	0.975
	No	7962	91.89	-0.086
Car	Yes	366	4.22	4.509
	No	8299	95.78	-0.199
Cooking fuel	Electricity	22	0.25	1.976
	Gas/LPG	797	9.2	3.76
	Paraffin	735	8.48	0.376
	Mud/Cement	7073	81.63	-0.474
	Other	38	0.44	0.999
Water type	Piped	2137	24.66	1.599
	Other	6528	75.34	-0.523
Toilet type	Flush toilet	822	9.49	3.618
	Pit latrine	6817	78.67	-0.27
	Other	302	3.49	-1.044
	None	724	8.36	-1.132

The descriptive statistics of the wealth index are reported in Table 3 below. Across the three products, take-up of financial services is consistent with higher-average asset holdings.[12]

[12] Negative values are a result of only taking the principal component/factor and discarding information contained in other components.

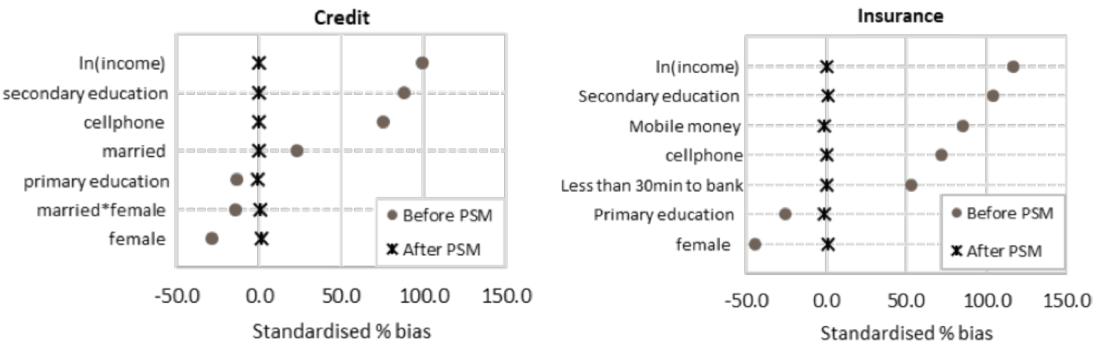
TABLE 3: DESCRIPTIVE STATISTICS OF WEALTH INDEX ACROSS FINANCIAL SERVICE TAKE-UP

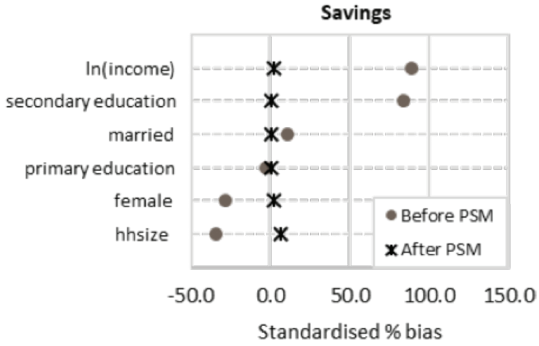
	Total	Savings		Credit		Insurance	
		Yes	No	Yes	No	Yes	No
Mean	0.00	0.55	-0.23	0.74	-0.12	0.93	-0.14
Median	-0.47	0.15	-0.56	0.28	-0.48	0.49	-0.48
Skewness	2.63	1.66	3.50	1.42	2.96	1.21	3.04
Kurtosis	11.05	5.67	19.02	4.56	13.95	3.92	14.87
Std. deviation	1.00	1.29	0.74	1.43	0.86	1.49	0.82
Min	-0.68	-0.68	-0.68	-0.68	-0.68	-0.68	-0.68
Max	5.81	5.81	5.66	5.79	5.81	5.67	5.81
N	8665	2550	6115	1169	7496	1141	7524

4.3. Matching by propensity score

Finally, we used the nearest NN, without replacement, to match individuals that had taken up financial services with individuals that had not. Figure 1 below shows the percentage bias between the treated and untreated before and after matching. The summary statistics that describe the balance of covariates across each treatment and control group, before and after matching, are depicted in Appendix 1.

FIGURE 1: BALANCE IN COVARIATES BEFORE AND AFTER MATCHING BY PROPENSITY SCORES (1:1 NEAREST NEIGHBOUR)





4.4. Robustness check

As discussed above, the success of the PSM as an approach is hinged on the ability to construct a statistical comparison group. As robustness checks and evaluating how good our matching based on the NN we also applied the same NN matching after imposing a threshold on the maximum allowed distance between propensity scores (caliper of 0.02) as seen in Austin (2011). As shown in Table 4, Table 5, and Table 6 the mean and median bias before PSM and after using the two different matching criteria results are similar for with and without caliper and consistently demonstrate a reduction in bias when using the PSM approach.

Table 4: Outcomes of the Matching

	Mean standardised bias (%)			**Median standardised bias (%)**		
Outcome	**Savings**	**Credit**	**Insurance**	**Savings**	**Credit**	**Insurance**
Before PSM	41.62	48.93	71.76	31.61	28.85	71.86
1:1 nearest neighbour	1.88	0.35	0.82	1.21	0.25	0.73
1:1 nearest neighbour (caliper = 0.02) (Robustness check)	1.75	0.31	0.80	0.97	0.19	0.59

Table 5: Balance in Covariates, before Matching

Covariates for savings propensity score model	Before PSM		
	Savings	**No savings**	**Standardised bias (%)**
N	2550	6115	
ln(income)	9.46	8.37***	89.0
secondary education	50.86	14.67***	83.6
Married	64.12	58.86***	10.8
primary education	32.12	33.59***	-3.1
Female	51.10	65.05***	-28.6
Hhsize	3.80	4.64***	-34.7
Covariates for credit propensity score model	**Before PSM**		
	Credit	**No credit**	**Standardised bias (%)**
N	1169	7496	
ln(income)	9.74	8.53***	98.8
secondary education	59.37	20.01***	87.8
cellphone	96.66	70.41***	75.7
married	69.97	58.91***	23.3
primary education	27.80	33.99***	-13.4
married*female	31.14	38.09***	-14.6
Female	48.67	62.86***	-28.9
Covariates for insurance propensity score model	**Before PSM**		
	Insurance	**No insurance**	**Standardised bias (%)**
N	1141	7524	
ln(income)	9.91	8.51***	117.1
Secondary education	64.94	19.31***	104.2
Mobile money	94.30	61.66***	85.7
cellphone	95.88	70.63***	71.9
Less than 30min to bank	78.73	54.66***	52.8
Primary education	23.14	34.68***	-25.7
Female	41.89	63.84***	-45.0

Notes: The covariates for each respective treatment were selected to minimise the standardised percentage bias between treated and untreated groups; *, ** and *** indicate that the mean values of subgroups are significantly different at the 10%, 5% and 1% significance levels, based on a t-test (continuous variables) or a Chi2 test (binary variables).

Table 6: Balance in Covariates, after Matching (1:1 nearest neighbour)

Covariates for savings propensity score model	**After PSM**					
	1:1 nearest neighbour			**1:1 nearest neighbour (caliper = 0.02)**		
	Savings	No savings	Standardised bias (%)	Savings	No savings	Standardised bias (%)
N	2520	2923		2512	2923	
ln(income)	9.46	9.44	1.9	9.45	9.43	1.8
secondary education	50.71	50.63	0.2	50.56	50.48	0.2
Married	64.29	64.05	0.5	64.17	64.09	0.2
primary education	32.26	32.18	0.2	32.36	32.29	0.2
Female	50.99	49.92	2.2	51.07	50.08	2.0
Hhsize	3.80	3.64*	6.3	3.80	3.65*	6.2
Covariates for credit propensity score model	**After PSM**					
	1:1 nearest neighbour			**1:1 nearest neighbour (caliper = 0.02)**		
	Credit	No credit	Standardised bias (%)	Credit	No credit	Standardised bias (%)
N	1155	3847		1153	3846	
ln(income)	9.74	9.74	0.1	9.73	9.73	0.1
secondary education	59.22	59.13	0.2	59.15	59.06	0.2
cellphone	96.71	96.80	-0.2	96.70	96.79	-0.3
married	70.13	70.13	0.0	70.08	70.08	0.0
primary education	27.79	28.05	-0.6	27.84	28.10	-0.6
married*female	31.26	31.08	0.4	31.22	31.14	0.2
Female	48.66	48.14	1.1	48.66	48.22	0.9
Covariates for insurance propensity score model	**After PSM**					
	1:1 nearest neighbour			**1:1 nearest neighbour (caliper = 0.02)**		
	Insurance	No insurance	Standardised bias (%)	Insurance	No insurance	Standardised bias (%)
N	1127	3069		1123	3069	
ln(income)	9.91	9.90	0.2	9.89	9.89	-0.2
Secondary education	65.04	64.42	1.4	64.92	64.29	1.4
Mobile money	94.50	95.03	-1.4	94.48	95.01	-1.4
cellphone	95.83	95.74	0.3	95.81	95.73	0.3
Less than 30min to bank	78.79	78.53	0.6	78.72	78.45	0.6
Primary education	23.16	23.69	-1.2	23.24	23.78	-1.2
Female	41.79	41.44	0.7	41.85	41.59	0.5

Notes: The covariates for each respective treatment were selected to minimise the standardised percentage bias between treated and untreated groups; *, ** and *** indicate that the mean values of subgroups are significantly different at the 10%, 5% and 1% significance levels, based on a t-test (continuous variables) or a Chi2 test (binary variables).

4.5. Causal effects of take-up

Table 7 reports the results of the average take-up effects estimated by propensity score matching. Across all financial products, we find that take-up of financial services has positive and significant effects on asset holdings at the 1% significance level. For 1:1 nearest neighbour matching without a caliper, take-up of insurance suggests 15% higher average household asset holdings relative to households that did not take up insurance, followed by a savings effect of 10.6%. Credit product take-up increases asset holdings by a more modest 8.7%. The results are consistent when matching with and without a caliper.

Table 7: Effect of Financial Service Take-up on Asset Holdings

PSM approach	**ATT**[a]		
	Savings	**Credit**	**Insurance**
1:1 nearest neighbour	0.106*** (5.176)	0.087*** (4.345)	0.150*** (6.580)
1:1 nearest neighbour (caliper = 0.02)	0.112*** (5.527)	0.099*** (5.053)	0.134*** (6.121)

Notes: z-scores in parentheses; * p<0.1, ** p<0.05, *** p<0.01;

[a] The average marginal effects are reported, for easy interpretability; reflecting the change in the probability of uptake given a one-unit change in an explanatory variable.

5. Discussions

The findings agree with many other studies which demonstrated that providing financial services particularly bank accounts in developing countries leads to increased savings, business activity and income (Burgess and Pande, 2005; Bruhn and Love, 2014; Young, 2015). Similarly for insurance, the findings corroborate that of Akotey and Adjasi (2014) who found taking up microinsurance providing positive and significant effects on Ghanaian households' asset holdings. The findings are also consistent with other studies which showed that those that were insured against adverse events were less likely to use their savings and/or sell their assets as a coping strategy (Aggarwal, 2010; Janzen and Carter, 2013; Levine *et al.*, 2016). Even though there is mixed literature on the impact of credit, our findings add to the body of literature suggesting positive impact of credit uptake as also documented by Banerjee *et al*. (2015b) who found microcredit to provide consistent modest effects on development outcomes of the average borrower.

5. Conclusion

The purpose of this study is to evaluate the impact of financial inclusion on wellbeing in Kenya. Specifically, we measured the impact of taking up formal savings, credit and insurance products on household asset ownership, as an indicator of economic welfare of households. We highlight that literature on the impact of financial services on economic wellbeing has largely relied on findings from RCTs, which has led to important gaps in the sector's understanding of financial inclusion as a development tool. This has resulted in a lack of consensus on whether financial inclusion as a strategy indeed leads to improved individual outcomes. To close this gap, we employ PSM to estimate the average treatment effect of taking up financial services. Our findings suggest that individual take-up of savings, credit and insurance all have positive effects on household economic welfare.

Funding

The work was supported by insight2impact, which is a resource centre that is funded by the Bill & Melinda Gates Foundation [Grant number OPP1114990] in partnership with The MasterCard Foundation.

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