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The Real Effects of Mobile Money: Evidence from a Large-Scale Fintech Expansion

by Manasa Patnam and Weijia Yao

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I N T E R N A T I O N A L M O N E T A R Y F U N D

**IMF Working Paper**

Strategy, Policy and Review Department

**The Real Effects of Mobile Money: Evidence from a Large-Scale Fintech Expansion<sup>1</sup>**

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**Abstract**

Mobile money services have rapidly expanded across emerging and developing economies and enabled new ways through which households and firms can conduct payments, save and send remittances. We explore how mobile money use can impact economic outcomes in India using granular data on transactions from Paytm, one of the largest mobile money service provider in India with over 400 million users. We exploit the period around the demonetization policy, which prompted a surge in mobile money adoption, and analyze how mobile money affects traditional risk-sharing arrangements. Our main finding is that mobile money use increases the resilience to shocks by dampening the impact of rainfall shocks on nightlights-based economic activity and household consumption. We complement these findings by conducting a firm survey around a phased targeting intervention which incentivized firms to adopt the mobile payment technology. Our results suggest that firms adopting mobile payments improved their sales after six-months of use, compared to other firms. We also elicit firms' subjective expectations on future sales and find mobile payment adoption to be associated with lower subjective uncertainty and greater sales optimism.

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## I. INTRODUCTION

Despite increased access to finance, the effective use of financial services remains low in India<sup>1</sup>. The World Bank Global Findex Database (2017)<sup>2</sup> reports that only 20 percent of adults in India save with a financial institution. Even within the population possessing a bank account, nearly half (48.5 percent) of the accounts remain inactive<sup>3</sup>, making India the country with the highest inactivity rate in the world in the 2017 survey. Moreover, only about 39 percent of the survey respondents reported sending or receiving domestic remittances using a financial institution. The bulk of remittance transfer are conducted with cash, either personally or using the network of relatives and friends (Demirguc-Kunt and others, 2018).

Mobile money and digital wallets offer an innovative technological solution to fill the financial infrastructure gap and alleviate frictions related to the limited use of formal financial services. This is because, the use of mobile money allows consumers to perform financial transactions in a relatively inexpensive and reliable way (Jack and Suri, 2014), eliminating spatial and temporal barriers, and can be used as a storage mechanism by both the banked and unbanked (Morawczynski and Pickens, 2009). In India, Paytm has been the largest mobile money payments firm since 2010, serving over 400 million users and 14 million businesses as of 2019 (BusinessWorld, 2019).

In this paper, we examine the economic impact from the two distinct use cases of mobile money, using large-scale data on monthly mobile money transactions of a little under half a billion users in India. First, does it improve the resilience to economic shocks by enabling a cheaper and more efficient way to save and transfer money? Second, can the adoption of this relatively costless payment technology help increase the sales of micro and small enterprises by reducing the frictions and costs associated with other payment methods. To investigate these questions, we use data from one of the largest mobile money provider in India, Paytm, and exploit (i) a policy episode which, by reducing the use of cash, suddenly increased the nation-wide adoption of mobile money and (ii) the sequencing of a phased targeted intervention that provided firms with an incentives to adopt the technology. In examining these questions, we aim to provide evidence on whether fintech innovations can have economy-wide economic impacts, and do so in a setting with a large informal sector presence and where considerable frictions to banking still exists.

Our investigation of the impact of fintech on economic outcomes is motivated, in part, by the observation that the adoption of Paytm mobile wallets surged in the immediate aftermath of

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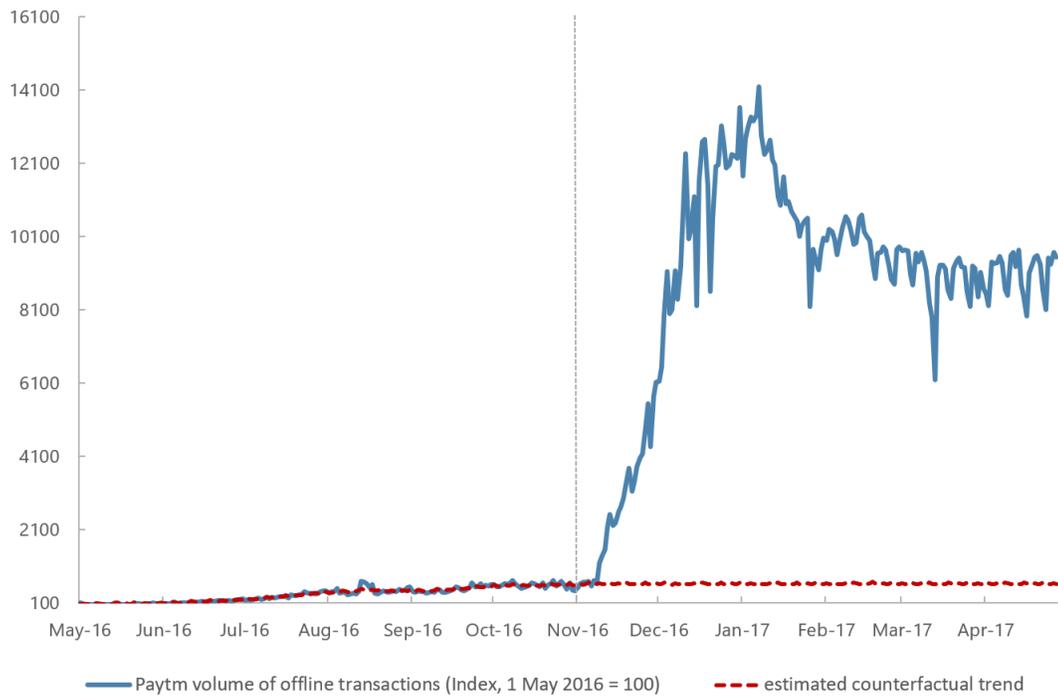
<sup>1</sup>Burgess and Pande (2005) document that between 1969 and 1990, bank branches were opened in roughly 30,000 rural unbanked locations. More recent evidence by Agarwal and others (2017) also show a large increase in the number of households having access to the formal banking services, as a result of the government's financial inclusion program in 2014. However they find that 81 percent of the new consumers do not deposit any money and 87 percent do not withdrawing any cash after opening the account. Finally, the World Bank Global Findex Database reports that, in 2017, over 80 percent of adults in India possess a bank account.

<sup>2</sup>Compiled using nationally representative surveys of more than 150,000 adults age 15 and above in over 140 economies. 3000 people were surveyed in India.

<sup>3</sup>Inactive accounts are defined as not making any deposit or withdrawal within a year.

the demonetization policy episode. This policy was enacted in November 2016, where the Government of India unexpectedly announced the withdrawal of major banknotes from circulation, effectively constraining the use of cash. Figure 1 plots the daily evolution of Paytm transaction volumes six-months before and after the announcement of the policy. As shown in the figure, there is a large spike in the level of transaction volumes immediately following the policy announcement, which contrasts with the relatively lower level of transaction activity in the days right before<sup>4</sup>. In contrast, the growth of debit cards transactions around this period (October-December 2016) was 129 percent (RBI (2017 Feb)). Chodorow-Reich and others (2020) also document a large increase in mobile money use following the demonetization policy announcement. Crouzet, Gupta, and Mezzanotti (2019) further show that this increase was persistent, in terms of the increase in the growth rate of the user base.

**Figure 1. Paytm Use around the Demonetization Policy Period**



The figure shows daily transaction volume from Paytm in blue (as an index with 1st May 2016 as base). The vertical dotted line corresponds to the date of demonetization. We also show in red the counterfactual prediction calculated based on a Bayesian structural time-series model (Brodersen and others, 2015), using proxies for economic/financial market activity growth in the post-demonetization period (based on daily industrial production index, stock market index, and consumption index).

This large scale-up of mobile money in a relatively short period of time provides a novel experimental setting that allows us to compare periods with and without mobile money that are

<sup>4</sup>Despite the distinct surge witnessed during demonetization, transaction volumes had been growing steadily before; the average growth rate of monthly transaction volume of Paytm was 52 percent in the six months prior to demonetization. It should also be noted that the level of transactions, attained from the adoption surge during the demonetization episode, was broadly sustained through the subsequent years (2017 and 2018).

in close proximity. With this in hand, we ask whether the negative effect of economic shocks, specifically rainfall shocks, can be mitigated by the use of mobile money, essentially testing for the regional risk-sharing benefits of mobile money. While the demonetization policy provided an economy-wide impact, suddenly increasing mobile money adoption, our specific research question makes additional use of the exogenous timing of region-specific rainfall shocks. As can be seen in the figure above, the period before the policy announcement was characterized with very low levels of adoption and use that allows us to construct a ‘placebo’ period and test the counterfactual of whether high-adoption districts were also special in their risk-bearing capacities when hit by rainfall shocks.

How can the mobile money innovation help improve risk-sharing outcomes? The principal implication of risk-sharing is that under complete markets, economic outcomes at the individual or regional level should respond only to aggregate economy-wide shocks and not to regional idiosyncratic shocks (Cochrane, 1991; Mace, 1991; Townsend, 1994). However the assumption of complete markets typically breaks down in a developing economy context where there are considerable frictions and market incompleteness in enabling risk-sharing arrangements. For instance, Kinnan and Townsend (2012) show that financial institutions help to some extent in smoothing consumption, but kinship networks play an important role too by providing indirect connections to the financial system when access is low. Jack and Suri (2014) show that mobile money innovation can help provide cheaper and more efficient alternatives to risk-sharing by reducing the transaction costs associated with transferring resources. They find that mobile money has had a significant impact on the ability of households to share risk providing welfare benefits of on average 3 to 4 percent of income.

Aron (2018) provides a review of the empirical evidence on mobile money and finds support for its role in improving risk-sharing. In addition to evidence from Jack and Suri (2014), notable other studies such as Mbiti and Weil (2015) and Wieser and others (2019) find that the increased use of mobile money lowers the use of informal savings mechanisms and increases remittance transactions (respectively). Almost all of the existing empirical studies surveyed by Aron (2018), examine household-level outcomes with the impact of mobile money based typically on adoption (i.e., households with and without a mobile money account). However one limitation of micro-studies, as pointed out by Aron (2018), is that they may be a poor guide to the economy-wide effect of a policy in the presence of spillover effects and network-wide externalities. In addition to this concern, Aron (2018) also suggests the data used by the existing evidence, typically on number of users, is subject to measurement error as it may not reliably measure activity.

The contribution of our paper is to examine the impact of mobile money on economy-wide outcomes by leveraging unique large scale data on mobile money transactions; to the best of our knowledge we are amongst the first to do so. Our analysis also addresses the data measurement issue by using data on both the number of users and transactions such that both, access to mobile money and its use, are measured precisely<sup>5</sup>. More importantly, our data on

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<sup>5</sup>We also benefit from the fact that Paytm leads the digital payment landscape in India (GSMA, 2018), having singularly dominated the market during the demonetization period, such that our data accurately represents the overall mobile money adoption and usage.

district-wise peer-to-peer transactions, are split, both within and across districts allowing us to explicitly test for the remittance channel through which mobile money can provide an insurance against shocks. We find that while rainfall shocks have a significant negative impact on economic activity, proxied by nighttime lights, – reducing it by 23 percent on average – this effect is partially mitigated in districts that use mobile money. Specifically a 10 percent increase in mobile money use in districts hit by a rainfall shock reduces the the negative effect of the shock by 3 percent.

Our analysis of district-level risk-sharing also contributes to the broader literature on regional risk sharing arrangements, as pioneered by [Obstfeld \(1995\)](#). The important role of finance in enabling regional risk sharing arrangements is documented by [Demyanyk, Ostergaard, and Sørensen \(2007\)](#) who find that the deregulation of U.S. banking restrictions, which increased financial integration, improved interstate insurance. [Hoffmann and Shcherbakova-Stewen \(2011\)](#) showing similar results also highlight the role of these deregulations in easing small firms’ access to credit, essentially reducing financial access frictions and transaction costs. We show in our paper that an innovative form of financial technology, mobile money, may also serve the same purpose by reducing the cost of sending inter-region remittances, and in this way improving financial integration to provide for better risk-sharing.

Our second question relates to more granular impacts at the firm level of enabling payments. The ability to conduct payments through mobile phones is arguably an important and appealing feature that has contributed to the large scale take-up of mobile payment technology by consumers and firms in several emerging and developing economies. For instance, in China, mobile payments for consumption reached RMB 14.5 trillion (or 16 percent of GDP) in 2017; Alipay (launched in 2004) and WeChat Pay (launched in 2011) now have over 500 million and 900 million monthly active users, respectively, or 36 percent and 65 percent of the Chinese population ([Frost and others, 2019](#)). In this context, we ask whether the adoption of the mobile money payment technology<sup>6</sup> can help increase the sales of micro and small enterprises by reducing the frictions and costs associated with other payment methods. The theoretical literature on the adoption of electronic payments has highlighted a few channels through which these effects may manifest. First, firms are likely to accept electronic modes of payment, despite, in some cases incurring costly surcharge fees to avoid ‘missed sales’. [Bourguignon, Gomes, and Tirole \(2019\)](#) describe missed sales as an occurrence when the customer has a high inconvenience cost of paying by cash, and is discouraged by either a high electronic payment surcharge or its non-acceptance. [Bolt, Jonker, and Van Renselaar \(2010\)](#) report, from survey data in the Netherlands, that 5 percent of consumers leave a store without purchasing when faced with card refusal or steep card surcharges. It is also possible that, in the presence of full information, consumers never even visit stores that do not accept electronic payments ([Chakravorti and To, 2007](#)). [Aggarwal, Brailovskaya, and Robinson \(2019\)](#) also posit that the improved transaction efficiency from the QR-code based payment technol-

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<sup>6</sup>The payment technology is based on the Quick Response (QR) code payment function in the mobile wallet, which enables users to receive and make immediate payments by generating their own QR code on the mobile phone app and complete a transaction by displaying or scanning QR codes. See [Aggarwal, Brailovskaya, and Robinson \(2019\)](#) for more details on how this technology can improve payments efficiency.

ogy could promote demand, leading to a genuine increase in consumer spending and provide evidence in favor of this.

Overall, this suggests that firms which are able to offer non-cash payment methods are likely to either acquire new customers, increase the retention of old customers or spur customer demand, thereby impacting sales. In the developing country context, there is an added effect of transaction efficiency that may also lead to an increase in firm sales. [Beck and others \(2018\)](#) study the effects of a payment technology innovation on entrepreneurship in a quantitative dynamic general equilibrium model, calibrating it with firm-level survey data from Kenya. They show that the adoption of mobile money improves firm performance by allowing for more efficient transfers, through the reduction in risky cash-holdings, and increased access to credit. In their model, the improvements in firm performance deriving from payment technology adoption lead to quantitatively significant macroeconomic effects.

To investigate this question, we take advantage of a phased targeting intervention that the mobile money company, Paytm, conducts to incentivize firms to adopt their mobile app. We exploit the sequencing of this intervention and identify ‘treatment’ and ‘control’ group firms, with the former set of firm having experienced this technology for six months. We then test the impact of this technology on subsequent firm sales using a difference in difference strategy. Our results show that Paytm using firms improve their sales, by approximately 26 percent relative to firms non-Paytm firms, after six months of use. We also elicit subjective expectations in our firm survey and find that Paytm using firms have lower subjective uncertainty around future sales. This complements our risk-sharing results at the district level on consumption, suggesting that mobile money use can have sizeable volatility reducing effects both at the firm and household level. These results are robust to different other methods of identification such as matching on a large vector of location and other characteristics.

Our analysis of whether payment technology adoption benefits micro and small enterprises, contributes to the broad literature on payment systems and, specifically, the still nascent literature on financial technology adoption by firms in developing countries. There are few empirical studies that directly study the effect of mobile payment technology on firm sales and their subjective expectations. [Dalton and others \(2019\)](#) use a randomized control trial, also incentivizing firms to adopt the technology, and find that adopting firms have better access to finance and experience a reduction in sales volatility. [Aggarwal, Brailovskaya, and Robinson \(2019\)](#) observe an increase in spending after the introduction of a mobile-payment technology on other electronic payment methods such as, debit and credit card sales, which promoted sales growth primarily for new businesses by facilitating customer acquisition.<sup>7</sup> Our paper contributes to this nascent strand of literature and links directly the effect of mobile payment adoption to the total sales of the firm (actual and future expectation) after six months of use.

We now proceed to describing the data used in our analysis. The paper is organized as follows: in Section II we discuss the data obtained from Paytm records on users and transactions

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<sup>7</sup>Other studies include [Aggarwal, Brailovskaya, and Robinson \(2019\)](#) who find that mobile money adoption among micro-enterprises raises their saving behavior and the likelihood of extending credit to consumers.

as well as the survey and intervention design for firm outcomes’ section III discusses the empirical framework for testing the risk-sharing and payments technology effects on economic activity and firm outcomes respectively; section IV reports the results obtained; Section V concludes.

## II. DATA ON TRANSACTIONS AND FIRM SURVEY

### A. Transaction level data

We obtain data from Paytm on their monthly users and transactions at the district level.<sup>8</sup> Our data is collected in total for 643 districts at a monthly frequency, between the time period of May 2016- April 2019. The transactions relate to both offline payments<sup>9</sup> and peer to peer transfer<sup>10</sup> and are disaggregated by value and volume. The payments data are further disaggregated by payments made to formal and informal sector firms. The peer to peer transfers data are further disaggregated by transfers made within a district and across-district transfers<sup>11</sup>. This enables us to identify remittance transfer from other districts in response to idiosyncratic district specific shocks.

We merge the transactions data with two other principal sources of data. The first is data on satellite measured night-time lights which we use as a proxy of district level monthly economic activity (Henderson, Storeygard, and Weil (2011); Chen and Nordhaus (2011); Kulkarni and others (2011); Alesina, Michalopoulos, and Papaioannou (2016)). The night time image data is obtained from the Earth Observations Group (EOG) at the National Centers for Environmental Information. The EOG produces a suite of average radiance composite images using nighttime data from the Visible Infrared Imaging Radiometer Suite (VIIRS) Day/Night Band (DNB)<sup>12</sup>. Prior to averaging, the DNB data is filtered to exclude data impacted by stray light, lightning, lunar illumination, and cloud-cover<sup>13</sup>. Each grid (one sq. km) is assigned a pixel value which has a radiance unit<sup>14</sup>. Luminosity is thus obtained as a sum of lights over the gridded area which in our case is defined as the district, using GIS data on the

<sup>8</sup>We use a list of total 654 districts, which is largely consistent with the 2011 Census of India.

<sup>9</sup>All Paytm data relate to offline transactions i.e., excluding online transactions through websites or online retail platforms. All other transactions conducted over the mobile phone and in physical shops are included under offline transactions.

<sup>10</sup>The peer to peer transactions data does not include a subset of these transactions made through the unified payments interface which Paytm joined, beginning November 2017. To ensure comparability over different time periods, we run the P2P analysis on data before November 2017, where these transactions were uniquely identified.

<sup>11</sup>Across district transfers for a specific district are defined as transfers received by this district from all other districts in India.

<sup>12</sup>The satellites collect a complete set of earth images twice a day. The data, in 15 arc-second resolution (1km grid interval), covers the world from 75° North to 65° South latitude.

<sup>13</sup>Cloud-cover is determined using the VIIRS Cloud Mask product (VCM). In addition, data near the edges of the swath are not included in the composites (aggregation zones 29-32).

<sup>14</sup>The radiance unit is the measured in  $W/cm^2$  and can be viewed as the intensity of electromagnetic radiation.

administrative boundaries of states and districts. We then detrend this district-wise luminosity time-series to account for monthly seasonality.

There are several reasons why we rely on luminosity data. The first is that monthly panel data on district GDP that could capture the evolution of incomes or consumption does not exist. The second reason is that, despite the measurement difficulties inherent in the use of such a proxy, there is convincing evidence to suggest that luminosity is strongly correlated with standard economic outcomes. [Chaturvedi, Ghosh, and Bhandari \(2011\)](#) and [Bhandari and Roychowdhury \(2011\)](#) examine this correlation at the district level in India and find similar effects. More recently [Beyer and others \(2018\)](#) measure monthly economic activity in South Asia at the district level using VIIRS lights data and find that for Indian districts, the luminosity measure has a high correlation with measured activity in national accounts. Finally, an additional reason for relying on luminosity evidence is that our identification strategy focuses on *changes* in outcomes rather than levels. This means that the sources of persistent heterogeneity across districts in the relationship between luminosity levels and levels of economic activity are not a concern.

To corroborate our measure of night-time lights, we use additional data from the 2017-18 Periodic Labor Force Survey (PLFS) that provides various labor market measures in both the urban and rural areas across India. With the rotational panel sampling design of the survey, we were able to calculate average labor supply hours and per-capita expenditure on NSS region level<sup>15</sup> for 4 quarters (July 2017 - June 2018)<sup>16</sup>.

The other key data we use are meteorological data on rainfall shocks, which serves as a measure of a district-specific economic shock. The Indian Meteorological Department (IMD) collects rainfall data from about 3500 stations spread over the entire country, which provides the basis for compiling district-level rainfall statistics. The IMD also provides long-term rainfall averages for each district, based on the rainfall records for the period from 1951-2000. We take the amount of deviation of each district's current recorded level of rainfall in any given month from its long term average in the same month (positive or negative). We construct a binary indicator of rainfall shock taking the value one if the district's rainfall is deficient or in excess of its long term average by 1.5 standard deviations of the cross-sectional distribution (roughly 100mm of rainfall). Essentially, it reflects conditions of either drought

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<sup>15</sup>Regions are hierarchical domains of study below the level of State/ Union Territory used in the surveys by National Sample Survey Organization. The average number of districts within a given NSS region is 7.3.

<sup>16</sup>PLFS adopted a stratified multi-stage survey sampling design. First stage strata were formed within each NSS region. In urban areas, the strata are based on the size of towns and the rural strata are divided into sub-strata with similar population size. Then the first stage units (the Urban Frame Survey blocks in urban areas and the 2011 Population Census villages in rural areas) were selected within each strata/sub-strata by a Probability Proportional to Size with Replacement (PPSWR) Scheme. Finally, the second stage strata (within each first stage unit) were formed based on the number of members in the households with secondary or above education and certain number of households were selected from each second stage strata. To implement the 2-year rotational scheme, 8 samples of first stage units were drawn within each urban and rural strata/sub-strata. For each quarter, they canvassed (as first visits) 25 percent annual allocation of first stage units. The rural samples were only visited once and the urban samples were re-visited 3 times in the subsequent quarters. But since the samples were drawn randomly, the data of each quarter should be representative at the NSS region level on average.

or flood within a district<sup>17</sup>. Our analysis also conditions on district-wise bank availability. We use quarterly data on the number of reporting / functioning offices at district level from the Reserve Bank of India (RBI).

To explore descriptively the cross-sectional determinants of mobile-money use, and as Paytm data are anonymized and stripped of any user or firm attributes, we gather data on district level from various sources. Specifically, we use data from 2011 Census of India to calculate some district-wise population characteristic measures such as share of rural households, literacy rate and share of unemployed/casual workers. We also collect data on additional measures such as the share of households with bank accounts and the average wealth index from the 2015 Demographic Health Survey by USAID. In addition, we construct an index of firm informality at the district level based on data from the 2013 Economic Census of Establishments<sup>18</sup>.

## B. Firm Survey around Intervention for Technology Adoption

In addition to the data on transaction, we also collaborated with Paytm to conduct a survey to obtain information on firm sales. The survey was designed around an intervention that Paytm conducted to incentivize unregistered informal sector firms to adopt the payments mobile app, enabling them to accept payments using a simple QR code<sup>19</sup>. The incentives provided varied but included cashback offers based on the first hundred payments received through the app or Paytm Gold benefits. We provide further details of the intervention in section III.B and discuss here the sampling and information collected through the survey.

The survey collected information on 3,046 firms out of which 1,417 belonged to the set of firms that were targeted early in January 2019 (treatment group) and 1,629 belonged to the set of firms that were targeted in July 2019 (control group). 925 out of the 3,046 firms provided their sales figures for both the month before the survey and six months ago, which are crucial to our analysis. The firms chosen to be surveyed were randomly sampled within each state by treatment group.

The survey was designed to capture information on total firm sales for two sets of firms – those that used the mobile payments technology and those that did not – six months before

<sup>17</sup>We check robustness to using continuous rainfall shock measures i.e., a district's month-wise deviations from its long-term average as in Bhalotra (2010). The pattern of our results is robust to using this measure but the interpretation of risk-sharing parameters is less easy, compared to the binary measure.

<sup>18</sup>Some variables in the Economic Census can help indicate whether this establishment is more likely to be formal or informal business. We assigned points to these measures. And the informality level of each establishment is measured by the sum of these points. Specifically, the points are: 1) type of house (1 point for "residential" or "residential cum commercial"); 2) type of business ownership (1 point for "proprietary", "self help group" or "co-operative"); 3) source of finance (1 point for "borrowing from non-institutions" or "loan from self help group", 2 points for "self finance"); 4) sector (1 point for "rural"); 5) number of workers (points = 1/"total workers"); 6) share of non-hired workers (points = "non-hired workers"/"total workers").

<sup>19</sup>Apart from the specific intervention for our analysis, firms can also adopt Paytm through their network of colleagues/friends and perceived benefits to their business.

and after the former were incentivized to adopt. As the survey was conducted only once, we asked firms to provide both their current monthly sales information as well as the amount of sales from six-months ago on a recall basis<sup>20</sup>. In addition to information on sales, we also obtained data on other basic firm characteristics such as number of employees, business category, whether they have access to a bank account and the amount of loan taken.

The survey also elicited information on firms' subjective expectations on their future sales. We asked each firm to report the minimum and the maximum monthly sales that they expect to earn over the next year and the probability of earning more than the midpoint of the support of the distribution (Altig and others, 2019; Manski, 2004). The survey question was as follows:

*In the future, one year from now, what are your expectations about your business:*

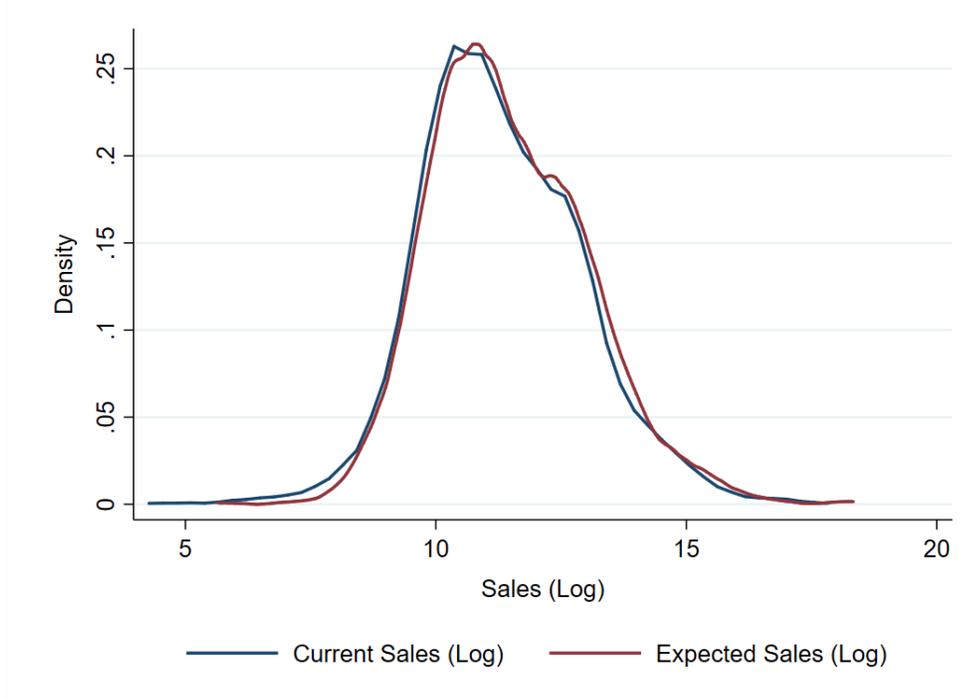
- *What is the monthly maximum amount of sales that you think your business will be able to earn, What is the monthly minimum amount of sales that you think your business will be able to earn,*
- *On a scale of 0-10, with 0 being not at all likely and 10 being certainly likely, what is the probability that your monthly sales in one year will be at least x [read out midpoint the value from excel sheet]?*

We then fit a triangular distribution to derive the mean (subjective expectation) and the standard deviation (subjective uncertainty) of future sales. The triangular distribution is typically a more plausible description of the probability distribution of sales, because outcomes further away from the midpoint receive less weight (see Guiso, Jappelli, and Pistaferri (2002) for a discussion).

To cross-examine the reliability of the subjective expectations data, we compare the average future sales across the cross-sectional distribution of firms with the actual current sales reported by them. Figure 2 plots the two distributions (in log values) which appear broadly similar with the future sales distribution slightly shifted to the right suggesting some optimism for the future.

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<sup>20</sup>The survey was conducted over two months which resulted in a small difference in the sales month for which current and recall sales were asked between treatment and control group firms. For treatment (control) group, the anchoring month for recall and current sales was February (January) and August (July) 2019. We use the World bank BEEPS database for India, to investigate if there are seasonal differences in the sales growth based on the one-month lag. We find a small positive sales differential such that the Jan-July sales differences is greater than than the Feb-Aug sales difference. This suggests that if anything we should expect treatment group firms to have a lower six-month sales growth differential.

**Figure 2. Distribution of Firm Sales: Current and Expected Sales**

This figure plots the density of current and expected firm sales. The expected firm sales distribution is obtained from the cross-section mean (first moment) of each firm's subjective expectations distribution for future sales (one year ahead).

### III. EMPIRICAL STRATEGY

#### A. Risk-Sharing Specification

In this section we present our empirical test where we ask how the rainfall shock and cross-sectional variation in mobile money use can affect economic activity, either as a way of savings for the unbanked or by allowing for faster and cheaper remittance transfer. We use the following empirical specification to test for the risk-sharing effects of mobile money, using night-time lights,  $Y_{it}$  as a proxy of economic activity in district  $i$  at month  $t$ :

$$Y_{it} = \beta S_{it} + \gamma M_{it} + \delta S_{it} \cdot M_{it} + \chi \mathbf{X}_{it} + \alpha_i + \eta_t + e_{it} \quad (1)$$

The variable  $S_{it}$  is a binary variable measuring the rainfall shock, as described in section II, and reflects conditions of drought or flood within a district. We expect rainfall shocks (both its surplus and deficit) to have a negative effect on economic activity.

The variable  $M_{it}$  measures the intensity of Paytm use in district  $i$  in month  $t$ . We capture intensity in two ways: first, we use the total number of users in a district at a given time;

second, we make use of information on across and within district peer to peer transfers. Transactions linked to peer to peer transfers are directly related to the intensity with which individuals transact among themselves, both within a given geographical area and across. The novel linking of transfers across districts allows us to examine the specific channel linked to across-district remittances in a district's ability to smooth its weather shocks. However, since the rainfall shocks tend to be spatially heterogeneous even within a district, it is possible that, even the within district peer to peer transfer activity reflects to some extent the ability to share risk.

Our hypothesis is that the negative effect of rainfall activity (i.e.,  $\beta < 0$ ) can be mitigated to some extent by efficient risk-sharing arrangements or some form of insurance. We test whether mobile money use can effectively serve this function by interacting the variable on rainfall shocks with mobile money use – the interaction term  $S_{it} \cdot M_{it}$  in equation (1) – and examining the magnitude and significance of its coefficient  $\delta$ . If indeed mobile money use can enable efficient risk-sharing arrangements then we would expect  $\delta > 0$  and the net shock-offsetting impact would depend on its magnitude.

We supplement the empirical specification with a rich set of fixed effects, both cross-sectional and time specific. We employ district fixed effects ( $\alpha_i$ ) and either a full set of year by month effects or year and month effects separately ( $\eta_t$ ). This ensures that our results are robust to district-level time-invariant unobserved heterogeneity as well as aggregate shocks over time. Since mobile money use tends to be highly correlated with banking activity, we also condition on the time-varying availability of bank branches in  $\mathbf{X}$  (Burgess and Pande, 2005).

Overall, our risk-sharing test predicates that  $\beta < 0$  and  $\delta > 0$ ; further if informal risk-sharing arrangements are more effective than formal sector in finance then we would also expect  $\delta > \zeta$ .

### Identification of risk-sharing effects

Our main identification restriction is for the rainfall shocks to be fully exogenous; specifically its timing and spatial occurrence to be orthogonal to mobile money use.

There is one possibility that mobile money use is endogenously adopted in districts that are accustomed to rainfall shocks and already have good risk-sharing arrangements in place. In this case, our results would reflect the effect of these unobservable factors. To ensure our results are fully robust to this concern, we exploit the period around the demonetization shock use both an alternative identification strategy and a placebo test. The sudden and unexpected large-scale take-up of Paytm, in the immediate aftermath of the government announcement on demonetization gives us an ideal setting to identify a counterfactual period. We are able to compare, thus, a period in which most districts had very limited use of mobile money and a period succeeding it within days, where the same districts experienced a massive adoption surge. It is important to note that our analysis exploits only the nation-wide demonetization policy<sup>21</sup> but still leaves room for district-specific rainfall shocks, which idiosyncratically impact economic activity.

<sup>21</sup>Chodorow-Reich and others (2020) show a contraction in aggregate employment and night lights-based output due to the demonetization policy of at least 2 percentage points and of bank credit of 2 percentage points.

*Leveraging the Regression Discontinuity Design:* To address the concern around confounding factors we make use of approaches based on the regression discontinuity design (RDD) which exploits the arbitrarily narrow window around the demonetization policy. The assumption is that within this interval, any unobserved factors related to a district’s risk-bearing capacity are likely to be similar so that observations right before the demonetization episode, that led to a spike in mobile money adoption, provide a comparison group for observations after. We therefore augment equation (1) with a flexible district-specific polynomial time trend,  $\mathbf{P}(t) \cdot \alpha_i$  (see [Davis \(2008\)](#) and [Auffhammer and Kellogg \(2011\)](#)) for a similar strategy):

$$Y_{it} = \beta S_{it} + \gamma M_{it} + \delta S_{it} \cdot M_{it} + \chi \mathbf{X}_{it} + \alpha_i + \eta_t + \lambda \mathbf{P}(t) \cdot \alpha_i + e_{it} \quad (2)$$

We obtain consistent estimates of the risk sharing parameter,  $\delta$ , from the RD specifications above under the presence of time-varying unobservable factors related to a district’s risk-bearing capacity, provided that the conditional mean of the unobservable is continuous around the threshold. In other words, we expect that while unobservable factors jointly related to the rainfall shock and mobile money use can affect economic activity, they do not change discontinuously at the threshold of the demonetization policy episode.

In equation (2), the parameter  $\eta_t$  also includes a dummy variable indicating the period after the demonetization policy was announce, to account directly for the impact of this policy on economic activity. Note that as the policy announcement was both unexpected and implemented at the aggregate level, the assignment variable – the district-specific time trend – could not have been manipulated. In other words, unobservables related to economic activity in districts could not have deliberately changed around the time cutoff in prior anticipation of the policy. Our identification assumption is also more flexible than a standard RDD which uses time as the assignment variable, but still exploits both cross-sectional and time variation through the timing of exogenous rainfall shocks.<sup>22</sup> In this sense, our strategy is closer to [Lalive \(2008\)](#) combining a difference-in-differences design with a regression discontinuity design. In other words, we obtain the difference between a district’s economic activity when hit by an exogenous district-specific rainfall shock by narrowly zooming in around the policy announcement date, conditioning on the impact of the policy itself on economic activity.

*Placebo test:* Another way of ensuring robustness to confounding factors is to construct a falsification exercise around the demonetization episode. To do so, we split the sample in two periods - a period just before demonetization (pre) and a period right after (post). We then artificially impute the post-demonetization district averages of mobile money use ( $\bar{M}_i^{Post}$  in the below specification) to the pre-sample.

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<sup>22</sup>In line with the fuzzy RDD approach, we can also estimate equation (2) using an instrumental variable strategy, by instrumenting the interaction term  $S_{it} \cdot M_{it}$  with the interaction term which identifies rainfall shocks in the post-demonetization period. In this set-up, we expect mobile money use to increase in districts hit with a rainfall shock in the post-demonetization period (satisfying the instrument’s informativeness criteria) and for the latter effect to be uncorrelated with unobservables related to a district’s time-varying risk-bearing capacity in the close time periods of pre and post demonetization (satisfying the instrument’s validity criteria). Our results are robust to implementing this strategy.

$$Y_{it}^{Pre} = \beta^{Pre} S_{it}^{Pre} + \delta^{Pre} S_{it}^{Pre} \cdot \bar{M}_i^{Post} + \chi^{Pre} \mathbf{X}_{it}^{Pre} + \alpha_i^{Pre} + \eta_t^{Pre} + e_{it}^{Pre} \quad (3)$$

If indeed, the districts that had intensive mobile money use were special in other dimensions, then we would expect similar results in the pre-period, with  $\delta^{Pre} > 0$ , when mobile money use was in fact quite limited.

## B. Effects of Mobile Payments Technology on Firms

In this section we discuss our empirical strategy which examines whether the adoption of the mobile QR code enable payment technology can improve firm sales. To do so, as described in the introductory section we make use of the sequencing of a targeted intervention that Paytm carried out to incentivize firms to adopt. We identify two sets of firms: firms who were targeted for adoption in January 2019 ('Treatment' firms) and firms targeted six-months later in July 2019 ('Control' firms). Both set of firms had not used the Paytm technology prior to the intervention. The treatment group firms would have had approximately six-months of experience accepting mobile payments, compared to the control group firms who, at the time of the survey, had little to no experience.

	Firms targeted for sign-up in:	
	January 2019 (6 months with Paytm)	July 2019 (No Paytm)
Sales 6 months ago	$S_0^P$ (No Paytm)	$S_0^C$ (No Paytm)
Sales current month	$S_1^P$ (Paytm)	$S_1^C$ (No Paytm)
6 months Sales Growth	$\Delta S^P$	$\Delta S^C$
<b>Treatment Effect</b>	$\Delta S^P - \Delta S^C$	

This setup allows us to use a difference-in-difference identification strategy to test for the six-month relative sales differential between treatment and control group firms:

$$\text{Sales}_{it} = \alpha \cdot \text{Treat}_i + \gamma \cdot \text{Post}_t + \beta \cdot \text{Treat}_i \cdot \text{Post}_t + e_{it} \quad (4)$$

where  $\text{Sales}_{it}$  is the sales of the firm  $i$  at time  $t$ . The time  $t$  refers to either the pre-treatment period where both sets of firms had no experience using the mobile payments technology, or the

post-treatment period where the treatment group of firms had six-months of experience using the technology. Correspondingly the variable  $Post_t$  identifies the post-treatment period and the variable  $Treat_i$  identifies the treatment group of firms. The relative sales growth differential is given by the interaction term  $Treat_i \cdot Post_t$ .

As the intervention campaign only incentivized firms to adopt, with the decision to adopt ultimately taken by the firms themselves, the effects identified by equation (4) represent intent to treat effects (see for e.g., [Crépon and others \(2013\)](#)). The average treatment effects can be recovered by using the intervention as an instrument for actual firm adoption; equation (4) can then be estimated using two-stage least squares.

**Identifying Assumption:** The difference-in-difference specification relies on the common trends assumption i.e., we should expect no sales growth differential between the two set of firms, absent the intervention and adoption of the payments technology. To validate this assumption we proceed in two steps. First, we recast equation (4) by adding fixed effects for firms' location ( $\tau_i$ ) and business-type ( $b_i$ ):

$$Sales_{it} = \alpha \cdot Treat_i + \gamma \cdot Post_t + \beta \cdot Treat_i \cdot Post_t + \tau_i + b_i + e_{it} \quad (5)$$

We then rely on a conditional common trends assumption and expect that both sets of firms would have followed a common sales trajectory, conditional on being in the same location and similar business groupings, in the absence of adopting the payment technology. In the second step we use the subset of both treatment and control group firms that never-adopt the technology despite being targeted in the intervention and check for their relative sales growth differential. As these firms have absolutely no experience with mobile payments, we should expect to see no difference in their sales between the pre and post period.

Finally, in addition to examining mobile money effects on firm sales, we also analyse whether it can have an impact on how firm's form expectation about future sales. For this we use the measure of subjective probabilities elicited in the survey, as described in Section II. We then fit a bi-triangular distribution on each firm's subjective sales probability and derive the first two moments of their future sales distribution. The first moment provides the firm's forecast of future sales, which we use with information on current sale to compute the impact of mobile money on expected sales growth, using a similar specification as equation (5). We also use the second moment to explore whether treatment group firms have lower subjective uncertainty around future sales, after six-months of adopting the mobile payments technology.

## IV. RESULTS

### A. Who Uses Mobile Money?

Before presenting the results on the impact of mobile money on aggregate economic activity and firm outcomes, we first explore the determinants of mobile money adoption across

districts, leveraging the data on users and firms across the entire economy. We had shown previously in Figure 1 that the demonetization policy episode, which constrained the use of cash provided a major economy-wide impetus for the take-up of mobile money. This could partly reflect the appeal of mobile money to ease transaction costs at a time when cash-constraints were severely binding. We now examine what factors - demographic, socio-economic and financial - drive the variation in mobile-money adoption across district, post the demonetization policy episode.

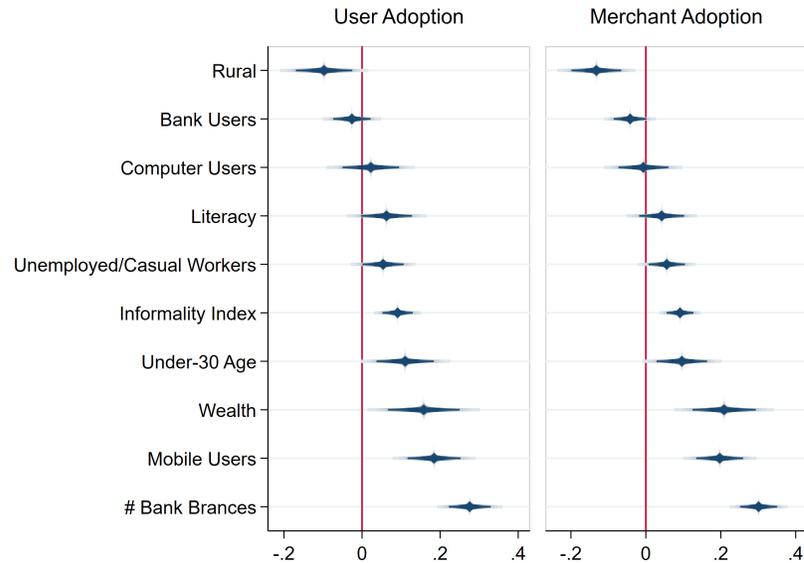
Figure 3 plots the standardized correlation coefficients for consumer and firm adoption at the end of 2018. The figure shows that the determinants of both consumer and firm adoption are largely similar. The largest correlate of adoption is bank availability with a one standard deviation increase in the number of bank branches associated with a 0.2 to 0.3 standard deviation increase in consumer and firm adoption respectively. This high correlation, between the bank availability and mobile money use, could be explained by the banking-led model of mobile money in India. In contrast to other countries (for e.g., Kenya) where mobile money users add money to their wallet through their telecommunication provider, in India mobile money wallets are largely linked to bank accounts<sup>23</sup>. As expected, we also find that the number of mobile users within a district strongly predicts adoption<sup>24</sup>. We find that mobile take up is relatively larger in districts with a younger population, with higher wealth and in urban areas.

Yet, conditional on the factors described above, we also find that adoption is higher in areas with a larger proportion of unemployed and casual workers, suggesting an appeal amongst the unbanked population. Another similarly important finding, especially in the Indian context where the degree of informal sector activity is high, is the positive correlation of mobile money adoption and a district's level of firm informality. As described in section II we construct the informality measure, directly based on the census of all firms domiciled within a district. We find evidence that a one standard deviation increase in a district's informality increases the take-up of mobile money by 0.1 standard deviation.

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<sup>23</sup>However, a person without a bank account can still add money to their mobile wallet through peer-to-peer transfers. It should be noted that since 2017, per the Reserve Bank of India guidelines, Paytm and other mobile operators are regulated as a Payments Bank. Such banks are primarily deposit-based and cannot issue loans and credit cards.

<sup>24</sup>The strength of this variable's correlation could be stronger than estimated, as we use data from the 2011 census to predict adoption rates in 2018. Given that the spread of mobile phones has increased significantly between the time periods, we expect there to be a larger correlation over time.

**Figure 3. Determinants of Mobile Money Adoption**

This figure plots the standardized effects from a regression exploring the determinants of user and merchant/firm adoption. Data Source for the explanatory variables are: 1) Census 2011: Rural; Computer Users; Literacy; Unemployed/Casual Workers; Under-30 Age; Mobile Users; 2) DHS Household Survey: Bank Users; Wealth; 3) Economic Census 2013: Informality Index; 4) RBI: # Bank Branches

## B. Effects of Mobile Money on Risk-Sharing

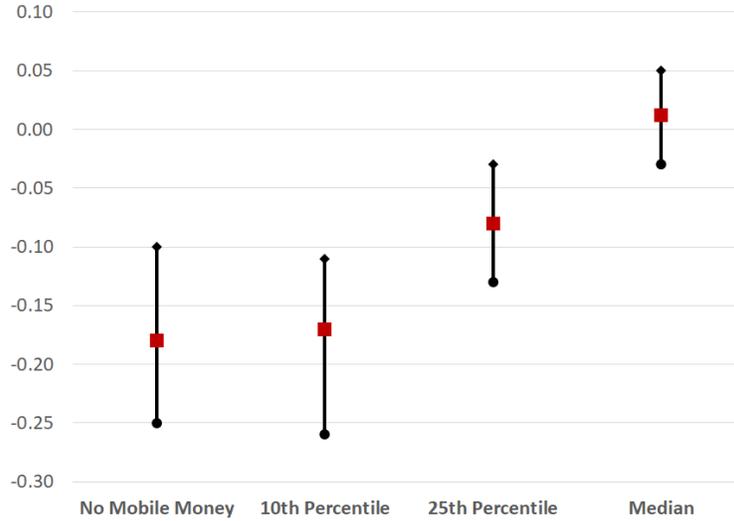
Table 1 presents results from our baseline risk-sharing specification, equation (1). The table shows that a rainfall shock has a significant negative impact on economic activity – reducing it by 17 percent on average. However, this effect is partially mitigated in districts with mobile money, depending on the intensity of use. Column (1) shows that the coefficient on the interaction term of mobile money use and rainfall shock is positive and significant. A 10 percent increase in mobile money use in districts hit by a rainfall shock reduces the negative effect of the shock by 3 percent. Column (2) conditions on the (log of) number of banks available in a district at time  $t$ , while Column (3) includes additionally fixed effects for each date (month-by-year) in the sample period; the results are robust to the inclusion of different types of controls.

**Table 1. Effect of Mobile Money on Risk Sharing: Economic Activity**

	(1)	(2)	(3)
Rainfall Shock	-0.169*** (0.025)	-0.169*** (0.026)	-0.184*** (0.027)
Mobile Money	0.014*** (0.002)	0.017*** (0.003)	-0.006 (0.008)
Mobile Money × Rainfall Shock	0.030*** (0.004)	0.030*** (0.004)	0.031*** (0.004)
Bank Availability		-0.363 (0.282)	-0.497 (0.318)
Time Fixed Effects			X
Observations	18827	18602	18602
r2	0.011	0.012	0.071

The table reports the average effects of rainfall shocks and mobile money adoption on economic activity. Mobile money adoption is measured as the total log value of Paytm users in the district; the dependent variable for all specifications is the demeaned log of the sum of night-time lights within each district. Standard errors clustered at the district are reported in parentheses. \* indicates significance at 10%; \*\* at 5%; \*\*\* at 1%.

Figure 4 shows how the risk-sharing effects vary by the intensity of mobile money use. For instance, a district with lower tenth percentile value of transactions, can reduce the negative effects of rainfall shock from 18 percent to 16 percent and this shock mitigating effect varies depending on the level of use. A district with the median value of transactions, on the other hand, can reduce the negative effects of rainfall shock from 18 percent to 1 percent.

**Figure 4. Risk Mitigation: Effect of Rainfall Shock by Intensity of Mobile Use**

This figure plots the predicted marginal effects of rainfall shock at different cross-sectional percentile levels of a district's mobile money adoption (10th, 25th and 50th percentile together with no mobile adoption). The effects with their 95 percent confidence bands are reported in the graph.

As raised in the discussion in section III.A, there is still a possibility that mobile money is endogenously adopted in districts that have good risk-sharing arrangements already in place such that all the results in Table 1 simply reflect this. To ensure that our results are fully robust to this source of endogeneity, we conduct the additional falsification exercise exploiting the period around demonetization. We split the sample into two – the period just before demonetization where Paytm was absent in most districts and the period after, when it expanded. We then artificially impute the post-demonetization district averages of mobile usage to the pre-sample and the idea is that, if indeed the districts that use mobile-money was special in other dimensions, then we would expect the same results to hold-out in the period just before demonetization, when in fact there was no mobile-money use. The results in Table 2, shows that this is not the case, and in our placebo period we find the risk-sharing interactive term to not just be insignificant but also reduced considerably in magnitude.

**Table 2. Effect of Mobile Money on Risk Sharing: Placebo Test & Robustness**

	Placebo		RDD	
	Pre (1)	Post (2)	(3)	(4)
Rainfall Shock	-0.209*** (0.078)	-0.146*** (0.050)	-0.204*** (0.025)	-0.116** (0.056)
Mobile Money $\times$ Rainfall Shock	0.008 (0.011)	0.025*** (0.006)	0.032*** (0.004)	0.026*** (0.005)
Time Fixed Effects	X	X	X	X
District-wise Time Trend Polynomial			X	X
Control for Post-Demonetization Period			X	X
Bank Availability & Shock Interaction				X
Observations	3626	14976	7239	7239
r2	0.043	0.070	0.366	0.366

The table reports the average effects of rainfall shocks and mobile money adoption on economic activity. Mobile money adoption is measured as the total log value of Paytm users in the district; the dependent variable for all specifications is the demeaned log of the sum of night-time lights within each district. The *pre* period relates to the sample between May and October 2016, while the post sample is for the six month period following the demonetization policy at the beginning of November 2016. *RDD* refers to results from a specification with flexible district-wise polynomial time trends between the period May 2016 and April 2017. Standard errors clustered at the district are reported in parentheses. \* indicates significance at 10%; \*\* at 5%; \*\*\* at 1%.

As an additional robustness exercise, in column (3) of Table 2 we also use an alternative identification strategy exploiting the time period over the demonetization policy episode. The regression discontinuity (RDD) specification used in column (3) fits a district specific polynomial trend such that any unobservables (conditional on the aggregate post-demonetization effect) related to risk-sharing are accounted for, to the extent that they do not change discontinuously around the threshold. The sample used for estimating this specification also narrows the time-window to six months before and after the policy episode. The results show that the effect of mobile money in reducing the extent of rainfall shock still holds, both in magnitude and direction. In column (4) we also include the interaction of bank availability with rainfall shock, to mitigate the concern that mobile money adoption may be picking up risk-sharing effects of banks, but the results remain robust to this inclusion.

Next, we relate our findings to the risk-sharing channel by using data on peer to peer transfers, specially across-district transfers. In Table 3 we report additional results from the regression discontinuity specification using peer-to-peer transfers to represent mobile money intensity. The first column shows the same result (mobile money based on total users) from Column (3) of Table 2. In column (2), using data on all peer-to-peer transfers received by a district, we find a negative effect of the rainfall shock, which is partially mitigated by districts that receive higher peer to peer transfers through mobile money. In column (3), we isolate only those peer-to-peer transfers received by a district from other districts, removing therefore within-district

transfers<sup>25</sup>, and find a similar pattern of results to hold. This suggests that remittance sent from other districts may help smooth shocks experienced in the target district.

There is some evidence that the magnitude of the risk-sharing effect from examining data on peer to peer transfers is reduced, almost by half, suggesting that other channels such as mobile wallet savings may also partially account for the total risk-sharing effect found previously (and reported again in Column (1)). We also test for possible channels through which the effects may manifest and find evidence that across-district money transfers increase following a rainfall shock lending further support to the explanation around the remittance channel.

**Table 3. Effect of Mobile Money on Risk Sharing: P2P Transfers**

	Users	P2P	Cross-District P2P
	(1)	(2)	(3)
Rainfall Shock	-0.204*** (0.025)	-0.283*** (0.048)	-0.237*** (0.043)
Mobile Money $\times$ Rainfall Shock	0.032*** (0.004)	0.017*** (0.004)	0.014*** (0.005)
Time Fixed Effects	X	X	X
Control for Post-Demonetization Period	X	X	X
District-wise Cubic Time Trend	X	X	X
Observations	7239	7239	7239
r2	0.368	0.366	0.366

The table reports the average effects of rainfall shocks and mobile money adoption and use on economic activity. Mobile money adoption is measured as the total log value of Paytm users in the district (column (1)); mobile money use is measured as the total log value of Paytm peer to peer transactions in the district (column (2)) and the total log value of across-district Paytm peer to peer transactions in the district (column (3)). The dependent variable for all specifications is the demeaned log of the sum of night-time lights within each district. Standard errors clustered at the district are reported in parentheses. \* indicates significance at 10%; \*\* at 5%; \*\*\* at 1%.

Finally, we confirm that our findings hold on household measures of consumption, as is typically examined in the risk-sharing literature. We use data from the recent labor force survey, conducted between 2017-2018 and link quarter-wise average per-capita consumption of households in different regions of India to its rainfall shock and mobile money use.<sup>26</sup> We find strikingly that, as before, the negative effect of a rainfall shock is mitigated by the region's intensity of mobile money use but these effects are concentrated around effects on

<sup>25</sup>On average, the cross-district peer-to-peer transfer received by a district is 18.6 percent of the total volume and 30.4 percent of the total value for our sample period of 2016 May - 2019 July.

<sup>26</sup>A region is at a higher level of aggregation compared to a district. The average number of districts within a given region is 7.3.

rural households (Column (2)). In fact, column (3) shows that neither rainfall shocks, nor its interaction with mobile money use have any significant effects on urban consumption. These results altogether suggest that the risk-sharing effects from mobile money use are particularly strong in rural areas that have low access to banks and where there are frictions on enabling effective risk-sharing arrangements.

**Table 4. Effect of Mobile Money on Risk Sharing: Labor Supply and Consumption**

	HH Per-Capita Consumption		
	Average (1)	Rural (2)	Urban (3)
Rainfall Shock	-0.090*** (0.032)	-0.095* (0.051)	-0.033 (0.036)
Mobile Money	-0.025* (0.014)	-0.025 (0.018)	-0.023 (0.015)
Mobile Money $\times$ Rainfall Shock	0.009*** (0.003)	0.011** (0.004)	0.003 (0.003)
Bank Availability	0.072 (0.045)	-0.098 (0.063)	0.098** (0.045)
Time Fixed Effects	X	X	X
Observations	325	325	325
r <sup>2</sup>	0.029	0.025	0.015

The table reports the average effects of rainfall shocks and mobile money adoption on household consumption. Mobile money adoption is measured as the total log value of Paytm users in the district. The dependent variable for column (1) is average household per-capita consumption within a region; for column (2) it is rural household per-capita consumption within a region; for column (3) it is urban household per-capita consumption within a region. Standard errors clustered at the district are reported in parentheses. \* indicates significance at 10%; \*\* at 5%; \*\*\* at 1%.

As Table 4 is based on a survey which covers the first two quarters of 2018, it could be noted that the risk-sharing effect last longer than the period during the demonetization policy which severely restricted cash usage. We also estimate our baseline results reported in Table 1, restricting it to the last quarter of 2018, and find that while the mobile money continues to dampen the negative effect of rainfall shocks, its risk-mitigation effect reduces. In this sample period, where the demonetization policy impact arguably dissipated, we still find that a 10 percent increase in mobile money use in districts hit by a rainfall shock reduces the negative effect of the shock by 3.8 percent. Overall, this suggests that our effects are not restricted to the specificity of an environment with binding cash constraints.

### C. Effects of Mobile Money on Firm Sales

In this section we discuss results on whether the adoption of the mobile QR code enable payment technology can improve firm sales. As described in section II.B we make use of the sequencing of a targeted intervention that Paytm carried out to incentivize firms to adopt. We identify two sets of firms: firms who were targeted for adoption in January 2019 ('Treatment' firms) and firms targeted six-months later in July 2019 ('Control' firms). Both set of firms had not used the Paytm technology prior to the intervention. The treatment group firms would have had approximately six-months of experience accepting mobile payments, compared to the control group firms who, at the time of the survey, had little to no experience.

Before presenting the results we test for differences in observable characteristics between treatment and control group firms before the intervention. Table 5 reports the mean value for a set of baseline covariates for treatment and control firms as well as the difference in means with its statistical significance. We find no statistically significant difference in firm characteristics, confirming that the pre-intervention covariates are balanced between the two set of firms<sup>27</sup>. Both treatment and control firms have on average 3-4 employees with an unbanked proportion of 30 percent, highlighting that our results essentially reflect the impact micro and small enterprises that have limited access to formal banking services. These firms are also relatively young, with an average age of 6-7 years and experience business losses equivalent to approximately 20 percent of baseline sales.

**Table 5. Balance of Firm Covariates**

	Control	Treatment	Difference	P-value of Difference
# Employees	4.92	3.05	1.86	0.30
Bank Account	0.72	0.70	0.02	0.48
Firm Age	6.72	6.28	0.43	0.38
Business Loss (thousands of Rs.)	22.96	19.31	3.27	0.71

The table reports the mean of baseline covariates for 558 control (no mobile technology) and 367 treatment (mobile technology) group firms. The p-value of the difference is reported in the last column with \* indicating significance at 10%; \*\* at 5%; \*\*\* at 1%.

Having ensured that the absence of statistically significant baseline differences between treatment and control group firms, we now present in Table 6, results on the impact of mobile

<sup>27</sup>It should be noted that even though the differences are statistically insignificant, given the sample size (925 firms), the t-tests could be underpowered to detect effects. As some of the differences (number of employees and business loss) are not very small, the results should be interpreted with caution. Even then, under the difference in difference strategy we can still allow small baseline differences to persist and rely mainly on common trends between the two groups to hold. We therefore provide additional robustness checks to verify our identification assumptions.

payment technology on firm sales. Column (1) shows that firms using Paytm technology improve their sales, by approximately 33 percent relative to firms not exposed to this technology, after six months of use. The addition of covariates (Column (2)), location fixed effects (Column (3)) and business-type fixed effects (Column (4)) marginally reduce the effect. However, our most robust specification in Column (5) includes location by business-type fixed effects and finds that the six months sales growth differential, between treatment and control firms, is 28 percent.

**Table 6. Effect of Payment Technology on Firm Sales: Intent to Treat Effects  
(Difference in Difference Specification)**

	Dep Var: Change in Sales (log)				
	(1)	(2)	(3)	(4)	(5)
Mobile Technology Firms (6 month sales growth differential)	0.331*** (0.028)	0.308*** (0.030)	0.234*** (0.053)	0.226*** (0.063)	0.276*** (0.081)
Covariates		X	X	X	X
Location Fixed Effects			X	X	
Business Type Fixed Effects				X	
Buisness Type by Location Fixed Effect					X
Observations	804	791	791	791	791
r2	0.035	0.050	0.211	0.232	0.485

The table reports the intent-to-treat effects of mobile payment technology on firm sales. The dependent variable in all specification is the change in firms' six-month sales. Standard errors clustered at the firm and treatment level are reported in parentheses. \* indicates significance at 10%; \*\* at 5%; \*\*\* at 1%.

A comparison of results in Column (1)-(2) with the other Columns (3)-(5), which include a rich set of location and business type dummies, also provide an insight into the net effects of mobile technology i.e, net of displacement effects. In the presence of externalities, treatment firms would benefit from the adoption of mobile payments technology but partially at the cost of displacing the sales of untreated firms. This would mean that the net effects of the technology on the overall population of firms could be lower than the baseline treatment effects reported in Table 6. We are unable to formally estimate the magnitude of these displacement effects as we lack data on the census of firms at each location (and by business type) but provide a simple test to gauge its importance. We take advantage of the variation in the proportion of treated firms at each location<sup>28</sup> and/or business type to conduct this test. As

<sup>28</sup>For instance, the average proportion of treated firm in each location is 25 percent with a standard deviation of 38 percent.

pointed out by Crépon and others (2012), in the absence of externalities, we would expect to find the effects estimated from specifications with location dummies (Columns (3)-(5)) to be greater than estimated effects obtained without their inclusion (Columns (1)-(2)). This is because the difference between firm within each location and/or business-type would be larger than the difference between firms in location with varying degrees of treatment. In fact, we find that the coefficients in all columns of Table 6 are broadly similar, with results including location/business-type dummies to be slightly lower even, suggesting that the presence and magnitude of displacement effects is small and that the net effect of mobile technology on firms is positive.

We also test the effect of using Paytm technology using an alternative identification strategy that involves matching firms on an extensive set of covariates (including location and business type dummies). The propensity matching method identifies treatment and control firms with similar probabilities (or propensity scores) of being selected in a treatment. In contrast to including the covariates and fixed effects as control variables, this method matches treated and control units according to their propensity score. In terms of estimation, the propensity score is used to calculate kernel weights following Heckman, Ichimura, and Todd (1997, 1998) such that the weights reflect a higher probability of being selected and serve as better matches. The outcomes of the treated firms are therefore compared to the weighted outcomes of control firms. is matched to the whole sample of control units instead of on a limited number of nearest neighbors.

Table 7, report results based on match by covariates, business type (Column (1)), location (Column (2)) and business type by location (Column (3)). These results are largely similar to the ones reported in Table 6 with Paytm using firms seeing a relative improvement in sales of 28 to 30 percent.

**Table 7. Effect of Payment Technology on Firm Sales: Intent to Treat Effects  
(Propensity Score Difference in Difference Specification)**

	Dep Var: Sales (log)		
	(1)	(2)	(3)
Mobile Technology $\times$ 6 months Post (6 month sales growth differential)	0.312*** (0.077)	0.257*** (0.095)	0.282** (0.118)
<i>Matching on:</i>			
Covariates	X	X	X
Location		X	
Business Type	X	X	
Buisness Type by Location			X
Observations	1172	923	738

The table reports the intent-to-treat effects effects of mobile payment technology on firm sales. The dependent variable in all specification is the change in firms' six-month sales. Standard errors clustered at the firm and treatment level are reported in parentheses. \* indicates significance at 10%; \*\* at 5%; \*\*\* at 1%.

More generally, the difference-in-difference specification, underlying both our identifying specifications, requires that the two groups of firms face a common sales trajectory in the absence of intervention. Unfortunately the survey did not collect information on sales in the months before the treatment group intervention took place. However, since the intervention only incentivized firms to adopt, and did not enforce the adoption decision, we are able to identify a subset of firms that never-adopted the technology. Table 8 shows the relative sales growth differential between the treatment and control never adopters. As these firms have absolutely no experience with mobile payments, we should expect to see no difference in their sales between the pre- and post- period and the results in Table 8 confirm this.

**Table 8. Common Trends: Evidence from Never Adopters  
(Propensity Score Difference in Difference Specification)**

	Sales (log)
Mobile Technology $\times$ 6 months Post (6 month sales growth differential)	0.131 (0.180)
<i>Matching on:</i>	
Covariates, Location & Business Type	X
Observations	106

The table reports the intent-to-treat effects of mobile payment technology on firm sales for the sample of firms that do not adopt this technology even after intervention. The dependent variable in all specification is the change in firms' six-month sales. Standard errors clustered at the firm and treatment level are reported in parentheses. \* indicates significance at 10%; \*\* at 5%; \*\*\* at 1%.

One other feature to firms from accepting mobile payments, is that they are offered insurance coverage at competitive rates to a certain amount in the event of a business loss; this feature can contribute to reducing firms' uncertainty and improve their optimism around future sales. We explore this by eliciting subjective expectations -- asking about the support and probability mass of each firm's future sales distribution to derive its first two moments. Table 9 reports these results and we find again that Paytm using firms are more optimistic and less uncertain about future sales which is partially corroborated by the fact that these firms also have a higher likelihood to take a loan, as documented in previous literature (Handley and Li, 2018; Tanaka and others, 2019).

**Table 9. Effect of Payment Technology on Firm Sales Expectations: Intent to Treat Effects (Difference in Difference Specification)**

	Future sales growth (growth rate)	Uncertainty (log s.d. future sales)	Bank loan (Yes/No)
	(1)	(2)	(3)
Mobile Technology Firms	-0.120*** (0.036)	-0.226** (0.102)	0.190** (0.096)
Covariates	X	X	X
Location	X	X	X
Business Type	X	X	X
Observations	712	699	603

The table reports the intent-to-treat effects effects of mobile payment technology on firm sales. The dependent variable in column (1) is the change in firms' current and expected sales (first moment of their subjective expectations distribution); in column (2) it is the log of firms' subjective uncertainty (second moment of their subjective expectations distribution); in column (3) it is a dummy for whether the firm borrows from a bank within the six-month treatment period. Standard errors clustered at the firm and treatment level are reported in parentheses. \* indicates significance at 10%; \*\* at 5%; \*\*\* at 1%.

Finally we examine the average treatment effects; as the intervention campaign only incentivized firms to adopt, with the decision to adopt ultimately taken by the firms themselves, the effects reported in Table 6 represent intent to treat effects. These estimates do not take into account the *intensity of Paytm use* which represents on average 5 percent of treatment firms' total sales (and ranging from 0 to 71 percent). The average treatment effects can be recovered by using the intervention as an instrument for actual firm adoption with equation (4) estimated using two-stage least squares.

Table 10 reports these results and we find again that increasing the intensity of mobile money use by 1 percent (i.e., higher Paytm sales as a percentage of total sales) are associated with a 4.2 percent sales growth differential over six-months.

**Table 10. Effect of Payment Technology on Firm Sales: Average Treatment Effects (Difference in Difference Specification)**

	Dep Var: Change in Sales (log)		
	(1)	(2)	(3)
Mobile Technology Use (% of sales) (6 month sales growth differential)	0.041*** (0.009)	0.040*** (0.011)	0.043*** (0.014)
Covariates	X	X	X
Location	X	X	
Business Type		X	
Business Type by Location Fixed Effect			X
First-Stage F-Statistic	48.6	46.9	38.9
Observations	780	780	780
r <sup>2</sup>	0.039	0.067	0.337

The table reports the average treatment effects of mobile payment technology on firm sales. The dependent variable in all specification is the change in firms' six-month sales. All columns report the second stage estimates from an instrumental variable regression using the treatment dummy as an instrument for the proportion of sales obtained through mobile payment technology. Standard errors clustered at the firm and treatment level are reported in parentheses. \* indicates significance at 10%; \*\* at 5%; \*\*\* at 1%.

## V. CONCLUSION

This paper provides novel evidence on the economic impact from the two distinct use cases of mobile money, using large-scale data on monthly mobile money transactions and a bespoke firm survey around a targeted intervention. First, does it improve the resilience to economic shocks by enabling a cheaper and more efficient way to save and transfer money? Second, can the adoption of this relatively costless payment technology help increase the sales of micro and small enterprises by reducing the frictions and costs associated with other payment methods.

We find that mobile money, serves as a powerful mechanism to improve the efficiency of risk-sharing arrangements. Our evidence shows that an economically meaningful effect of mobile money use in reducing the negative impact on economic activity. We also analyse the impact of mobile based payments technology on firm sales, by taking advantage of a phased targeting intervention that incentivized firms to adopt the technology. Our results show that firms adopting the novel payment technology improve their sales, by approximately 26 percent relative to non-adopting firms. Our results are overall robust to different methods of identification and placebo tests for validating assumptions.

While our findings provide insights into the benefits to (mainly rural) households and informal sector firms from the use of remittances and payments, there are still several open questions regarding the sustainability of the impact and its use-cases. Our evidence is situ-

ated in an environment where cash dependence and informality is high and it is yet unclear whether these impacts will continue to hold in the longer run where such conditions could ease, especially with increased regulatory focus, and as the competition for mobile money provides picks up. Similarly, in terms of its use-cases [Suri \(2017\)](#) finds that while they remain limited to payments and peer-to-peer transactions, several innovations are underway which build on existing mobile money innovations, and that may potentially increase the benefits from its use. Research is already growing on the broader impact of fintech on alleviating credit frictions ([Bharadwaj, Jack, and Suri, 2019](#); [Hau and others, 2018](#)), in improving financial decision making ([Carlin, Olafsson, and Pagel, 2019](#)) and in reducing barriers to financial intermediation ([Berg and others, 2018](#); [Philippon, 2019](#)).

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