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“Short Term Cost of Cash and Mobile Financial Services: Evidence from a natural experiment in India”

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Short Term Cost of Cash and Mobile Financial Services: Evidence from a natural experiment in India

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Abstract

The use of digital financial services (DFS) in developing countries can be a tool for financial inclusion, curbing tax evasion, and facilitating the efficient delivery of public services. Using a unique event – an un-announced and large scale demonetization process that took place in 2016 in India that increased the short-term costs of holding and transacting in cash, we study the uptake of a specific form of DFS, namely mobile payments, in India. We find that in states where the labour market was less formal, and where workers were more likely to be affected by the demonetisation process, this shock led to a larger increase in the use of platforms larger than in states where the labour market is more formal. The effect of this "forced experimentation" was, however, short lived. At the individual level, people who were more exposed to the shock were more likely to adopt mobile payments and this effect persists over the next two years. Strikingly, the marginal effects of the shock for high-exposure women was almost twice as high as for high-exposure men. Our results contribute to understanding user behaviour and persistence of habits, with important implications for the design of policies aimed at increasing the uptake of digital payment technologies.

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1 Introduction

Digital financial services (DFS), that include mobile payment, banking and finance, are increasingly valuable to further financial inclusion and simplify the delivery of public services in developing countries. A large proportion of the unbanked population in these countries has access to mobile phones (Donovan, 2012), and mobile based solutions are often quicker and cheaper alternatives than expanding traditional banking infrastructure. Additionally, governments in developing countries are interested in the substitution from cash to mobile financial services not just for the motive of financial inclusion but also to address tax evasion (Immordino and Russo, 2018). In this paper, we study a unique natural experiment carried out in India in 2016 that increased the short term costs of holding and transacting in cash. In particular, we examine if this short term inconvenience in using cash led to a sustained uptake of mobile financial services among individuals. We do this in two separate ways. First, we take an aggregate view and analyze the impact of this currency shock on the inclination to use DFS platforms at the state level using monthly data from Google Trends between 2014 and 2020. Second, we use a novel annual household survey to dig deeper and evaluate the impact of this shock on the individual probability of adopting mobile based DFS between 2014 and 2018.

On the evening of November 8 2016, without prior notice, the Prime Minister of India announced that the two highest denomination currency notes (Rs. 1000 and Rs. 500) were to be demonetized with immediate effect.[1] New currency was to be issued in the denominations of Rs 2000 and Rs 500. People had until 31 December 2016 to either exchange their old currency for the new notes, or deposit the old currency in their bank accounts. The total value of currency that could be exchanged or deposited had strict limits. As a result, 86.9% of the value and 75% of the volume of the currency in circulation was wiped out. For a largely informal economy with cash used for over 95% of transactions, this shock, henceforth called demonetization, translated into a decrease in the year on year growth rate in 2016Q4 by 2 percentage points(Chodorow-Reich et al. 2019). There was also a corresponding decline in employment by 2 percentage points (Chodorow-Reich et al. 2019). However, output declined relatively less than the decline in currency circulation which suggests that at least some individuals substituted from cash to electronic forms of payment.

[1] Approximately equal to $16 and $8 respectively.

The focus of this paper is this substitution from cash to electronic payment platforms and DFS resulting from the short term currency contraction caused by demonetization. Specifically, we are interested in the following three questions: 1) Did demonetisation lead to an aggregate change in the inclination to use platforms (measured by the total hits of specific keywords on Google search) across different Indian states ? 2) Did demonetisation have an impact on the individual probability of adopting mobile based DFS and did this impact persist over time ? 3) Did men and women respond differently to the shock ?

Note that we do not evaluate whether or not the demonetization policy achieved its objectives, nor do we compute welfare effects of this shock. Demonetization affected all sectors of the economy and the computation of overall welfare effects is outside the scope of this paper. Instead, the focus of the paper is on understanding the impact of this "forced experimentation", that temporarily increased the cost of cash transactions, on the probability of using mobile transactions.

We answer our research questions using a difference in differences framework. The main empirical challenge in this exercise is that demonetization was a shock that affected all the individuals in the economy at the same time. To address this, following recent literature (Chodorow-Reich et al. 2019), we construct measures of exposure to the shock that differ across states and individuals. For the aggregate analysis focused on the inclination to use digital platforms (measured by the total number of web hits on relevant keywords), we classify states by the degree of formality of their labour force. To measure formality, we use the state-wise proportion of workers receiving paid leaves- the fewer the proportion of workers receiving paid leaves, the more informal the state is said to be. We use a report published in 2013 by the Ministry of Labour and Employment, Government of India to obtain the data on workers.This implies that we assume that states have the same ranking of informality in our period of analysis (2014-18) that they did in 2013. We hypothesize that the more informal a state, the more it is exposed to demonetization, since it is likely that informal states are more cash dependent. We then compare more exposed states to less exposed states before and after the shock.

To understand the impact of the shock at the individual level, and answer questions 2) and 3), we use detailed survey data published by Financial Inclusion Insights (FII). In the same spirit as the previous exercise, we build a measure of individual exposure to demonetisation based on the observed distance of individuals from their nearest bank branch. In the aftermath of the shock, the only way old currency notes could be deposited or exchanged was at bank branches. Hence, distance from the bank branch is likely to be a good measure of exposure to the shock. Our hypothesis in this case is that the further away an individual lives from a bank branch, the more exposed they are to demonetisation and ceterus paribus, the more likely they are to adopt DFS and payment platforms. We thus compare the difference in adoption of mobile based payments and DFS of individuals living further from bank branches to those living closer before and after the shock.

From the aggregate analysis at the state level, we find that in the quarter of demonetization, the more informal the state, the greater the inclination to use digital platforms. However, this effect does not persist over time. For the individual level analysis, we find that conditional on individual demographics, people that were more exposed to the demonetisation shock were more likely to use their mobile phones for financial transactions in the year of the shock. Unlike at the state level, we find that this effect also persisted in 2017 and 2018. Note, however, that the magnitude of these effects are relatively small. On average, for individuals with high-exposure to demonetisation, the probability of using mobile based transactions increased by 2.9% in 2016, 1.3% in 2017 and 2.6% in 2018 (relative to low-exposure individuals). We also conduct this analysis separately for men and women. We find that the effects of demonetization on the probability of using mobile transactions is positive for both men and women, though the estimates are less precise for women. Notably, conditional on individual characteristics, the marginal effects for women in the high-exposure group are larger than men in the high-exposure group. It would be useful to understand the mechanisms driving this difference, however, our current data limitations do not allow us to do so.

There is a small but growing literature analysing the demonetization episode. Our paper is closest in spirit to Chodorow-Reich et al (2019). They build a theoretical model for cash holdings, and test the predictions of the model using a cross section of data at the district level. Contrary to this, we use cross-sectional data over 5 years at the individual level to study the impact of this shock on individual outcomes. We also differ in the main identifying assumption: we do not rely on assumptions about the behaviour of the central bank to construct a measure of exposure to the shock, instead using the distance from the nearest bank branch (which cannot change in response to the shock in the short run). Crouzet et al (2020) look at coordination failures in technology adoption on the merchant side of digital payments and use the demonetisation shock to study if these failures were overcome by a short-term cash shortage. Kisat and Phan (2020) investigate whether the demonetization shock propogates through the input-output networks. Agarwal et al (2021) study the relationship between consumer spending and digital payments, using demonetization as a source of exogenous variation.

We proceed as follows: section 2 discusses the institutional details of demonetization, section 3 describes the details of the data, section 4 provides the empirical model and results for the aggregate analysis, section 5 provides the empirical model and results for the individual level analysis, and section 6 concludes.

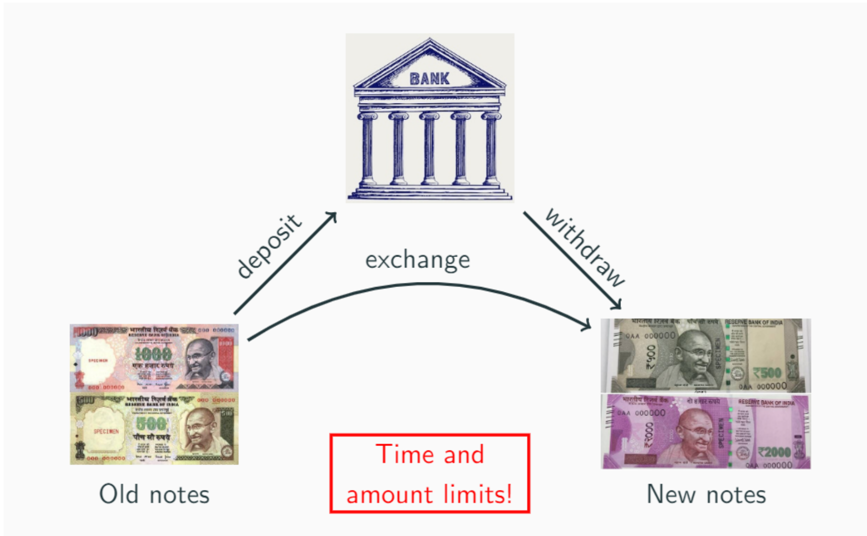
2 Demonetisation

In this paper, demonetisation refers to the unexpected macroeconomic liquidity shock that happened in India in November 2016. Even though it wasn't the first time currency was demonetized in India or elsewhere, it was a unique episode as it happened in an otherwise economically stable environment (Lahiri, 2020). On the evening of November 8 2016, without prior notice, the Prime Minister of India announced that the two highest denomination currency notes (Rs. 1000 and Rs. 500) were to be demonetized with immediate effect.[2] This amounted to 86% of the value of the currency in circulation being wiped out with no prior notice to households, businesses or banks. The volume of cash in circulation fell by 75% and there was a corresponding increase in bank deposits (Figure 2). Demonetized currency could be deposited in banks or exchanged, with a daily limit for new denominations of 500 and 2000 rupees until December 31st 2016 (Figure 1).[3] Households with bank accounts could access funds using cheques, transfers, debit and credit card and the main difficulty was in using cash for transactions ((Chodorow-Reich et al. 2019).

[2] Using 2016 USD-INR exchange rates, the value of the demonetized notes corresponded to $ 8 and $ 16.

[3] A daily limit of Rs. 4000 was imposed initially, which was then increased Rs 4500 and subsequently reduced to Rs 2000. Weekly limits on the amount that could be exchanged were also put in place. Additionally, for bank deposits of demonetized currency, taxpayer identification number had to be provided for deposits above 50,000 rupees.

Figure 1: Remonetization process



The motivation for the policy was threefold. First, it aimed to target corruption and tax evasion through undeclared income held in cash. It also attempted to address the issue of counterfeit currency circulating in the economy. Lastly, as a more long-term objective, it hoped to steer the heavily cash dependent Indian economy towards a more formalized system of digital payments. The remonetization of the economy was not a smooth process- caught off-guard by the abruptness of the policy, both the Reserve Bank of India and commercial banks struggled to stock automatic teller machines (ATMs) with cash. Moreover, there was a relative excess of 2000-rupee bills, which were less useful for low value daily transactions (Lahiri, 2020). Thus, in the days following demonetization, the cost of cash relative to other methods of payments increased substantially. This translated into a significant substitution towards digital platforms and electronic payments. As observed in figure 3, there was a sizeable jump in the volume of transactions taking place by debit cards and payment platforms. In the two months immediately following demonetization, the volume of debit card usage grew by an average of 72% per month, and the volume of platform transactions grew by nearly 47 %. At the same time, on the side of merchants there was a corresponding increase in the number of point of service terminals for electronic payments as well.

Two noteworthy caveats emerge from figures 2 and 3. First, the demonetisation shock was short term - from figure 2, we can see that cash in circulation is restored to its pre-demonetisation levels

Figure 2: Demonetisation and cash in circulation

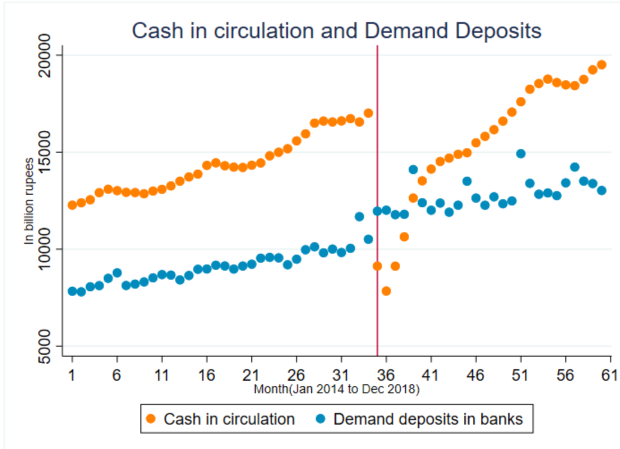
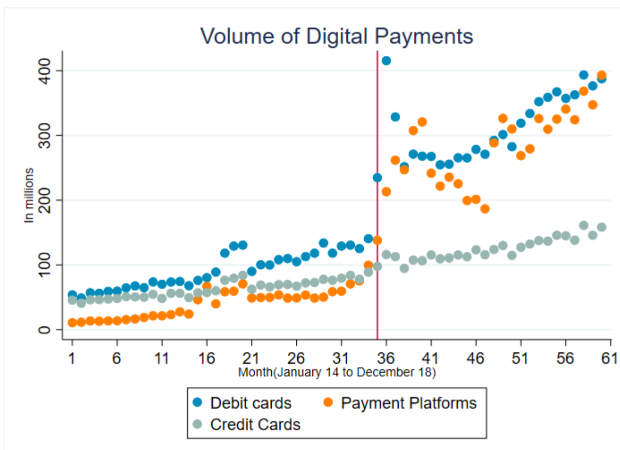


Figure 3: Demonetisation and electronic payments



in six months after the shock. In fact, by the end of the period of analysis, the cash in circulation was higher than pre-demonetisation levels. Additionally, although figure 2 points to an increase in volume of transactions, it is unclear if this increase happened because more individuals started transacting digitally or if existing users were transacting more.

3 Data

3.1 Google Trends

In our final data set obtained from Google Trends, we observe state wise monthly hits of keywords linked to 126 unique electronic payments platforms. The period of analysis is January 2014 to

November 2020. Although we observe a total of 36 states and union territories, to be consistent with the geographical scope of the individual level analysis, we restrict our sample to the 21 states that are also present in the household survey data. We add the total platform hits by state and month, to end up with 2241 unique combinations of total state hits-state-month.

Figure 4: Combined monthly hits of all platforms

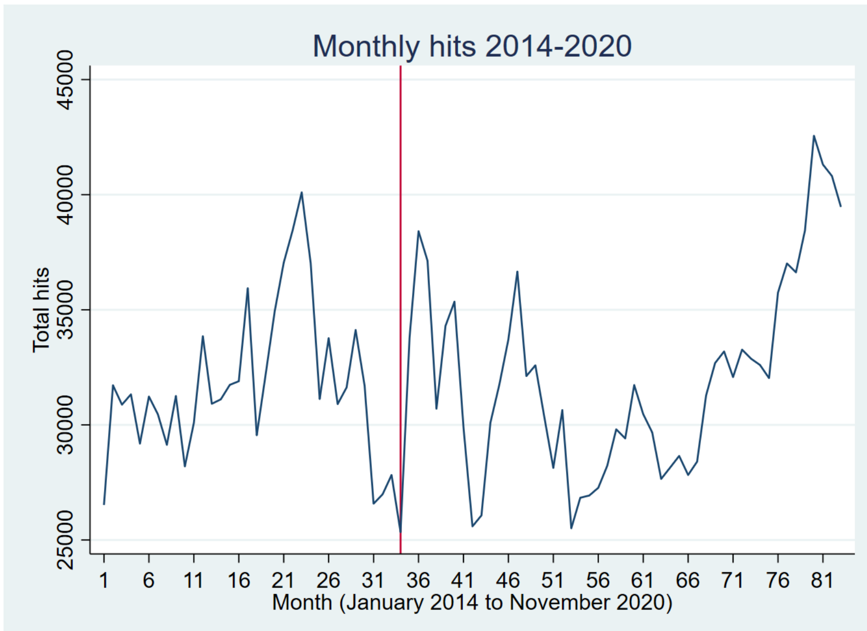


Figure 5: Digitisation in India

3.2 Data on formality of states

Our aggregate analysis rests on formality of the workforce as a measure of exposure to the demonetisation shock. We obtain the data on this from the report on Employment in Informal Sector and Conditions of Informal Employment published by the Ministry of Labour and Employment, Government of India in 2013. The measure of formality is defined as the number of workers per thousand that receive paid leaves in a given state. This means that this measure varies between 0 and 1000. A state which has a higher proportion of workers getting paid leave is said to be more formal than a state that has a lower proportion of workers getting paid leave. This enables us to have a continuous measure of formality of states. Since we use data from 2013 (before the start of our period of analysis) and no subsequent data is available for this indicator, we assume that the measure of formality of states remains constant over time.

Table 1: Number of workers/1000 receiving paid leave across states in 2013-14

PL workers/1000	No. of states
< 100	1
Between 100 & 200	4
Between 200 & 300	8
Between 300 & 400	3
Between 400 & 500	3
Between 500 & 600	3
< 700	2

3.3 Household Survey Data

To provide preliminary evidence on the effects of demonetisation on the individual probability of platform usage, we use 5 rounds of new household survey data between 2014 and 2018. This nationally representative data is collected by Financial Inclusion Insights, and samples approximately 45 thousand households and individuals per year. However, the data doesn't follow the same individuals over time and contains repeated cross-sections instead. In addition to information on household financial behaviour, the survey also provides a rich set of demographic variables.

Table 2: Year wise sample size

Year	Households
2014	45,087
2015	45,036
2016	45,540
2017	47,132
2018	48, 027

3.3.1 Digitisation in India

Descriptive evidence from the survey shows a swiftly changing digital landscape in India. The proportion of individuals that own a mobile phone has increased consistently over the period 2014 to 2018 (Figure 6). Similarly, the proportion of adults browsing the internet, using mobile phones to make transactions, as well as using payment platforms has increased substantially between 2014 and 2018 (Figure 7).

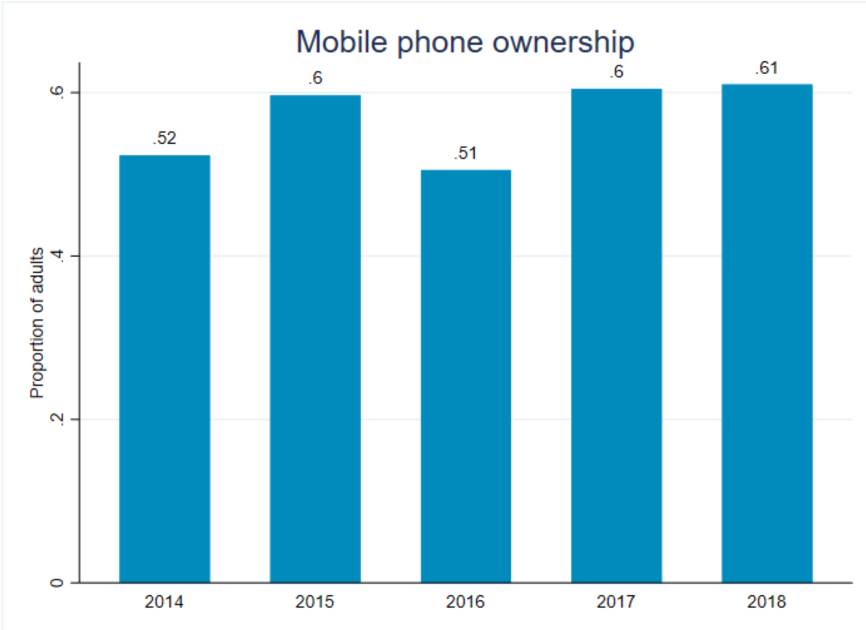


Figure 6: Mobile Ownership in India

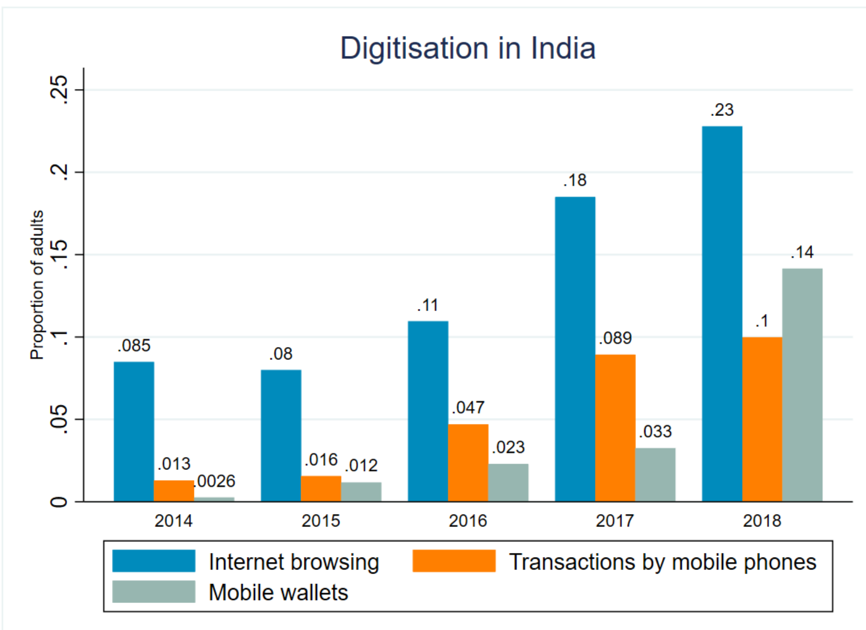


Figure 7: Digitisation in India

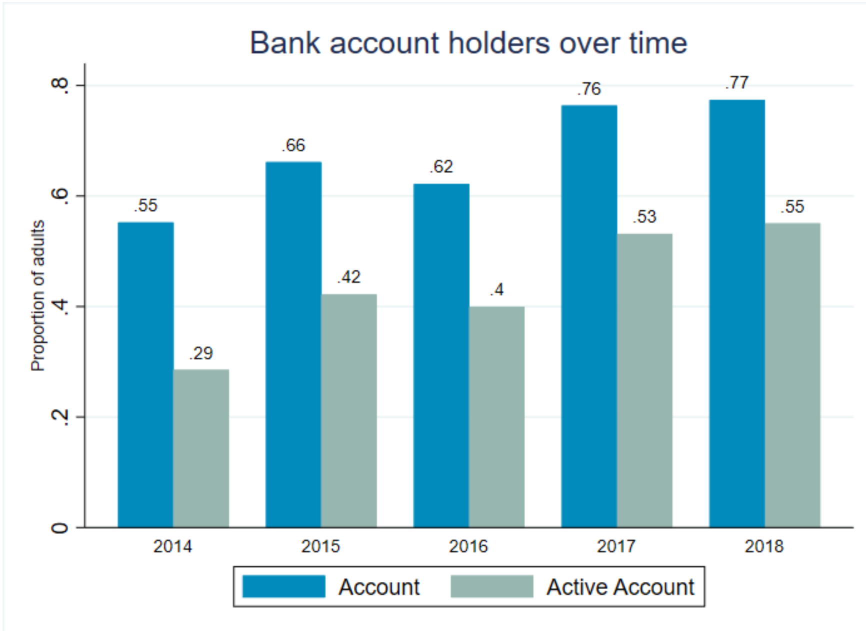


Figure 8: Bank Account Penetration

3.3.2 Bank Accounts and Savings

The proportion of the population having bank accounts increased from 55% in 2014 to 77% in 2018. However, the proportion of active users of bank accounts was significantly lower, though increasing, over the same period (Figure 8). At the same time, the proportion of people having any type of savings (formal, cash, gold, informal) declined substantially over this period (Figure 9). This was particularly true for the proportion of people having savings in cash: before demonetisation, in 2015, 71% of the population held some savings in cash, this decreased to 10% in 2017 (Figure 9). This was not compensated by an increase in savings in formal institutions, which declined as well during this period. By the end of the period, even though the proportion of people saving in formal institutions did not go back to the pre-shock levels, the proportion saving in cash did, reaching 60% in 2018.

4 State-level analysis

In this section, we provide an empirical model of the effect of demonetisation on the inclination to use platforms among different Indian states across different months. We proxy the inclination to use platforms in a particular state by the total hits of relevant keywords. We assign a level of

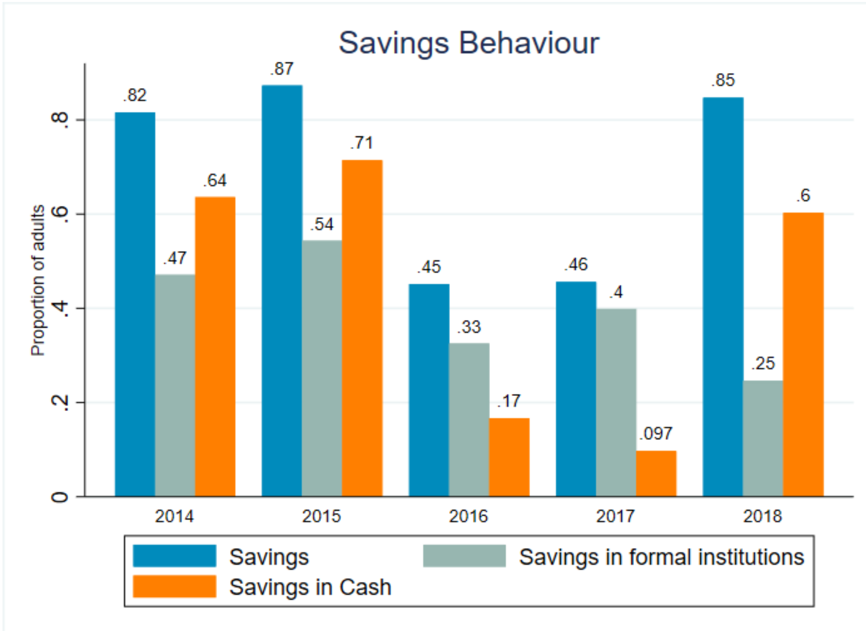


Figure 9: Proportion of Adults Saving

formality to states based on the share of the labour force receiving paid leaves. The states that have a larger share of workers receiving paid leave are said to be more formal. In order to classify states based on their exposure to demonetisation, we argue that states with a more informal labour force are more exposed to the shock. Workers in the informal labour force are likely to be more cash dependent than formal workers, especially since the former are predominantly paid wages in cash.

We estimate the following model using OLS:

$$TH_{sm} = \beta Formality_s \times \mathbb{1}_{m>34} + \lambda_m + \lambda_q \times \lambda_s + \epsilon_{sm} \tag{1}$$

The indices m, q and s denote month, quarter and state respectively. The dependent variable is total hits of relevant keywords in state s, in month m. $Formality_s$ measures the share of workers in state s receiving paid leaves. The main variable of interest is $Formality_s \times \mathbb{1}_{m>34}$ which measures the exposure of the state to the demonetization shock. Demonetization took place in month number 34 of the period of analysis and $\mathbb{1}_{m>34}$ is an indicator for the months after the shock. The main parameter of interest β then measures the change in total hits after the shock for a 1 unit change in the formality variable. A negative estimate of β would mean a higher inclination to use platforms after the shock for informal states relative to formal states. We control for time varying heterogeneity by including month fixed effects λ_m and state specific time varying heterogeneity by including state-quarter fixed effects $\lambda_q \times \lambda_s$. The latter helps us control for changes in economic outcomes (for example, GDP) that vary across state and quarters.

4.1 Persistence

In order to study the persistence in the effects of the shock on the inclination to use platforms, we decompose the econometric model in equation (1) as follows:

$$TH_{sm} = \sum_{i=0}^{i=9} \beta_i Formality_s \times \mathbb{1}_{q0_i} + \beta_{10} Formality_s \times \mathbb{1}_{m>61} \lambda_m + \lambda_q \times \lambda_s + \epsilon_{sm} \qquad (2)$$

The dependent variable and the fixed effects remain the same as in equation (1). However, instead of having one variable that captures the average effect on total hits for all the months after the shock place, we decompose this to allow for the effect to vary with quarters. Specifically, $q0_i$ refers to quarters after the shock with i going from 0 (the quarter of demonetisation: November to January 2016) to 9 (November to January 2019). $\mathbb{1}_{m>61}$ is an indicator for all the months after January 2019. The main parameters of interest then are β_i.

4.2 Results

As mentioned in the previous section, we use two specifications for our analysis. In the first (column 1 of table 3), we look at the average impact of the shock on the total state hits after the month of demonetisation. We find that the main parameter of interest on our exposure variable (the interaction of formality with the months after demonetisation) has a negative sign and is significantly estimated at 99% level of confidence. This means that after the shock, the more informal the state, the higher the total hits of the relevant keywords, and thus, the higher the inclination to use platforms. This result is consistent with economic intuition - in states which are more exposed to the shock, individuals have a greater incentive to switch to electronic payments and transactions through platforms.

Table 3: Aggregate Analysis Results

	(1) Total State Hits	(2) Total State Hits
Formality × After November 2016	-1.224**	
	(0.523)	
Formality × Nov'16-Jan'17		-1.224**
		(0.524)
Formality × Feb'17-Apr'17		-0.372
		(0.600)
Formality × May'17-Jul'17		1.447
		(0.742)
Formality × Aug'17-Oct'17		0.970
		(0.863)
Formality × Nov'17-Jan'18		0.408
		(1.144)
Formality × Feb'18-Apr'18		0.919
		(1.168)
Formality × May'18-Jul'18		1.786
		(1.235)
Formality × Aug'18-Oct'18		1.864
		(1.249)
Formality × Nov'18-Jan'19		2.094
		(1.291)
Formality × After Jan'19		2.642*
		(1.304)
Constant	720.3***	720.3***
	(88.17)	(88.42)
Month FE	yes	yes
State × Quarter FE	yes	yes
N	2241	2241
adj. R^2	0.950	0.951

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

However, as we see in column (2) of table 3, the relative effect of demonetisation on informal states is very short lived. The parameter of interest is negative and significant only for the month of demonetisation and the two months thereafter (November 2016 to January 2017). We find no significant relative effects of the shock after this. This finding is consistent with what the literature has found for adoption of electronic payments (by merchants) in India as a result of the shock (Crouzet et al, 2019). With a large extent of the remonetisation process occurring in the six months after demonetization, these authors conclude that the effects of this shock on merchants was short term.

5 Individual level analysis

In this section, we turn to individual level effects of demonetization and employ household survey data. The main dependent variable that we consider is the individual's probability of using their mobile phone to carry out a financial transaction. As mentioned earlier, since demonetization was a shock that affected all individuals in the country, we create a measure of exposure to the shock that classifies individuals into low-exposure and high-exposure groups.

5.1 Individual level exposure to demonetization

We use the distance of the individual from their nearest bank branch as a measure of exposure to the shock. In the aftermath of demonetisation, the only way for individuals to get rid of old bills was to either deposit it in their bank accounts or exchange it for new notes at the bank branch. Given individual level demographics, the further away an individual lives from the bank, the more exposed they were likely to be to the shock and the greater the incentive they have to switch to electronic payments.

In the survey, we do not observe individual's distance from the nearest bank branch as a continuous variable. We observe a categorical variable with 4 categories: i) bank branch located within 0.5km of residence; ii) bank branch located between 0.5 km and 1 km of residence; iii) bank branch between 1km and 5km of residence; iv) bank branch located at more than 5km of residence. In terms of demographic characteristics of individuals in these groups, we find that groups i) and ii) are more similar to each other than they are to groups iii) and iv) (Section A.1). The same holds for groups iii) and iv). Hence, for the empirical analysis, we collapse these 4 categories into two: Near ($<$ 1km) and far ($>$ 1km).

Table 5: Distance regressed on demographics

	Far
Age	-0.00435***
	(0.000486)
Gender	0.141***
	(0.0162)
Education	-0.0290**
	(0.00890)
Income score	0.000651
	(0.000333)
Bank Account	0.0260
	(0.0161)
Urban	-2.361***
	(0.0235)
Mobile	-0.0765***
	(0.0156)
Smartphone	0.0336
	(0.0269)
Employed	0.164***
	(0.0157)
Internet	-0.0546*
	(0.0267)
Pseudo R^2	**0.123**

Table 4: Number of individuals in each distance bin

Year	< 0.5km	0.5 to 1km	1 to 5km	> 5km	Missing
2014	4058	4492	6830	5354	24353
2015	10371	8320	12998	10532	2815
2016	6242	9107	15243	9650	5298
2017	9193	9060	15957	10238	2684
2018	5749	7738	19939	12802	1799
Total	39773	42335	77140	53502	63096

5.2 Distance to bank and demographics

To check that the individual's nearest bank branch is not entirely explained by observed demographic and economic variables, we regress distance on age, gender, education level, income score, bank account ownership, urban-rural classification, mobile phone ownership, smartphone ownership, employment status and internet usage (results table in appendix). We find that the distance to the nearest bank branch is not deterministic in individual level covariates of interest (Table 5).

5.3 Econometric specification

For an individual i in year t:

$$Y_{it} = \bar{\alpha} + \beta X_{it} + \gamma_1 Far_t + \gamma_{2t} \mathbb{1}_t + \gamma_{3t}(\mathbb{1}_t \times Far_t) + \epsilon_{it} \tag{3}$$

Y_{it} is a binary variable which records whether the individual uses their mobile phone for financial transactions.[4] X is a vector of demographic variables including gender, age, employment status, whether the individual has a high school diploma, whether the individual lives in an urban or rural area and fixed effects for the state in which the individual resides in. Far_t is an indicator variable that denotes that individual lives in a high exposure area (nearest bank branch at more than 1 km away). $\mathbb{1}_t$ is an indicator variable for year fixed effects. The main variables of interest $\mathbb{1}_t \times Far_t$ capture the interaction between individuals in high exposure areas and the year.

5.4 Identification

In order to identify the effect of the shock on the individual probability of using mobile financial transactions, parallel trends need to hold. This would require that the high-exposure group and low-exposure groups have similar trends in adoption of mobile transactions before the shock. Moreover, we have to assume that the demonetization shock itself did not have an impact on our measure of exposure to the shock. This means that we assume that demonetization did not affect the geographical location of bank branches. It is reasonable to expect that banks did not take the costly (sometimes infeasible) decision of building more bank branches (or closing them) in response to the shock. The other factor that would affects the measure of exposure to the shock is if individuals migrated to be closer to bank branches as a result of demonetization. Given individual characteristics (which we explicitly control for) and the fact that demonetization was a temporary shock, we assume that this was not the case.

The main parameters of interest are γ_{3t}: capturing the relative probability of using mobile financial transactions for high exposure individuals in every year of the analysis. We expect the estimates for these parameters to be insignificant in the years before the shock. This would verify the parallel trend assumption. A positive sign of any of the γ_{3t} means that, controlling for other individual char-

[4]The precise question in the survey is: Have you ever used your mobile phone to carry out a financial transaction?

acteristics, high exposure individuals have a higher probability of using platforms/mobile phones for transactions.

5.5 Results

Table 6 provides the results of the logistic regression specified in section 4.3. We find that parallel trends hold: the coefficient on the interaction of the year before demonetization (2015) and the high-exposure group (Far) is not statistically significant. We find a positive and statistically significant effect of the shock on the probability of using mobile transactions for the high exposure group in the year of the shock (2016). This effect persists in 2017 and 2018, though the estimate for 2017 is less precise than the other years. The parameter estimates of the control variables have expected signs and are precisely estimated. A higher probability of using mobile phones for financial transactions is associated with younger individuals, men, individuals with high school diplomas, individuals living in urban areas and individuals that are employed.

Table 7 provides the average marginal effects of the shock on the probability of using mobile transactions for 2016, 2017 and 2018. Even though the effects are positive and statistically significant, their magnitude is relatively small. For instance, on average, for high-exposure individuals, the probability of using mobile phones for financial transactions increased by 2.9% in 2016, 1.3% in 2017 and 2.6% in 2018.

6 Heterogeneity Analysis: Gender

In this section, we examine the heterogeneous effects of this shock across men and women. The existence of a gender digital divide is now well-documented, especially in developing countries (Antoine and Tuffley, 2014). The individual level survey data for India also demonstrates this gender digital divide. Figure **??** clearly shows the gap in mobile phone ownership between men and women: in 2014, 68% of all men but only 34% of all women owned a mobile phone. In 2018, 76% of all men owned a mobile phone as opposed to 45% of all women. The gender gap in mobile phone ownership stayed roughly the same: in 2014 and in 2018, nearly 2 times as many men owned mobile phones as women. Figure **??** shows the gender gap in the proportion of people that access/browse the internet. In 2014, nearly 4 times as many men accessed the internet as women. Even in 2018,

Table 6: Ever-use of mobile for financial transactions

	Mobile Transaction Indicator
Far	-0.539***
	(0.111)
Far × 2015	0.126
	(0.144)
Far × 2016	0.541***
	(0.120)
Far × 2017	0.267*
	(0.116)
Far × 2018	0.519***
	(0.115)
Age	-0.023***
	(0.0008)
Men	0.44***
	(0.025)
High School Diploma	1.220***
	(0.022)
Urban	0.675***
	(0.022)
Employed	0.731***
	(0.026)
Time FE	Yes
State FE	Yes
N	205825
pseudo R^2	0.200

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Average Marginal Effects

Far × 2016	0.029***
	(0.007)
Far × 2017	0.013*
	(0.006)
Far × 2018	0.026**
	(0.006)

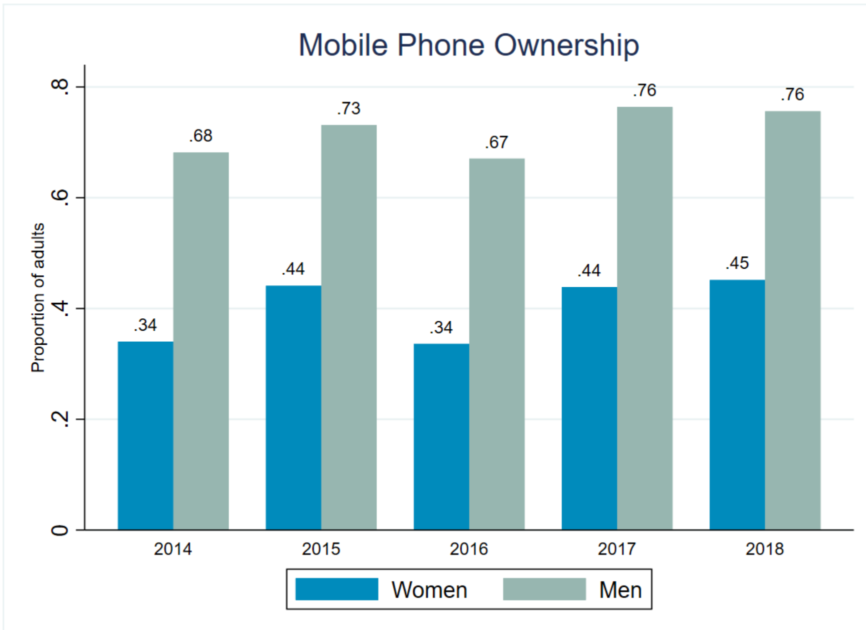


Figure 10: Mobile phone ownership by gender

the gender gap persisted (though declined) and 2 times as many men accessed the internet as women. Figure 12 highlights the gender gap in the main variable of interest: the proportion of people using their mobile phones to make financial transactions. In 2014, 3 times as many men used mobile transactions as women. The gap between men and women reduced slightly by the end of the period and in 2018, twice as many men were using mobile transactions as women. In the rest of this section, we focus on the gender differences in the response (of the probability of using mobile transactions) to the demonetization shock. We estimate the regression specified in Equation 3 separately for the sample of men and women.

Table 8 summarizes the results of the estimation. Parallel trends hold for both the samples. For the sample of women, the demonetization shock has a positive impact on the probability of using mobile transactions for the high-exposure group relative to the low-exposure group. Estimates from

Table 8: Ever-use of mobile for financial transactions

	Women	**Men**
	Mob. Transaction	Mob. Transaction
Far	-1.140***	-0.331***
	(0.233)	(0.128)
Far × 2015	0.557	0.0237
	(0.290)	(0.167)
Far × 2016	1.100***	0.340**
	(0.248)	(0.141)
Far × 2017	0.888***	0.0606
	(0.240)	(0.136)
Far × 2018	1.188***	0.273**
	(0.239)	(0.135)
Age	-0.0182***	-0.0246***
	(0.00139)	(0.00100)
High School Diploma	1.162***	1.246***
	(0.0390)	(0.0268)
Urban	0.736***	0.653***
	(0.0377)	(0.0280)
Employed	1.173***	0.402***
	(0.0359)	(0.0319)
Time FE	Yes	Yes
State FE	Yes	Yes
N	110566	95259
pseudo R^2	0.196	0.180

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

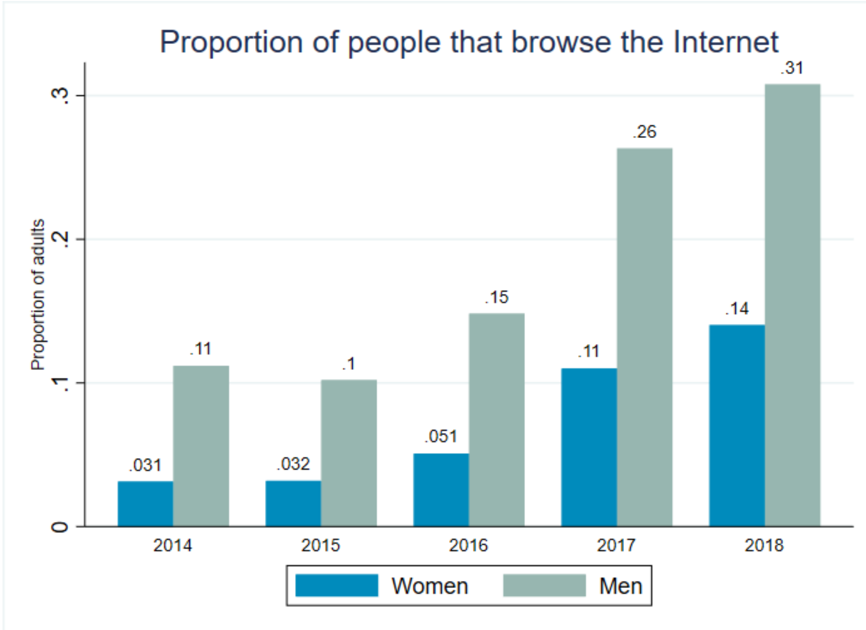


Figure 11: Internet browsing by gender

Table 9: Average Marginal Effects Across Gender

	Women	**Men**
Far × 2016	0.049***	0.024**
	(0.015)	(0.018)
Far × 2017	0.035***	0.003
	(0.012)	(0.009)
Far × 2018	0.049***	0.018**
	(0.013)	(0.009)

the sample of men also show a positive effect of the shock on the dependent variable, however, the estimates are much less precise than for women. Strikingly, conditional on individual characteristics, the marginal effects for women in the high-exposure group are larger than men in the high-exposure group (Table 9). For example, in 2016, the probability of using mobile phones for transactions increased by nearly 5% for women, compared to only 2.4% for men.

7 Conclusion

In this paper, we focus on an un-announced and large scale natural experiment that took place in India in 2016 that increased the short-term costs of holding and transacting in cash. As mentioned previously, we do not evaluate the welfare effects of this policy or whether the policy met its intended

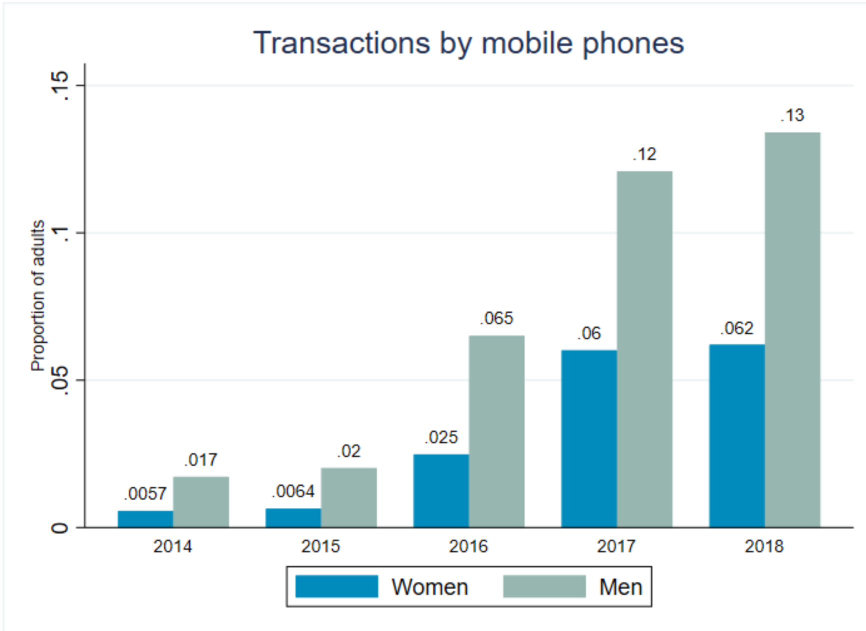


Figure 12: Mobile financial transactions by gender

objectives. Instead, we focus on how this temporary cash shortage affected the uptake of a specific form of DFS, mobile based payments/transactions. Using two new sources of data in a difference in differences framework, we conduct this analysis at the state level and also at the individual level. We build new measures of exposure to the shock, both at the state and the individual level. We find that in states where the labour market is less formal, and where workers were more likely to be affected by the demonetisation process, this shock led to a larger increase in the inclination to use mobile based payments than in states where the labour market is more formal. The effect of this "forced experimentation" was, however, short lived. At the individual level, people who were more exposed to the shock were more likely to use their mobile phones for transacting and we find that this effect persisted over the next two years. Strikingly, the marginal effects of the shock for high-exposure women was almost twice as high as for high-exposure men. However, the magnitude of all effects measured at the individual level was relatively small. Finally, ongoing work seeks to explore the mechanisms that explain the difference in marginal effects for men and women.

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A Additional Descriptive Statistics

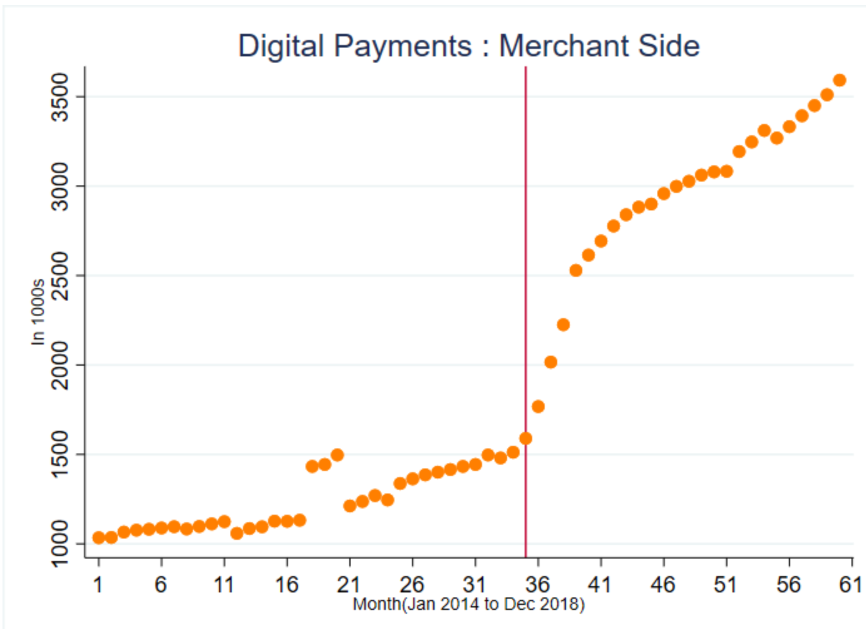

Figure 13: Number of Debit/Credit card terminals

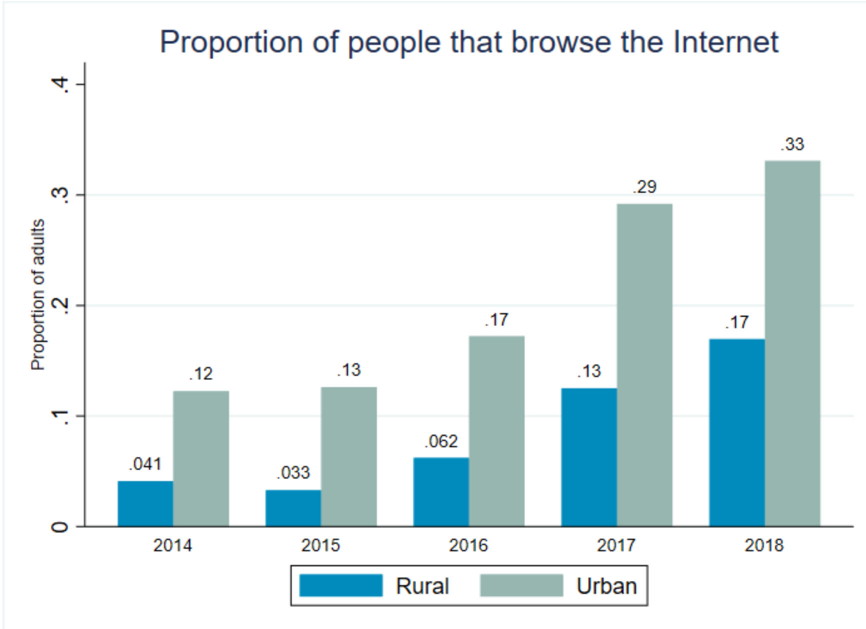

Figure 14: Internet browsing by region

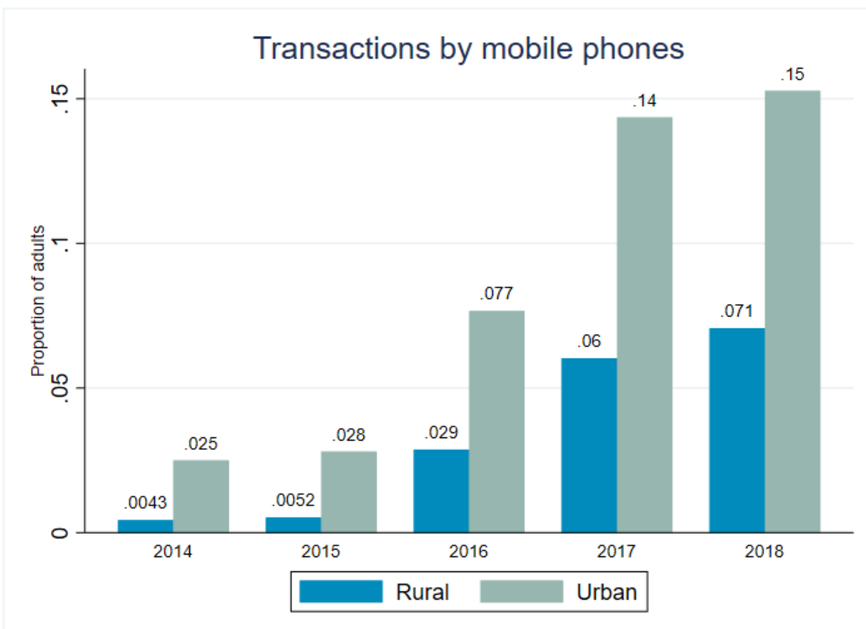

Figure 15: Mobile financial transactions by region

A.1 Distance Bin-Wise Demographics

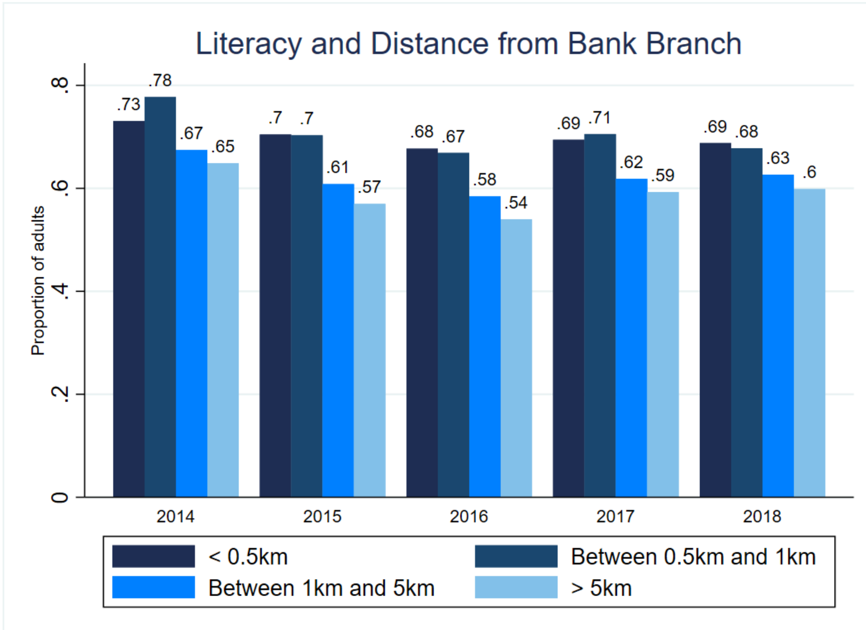

Figure 16: Literacy rates across distance bins

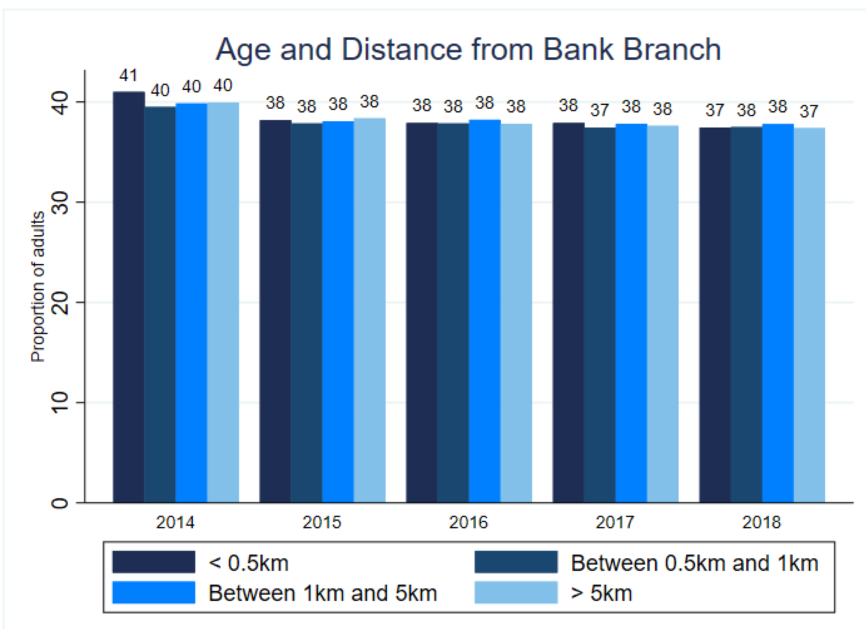

Figure 17: Mean age across distance bins

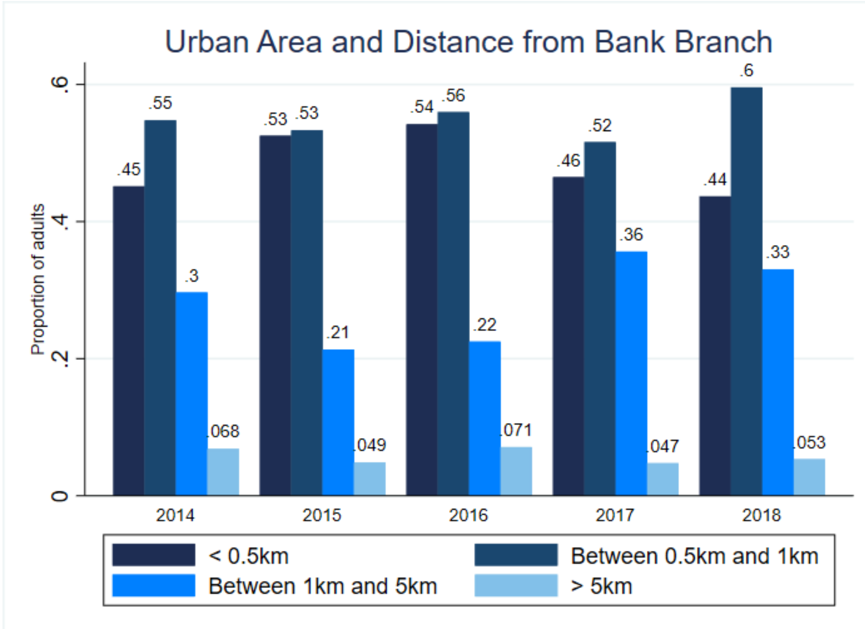

Figure 18: Proportion of people living in urban areas

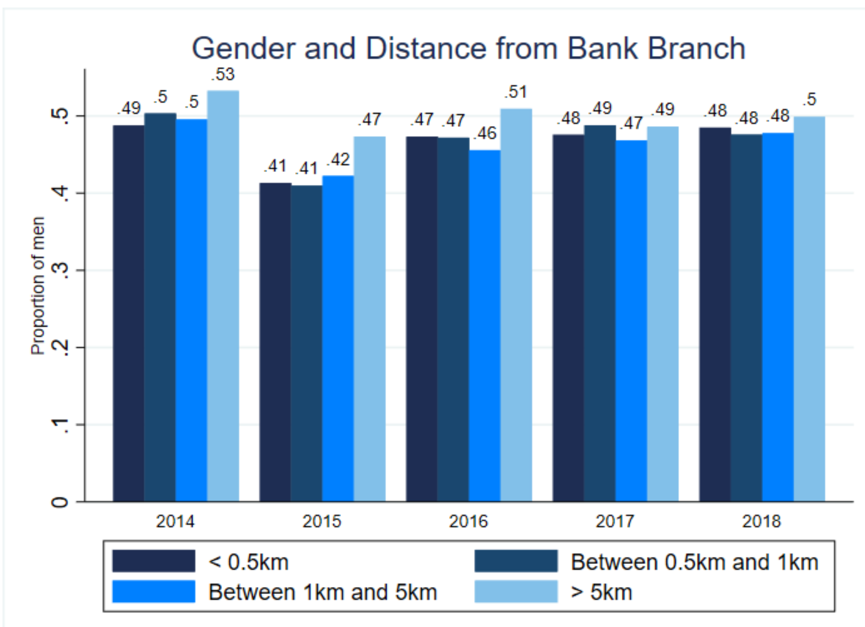

Figure 19: Proportion of men across distance bins

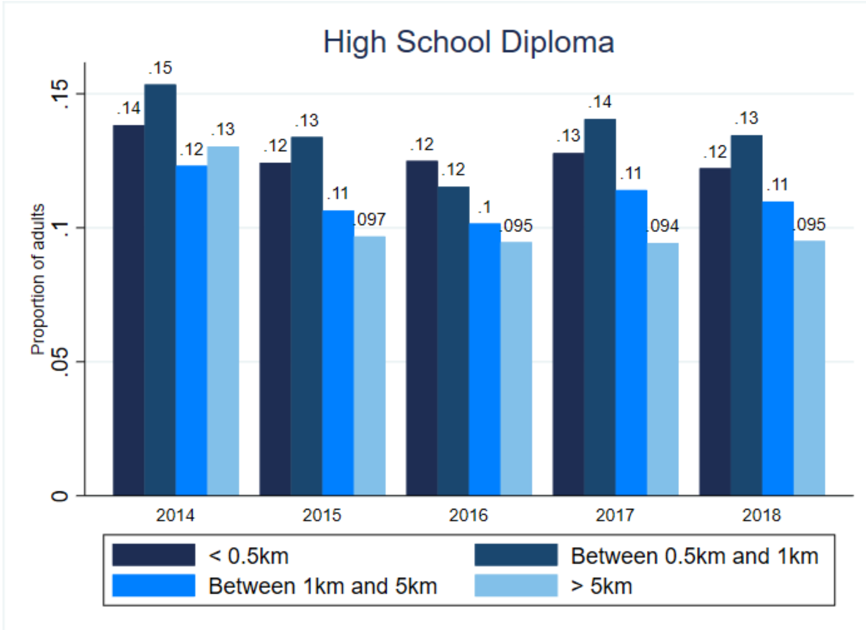

Figure 20: Proportion of high school diploma holders distance bins

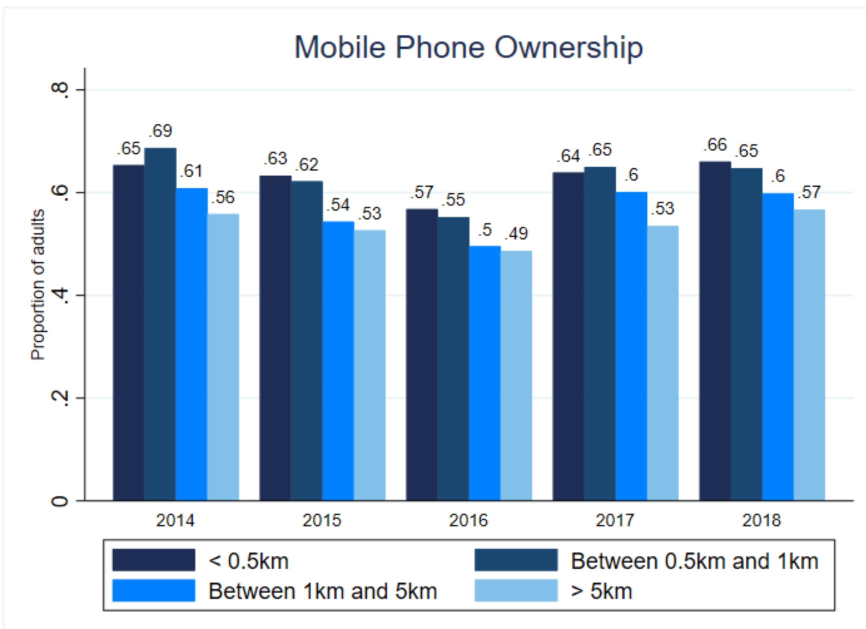

Figure 21: Mobile phone ownership across distance bins

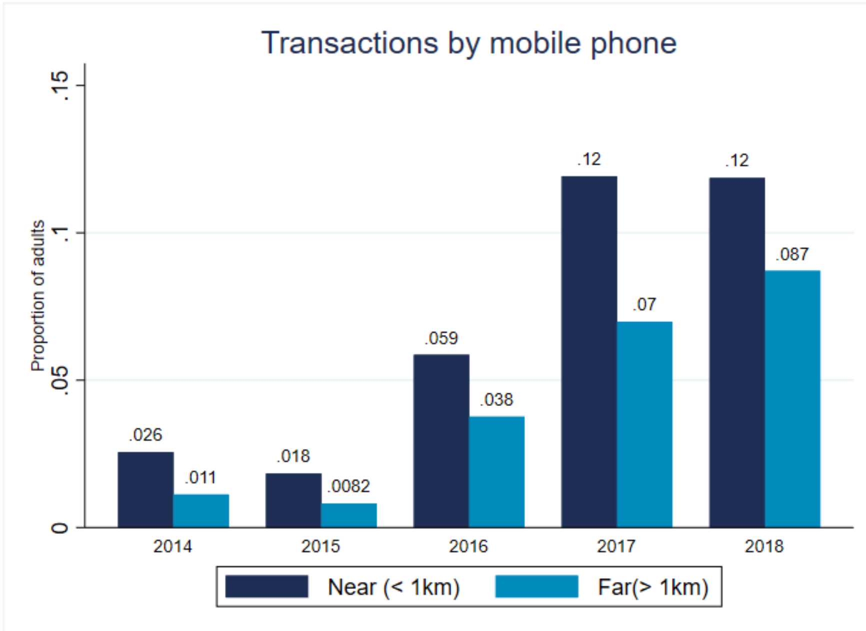

Figure 22: Mobile transactions across distance bins