Cash Is King: The Role of Financial Infrastructure in Digital Adoption

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Abstract

This paper examines whether a one-time, extensive, but temporary shock to cash supply can affect the adoption of digital payments. We exploit the 2016 demonetization episode in India, which overnight discontinued 86% of cash in circulation. Using novel administrative data from retail debit card transactions, we identify a 12% increase in digital payments in areas adversely affected by the cash shortage, which persisted well after the restoration of cash supply. Examining mechanisms, we find a limited role for social networks and stronger support for learning by doing. Further, information frictions hinder the immediate adoption of digital payments (*JEL* E5, 023).

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The past decade has witnessed a massive increase in the use of digital transactions through electronic cards and mobile payments. This has been led by developing economies, where the share of adults transacting using digital payments grew from 35% to 57% between 2014 and 2021 (Demirgüç-Kunt et al. 2021). As opposed to cash, digital payments offer unique advantages in the form of increased security, lower transaction, and monitoring costs, and can foster trust in financial institutions (Galiani, Gertler, and Navajas-Ahumada 2022). As digital transactions are often linked to bank accounts, they facilitate formalization and financial inclusion. Seamless transfer of funds across space can also aid risk-sharing when insurance markets are incomplete (Jack and Suri 2014). Conversely, the ease of monitoring digital transactions can hinder adoption through higher compliance costs and tax burdens.

Causally identifying factors facilitating the adoption of digital technology, however, is fraught with challenges as agents select into adoption (Galiani, Gertler, and Navajas-Ahumada 2022). Moreover, inadequate financial infrastructure, information frictions, low financial literacy, and limited trust in financial institutions can deter adoption (Gupta, Ponticelli, and Tesei 2020). This paper bears evidence to this question by studying whether an extensive, unanticipated, but temporary shortage in cash availability can induce households to switch to digital payments.

We study the "demonetization" episode, namely, the sudden decision by India's federal government in November 2016 to discontinue overnight two of the largest currency denominations accounting for 86% of the cash in circulation. The policy directed citizens to deposit the discontinued currency into bank accounts and gradually replaced them with new denominations. Severe restrictions were placed on cash withdrawals from banks and ATM terminals, which were compounded by operational constraints on the central bank's printing presses. This contributed to an adverse reduction in cash supply that was most severe until April 2017 (Chodorow-Reich et al. 2020). As electronic and digital payments remained unaffected, we examine whether an extensive shock to cash supply can induce digitization.

India serves as an ideal context to study this question. Notwithstanding the wide network of commercial banks, government policies promoting financial inclusion, and a high degree of trust in financial institutions, the economy continues to have high cash dependence (Rogoff 2015).[1] The ubiquitous use of cash, combined with the exceptional secrecy surrounding demonetization, implied that while almost all households were affected by the shock, no household could have anticipated it. This makes the intervention akin to an exogenous economy-wide shock to cash supply, which we exploit for causally identifying the adoption of card-based digital payments.

Despite the recent surge in mobile payments — particularly accelerated during the COVID-19 pandemic — transactions using credit and debit cards continue to form a key component of digital payments. As reported by Demirgüç-Kunt et al. (2021), with the exception of select economies, such as China, Kenya, and Côte d'Ivoire, the majority of individuals used a combination of electronic cards and mobile payments to conduct digital transactions. Moreover, even in advanced economies, such as the United States, perceived security risks regarding mobile-based payments have resulted in electronic cards being the primary mode of digital transactions (Pew Trust 2019).[2] In the context of India, digital transactions using credit and debit cards at PoS terminals equaled 10% of gross domestic product (GDP) for 2021-2022, making the adoption of electronic cards and frictions deterring such adoption a relevant question of the study.

We use novel administrative data at the level of pincodes to examine the adoption of digital payments in response to an unexpected (but temporary) reduction in cash availability. Digital payments are observed through point-of-sale (PoS) transactions undertaken using debit cards issued by the major national payment vendor RuPay.[3] For

[1] The Indian Human Development Survey (IHDS) in 2011-12 reported that over 80% of households reported confidence in commercial banks to keep their money safe. This is possibly linked to the predominance of government-owned banks in the banking system, which comes with a sovereign guarantee. The All India Debt and Investment Survey (AIDIS) of 2013 also reported over three-fourths of Indian households to have access to a bank account. In August 2014, the federal government launched a massive financial inclusion drive, aiming to provide each adult with a bank account.

[2] For instance, Pew Trust (2019) surveyed households in the United States where only 22% of respondents considered mobile payments to be well-protected and 38% considered them to be poorly protected. The corresponding figures for debit (credit) cards were 43% (22%).

[3] RuPay is an indigenous digital platform promoted by India's central bank in 2012 with the goal of expanding access to electronic payment systems to rural and low-income households. This makes it particularly suited for studying digital financial inclusion.

causal identification, we compare digital payments across pincodes located in the same geographical region and quarter, with the identifying variation stemming from pincodes' exposure to the cash supply shock as a function of their physical distance from repositories of cash.

Similar to Chodorow-Reich et al. (2020), our empirical strategy exploits the "hub and spoke" model for currency distribution,[4] whereby India's central bank – the Reserve Bank of India (RBI) – distributes currency to 4,000 "currency chests" (CC), which in turn transmit cash to bank branches and ATM terminals.[5] Under the assumption that transportation costs and logistical challenges in cash disbursement are an increasing function of distance (and such challenges were exacerbated post-demonetization), we would expect faster cash replenishment to pincodes located near currency chests (control group) relative to those farther away (treatment group). This generates cross-sectional variation in treatment intensity, which we apply in a differences-in-difference framework.

We empirically confirm the validity of our distance-based measure of exposure to the cash shortage by examining the treatment's impact on cash withdrawals from ATM terminals. We find that a 1-kilometre (km) increase in a pincode's distance to the nearest CC reduced average monthly ATM cash withdrawals by 0.4 % in the first 3 quarters after demonetization, which dissipated over the long term (6 quarters post demonetization) as the cash supply recovered to pretreatment levels. This lends support to our hypothesis that the cash shortage was more acute in pincodes farther from CCs.

We find that this adverse (but temporary) reduction in cash supply generated a positive and persistent impact on digital payments: a 1-km increase in pincodes' distance from CCs increased average monthly digital payments using RuPay debit cards by 0.6 % in the first 3 quarters after demonetization, increasing to 1.3 % over a 6-quarter period. Areas facing higher cash shortage thus witnessed higher adoption of digital payments, which also persisted beyond the disruption to cash supply.

[4]In contemporaneous work, Crouzet, Gupta, and Mezannotti (2023), Das et al. (2022), and Ghosh, Vallée, and Zeng (2021) also use a similar empirical strategy of exposure to currency chests to study the impact of demonetization on alternative outcomes.

[5]While the RBI directly operates the printing presses, the currency chests are managed by individual bank branches, operating under a broad agreement with the central bank.

Our empirical strategy includes pincode and district-time[6] fixed effects, in addition to predemonetization pincode covariates, such as distance to urban centers, availability of financial infrastructure, and proxies for local economic activity. We use an event-study design to verify the absence of differential pretreatment trends in cash withdrawals and digital payments. The event study establishes a sharp decrease in ATM cash withdrawals in the quarter of demonetization, accompanied by a corresponding (and persistent) increase in digital payments which, however, begins in the subsequent quarter. Importantly, we show that conditional on pincode observables, pincodes' distance from CCs do not predict pre-treatment digital payments (cash withdrawals). Thus, a pincode's distance to repositories of cash only affected digital payments (cash withdrawals) in the post-treatment period, assuaging concerns that the distance metric might be proxying for unobservables correlated with the adoption of digital payments. We also demonstrate robustness to a permutation-based placebo test whereby pincode-CC distances are randomly assigned.

Higgins (2019) describes digital payments as a two-sided market which requires retailers to have access to PoS terminals and consumers to have debit cards. Likewise, Suri (2017) considers the absence of financial infrastructure and financial literacy to constrain economic agents from using digital technology. Consistent with the above hypotheses, we find that conditional on the intensity of cash shortage, digital payments increased only in areas with a relatively high pretreatment density of PoS terminals and bank branches.

Exploring mechanisms, we begin by examining treatment heterogeneity across regional characteristics likely to affect the adoption of digital payments. Conditional on the cash supply shock, we find local financial infrastructure and household awareness in terms of newspaper readership and television viewing to positively affect the adoption of digital payments over the near and the long term. We contend that limited information about policies facilitating the use of digital payments — for instance, a temporary waiver on PoS transaction fees announced right after demonetization — could have stymied the

[6]Comparable to the U.S. county, districts form the third administrative tier in India, after federal and state.

adoption of digital payments, even for agents with access to digital technology. Indeed, we find no lag in digital payments adoption in regions with relatively high ex ante financial infrastructure *and* household awareness, while regions with relatively high financial infrastructure but *low* household awareness saw a two-quarter lag in adoption. This echoes the findings of Gupta, Ponticelli, and Tesei (2020), who show information frictions to impede households' ability to realize the gains from technology adoption fully.

Next, we test whether consumers or retailers drove the adoption of digital payments. We first identify how the cash shortage affected households' adoption of digital technology. In the absence of data on debit cards, we gauge this from households' adoption of credit cards and show that exposure to the cash supply shock significantly increased households' likelihood of owning a credit card. As credit and debit cards exhibited comparable aggregate trends during this period, we contend that part of the increase in digital payments in response to the cash supply shock can be attributed to new adopters of digital technology. Event study plots show that the adoption of credit cards lasted only for two quarters and started in the quarter succeeding demonetization. This is likely because of the nontrivial transaction costs involved in the processing of applications for electronic cards by financial institutions. If new adopters subsequently conducted retail transactions using electronic cards, the time costs of adoption can serve as a plausible explanation for the temporary lag in digital payments in response to the cash shortage.

Importantly, we find the adoption of credit cards to be restricted to households with prior exposure to financial products, such as life insurance and retirement savings, pointing to the presence of learning by doing and a limited role for social learning (Breza, Kanz, and Klapper 2020). Finally, we identify no impact of demonetization on PoS terminal adoption by retailers, over either the near or the long term. This rules out that the increase in digital payments emanated from the targeted rollout of financial infrastructure to areas adversely affected by the cash shortage. Moreover, this indicates that the adoption of cards amongst households was insufficient to mandate higher adoption among retailers. Thus, while we find evidence supporting the adoption of electronic cards in response to the cash supply shock, muted adoption by retailers rules out widespread adoption across households, which could have generated the continual cycle of adoption found in Higgins (2019). This suggests that the increase in digital payments in response to the cash supply shock was primarily along the intensive margin by consumers who had prior access to financial infrastructure and necessary information about digital technology.

Finally, we offer evidence consistent with habit formation to explain the persistence of digital payments. If the increase in digital payments was driven by agents with prior access to financial infrastructure, continued usage even after the restoration of cash supply (with no other major intervention affecting digital payments) points to households gaining familiarity through the usage of cards during the period of cash shortage, leading to habit formation. This applies to both new adopters of digital technology and households who had access to it but were reluctant to use it prior to demonetization due to unfamiliarity or inertia. While our data does not track usage at the individual level, we find that exposure to the cash shortage led to higher digital transactions along the intensive margin, pointing to a higher number of digital technology users. A formal test of habit formation posited by Schaner (2018) implies that individuals adopting the technology over the short run continue to use it over the long run. Our results align with this hypothesis: conditional on the cash supply shock, the adoption of digital payments remained significantly higher in regions with higher awareness and financial infrastructure across both the near and long term.

Our paper contributes to a growing literature on the adoption of financial services across developing economies. While a number of papers have used field experiments to study the adoption of financial products, we exploit a natural experiment to study the adoption of digital payments. Our work is closely related to Higgins (2019) and Bachas et al. (2021), both of whom study the administrative rollout of debit cards in Mexico to show that it led to higher savings through increased trust in financial institutions and widespread adoption through spillovers between retailers and households. Contrary to these papers studying the large-scale rollout of debit cards, the demonetization episode only increased the costs of cash transactions, with no accompanying interventions to expand financial infrastructure for either households or retailers. We show that while a decline in cash availability does result in a persistent increase in digital payments, this is majorly along the intensive margin, with limited adoption of digital technology by households and no evidence of PoS terminal adoption by retailers.

Our paper also closely relates to Crouzet, Gupta, and Mezannotti (2023), who use the same policy intervention but studies the adoption of digital payments using mobile-based networks. The key distinction between our paper and Crouzet, Gupta, and Mezannotti (2023) is the mode of digital payments studied: electronic cards as opposed to digital payments. As noted by Crouzet, Gupta, and Mezannotti (2023), transaction costs to participate in mobile payments are minimal, but a threshold number of users are necessary for adoption. Digital payments using electronic cards, on the contrary, involve nontrivial adoption costs in the form of processing of applications by banks, time costs involved in dispatching cards post-approval, and fees for the adoption of PoS terminals. We argue that time and processing costs associated with card-based digital payments form a key friction which hinders households from immediately switching to this payment method in response to the cash shortage. The presence of substantial adoption costs also hinders technology adoption through social learning, as adoption costs remain constant, regardless of the number of digital technology users. Consequently, the pretreatment endowment of local financial infrastructure, as opposed to the existing network size, becomes the key facilitator to the adoption of digital technology using electronic cards.

In this respect, we consider our paper to lie at the intersection of Higgins (2019) and Crouzet, Gupta, and Mezannotti (2023). High fixed costs of adoption imply that limited entry into card-based digital payments along the extensive margin by consumers also stymies the adoption of PoS terminals by retailers. Thus, while we identify continual benefits from the one-time adoption of digital technology, overall adoption remains limited in the absence of a concerted push to financial infrastructure, such as the one studied in Higgins (2019).

As digitization is closely linked with formalization, our paper also relates to the extensive literature studying informality and development. A number of papers have studied incentives to aid the financial inclusion of households and the formalization of firms, primarily through lowering the costs of participation in the formal economy. In contrast, De Andrade, Bruhn, and Mckenzie (2014) show that increasing the costs of informality can be more effective in inducing formalization. We add to this literature by identifying that a temporary increase in the cost of cash transactions can increase participation in the formal economy through digital payments, but conditional on the availability of financial infrastructure. To this effect, our paper reconciles the two approaches to induce formalization: lowering the costs of formalization through improved access, while increasing the costs of informality.

Finally, we add to the growing literature studying the economic effects of demonetization (for a recent review, see Lahiri 2020) and its impact on digital payments. Our baseline results are consistent with the findings of Chodorow-Reich et al. (2020), although technology adoption is not the central focus of their paper.[7] Relatedly, Das et al. (2022) examine its effect on firms' tax compliance; Ghosh, Vallée, and Zeng (2021) study its impact on FinTech lending.

1 Background: Cash in India

This section documents the cash dependency of the Indian economy and describes the policy intervention of interest. We also discuss the logistics of currency circulation.

1.1 Cash and the Indian economy

India has one of the highest cash-GDP ratios (Internet Appendix Figure E1), surpassed only by Japan and Hong Kong (Rogoff 2016). We estimate that 95% of household consumption in the median district is undertaken using cash.[8] Internet Appendix Figures A1-A3 shows that areas with high cash dependency were negatively selected along ob-

[7] Agarwal et al. (2022) also study the impact of demonetization on digital payments but focuses on three large cities. In contrast, our paper exploits rich geographic heterogeneity across the entire economy, allowing us to precisely pinpoint factors contributing to (or hindering) the adoption of card-based digital technology.

[8] Internet Appendix A1 combines proprietary data on digital payments and district-level household consumption to estimate regional cash dependency.

servables, such as urbanization, informality, education, consumption, and financial infrastructure, indicating low shock-coping abilities.

1.2 The demonetization intervention

On the evening of November 8, 2016, India's Prime Minister made the shock announcement that the two largest currency denominations – INR 500 and INR 1,000 – accounting for 86% of the currency in circulation[9] would cease to remain official tender effective midnight, November 9, 2016. The remaining denominations were left unaffected, and the government undertook to replace the discontinued currency with a fresh set of INR 500 bills and introduced the INR 2,000 denomination.[10]

Citizens were directed to deposit the discontinued currency into bank accounts by December 31, 2016.[11] Anticipating shortages in cash supply, a weekly ceiling of INR 20,000 was placed on cash withdrawals from banks.[12] Citizens were eligible to exchange a limited quantum of old currency at any bank branch, while cash withdrawals from ATM terminals were capped at INR 2,000, to be raised later to INR 4,000.[13] No restrictions were imposed however on transactions undertaken through bank transfers, cheques, and digital payments undertaken through e-wallets or debit/credit cards.[14]

We exploit two key features of the "demonetization" intervention. First, only a handful of advisors associated with the Prime Minister had prior knowledge about the intervention, making it an exogenous event as agents neither anticipated the shock, nor could have planned their response in advance. Second, the discontinuation of 86 % of the currency in circulation, combined with printing press constraints and restrictions on

[9]The remaining denominations were INR 100, 50, 20, 10, 5, 2, and 1. For the sake of comparison, the average per capita monthly consumption of an urban household in 2011-2012 equalled INR 2,000.

[10]The disruption was justified as a step towards unearthing corruption and undisclosed wealth, noting the ubiquitous use of cash for transactions accruing to the "shadow" economy. A second reason offered was that cash was also used to fund terrorism (Business Standard 2017). Over time, additional rationales were provided by the government.

[11]The discontinued currency also could be deposited in savings accounts at post offices.

[12]The government mentioned that the ceiling would be revised upwards once the cash supply was restored.

[13]Initially, the daily ceiling for such transactions was INR 4,000, until November 24, 2016. Unlike bank transactions, no specific timeline was provided for when the ceiling on ATM cash withdrawals would be relaxed. ATM terminals also remained closed on November 9 and 10.

[14]E-wallets are akin to prepaid cards.

currency withdrawals, significantly increased the cost of cash transactions, but digital payments remained unhindered. This allows us to study whether the unanticipated increase in the cost of cash transactions pushed agents to transact using digital payments in lieu of cash.

Figure 1 summarizes the aggregate impact of the policy on cash supply and digital payments. The left panel shows monthly trends in currency in circulation and cash held by banks, while the right panel compares cash withdrawals from ATMs and digital payments undertaken through PoS terminals. The vertical line represents the timing of demonetization. In line with the unanticipated nature of the policy and its goal to deposit outstanding high-denomination currency in banks, the left panel shows a sharp decline in currency in circulation in the month of demonetization, accompanied by a sharp increase in cash held by banks. While there was an immediate withdrawal of cash from banks in the following 2 months, currency in circulation took almost a year to recover to pretreatment levels.

The right-hand panel of Figure 1 shows a sharp reduction in cash withdrawals from ATM terminals coinciding with the "treatment" intervention. ATM cash withdrawals returned to their pretreatment levels within 4 months post-demonetization, supporting the observation of Chodorow-Reich et al. (2020) that the currency shortage was most acute in the first 3 months after the event. In contrast, there was a sharp irreversible increase in digital payments, which shifted to a new equilibrium.

1.3 Regional variation in treatment intensity

As the central bank, the RBI manages currency supply using a "hub-and-spoke" framework. The RBI operates four printing presses to print currency, which is then sent to 19 issue offices, from where it is distributed to 4,034 currency chests (CC). Printing presses are strategically located, with one each in eastern, southern, central and western India. The CCs serve as repositories of cash and are operated by commercial banks. From the CCs, the currency is further distributed to individual bank branches, which then supply cash to ATMs.[15]

As our unit of analysis is the pincode, the ideal experiment would have involved the random assignment of pincodes to treatment and control status in terms of cash shortage.[16] Absent such randomization, we exploit cross-sectional variation in the distance between CCs and pincodes. The core assumption is that transportation costs and logistical challenges are an increasing function of distance, implying that transmitting currency to pincodes located farther from a CC would involve higher costs and entail greater time. In a period of significant disruption and currency shortage, it is plausible that such logistical challenges would have been accentuated, necessitating a longer wait for currency replenishment in pincodes farther from CCs.

We construct cross-sectional variation in treatment intensity based on the distance between a pincode and its nearest CC. Importantly, as CC locations are fixed, the set of CC-pincode distances is, by construction, orthogonal to the treatment. There were also no new CC openings in the post-demonetization period (or immediately prior to demonetization), alleviating concerns about strategic manipulations in the pincode-CC distance.

2 Data and Descriptive Trends

This section describes the various data sets used in the paper and constructs the cross-sectional variation in treatment intensity.

2.1 Digital transactions and cash withdrawals

Our primary outcome of interest – digital transactions – is measured using proprietary data from the National Payments Corporation of India (NPCI), comprising of transactions

[15] In all, the currency is distributed from 4,000 CCs to 135,000 bank branches and 200,000 ATM terminals.

[16] An alternative strategy would have been to exploit cross-sectional variation in pincodes' pretreatment cash dependency. However, data on currency circulation at the pincode level are unavailable.

conducted using RuPay debit cards.[17] The data are available at a monthly frequency at the pincode level between January 2016 and April 2018. We smooth out monthly fluctuations by taking quarterly averages, leaving us with a panel of 17,289 pincodes across 9 quarters, that is, 3 pretreatment and 6 post-treatment.[18] In the absence of pincode-level data on currency in circulation, we obtain from the NPCI proprietary data on pincode-level cash withdrawals from ATM terminals to assess the treatment's impact on cash supply.[19]

We use the pincode-CC distance to construct the local exposure to demonetization[20] as

$$Dist_{CC,i} = \min[Dist(z_i, z_c)]; \forall c \in \mathbf{C}. \quad (1)$$

In (1), $Dist$ is the Euclidean distance between pincode i and currency chest c, computed using the latitude and longitude of each pincode's centroid, and the centroid of the currency chest pincode.[21] Distance to the nearest CC ($Dist_{CC}$) is the minimum from this vector of distances. Figure 2 shows a bimodal distribution, with a mass of pincodes where the nearest CC is located within the pincode ($Dist_{CC,i} = 0$), and a second concentration around 10 km. The median (mean) pincode to nearest CC distance is 10.8 (11.8) km.[22]

A potential concern with $Dist_{CC}$ relates to measurement error arising from a mismatch between pincodes and the nearest CC. For instance, it is not implausible that the CC nearest to a pincode in terms of Euclidean distance does not supply currency to that

[17]RuPay is a network of credit and debit cards, handled by the NPCI, and supervised by the RBI. In terms of transaction volumes, RuPay debit card transactions from PoS terminals in the first 10 months of 2016 accounted for 4 % of total debit card transactions through PoS terminals. The absence of credit card transactions is because RuPay introduced credit cards only in June 2017.

[18]There are approximately 19,000 unique pincodes in India. We do not consider the April-June quarter in 2018 as we have data for a single month in that quarter. Pincodes lacking at least 2 quarters of data in both the pre- and the post-treatment periods are excluded.

[19]The NPCI data on cash withdrawals cover only a fraction of the total cash withdrawals from ATMs in the economy. This pertains to transactions undertaken only when a consumer uses a debit or credit card issued by bank A, in an ATM terminal operated by bank B; $A \neq B$. In the 3 quarters prior to demonetization, our data covers approximately 50% of the total cash withdrawals undertaken from ATM terminals in India.

[20]Other papers using a similar empirical strategy exploiting exposure to currency chests include Crouzet, Gupta, and Mezannotti (2023), Das et al. (2022), and Ghosh, Vallée, and Zeng (2021).

[21]The All India Pincode Directory maintained by the Government of India contains the latitude and longitude of each pincode. For CC pincodes, we use RBI's publicly available list of CC physical addresses.

[22]About a sixth of the pincodes (2,726) have a CC located within the pincode

pincode as geographical factors impose a higher travel/time cost.[23] While this classical measurement error should attenuate the regression coefficients, we empirically verify that our findings are not driven by any spurious correlations using a permutation-based placebo test where we randomly assign $Dist_{CC}$ to pincodes and verify that the treatment effect cannot be replicated using this "pseudo" set of distances.

2.2 Other data sources

The proprietary data from the NPCI are combined with data from a number of other sources.

2.2.1 Bank branch data: Basic statistical returns.

We use data from the Basic Statistical Returns (BSR) to generate additional pincode-level characteristics. These are proprietary data collected by the RBI on branch-level loans and deposits for all commercial banks in India. We scrape the physical addresses of bank branches to map 90% (120,432 out of 134,445 bank branches) of commercial bank branches in March 2016 to pincodes. This provides us with the stock of local financial infrastructure and economic activity in the pincode.[24] We also use RBI's classification of bank branches into urban and rural to assess urbanization at a granular level.[25] Finally, assuming that deposit accounts are proportional to population, we impute a pincode's population by assigning to each pincode a fraction of the district's population, equivalent

[23]The likelihood of mismatch increases in the presence of natural obstacles and other topographical factors. For instance, if the closest CC to a pincode is located on the other side of a hill through which no motorable road passes, the pincode might be receiving currency from an alternative CC, which is possibly located at a greater Euclidean distance but involves lower travel time. Other factors giving rise to such mismatches is if bank branches in the pincode arrange to receive currency from CCs maintained by their own banks.

[24]We use the stock of bank deposits in each pincode in March 2016 as proxies for local pretreatment economic activity. As we calculate bank branches and deposit accounts in March 2016, this includes all the deposit accounts opened under the Pradhan Mantri Jan Dhan Yojana (PMJDY) which aimed to provide every adult with a bank account.

[25]The RBI classifies bank branches as metropolitan, urban, semi-urban and rural. Branches are "semiurban" if they operate in an area with a population between 10,000 and 100,000, and "rural" if they operate in an area with a population less than 10,000. The respective cutoffs for "urban" and "metropolitan" branches are populations of 100,000-1 million, and in excess of 1 million. A pincode is classified to be rural if every bank branch in the pincode is classified as rural or semiurban. 83% of the pincodes in our sample are classified as rural.

to the fraction of a district's deposit accounts located in the pincode. We use this imputed pincode population to estimate pincode financial infrastructure, namely, pretreatment per capita bank branches, PoS, and ATM terminals. Admittedly, this involves a significant degree of extrapolation, and we replicate these results using district-level population, which is measured with higher accuracy.

2.2.2 Consumer pyramids: Household consumption.

We use the Consumer Pyramids Household Surveys (CPHS), collected by the Centre for Monitoring the Indian Economy (CMIE) to identify the treatment's impact on households' adoption of digital technology, as well as other household outcomes, such as consumption, investment, and borrowings. Information on household ownership of credit cards, along with their experience with financial products, such as insurance and retirement savings, are used to assess households' financial inclusion. The surveys started in 2014 and cover 27 states and 514 districts with each household being interviewed thrice every year.[26] Each survey wave covers approximately 135,000 households, and our sample includes 123,000 households surveyed between January 2015 and April 2018 for whom we have at least 2 two pretreatment, and two post-treatment observations.[27]

2.2.3 District covariates.

We use data from the 2011 Census of India, along with nationally representative household and enterprise surveys conducted by the National Sample Survey Organisation (NSS) and the Indian Human Development Survey (IHDS) to generate regional characteristics on physical infrastructure, informality, consumption, education, demographics, financial inclusion, strength of social networks, awareness and trust. These variables are used to examine heterogeneity in digital payments adoption. Internet Appendix B1 provides additional details.

[26] The three survey waves are January-April, June-August, and September-December, respectively.

[27] One drawback of the CPHS data, however, is that it oversamples urban households, with 60 % of the sample residing in urban areas. We address this by weighting our results using the sample weights provided by the CPHS; the weights reflect the inverse of the sampling probability for each household.

2.3 Descriptive trends

Internet Appendix Figures E2 and E3 depict pretreatment pincode characteristics as a function of their distance from CCs. Unsurprisingly, we see that pincodes located farther from CCs are more likely to be rural, located farther from major urban centres, and have lower financial infrastructure. These pincodes also have lower economic activity as estimated by the fraction of pretreatment cash withdrawals, bank deposits, and digital transactions.

The paucity of pincode-level data on consumption or demographics leads us to use district data to examine the pretreatment characteristics of areas located farther from centres of cash. We aggregate pincode-CCs distances to the district by computing the within-district median of the $Dist_{CC}$ distribution. Specifically, for district d,

$$Dist_{CC,d} = \overline{Dist_{CC,i}}, \tag{2}$$

where $\overline{Dist_{CC,i}}$ denotes the median operator. Internet Appendix Figures E4 and E5 are consistent with the pincode characteristics discussed previously: districts with relatively high $Dist_{CC,d}$ have lower urbanization, higher informality, lower educational attainment, lower financial infrastructure, and lower consumption. Importantly, regions with higher distances to CCs are also more cash-dependent and, by extension, more likely to be adversely affected by a disruption to cash supply.

Figure 3 motivates our empirical strategy. We classify pincodes into 5-km $Dist_{CC}$ bins and plot the unconditional mean of ATM cash withdrawals and digital transactions within each bin. To control for initial differences in transaction volumes, we normalize bin-specific quarterly averages using transaction volumes in the first quarter (January-March 2016). Figure 3 shows comparable evolution of cash withdrawals and digital transactions across the four pincode-CC distance bins in the first three-quarters of the pretreatment period. The onset of demonetization in November 2016 (quarter 4) is accompanied by a sharp drop in ATM cash withdrawals and a concomitant increase in digital transactions. Consistent with the aggregate trends in Figure 1, the reduction in cash withdrawals is reversed within 2 quarters, but the increase in digital transactions persists over 6 quarters. The reduction in cash withdrawals, however, was the smallest for pincodes located within 5 km of a CC and largest for those located beyond 10 km from a CC. This group of pincodes also saw the sharpest increase in digital transactions.

Figure 3 provides three key insights: first, pincodes located farther from CCs saw a sharper reduction in cash withdrawals post-demonetization, consistent with our expectations that logistical challenges would hinder cash replenishment to such areas. Second, these pincodes also saw the highest increase in digital transactions post-treatment. Third, for both cash withdrawals and digital transactions, there is no evidence of differential pre-treatment trends across the four pincode-CC distance bins. This motivates our empirical strategy to identify the causal impact of the policy intervention using a difference-in-difference (DiD) design.

3 Results

We will now discuss the key findings of our paper.

3.1 Baseline results

Figure 4 provides a nonparametric depiction of the treatment effects. To test for persistence, we split the post-demonetization period into near and long terms. The former refers to the 4 quarters between quarter 3, 2016, and quarter 3, 2017; the latter is the remaining 2 quarters, ending with quarter 1, 2018. The horizontal axis is divided into 25 1-km bins, based on pincodes' distance to the nearest CC. The treatment effect is computed as the unconditional within-pincode first difference estimator:

$$Difference_i = \ln(\bar{Y}_i^{Post^j}) - \ln(\bar{Y}_i^{Pre}), \tag{3}$$

where j is the near or long term, and i refers to the pincode. The outcomes of interest ($\bar{Y}$) are average cash withdrawals (top row) and digital payments (bottom row). Each point in Figure 4 is the unconditional mean of $Difference_i$ for pincodes located in that distance bin. The top row of Figure 4 shows a sharp reduction in cash withdrawals in pincodes located farther from CCs over the near term, which subsequently eased over the long term. On the contrary, the bottom row of Figure 4 shows higher digital transactions in pincodes located farther from CCs across both the near and the long-terms.

We rigorously test the trends in Figure 4 using the reduced-form DiD specification:

$$ln(Y_{idt}) = \alpha_i + \delta_{dt} + \beta Dist_{CC,i} \times Post_t + \phi \mathbf{X}_{idt} + \epsilon_{idt}. \tag{4}$$

In (4), the unit of observation is pincode i, located in district d. The primary outcome of interest is the logged volume of digital transactions conducted through PoS terminals. We also consider ATM cash withdrawals to gauge the policy's impact on cash availability. $Post$ is a dummy equaling one for all quarters since quarter 3, 2016 – the quarter of demonetization – and is split into near and long terms, akin to Figure 4. $Dist_{CC}$ is the Euclidean distance between a pincode and its nearest CC, defined in Equation (1).

Specification (4) includes pincode and district-quarter fixed effects – α and δ. District-quarter fixed effects imply that we are comparing changes in digital payments across pincodes located in the same district and quarter. Identification exploits variations in $Dist_{CC}$ across pincodes within a district. Pincode fixed effects control for time-invariant differences in outcomes across pincodes. We also include pretreatment pincode covariates in $\mathbf{X}$, interacted with a post-treatment indicator.[28] The coefficient of interest is β, estimating the impact of a unit increase in the minimum pincode-CC distance on digital transactions (cash withdrawals) in the post-treatment period.

Column 1 of Table 1 includes only pincode and district-time fixed effects and shows that pincodes farther from CCs saw substantially lower cash withdrawals from ATMs in the first three-quarters post-demonetization. The inclusion of covariates in column 2 reduces the coefficient magnitude (in absolute value), but it continues to remain negative and statistically significant over the near term, while the long-term coefficient becomes

[28]The pretreatment pincode covariates considered are: (a) ATM and PoS terminals per capita; (b) per capita bank branches; (c) distance to nearest urban center; (d) total deposit accounts (deposit volume), as a share of national deposit accounts (deposit volume); (e) imputed pincode population; and (f) whether the pincode has a CC located in it.

statistically non-significant. The coefficient implies that an unit increase in a pincode's distance from the nearest CC resulted in a 0.4 % decline in cash withdrawals from ATMs over the near term. Compared to a pincode at the 25th percentile of the $Dist_{CC}$ distribution ($Dist_{CC} = 4.3$ km), a pincode at the 75th percentile ($Dist_{CC} = 17.3$ km) saw 5 % [(17.303 - 4.281)*0.004] lower ATM cash withdrawals per month in the post-treatment period.

Columns 5 and 6 of Table 1 show the impact of demonetization on digital payments – measured using RuPay card transactions at PoS terminals – and identifies a positive and statistically significant treatment effect in pincodes located farther from CCs. Akin to cash withdrawals, the inclusion of pincode covariates in column 6 attenuates the coefficient in magnitude, but it continues to remain positive and statistically significant at the 1% level. Moreover, the treatment continues to positively affect digital payments over the long term, signalling persistence, and we can reject the equality of the near and long-term coefficients at the 1% level. The estimated coefficients imply that a unit increase in the pincode-CC distance increased the volume of digital transactions by 0.6% in the near term and 1.3% in the long term. Moving from the 25th to the 75th percentile of the pincode-CC distance distribution would increase digital payments by 8% (17%) over the near (long) term.

Columns 3 and 7 of Table 1 replace the continuous $Dist_{CC}$ measure with the binary $HighDist$ which equals one if $Dist_{CC}$ exceeds 11 km – the median pincode-CC distance – and reports a 7% reduction in monthly ATM cash withdrawals in the first 4 quarters succeeding demonetization for pincodes located farther from CCs. Evaluated at the pretreatment mean for pincodes located near CCs ($HighDist = 0$), column 3 reflects an average monthly decline in cash withdrawals of INR 6 million in the near term. Akin to column 2, this negative effect dissipates over the long term. For digital payments (column 7), pincodes located farther from CCs saw a 12% (22%) increase in digital transactions in the near term (long term). Relative to the pretreatment mean for pincodes located near CCs, this equates to an INR 0.10 (0.18) million increase in average monthly digital payments over the near term (long term).

Columns 4 and 8 of Table 1 separately identify the treatment effect in 5-km bins of the pincode-CC distance. In line with Figure 3, the coefficients show that the treatment reduced cash withdrawals across all four distance bins in the near term, with the negative effect accentuated for pincodes located at distances in excess of 10 km.[29] In line with the explanation that the adoption of digital payments was in response to the cash shortage, the positive treatment effect on digital payments was driven by pincodes located in excess of 10 km from CCs.[30]

We use columns 3 and 7 of Table 1 to provide a back-of-the-envelope estimate of the extent to which digital payments offset the decline in cash supply. As our data on cash withdrawals and digital payments capture only a subset of total transactions undertaken through these mediums,[31] we assume that the share of transactions covered (relative to the national aggregate) remained constant across all pincodes. Applying the corresponding coefficients from columns 3 and 7 of Table 1, we estimate an aggregate decline in monthly average ATM cash withdrawals of INR 12.24 million for pincodes located farther from CCs.[32] The accompanying increase in digital payments is INR 4.39 million, which is approximately 36% of the reduction in cash withdrawals.[33]

3.1.1 Identification concerns.

The identifying assumption for a causal interpretation of β in the specification (4) is that conditional on the fixed effects and pincode covariates, digital transactions would have

[29] We can reject the equality of the coefficients corresponding to pincodes located at a distance of 5-10 km from CCs, relative to those located at 10-15 km, 15-20 km, and in excess of 20 km. We are, however, unable to reject the equality of the treatment effect for the latter three bins.

[30] Statistically, we can reject the equality of the treatment effect for pincodes located between 5 and 10 km from CCs, and those located between 15 and 20 km from CCs across both the near and long terms. Over the long term, we can reject the equality of the treatment effect for pincodes located between 5 and 10 km of CCs, and those located between 10 and 15 km.

[31] In the 4 quarters succeeding demonetization (including the quarter of demonetization and three quarters thereafter), our data accounted for 47% of cash withdrawals from ATM terminals, and 10% of transactions undertaken through PoS terminals using debit cards.

[32] The average volume of cash withdrawals (digital payments) in pincodes located near CCs in the first four quarters post-demonetization was INR 86.68 (3.87) million. After applying the corresponding scaling factors, this equals INR 183 (38) million in cash withdrawals (digital payments).

[33] In the aggregate data, we find the increase in digital payments during this period equaling 33% of the decline in cash withdrawals. Note, however, that we only count reductions in ATM cash withdrawals and not the reductions in cash withdrawals from banks. We also don't include the increase in digital payments from credit cards or through mobile money applications.

evolved comparably in the absence of the treatment across pincodes located near and farther from CCs.[34] While the counterfactual cannot be empirically tested, we exploit the pretreatment data to verify that pincodes located farther from CCs did not exhibit differential pretreatment trends in outcomes. Figure 3 hinted at the absence of any differential pretreatment trends across $Dist_{CC}$ bins. We formally test this using the event-study design:

$$\ln(Y_{idt}) = \alpha_i + \delta_{dt} + \sum_{j=-3}^{6} \beta_j Dist_{CC,i} \times \mathbb{1}(Shock_{t+j}) + \phi \mathbf{X}_{idt} + \epsilon_{idt}, \tag{5}$$

where $Shock_t$ in (5) is a quarter dummy, with $t = 0$ denoting the quarter of demonetization (quarter 4, 2016). β_j identifies the treatment effect corresponding to each quarter j before and after the treatment intervention, with the quarter prior to demonetization (quarter 3, 2016, $t = -1$) serving as the reference period. The inability to reject the null of $\beta_{-3} = \beta_{-2} = 0$ would support the identifying assumption that digital transactions in pincodes farther from CCs were not on a separate trend prior to demonetization.

Figure 5 shows the quarterly treatment effects with the vertical broken line signifying the quarter prior to the treatment intervention. Consistent with the descriptive trends in Figure 1, the left-hand figure shows a sharp decline in cash withdrawals in the quarter of demonetization for pincodes located farther from CCs, which declined further in the succeeding quarter and persisted into the second quarter of 2017. The cash supply stabilized over the second half of 2017 and there is no differential effect across $Dist_{CC}$ in the last two quarters. Importantly, there is no differential trend in cash withdrawals as a function of $Dist_{CC}$ in the pretreatment period – the coefficients are small, attenuated toward 0, and not statistically significant.

The right panel of Figure 5 plots the quarterly treatment effects on digital payments. Again, we note the absence of differential pretreatment trends as a function of pincodes'

[34] The principal threat to causal identification stems from omitted pincode-level time-varying factors correlated with $Dist_{CC}$, which also affect digital payments. The rural employment guarantee scheme is a prime example. If the government disproportionately targeted this workfare scheme to pincodes located farther from CCs in the post-treatment period, this would potentially bias our results upwards.

distance from CCs. Post-demonetization, there is a steady uptake in digital payments for pincodes located farther from CCs, starting with the quarter succeeding demonetization. Like Table 1, we identify persistence in the treatment effect on digital payments, even when there was no differential reduction in cash withdrawals. Consequently, in the absence of any other intervention which (a) reduced cash supply in pincodes located farther from CCs and (b) increased digital payments in the same set of pincodes, we can assign a causal interpretation to the coefficients in Table 1.

A second concern with our empirical strategy pertains to the nonrandom location of CCs. In particular, we would expect CCs as centers of cash to be strategically located in pincodes with relatively high economic activity and, thereby, more likely to switch to digital payments if overall economic development is positively correlated with technology adoption. This is borne out in Figure E2 (Internet Appendix), which shows pincodes near CCs were closer to major urban centers, had the higher pretreatment financial infrastructure and accounted for a higher share of bank deposits and credit. This raises the concern as to whether the independent variable of interest – $Dist_{CC}$ – measures the intensity of the cash supply shock or proxies for factors affecting technology adoption.

We address this in two ways. First, note that we continue to identify a positive treatment effect on digital payments upon the inclusion of pincode controls. This implies that pincodes' distance to the nearest CC are not proxying for *observable* pretreatment pincode characteristics. Second, Appendix Table E2 shows that conditional on pincode observables, $Dist_{CC}$ did *not* predict pretreatment digital transactions or cash withdrawals. As seen from columns 1 and 4 of Internet Appendix Table E2, there exists a sizeable unconditional negative correlation between $Dist_{CC}$ and pretreatment cash withdrawals/digital transactions. While district-specific factors do not explain this, the inclusion of pincode controls for population, rural location, financial infrastructure, and share of bank deposits sharply attenuates the coefficient toward zero and it is statistically nonsignificant (columns 3 and 6.

Resultantly, while pincodes' distance to the nearest CC did not predict pretreatment cash withdrawals and digital payments (conditional on observables), it does so in the post-treatment period, regardless of whether we condition on the same set of observables. This lends credibility to the contention that conditional on pincode observables, $Dist_{CC}$ proxies for logistical challenges involved in supplying currency to pincodes located farther from CCs, which are accentuated during a period of currency shortage, and not other pincode unobservables which may predict digital technology adoption.

A third concern relates to the ability of individuals to travel across pincodes to mitigate local reductions in cash supply. For instance, if transportation costs are small, individuals could offset the cash shortage by travelling to pincodes closer to CCs witnessing faster cash replenishment. This can accentuate the reduction in cash withdrawals from ATMs located farther from CCs and dampen the treatment effect for digital payments if households adopt digital payments as a substitute for cash. Internet Appendix Table E3 tests for treatment heterogeneity by regional connectivity and shows it to be unlikely that the negative treatment effect on cash withdrawals was generated solely by individuals travelling from pincodes with low cash availability to those with high availability. Namely, columns 2 and 3 of Internet Appendix Table E3 continue to identify a negative and significant impact of demonetization on cash withdrawals in pincodes with relatively low connectivity, which can be used as a conservative upper-bound of the treatment's impact on ATM cash withdrawals.[35] Moreover, columns 4 and 6 of Internet Appendix Table E3 show the adoption of digital payments to be significantly higher in areas with enhanced connectivity. This alleviates concerns that we are underestimating the treatment effect on digital payments, as select agents could have travelled to areas with faster cash replenishment.[36]

In summary, our DiD results show that the treatment caused a sharp decline in

[35]In the absence of pincode data on connectivity, we use district-level data from the 2011 Census: namely, (a) the fraction of villages in a district which are connected by a national or state highway, or a major district road; (b) the fraction of districts reporting a bus service; and (c) the fraction of districts reporting a private taxi service. The implicit assumption is that individuals would be more likely to travel from pincodes with better access to roads or public transport.

[36]As seen in Internet Appendix B1 and Section 4.1, physical infrastructure is positively correlated with financial infrastructure and overall household awareness, both of which positively affected the adoption of digital payments. Thus, any reduction in digital payments due to increased access to cash in areas with high transport connectivity is likely to be offset by the availability of financial infrastructure and higher levels of awareness.

ATM cash withdrawals in pincodes located farther from CCs. This was accompanied by a persistent increase in the adoption of digital payments. The decline was most severe in pincodes located between 10 and 20 km from currency chests, and the corresponding increase in digital payments was the strongest in these pincodes. As noted by Chodorow-Reich et al. (2020), the increase in digital payments rules out that the reduction in cash withdrawals was driven by a negative demand shock. In Internet Appendix C1, we rule out concerns that the increase in digital payments can be attributed to changes in household portfolio choices or access to credit in areas with slower cash replenishment. There is also no evidence of higher consumption in these areas. If anything, we find consumption to be lower in areas with higher exposure to the cash supply shock, assuaging concerns that the increase in digital transactions was an upshot of public transfers to regions adversely affected by demonetization. This strengthens the contention that the increase in digital transactions was driven by agents switching to alternative payment methods in response to the cash shortage. Importantly, unlike the treatment effect on cash withdrawals which reverses over time as the cash supply stabilizes, there is no such reversal for digital transactions.

3.2 Robustness

We show the stability of our baseline results to a number of alternative sample and specification choices. Table 2 shows robustness to alternative sample choices: columns 1 and 5 extends the sample to all pincodes; columns 2 and 6 exclude pincodes in which a currency chest is located as these might be systematically different; columns 3 and 7 exclude pincodes located in six major metropolitan centers;[37] and columns 4–8 restrict the sample to June 2017, prior to the introduction of a nationwide harmonized consumption tax.[38] Irrespective of sample choice, the treatment reduced near-term ATM cash withdrawals in pincodes located farther from CCs, which dissipated over the longer term. This was

[37]These are namely the metropolises of New Delhi, Mumbai, Kolkata, Chennai, Hyderabad and Bangalore.

[38]This was the Goods and Services Tax (GST), which came into effect on July 1, 2017.

accompanied by higher digital payments across both the near and long terms.[39]

Additional specification checks are presented in Internet Appendix Tables E4 and E5, documenting robustness to alternative specification choices and outcome variables. Internet Appendix Table E4 shows the stability of the baseline results to the inclusion of a linear time-trend in the initial value of the outcome variable (measured in quarter 1, 2016), or clustering by district (allowing outcomes to be correlated across pincodes within a district). We also show robustness to alternative distance bins based on select percentiles of the $Dist_{CC}$ distribution. Internet Appendix Table E5 shows the results to be stable to scaling the outcome variable by imputed pincode population, using a hyperbolic sine transformation to account for zeroes, or measuring the outcome in terms of transaction counts, instead of transaction volumes. While we cannot distinguish between repeat and new users, the positive treatment effect on transaction counts reassures us that the results are not driven by a few high-value transactions.

Finally, we undertake a permutation-based placebo test where we randomly assign values of $Dist_{CC}$ to pincodes and reestimate our reduced-form specification. The process is replicated 100 times and Figure 6 plots the empirical CDF of the coefficients. The vertical line in each figure is the "true" treatment effect. For digital transactions (cash withdrawals), we find only 6 (4) of the near-term and 4 (2) of the long-term coefficients to be statistically distinguishable from zero. Moreover, almost none of the placebo coefficients exceeds the "true" coefficient size obtained in Table 1. This confirms that the treatment effects are not generated through unobservable pincode-specific factors correlated with pincodes' distance to the nearest CC.

3.3 Heterogeneity by financial fnfrastructure

Digital payments in our paper involve retail transactions made using debit cards. This makes financial infrastructure a necessary condition for digital transactions and we would expect the positive treatment effect to be accentuated in areas with a high density of PoS

[39]Dropping the pincodes with CCs in the sample results in a further reduction in coefficient magnitude for cash withdrawals and the coefficient now is significant only at the 15% level (p-value .114). This suggests that the cash supply shock eased much faster in pincodes with a currency chest.

terminals and bank branches. We verify this in Table 3 by examining treatment heterogeneity across three separate measures of local financial infrastructure: bank branches, ATMs and PoS terminals per capita.

Expectedly, column 1 of Table 3 shows that conditional on distance from CCs, pincodes with a relatively high (above-median) concentration of pretreatment PoS terminals witnessed significantly higher digital payments in the post-demonetization period. The triple interaction coefficient is positive and significant, and the results are very similar if we consider heterogeneity by district-level PoS terminal density (column 4). Consistent with PoS terminals being a necessary condition for the adoption of digital payments, the $Dist_{CC} \times Post$ coefficient is nonpositive across both the near and the long term, implying that the treatment left digital payments unaffected in pincodes with low ex ante PoS terminal density.

The results are directionally equivalent when considering other measures of financial infrastructure, albeit weaker for pincode-level banking infrastructure (column 2). Intriguingly, we identify positive treatment heterogeneity across ATM terminal density, possibly because of the high correlation between PoS and ATM terminal density. Alternatively, ATM terminals could have aided households in gaining familiarity with debit cards, and by contributing towards overall learning and fostering trust in the technology (Higgins, 2019).

A contrary explanation to the above findings is that policy makers prioritized cash replenishment to areas with poor financial infrastructure and limited access to alternative payment systems. Internet Appendix Table E7 however finds no evidence of a differential impact of financial infrastructure on ATM cash withdrawals.[40] This is consistent with cash replenishment being undertaken in a haphazard manner, with the physical distance from centers of cash being the key friction. Consequently, while being endowed with financial infrastructure did not ameliorate the cash shortage, it did allow agents to switch to digital payments in the absence of cash.

[40]If anything, weak evidence suggests that cash supply restoration was quicker in areas with higher financial infrastructure. The triple interaction coefficients are positive, albeit not precisely estimated.

4 Mechanisms

This section explores mechanisms explaining the adoption of digital payments in response to the cash supply shock. We first identify heterogeneity across regional characteristics. Next, we examine which side of the market – households or retailers – adopted digital payments. Finally, we consider factors explaining persistence in adoption.

4.1 Factors driving adoption

We begin by exploiting the rich geographical variation in our data and identifying heterogeneity in adoption across regional characteristics. Existing papers highlight the roles of access to technology, education, information, social networks, learning by doing, and habit formation as some of the prime factors affecting technology adoption (Gupta, Ponticelli, and Tesei 2020). Hence, we test heterogeneity across seven broad factors: financial and physical infrastructure, financial inclusion, education, awareness, trust, and social networks. As multiple variables can be used to measure each factor, we create indexes for each characteristic of interest:

$$Index_d^k = \frac{1}{n}\sum_{j=1}^{n} StdChar_d^j. \tag{6}$$

Equation (6) defines $Index^k$ as the equally weighted mean of individual standardized indexes used to measure characteristic k (details in Internet Appendix B1). With the exception of financial infrastructure, the remaining indexes are measured at the level of the district.

Expectedly, Internet Appendix Table B2 reports a high degree of correlation across indexes which could lead to spurious inference if we separately examine treatment heterogeneity across each index. We address this by estimating a triple difference specification which simultaneously tests for treatment heterogeneity across all indexes. Specifically, we estimate

$$\begin{aligned} ln(Y_{idt}) = \alpha_i + \delta_{dt} + \beta Dist_{CC,i} \times Post_t \\ + \sum_{k=1}^{9} \theta_k Dist_{CC,i} \times Index_d^k \times Post_t + \phi \mathbf{X}_{idt} + \epsilon_{idt}. \quad (7) \end{aligned}$$

As each index is the mean of standardized indexes, they are centered around zero. Consequently, β in (7) estimates the impact of a unit increase in $Dist_{CC}$ on the adoption of digital payments for pincodes located in a district with the mean score across all nine indexes. Conditional on distance from currency chests, each θ_k estimates the differential adoption of digital payments arising from a unit increase in index k. Effectively, specification (7) identifies the differential effect of each characteristic on digital payments, conditional on the remaining factors. In addition to the seven indexes mentioned above, we also include an index for household consumption and district informality. While the former directly captures heterogeneity by regional purchasing power, the latter is included since informality and financial infrastructure are negatively correlated at the district level.

Table 4 shows the results from this exercise. The outcome of interest in panel A is the logged volume of digital payments; in panel B, logged transaction counts. Column 1 shows the double-difference coefficient corresponding to β; columns 2–10 show the θ_k coefficients. Even in this saturated regression, column 2 confirms that local financial infrastructure positively affected the adoption of digital payments, alleviating concerns that the variables used to measure financial infrastructure in Section 3.3 were proxying for other correlated factors.

Column 7 shows that conditional on the cash supply shock, household awareness positively affected the adoption of digital payments across the near and long term for both transaction counts and volumes. Similar to Bachas et al. (2021), we find households' trust in public institutions to positively affect digital payments, especially along the intensive margin (column 8, panel A). Surprisingly, we find no heterogeneity across regions with strong social networks. Nor is there evidence of heterogeneity by physical infrastructure, educational attainment, or enterprise financial inclusion, although households' financial inclusion positively affected transaction counts over the long term (column 4, panel B).

Finally, conditional on distance from CCs, regional informality has a negative impact on the adoption of digital payments, with the triple interaction coefficients being large in magnitude and statistically significant. As districts with relatively high informality — namely, lower urbanization, higher self-employment, and lower salaried workers — are also negatively selected on observables and have a higher reliance on cash, this points to the negative distributional consequences of demonetization. They support concerns voiced by Lahiri (2020) that the adverse effects of the cash shortage were likely to have been borne by the informal sector. We explore this further in Internet Appendix D1 and show that conditional on the cash supply shock, digital payments remained dampened by a factor of 6 in pincodes in areas with relatively higher informality over the long term. This was, however, reversed in pincodes with relatively high financial infrastructure, suggesting that the lack of financial infrastructure precluded the adoption of digital payments in these regions, despite comparable reductions in cash supply.

Table 4 points to the significant role played by household awareness and financial infrastructure for the adoption of digital payments. The positive treatment heterogeneity across household awareness – measured using newspaper readership, listening to the radio, and television viewership – is consistent with Gupta, Ponticelli, and Tesei (2020), which shows that incomplete information can constrain technology adoption. To this effect, we highlight two information frictions – namely, transaction fees and data security – which could have muted agents' adoption of digital payments, notwithstanding access to technology.

First, fees levied on PoS transactions were temporarily waived post-demonetization (until December 31, 2016) to incentivize the use of digital payments in the absence of cash (Business Line 2018; *Economic Times* 2016). Borzekowski, Kiser, and Ahmed (2008) in an early study uncovers the high elasticity of debit card usage to fees in the Unite States, while Higgins (2019) mentions that competitive markets limit the pass-through of such fees to consumers.[41] Resultantly, it is plausible that retailers initially unaware of the fee

[41]Borzekowski et al. (2008) report a 12% decline in the likelihood of card usage in response to a 2%

waiver were less agreeable to accepting card payments.[42]

Second, concerns about digital security could also have limited the adoption of digital payments in response to the cash shortage. For instance, a month prior to demonetization, there were apprehensions regarding a major security breach pertaining to cards issued by India's largest bank (Saha, M, 2016; *Economic Times*, 2018; Vishwanathan, V, 2016). In the following weeks, a number of newspapers reported on the potential data breach and provided directions regarding the safe usage of credit/debit cards. If security concerns hindered adoption and access to credible information reassured users regarding the safety of digital transactions, we would expect adoption to be concentrated in areas with high newspaper readership, conditional on the cash supply shock. In the absence of such information, households may have been wary of immediately responding to the cash shortage through digital payments, consistent with the one-quarter lag in adoption seen in Figure 5.

We empirically test this by identifying the impact of the cash supply shock on digital payments, conditional on access to financial infrastructure and household awareness. If information frictions indeed delayed the adoption of digital payments, the corollary would be that adoption would be immediate in regions with access to both financial infrastructure and information. We assess this by splitting our sample into four groups according to their pretreatment scores on the infrastructure and awareness indexes and reestimating the event-study plots corresponding to Figure 5 for each subsample.

The results in Figure 8 are striking. In the top panel, where the sample is restricted to pincodes with relatively low (below-median) scores on the financial infrastructure index, there is little impact of the treatment on digital payments. The bottom panel restricts the sample to pincodes with relatively high scores on the financial infrastructure index, and the positive treatment effect materialized with a 2-quarter lag (left panel) for districts with limited awareness. However, for pincodes with high financial infrastructure *and* located in districts with high awareness (right panel), there is a sharp increase in digital

increase in transaction fees.

[42] Alternatively, consumers unaware of the fee waiver may not have initially used their cards in the belief that retailers would decline card payments.

payments from the quarter of the policy intervention. This is consistent with information frictions hindering technology adoption, even in areas where agents would be expected to have access to the technology. The results are akin to Gupta, Ponticelli, and Tesei (2020), which documents adoption to be highest when agents have access to technology, *and* information pertaining to the technology.[43]

4.2 Sources of adoption: Households versus retailers

Section 4.1 showed that conditional on the cash supply shock, the increase in digital payments was driven by areas with significantly higher financial infrastructure and household awareness. We now explore which side of the market responded to the cash supply shock and adopted digital payments: consumers or retailers.

4.2.1 Digital technology adoption by households.

We use the CPHS data to identify whether the policy intervention affected technology adoption by households in the form of credit cards. While the digital payments data only cover debit card transactions, Internet Appendix Figure E8 shows that the number of credit and debit cards exhibited comparable aggregate trends between January 2016 and December 2017. In the absence of time-varying data on debit cards, we infer digital technology adoption from households' credit card ownership. Specifically, we estimate the linear probability model:

$$\Pr(Card = 1)_{hdt} = \alpha_h + \delta_{st} + \beta DistCC_d \times Post_t + \mathbf{X}_{hdt} + \epsilon_{hdt}. \quad (8)$$

The unit of observation is household h, and the outcome of interest is a dummy equaling one if the household had a credit card in survey period t. As the CPHS provides household identifiers only at the level of district, we exploit district-level variation in

[43]An alternative explanation would be that predemonetization users of digital technology were not infra-marginal, and the increase in digital payments was driven entirely by new adopters. We cannot conclusively establish or negate this hypothesis in the absence of transaction-level data, but the limited adoption of digital technology along the extensive margin, documented in Section 4.2 makes this unlikely.

distance to currency chests from Equation (2). Household fixed effects (α) partial out time-invariant factors affecting credit card adoption; state-time fixed effects (δ) imply that we are comparing credit card ownership for households in the same state and survey wave. Identification exploits variations in the median pincode-CC distance across districts. Standard errors are clustered by district and CPHS household weights are used to weight the regressions.

Columns 1 and 2 of Table 5 indicate higher adoption of digital technology by households in response to the cash supply shock. The coefficient is positive and statistically significant over the near term, implying that a 10-km increase in the median distance to CCs resulted in a 1-percentage-point increase in the likelihood of a household owning a credit card. The coefficient is large in magnitude when considering that less than 4% of households prior to demonetization had a credit card. The corresponding event-study plot in Figure 7 identifies positive and statistically significant coefficients in the first and second survey waves succeeding demonetization, coinciding with the increase in digital payments in areas farther from CCs. We, however, find no evidence of credit card adoption either during the period of demonetization or over the long term, once the cash shortage had abated.

These results offer two key insights. First, households exposed to higher cash shortages did adopt digital technology along the extensive margin.[44] If credit and debit cards exhibited comparable trends, participation along the extensive margin could also have contributed to increased digital payments in response to the cash shortage. An implicit assumption is that new adopters also used cards to conduct retail transactions. While we cannot ascertain this empirically, we offer two pieces of evidence supporting this assertion: first, Internet Appendix Table E5 shows that the treatment also increased the number of digital transactions, consistent with a higher number of users of digital technology.[45] Second, aggregate data from the nationally representative All India Debt and

[44]A potential confounding factor is whether households adopted credit cards to alleviate credit constraints imposed by the adverse shock to cash supply. In unreported results, we rule this out by confirming that the higher adoption of credit cards in response to the cash supply shock was driven by relatively richer households, who would have a lower likelihood of being credit-constrained.

[45]We cannot, however, rule out an increase in the intensity of use amongst existing users.

Investment Survey (AIDIS) in 2019 reported that 75% of individuals who owned a debit or credit card made at least one transaction with the same during the year, suggesting usage amongst those with access to digital technology. This is consistent with studies documenting high usage of financial products by adopters (Dupas et al. 2018; Jack and Suri 2014).

Second, while the results point to digital technology adoption along the extensive margin, there is limited evidence of adoption immediately after demonetization, consistent with the muted increase in aggregate digital payments during the quarter of demonetization. We contend this is likely because of the nontrivial transaction and time costs associated with the adoption of electronic cards. Unlike mobile payments, credit and debit card adoption requires the processing of applications by banks, and post-approval, there is an additional time lag for the issuance of the cards. If banks were capacity constrained during demonetization, with the primary focus being currency replenishment, the processing time for credit and debit card applications was likely to have been higher, reducing households' ability to respond immediately to the policy through the adoption of digital payments. Nontrivial transaction costs in the adoption of cards thereby serve as a second plausible mechanism to explain the lagged adoption of digital payments in response to the abrupt reduction in cash supply.

Columns 3–9 of Table 5 identify treatment heterogeneity in households' adoption of credit cards across the district and household characteristics to infer whether adoption is driven by social learning or household financial literacy. Learning models underline the role of network effects, whereby households learn from the experience of other households (Gupta, Ponticelli, and Tesei 2020). If social learning is the predominant channel, we would expect widespread adoption across households and not just restricted to households with high financial literacy. Alternatively, if the latter group drives adoption, it points to selection into digital payments through learning by doing (Breza, Kanz, and Klapper 2020).

Columns 3 and 4 of Table 5 identify positive and significant coefficients on the triple interaction terms corresponding to whether households had prior experience with insurance or retirement savings – our proxies for household financial literacy. The double-difference coefficient in both instances remains small and non-significant, signifying that credit card adoption in response to the cash supply shock was driven exclusively by households with prior exposure to financial products. Column 5 rules out heterogeneity across the strength of local networks, while columns 6–8 identify no heterogeneity across local financial infrastructure, negating the role of supply-side interventions.[46] The results offer little evidence in favour of the social learning channel, which also eliminates the role of network effects manifesting over time as a plausible explanation for the lagged adoption of digital payments.

4.2.2 Digital technology adoption by retailers.

Section 4.2.1 established digital technology adoption by households in areas adversely affected by the cash supply shock. We now examine whether the cash shortage also led to higher adoption of digital technology by retailers in the form of PoS terminals. This would occur if the widespread use of credit and debit cards nudged retailers to adopt digital payments to cater to the higher demand for card payments from consumers. Alternatively, policymakers could have engaged in the targeted rollout of financial infrastructure to areas where cash replenishment was slower. Consequently, we use our proprietary pincode-level data to identify whether exposure to the cash shortage induced the adoption of PoS terminals.

Internet Appendix Figure E7, however, offers little evidence of higher adoption of PoS terminals in areas most affected by the disruption to cash supply. The left panel plots unconditional quarterly trends in per capita PoS terminals across four $Dist_{CC}$ bins and depicts an aggregate increase in PoS terminal density in the 3 quarters following demonetization. The sharpest increase, however, is for pincodes located between 5 and 10 km from currency chests. The event-study plots in the right panel of Internet Appendix Figure E7 confirm this and point to the significantly *lower* adoption of PoS terminals in pincodes located farther from CCs in the 2 quarters following demonetization. Thus, while

[46] We use in column 5 the social networks index described in Internet Appendix B1.

overall adoption of PoS terminals increased, there was no *differential* increase in pincodes disproportionately affected by the cash supply shock, which also witnessed significantly higher digital payments.

Internet Appendix Figure E7 confirms that the increase in digital payments in areas most affected by the cash shortage was unaccompanied by higher adoption of PoS terminals by retailers. This rules out that the increase in digital payments emanated from the targeted expansion of digital financial infrastructure. The limited adoption of PoS terminals also cannot be explained by capacity constraints faced by financial institutions during demonetization. While POS terminal adoption also involves substantial time costs due to the processing of applications by banks, this cannot explain the lack of adoption a full year after demonetization when banks' primary focus had shifted from cash replenishment.

Higgins (2019) posits that network effects play a critical role in the adoption of digital technology by retailers, and a sufficiently large number of consumers would need to adopt digital payments before retailers undertake the fixed costs of adoption. While the previous section argued that households adopted digital technology in areas with higher exposure to the cash supply shock, the lack of adoption of PoS terminals by retailers points to the limited overall adoption of digital technology by consumers.[47] This is supported by the AIDIS 2019, which reported that less than 40% of India's adults had access to a credit or debit card in 2019. The top-right-hand panel of Internet Appendix Figure E8 also depicts modest aggregate adoption of debit cards post-demonetization.

Collectively, while the absence of transaction-level data precludes us from precisely quantifying the share of digital payments arising from the extensive and intensive margins, the aggregate evidence points to an increase in digital transactions primarily along the intensive margin in response to the unanticipated cash shortage. The absence of adoption from retailers, combined with sluggish growth in aggregate electronic cards post-demonetization rules out any major extensive margin response. Along the intensive

[47] An alternative explanation to the limited adoption of PoS terminals by retailers would be due to concerns about information disclosure to public authorities through participation in formal payment systems.

margin, the adoption of digital technology is driven by areas with prior access to financial infrastructure, with negligible roles of social learning and network effects in aiding the adoption process along either the intensive or extensive margins. The results point to financial infrastructure serving as a key friction to the adoption of digital payments using electronic cards. The absence of any extensive rollout of electronic cards or PoS terminals by policymakers restricted the adoption of digital payments to select areas with high ex ante financial infrastructure. This is distinct from the findings of Higgins (2019), who showed that the large-scale distribution of debit cards in Mexico successfully aided the adoption of digital technology along both the intensive and the extensive margins (in the form of PoS terminal adoption by retailers).

4.3 Persistence of digital payments

A key feature of our results is the persistence in digital payments. As seen from Figure 5, while the cash supply stabilized within 3 quarters post-demonetization, the increase in digital payments extended over 5 quarters. We argued that the absence of cash triggered the increase in digital transactions. As no other interventions were affecting digital payments in the post-demonetization period, the continual use of digital payments in areas with higher exposure to the cash supply shock, even after the restoration of cash supply, points to habit formation among agents. In this final section, we draw from our prior findings to highlight evidence consistent with habit formation.

Internet Appendix Figure E7 shows that the cash supply shock did not affect the adoption of PoS terminals by retailers, either because of high costs of switching or because of a lack of widespread adoption of digital technology among consumers.[48] This is contrary to the findings of Higgins (2019), who showed that an extensive rollout of debit cards in Mexico influenced small retailers to adopt PoS terminals, which in turn fueled further adoption amongst consumers. Persistence in the use of digital transactions in the present context is not driven by higher adoption of PoS terminals across retailers

[48] This could be due to limited adoption of the cards themselves, or limited use of the cards post-adoption.

and is unlikely to be driven by the widespread adoption of digital technology by consumers along the extensive margin. Indeed, as seen from the top-right panel in Internet Appendix Figure E8, there was only a modest jump in debit cards as a fraction of total bank accounts between 2016 and 2018.

In the absence of a cycle of digital technology adoption in response to the cash supply shock, the alternative channel explaining persistence is continued usage amongst consumers with access to digital technology. This could be through consumers who had access to debit cards prior to demonetization, or the subset of consumers who adopted debit cards in response to the cash supply shock. As debit cards can be used continually post-issuance, even moderate adoption of cards by households can translate into high long-term usage.[49] This points to the long-term benefits accruing from a one-time adoption of digital technology.

The increased usage can occur through social learning, or learning by doing, as consumers gain familiarity with the technology through retail transactions as a substitute for cash (Breza, Kanz, and Klapper 2020). Our empirical results, for both household credit card adoption and aggregate digital payments, rule out the social learning channel and point to learning by doing. For new users, Table 5 shows that only households with prior experience in financial products adopted credit cards in response to the cash supply shock. Consistent with this, column 3 in panel B of Table 4 shows that conditional on the cash supply shock, the number of digital transactions over the long run was significantly higher in areas with relatively high pretreatment household financial inclusion. This indicates that for consumers who adopted digital technology in response to the cash shortage, usage during the period of cash shortage contributed to learning about digital technology, resulting in habit formation.

A direct test of habit formation proposed by Schaner (2018) is whether regions which adopted digital payments over the near term also continued to use digital payments over the long term. Table 4 and Figure 8 lend support to this hypothesis. The adoption

[49]This, for instance, can be seen from Internet Appendix Figure E8: prior to demonetization, the volume of PoS transactions involving credit cards was significantly larger than PoS transactions undertaken using debit cards, even though the credit to debit card ratio was less than 4%.

of digital payments in response to the cash shortage was immediate in areas with high financial infrastructure *and* high awareness, and the positive treatment effect persisted in these areas over the long term. This would be consistent with the explanation that the absence of cash-induced households with access to digital technology and a threshold level of awareness to switch to digital payments in the near term. Usage during the period of cash shortage facilitated learning for this subset of users, leading to habit formation and continued usage even after the restoration of cash supply (Breza, Kanz, and Klapper, 2020).

5 Conclusion

This paper explores whether a temporary but unexpected increase in the cost of cash transactions in a developing economy can induce the adoption of digital payments as a substitute for cash. We study the demonetization episode in India, which discontinued overnight 86% of the currency in circulation, but left unaffected digital transactions undertaken using credit or debit cards. Using proprietary data on pincode-level digital and cash transactions, we show that regions with high exposure to the cash supply shock exhibited significantly higher adoption of digital payments, which persisted 6 quarters after the policy intervention, even after the cash supply had stabilized. The cash supply shock however, had a negative impact on per capita household consumption over the long term. The increased adoption of digital payments was driven by areas with relatively high financial infrastructure and primarily along the intensive margin. Indeed, we find no evidence of adoption of financial infrastructure by retailers in the form of PoS terminals. Households' adoption of electronic cards in areas adversely affected by the cash supply shock too was modest. We also find a limited role for network efforts in driving the adoption of digital payments. We contend this to be linked to the nontrivial adoption costs associated with the adoption of card-based digital payments in the form of time and processing costs of card/PoS applications. Overall, our paper highlights the availability of financial infrastructure to serve as the key friction in the adoption of digital payments.

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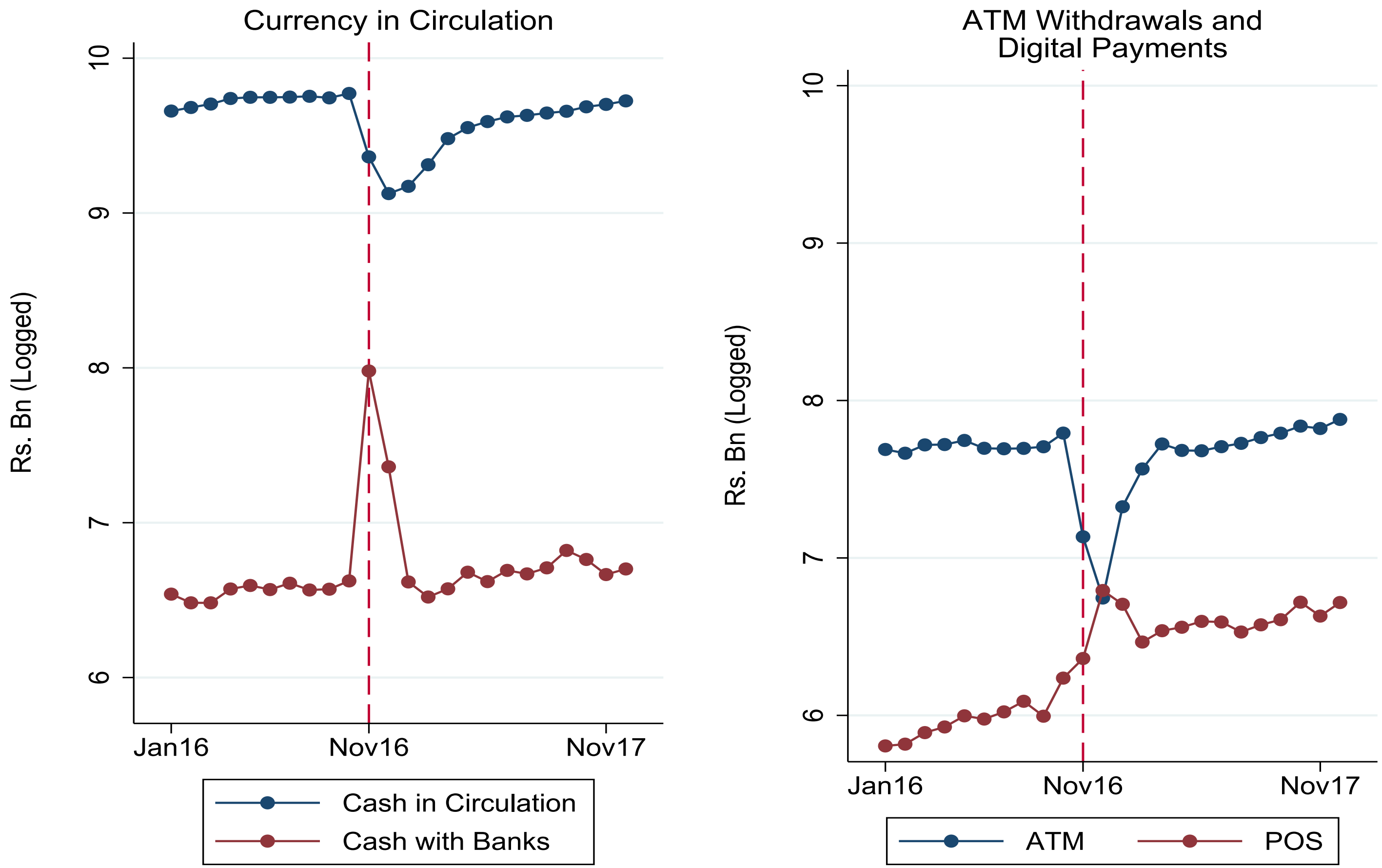
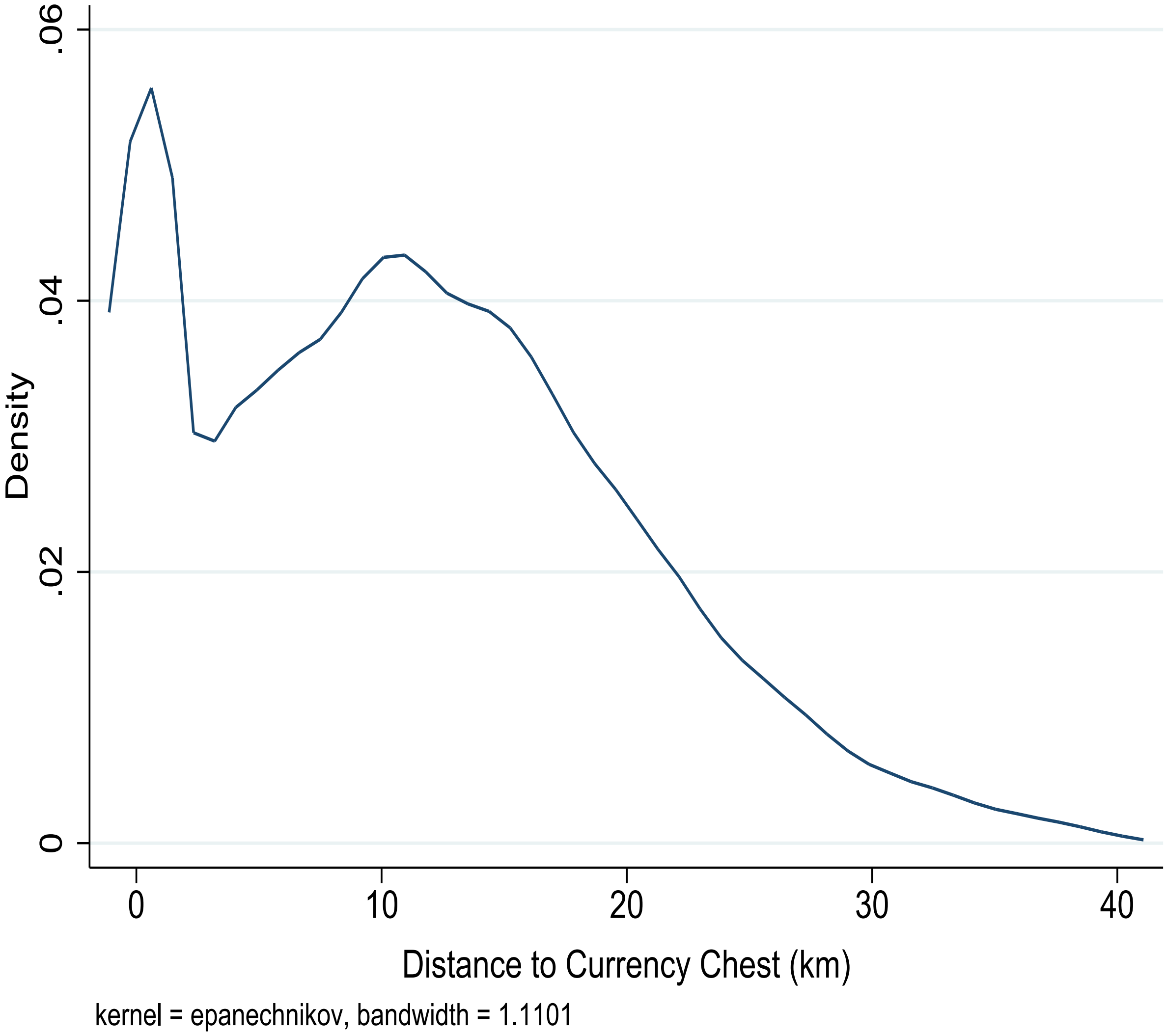
Figure 1
Demonetization, cash and digital transactions



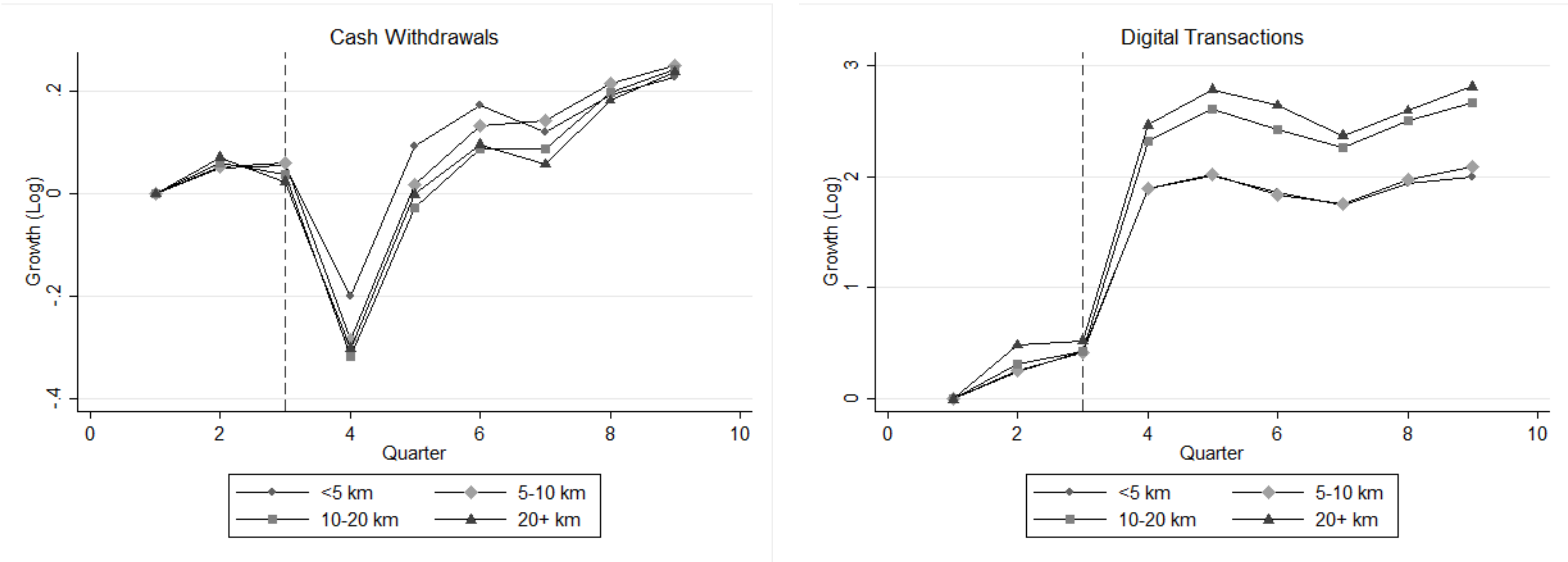
The above figures plot the evolution of currency in circulation, currency in banks, cash withdrawals from ATMs and digital payments undertaken using RuPay cards at a monthly frequency between January 2016 and December 2017. The data are aggregated to the all-India level. The broken vertical line represents the month of the demonetization intervention.

Figure 2
Distribution of pincode distance to nearest currency chest



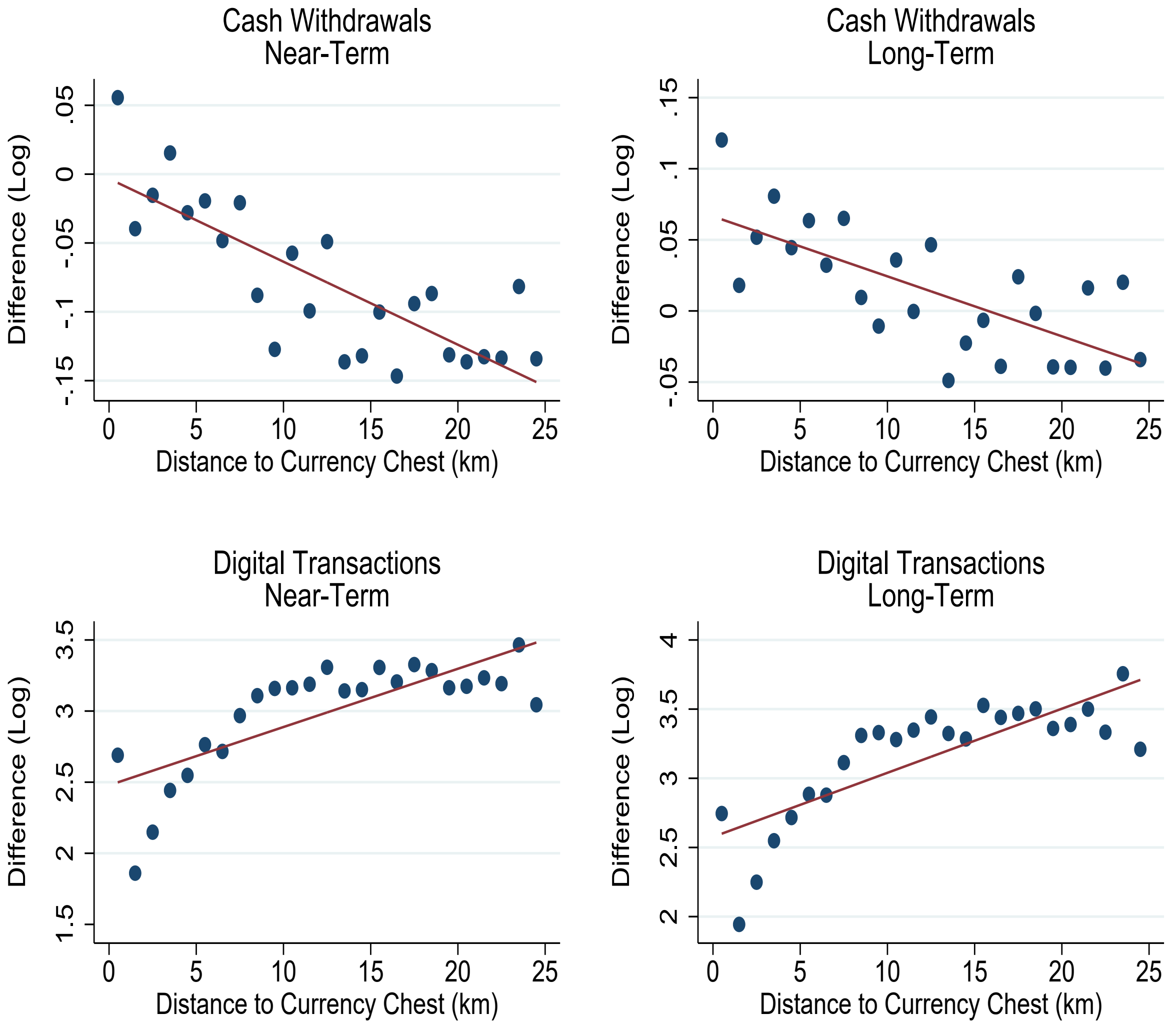
The above figure plots the kernel density distribution of pincodes' (Euclidean) distance to the nearest currency chest, measured in kilometres.

Figure 3
Quarterly trends in cash withdrawals and digital transactions by distance from currency chest



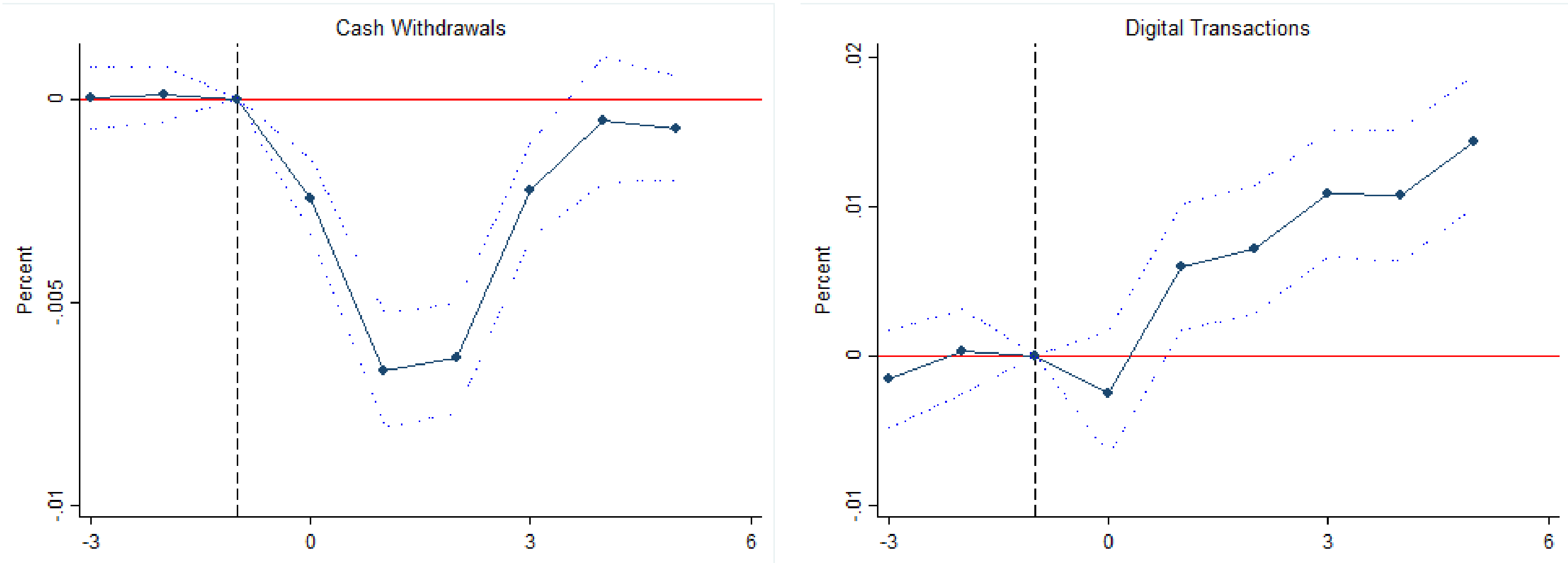
The above figures present quarterly trends in cash withdrawals and digital transactions averaged across 4 pincode distance bins. The vertical dashed line represents the quarter corresponding to the demonetization intervention. The left-hand figure plots the outcomes for cash withdrawals from ATMs; the right-hand figure, digital payments undertaken through POS terminals using RuPay cards. Each point represents the (logged) quarterly growth in the average volume of cash withdrawals (digital payments) for pincodes corresponding to each distance bin, relative to the first quarter in the sample (January-March 2016).

Figure 4
Cash supply shock, cash withdrawals and digital transactions: Unconditional first differences



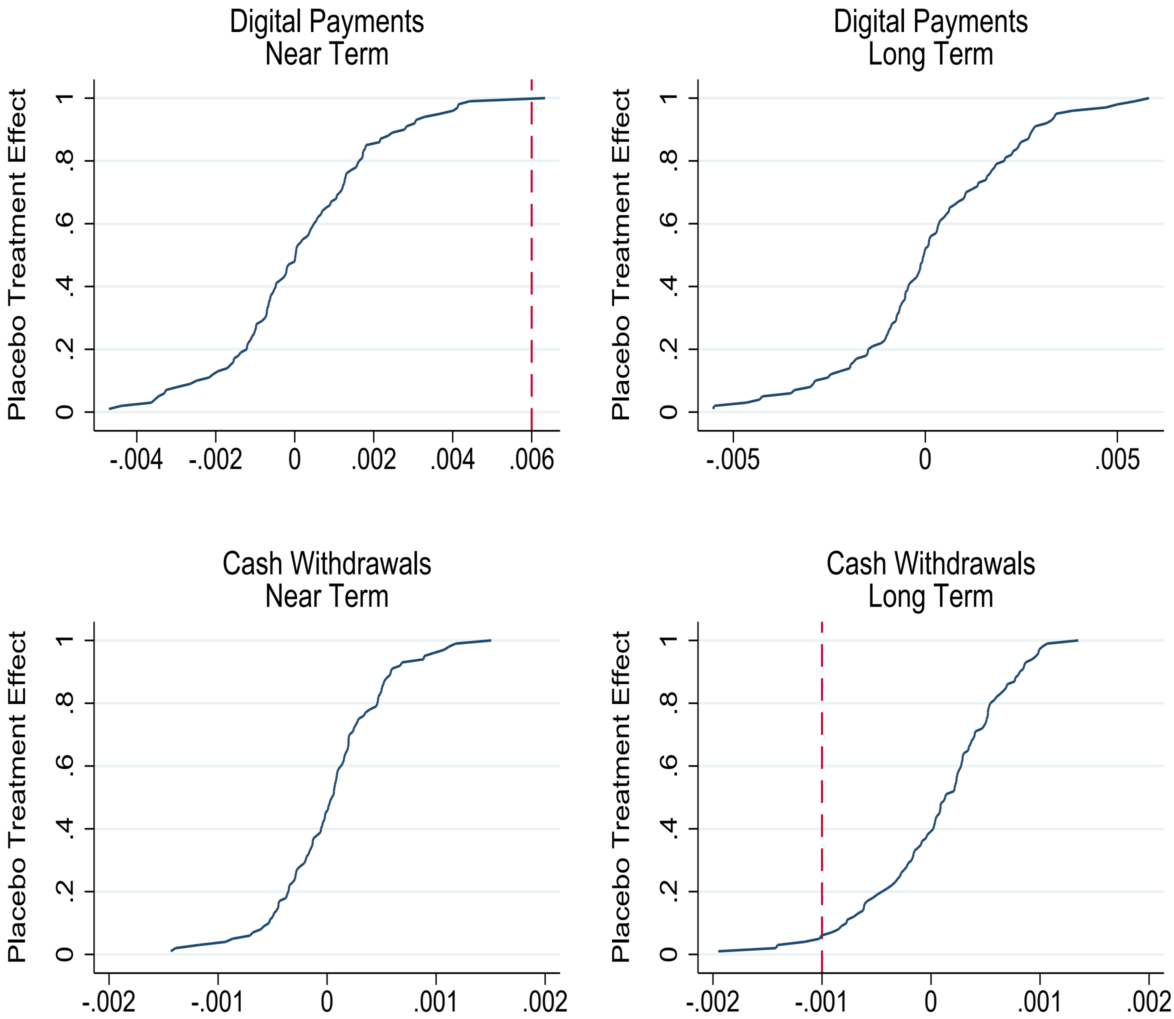
The above figures plot the pincode-level long-differences in cash withdrawals and digital transactions by distance to currency chest. The horizontal axis is divided into 25 1-km bins of pincodes' distance to the nearest currency chest. Each point on the figure represent the unconditional average difference (log) between the post-treatment value, and the pretreatment value, corresponding to pincodes located within that distance bin. Near-term refers to the logged difference between average pincode outcome values between September 2017 and January 2016; long-term refers to the logged difference in outcome values between March 2018 and January 2016.

Figure 5
Cash supply shock, cash withdrawals and digital transactions:
Event study plots



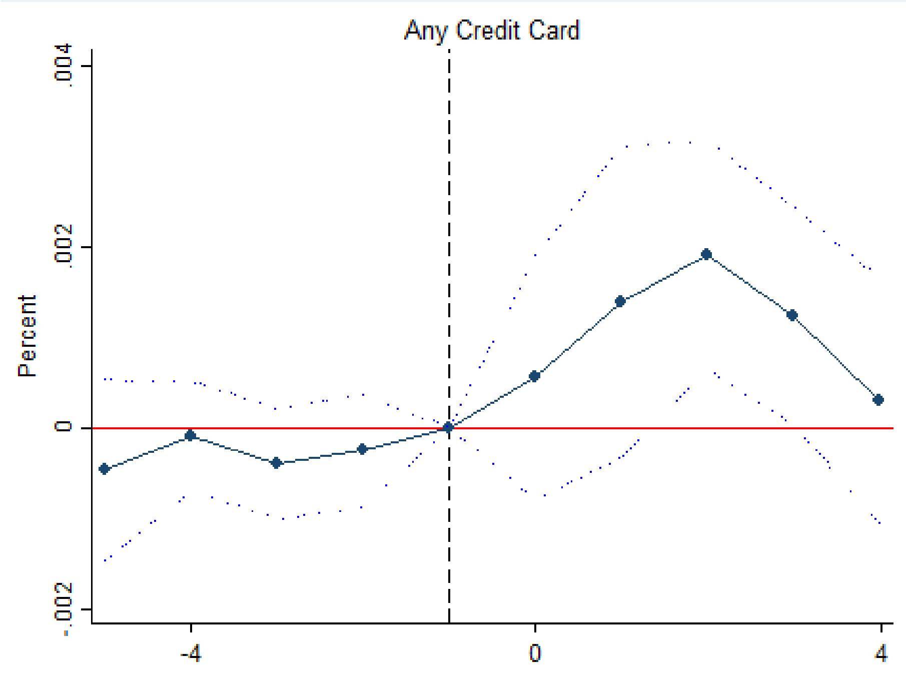
The above figures depict the average quarterly coefficients from an event-study design, regressing the outcome of interest on pincodes' distance to the nearest currency chest, interacted with an indicator for each quarter. The unit of observation is the pincode. The quarter prior to the demonetization intervention (June-September 2016, shown as the vertical dashed line) is taken as the reference period. The vertical line plots the coefficient values, while the dashed lines represent the 95% confidence intervals. All specifications include pincode and district-quarter fixed effects. Standard errors are clustered by pincode. Cash withdrawals is the volume of cash withdrawals from ATMs; digital transactions is the volume of digital payments undertaken through POS terminals.

Figure 6
Empirical CDF of placebo treatment rffects



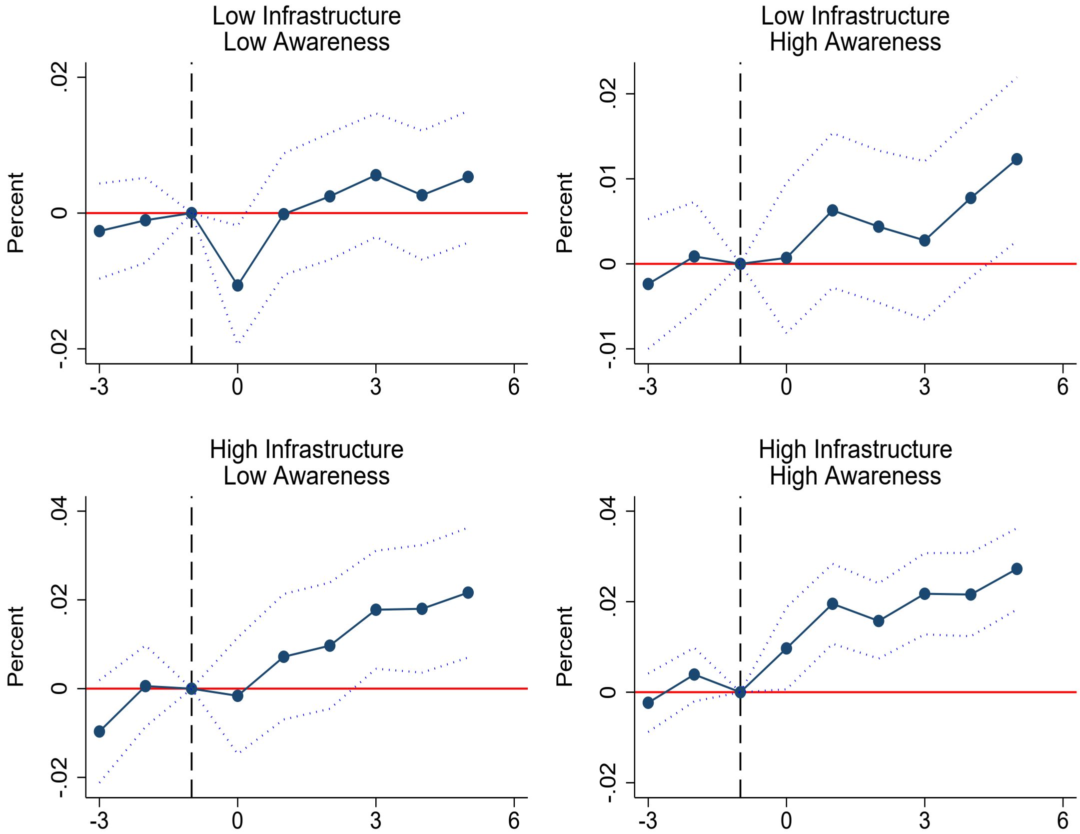
The above figures plot the empirical CDFs from a permutation-based placebo test where pincodes are randomly assigned to distances from currency chests. The baseline empirical specification in Equation (4) is then estimated using these "pseudo-distances" and the process is repeated 100 times. The red vertical line represents the "true" coefficient value corresponding to the actual distance of pincodes from currency chests.

Figure 7
Cash supply shock and household adoption of credit cards



The above figure shows the impact of the cash supply shock on households' adoption of credit cards. The unit of observation is the household; the outcome of interest is a dummy equaling one if the household has a credit card. The dashed vertical line represents the survey wave conducted between May and August 2016, the period prior to demonetization. The solid line represents the coefficients while the dashed lines represent the 95% confidence intervals. The regression includes household and state-survey wave fixed effects, in addition to district and household covariates. Household weights are included; standard errors are clustered by district.

Figure 8
Cash supply shock and digital payments:
Heterogeneity by financial infrastructure and household awareness



The above figures show the impact of the cash supply shock on digital payments, conditional on local financial infrastructure and regional awareness. The unit of observation is the pincode. The outcome of interest is (logged) digital payments. The top-left figure restricts the sample to pincodes with below-median scores on the infrastructure and awareness indices; the top-right figure restricts the sample to pincodes with below-median scores on the infrastructure index but above-median scores on the awareness index; the bottom-left figure restricts the sample to pincodes with above-median scores on the infrastructure index and below-median scores on the awareness index; the bottom-right figure restricts the sample to pincodes with above-median scores on both the infrastructure and awareness indexes. The quarter prior to the demonetization intervention (June-September 2016, shown as the vertical dashed line) is taken as the reference period. The vertical line plots the coefficient values, while the dashed lines represent the 95% confidence intervals. All specifications include pincode and district-quarter fixed effects. Standard errors are clustered by pincode.

Table 1
Cash supply shock, cash withdrawals, and digital transactions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. variable	log(Cash withdrawals)				log(Digital transactions)			
Dist_{CC} × Near term	-.007***	-.004***			.017***	.006***		
	(.000)	(.001)			(.002)	(.002)		
Dist_{CC} × Long term	-.003***	-.001			.024***	.013***		
	(.001)	(.001)			(.002)	(.002)		
High Dist_{CC} × Near term			-.067***				.115***	
			(.009)				(.031)	
High Dist_{CC} × Long term			-.007				.220***	
			(.011)				(.036)	
Dist_{CC} 0-5km × Near term								
Dist_{CC} 0-5km × Long term								
Dist_{CC} 5-10km × Near term				-.050***				.076**
				(.011)				(.037)
Dist_{CC} 5-10km × Long term				.010				.141***
				(.014)				(.043)
Dist_{CC} 10-15km × Near term				-.091***				.132***
				(.013)				(.042)
Dist_{CC} 10-15km × Long term				-.013				.250***
				(.014)				(.048)
Dist_{CC} 15-20km × Near term				-.110***				.206***
				(.014)				(.047)
Dist_{CC} 15-20km × Long term				-.024				.332***
				(.016)				(.054)
Dist_{CC} 20km+ × Near term				-.095***				.163***
				(.014)				(.048)
Dist_{CC} 20km+ × Long term				-.013				.309***
				(.018)				(.055)
Observations	104,789	104,789	104,789	104,789	101,803	101,803	101,803	101,803
R^2	.96	.96	.96	.96	.92	.92	.92	.92
Dep var mean	53.10	53.10	53.10	53.10	.48	.48	.48	.48

* $p < .10$, ** $p < .05$, *** $p < .01$.

This table estimates the impact of the treatment intervention on cash withdrawals and digital payments. The unit of observation is the pincode. The outcome of interest in columns 1–4 is the (logged) average volume of monthly cash withdrawals from ATMs in a quarter; in columns 5–8, the (logged) average volume of monthly digital payments undertaken through POS terminals. The sample is restricted to pincodes within 40 km of a currency chest (CC). *Near-term* refers to 4 quarters between October 2016 and September 2017; *Long-term* refers to 2 quarters between October 2017 and March 2018. *High* $\textit{Dist}_{CC}$ is a dummy equaling one if the pincode is located at a distance in excess of 11 km from a CC. Columns 4 and 8 test for nonlinearities in the treatment across 5-km bins of pincode-CC distances. All specifications include pincode and district-quarter fixed effects. Columns 2–4 and 5–8 also include pincode-specific time-varying covariates. Standard errors are in parentheses, clustered by pincode.

Table 2
Cash supply shock, cash withdrawals, and digital transactions: Robustness to alternate samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. variable	log(Cash withdrawals)				log(Digital transactions)			
	All pincodes	Exclude CC pincodes	Exclude metros	Shorter sample	All pincodes	Exclude CC pincodes	Exclude metros	Shorter sample
Dist_{CC} × Near term	-.004***	-.001	-.004***	-.004***	.004**	.016***	.005***	.009***
	(.001)	(.001)	(.001)	(.001)	(.002)	(.003)	(.002)	(.002)
Dist_{CC} × Long term	-.001	.001	-.001		.012***	.021***	.013***	
	(.001)	(.001)	(.001)		(.002)	(.003)	(.002)	
Observations	105,643	81,576	101,378	69,956	102,620	78,760	98,394	66,897
R^2	.96	.95	.96	.97	.92	.91	.92	.93

* $p < .10$, ** $p < .05$, *** $p < .01$.

This table shows the robustness of our baseline results to alternative sampling choices. The unit of observation is the pincode. The outcome of interest in columns 1–4 is the (logged) average volume of monthly cash withdrawals from ATMs in a quarter; in columns 5–8, the (logged) average volume of monthly digital payments undertaken through POS terminals. All specifications include pincode and district-quarter fixed effects, along with pincode-specific time-varying covariates. *Near-Term* refers to 4 quarters between October 2016 and September 2017; *Long-Term* refers to 2 quarters between October 2017 and March 2018. Columns 1 and 5 include all pincodes in our sample; columns 2 and 6 exclude pincodes which have a CC located in them; columns 3 and 7 exclude six major metropolitan districts; columns 5 and 8 restrict the sample to the quarter ending in June 2017, prior to the introduction of the Goods and Services Tax. Standard errors are in parentheses, clustered by pincode.

Table 3
The role of financial infrastructure

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable	log(Digital transactions)					
	Pincode measure			District measure		
Dist_{CC} × Near term	-.005**	.004*	-.002	.000	-.002	.005**
	(.002)	(.002)	(.002)	(.002)	(.003)	(.002)
Dist_{CC} × Long term	.001	.010***	.004	.007**	.006**	.011***
	(.003)	(.002)	(.003)	(.003)	(.003)	(.002)
Dist_{CC} × High POS × Near term	.026***					
	(.003)					
Dist_{CC} × High POS × Long term	.022***					
	(.004)					
Dist_{CC} × High branch × Near term		.006*				
		(.003)				
Dist_{CC} × High branch × Long term		.010***				
		(.004)				
Dist_{CC} × High ATM × Near term			.022***			
			(.003)			
Dist_{CC} × High ATM × Long term			.023***			
			(.004)			
Dist_{CC} × High POS × Near term				.013***		
				(.003)		
Dist_{CC} × High POS × Long term				.014***		
				(.004)		
Dist_{CC} × High branch × Near term					.015***	
					(.003)	
Dist_{CC} × High branch × Long term					.014***	
					(.004)	
Dist_{CC} × High ATM × Near term						.004
						(.003)
Dist_{CC} × High ATM × Long term						.007*
						(.004)
Observations	101,803	101,803	101,803	101,794	101,803	101,794
R^2	.92	.92	.92	.92	.92	.92

* $p < .10$, ** $p < .05$, *** $p < .01$.

This table estimates treatment heterogeneity across pincode and district measures of financial infrastructure. The unit of observation is the pincode. The outcome of interest is (logged) average volume of monthly digital payments undertaken through POS terminals. The sample is restricted to pincodes within 40 km of a currency chest (CC). *Near-term* refers to 4 quarters between October 2016 and September 2017; *Long-term* refers to 2 quarters between October 2017 and March 2018. Columns 1–3 considers pincode-level heterogeneity; columns 4–6 consider district-level heterogeneity. *High POS* is a dummy equaling one for pincodes/districts where the per capita POS terminals in the pincode/district exceed the national median. *High branch* is a dummy equaling one for pincodes/districts where the per capita bank branches in the pincode/district exceed the national median. *High ATM* is a dummy equaling one for pincodes/districts where the per capita ATM terminals in the pincode/district exceed the national median. All specifications include pincode and district-quarter fixed effects along with pincode-specific time-varying covariates. Standard errors are in parentheses, clustered by pincode.

Table 4
Cash Supply Shock and Digital Payments: Heterogeneity by Regional Characteristics

A: Volume

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. variable	Log(Digital transactions: Volume)									
	β	θ_k								
	DiD	Fin. infra.	HH fin. incl.	Ent. fin. incl.	Phy. infra.	Educ.	Aware	Trust	Network	Informal
Near term	.006**	.008***	.003	.000	.003	-.007**	.006**	.006*	-.001	-.011***
	(.003)	(.003)	(.003)	(.003)	(.004)	(.003)	(.003)	(.004)	(.003)	(.004)
Long term	.014***	.008***	.005	-.003	-.003	-.004	.006**	.011**	.005	-.010**
	(.003)	(.003)	(.004)	(.004)	(.004)	(.004)	(.003)	(.004)	(.004)	(.004)
Observations	68,718	68,718	68,718	68,718	68,718	68,718	68,718	68,718	68,718	68,718
R^2	.92	.92	.92	.92	.92	.92	.92	.92	.92	.92

B: Count

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. variable	log(Digital transactions: Counts)									
	β	θ_k								
	DiD	Fin. infra.	HH fin. incl.	Ent. fin. incl.	Phy. infra.	Educ.	Aware	Trust	Network	Informal
Near term	.011***	.007***	.004	.002	.002	.000	.006***	.005	-.001	-.010***
	(.002)	(.002)	(.003)	(.003)	(.004)	(.003)	(.002)	(.003)	(.003)	(.003)
Long term	.016***	.006**	.007**	-.001	-.005	.003	.005*	.005	-.001	-.008**
	(.003)	(.002)	(.003)	(.004)	(.004)	(.003)	(.003)	(.004)	(.003)	(.004)
Observations	68,718	68,718	68,718	68,718	68,718	68,718	68,718	68,718	68718	68,718
R^2	.92	.92	.92	.92	.92	.92	.92	.92	.92	.92

This table identifies heterogeneity across regional characteristics in the adoption of digital payments in response to the cash supply shock. The unit of observation is the pincode. The outcome of interest in panel A is the volume of digital payments (logged); in panel B, the number of digital transactions undertaken (logged). The results are based on estimating specification (7). Column 1 in both panels correspond to the double difference β coefficient in specification (7); columns 2-10 correspond to the triple-interaction θ_k coefficients in specification (7). The column headers note the characteristic of interest. *Fin. infra.* is the financial infrastructure index. *HH. fin. incl.* is the financial inclusion index for households; *Ent. fin. incl.* is the financial inclusion index for informal enterprises; *Phy. infra.* is the physical infrastructure index; *Educ.* is the local education index; *Aware* is the awareness index; *Trust* is the trust index; *Network* is the network index; and *Informality* is the informality index. See Internet Appendix B1 for more detail on the indices. All specifications include pincode and district-quarter fixed effects, along with pincode-specific covariates. Standard errors are in parentheses, clustered by pincode.

Table 5
Cash supply shock and credit card adoption

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. variable				Pr(Credit card = 1)				
$Dist_{CC}$ × Near term	.002***	.001**	.001	.001	.001**	.001	.001	.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
$Dist_{CC}$ × Long term	.001*	.000	-.000	-.000	.000	.001	.001	.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
$Dist_{CC}$ × Any life ins. × Near term			.001***					
			(.000)					
$Dist_{CC}$ × Any life ins. × Long term			.001***					
			(.000)					
$Dist_{CC}$ × Any ret. save × Near term				.003***				
				(.001)				
$Dist_{CC}$ × Any ret. save × Long term				.006***				
				(.001)				
$Dist_{CC}$ × Std. network × Near term					.000			
					(.001)			
$Dist_{CC}$ × Std. network × Long term					-.001			
					(.001)			
$Dist_{CC}$ × High branch × Near term						-.000		
						(.001)		
$Dist_{CC}$ × High branch × Long term						-.002*		
						(.001)		
$Dist_{CC}$ × High POS × Near term							.000	
							(.001)	
$Dist_{CC}$ × High POS × Long term							-.001	
							(.001)	
$Dist_{CC}$ × High ATM × Near term								.001
								(.001)
$Dist_{CC}$ × High ATM × Long term								-.001
								(.001)
Observations	1,089,151	1,089,151	1,088,320	1,088,320	888,280	1,089,151	1,089,151	1,089,151
R^2	.58	.58	.58	.58	.60	.58	.58	.58
Dep var mean	.03	.03	.03	.03	.03	.03	.03	.03
Covariates	N	Y	Y	Y	Y	Y	Y	Y

* $p < .10$, ** $p < .05$, *** $p < .01$

This table estimates the impact of the cash supply shock on households' likelihood of adopting credit cards. The unit of observation is the household. The outcome variable is a dummy equaling one if the household has a credit card. $Dist_{CC}$ is measured at the level of the district, as the median value of $Dist_{CC}$ across all pincodes located in the district. All specifications include household and state-time fixed effects. No other covariates are included in column 1; columns 2–9 include household and district-specific covariates. *Any life ins.* is a dummy equaling one if any household member subscribed to a life insurance policy in the period prior to demonetization; *Any ret. save.* is a dummy equaling one if any household member had an employer-sponsored retirement savings account (provident fund) in the period prior to demonetization. *Std. network* is the equally weighted average of seven standardized indexes capturing the strength of local social networks. *High branch*/*High POS*/*High ATM* is a dummy equaling one if the district's per capita bank branch/POS terminal/ATM terminal density exceeds the median bank branch/POS terminal/ATM terminal density across all districts. All specifications are weighted using household-specific weights. Standard errors are clustered by district.

Cash is King:

The Role of Financial Infrastructure in Digital Adoption

Internet Appendix

A1 Regional Cash Dependence

This section details our measure for regional cash dependency. Administrative data on the volume of transactions undertaken by households and firms in cash as a fraction of their overall transactions is limited. To this effect, we combine administrative data on digital payments with survey data on household consumption to gauge regional cash dependency prior to demonetization. The fundamental assumption in this exercise is that households' consumption is undertaken using either cash, or credit/debit cards, and limited consumption is undertaken through cheques, bank transfers and mobile payments.

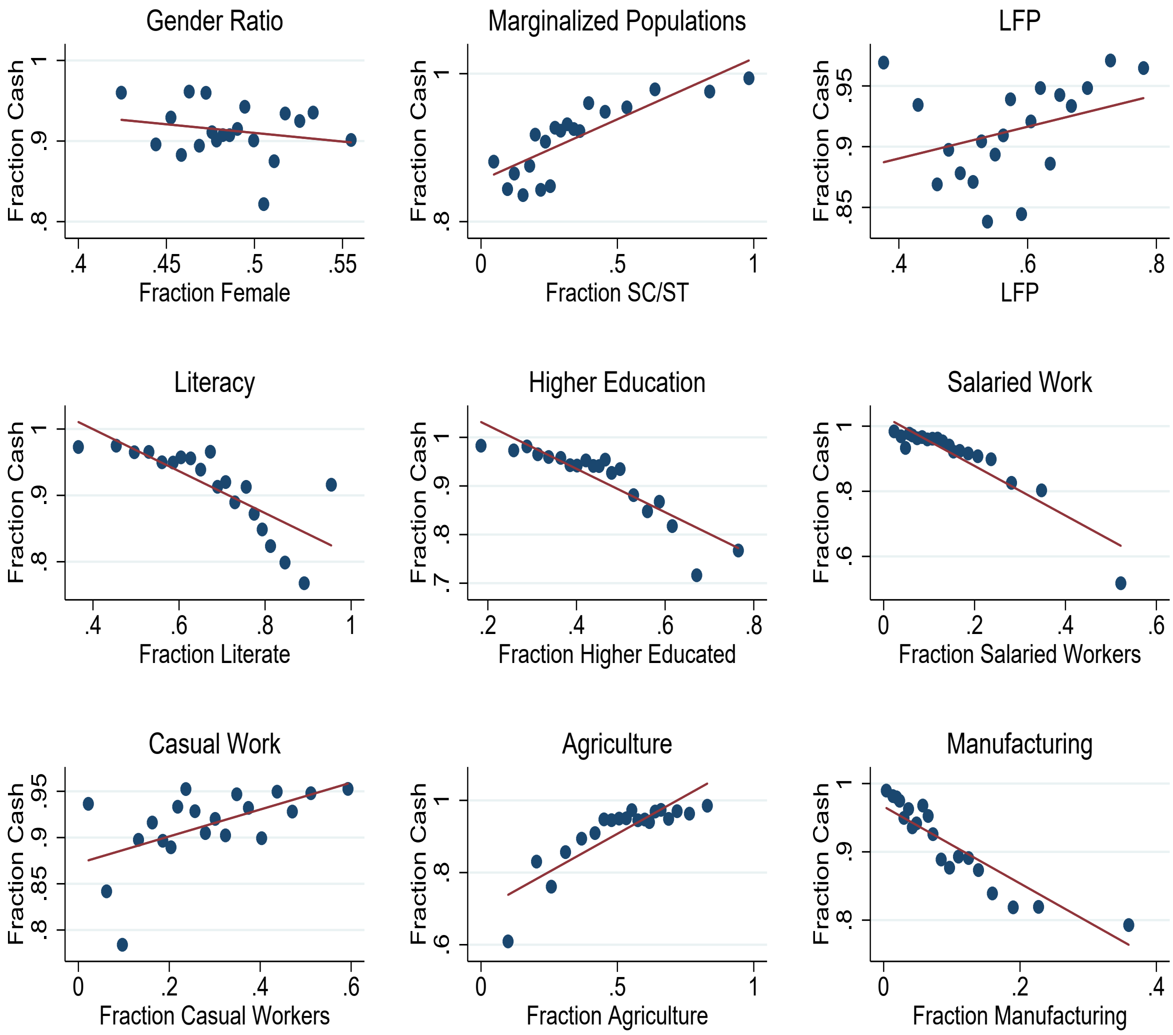
The last nationally representative detailed household consumption survey in India was undertaken in 2011-12. In 2014-15, a nationally representative survey was undertaken to gauge households spending on education but also enquired about households' total monthly expenditure. Both surveys identify households at the level of districts, which permits us to estimate district-level aggregate consumption in 2011-12 and 2014-15 by applying the appropriate household weights. We estimate the district-level growth in household consumption between 2011-12 and 2014-15 and apply this to compute aggregate consumption in 2015-16 for district d – $Cons_{d,2016}$.[50]

Next, we use the administrative data on transactions undertaken using RuPay PoS cards to estimate the use of digital payments. As pincodes can be mapped to districts, this gives us the monthly volume of payments undertaken through credit and debit cards in a district. For each district, we average over the first 10 months of 2016 (pre-demonetization) the volume of payments undertaken using RuPay credit and debit cards. The second challenge in estimating regional cash dependency is that RuPay card transactions in our administrative data account for approximately 4% of the total transactions undertaken using debit cards through PoS terminals in the pre-demonetization period. We address this in the spirit of Mian, Rao, and Sufi (2013), assuming that the volume of digital payments in a region as a fraction of the national average is uniform across card providers.[51] Under this assumption, we use the data on aggregate national payments undertaken using credit and debit cards to impute the volume of transactions undertaken using credit and debit cards in each district. Subsequently, regional cash dependency, based on household consumption, is defined as: $CashDep_d = \frac{Consumption_{d,2016} - PoS_{d,2016}}{Consumption_{d,2016}}$, where $PoS_{d,2016}$ is the imputed volume of PoS transactions undertaken using credit and debit cards in the district.

[50] This assumes that the district-level consumption growth was equal between 2011-12 and 2014-15, and 2014-15 and 2015-16.

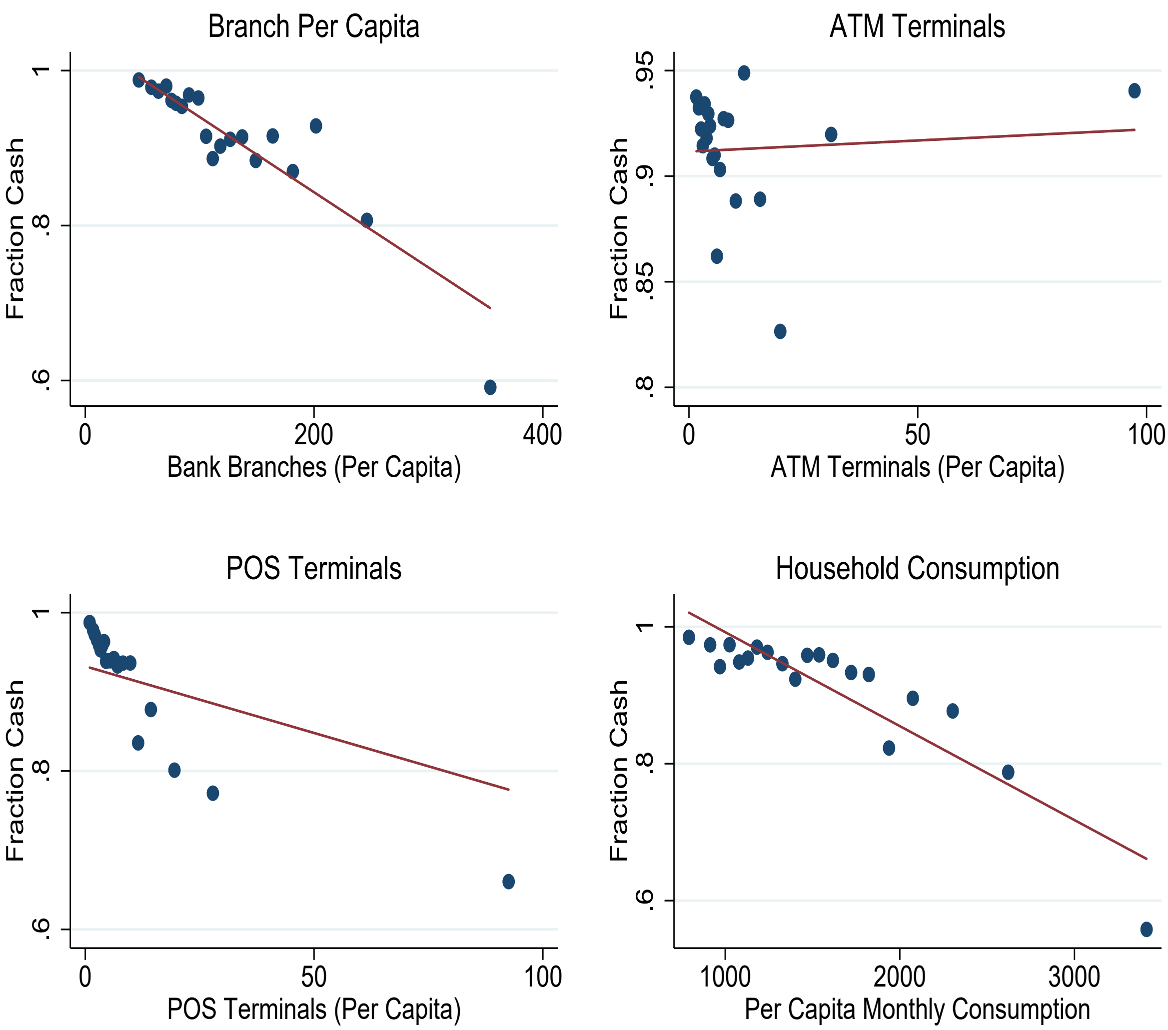
[51] Thus, if district d accounts for x% of national transactions using RuPay cards, it would also account for x% of national transactions using all other card providers such as Visa and Master Card.

Figure A1
Correlates of Regional Cash Dependency



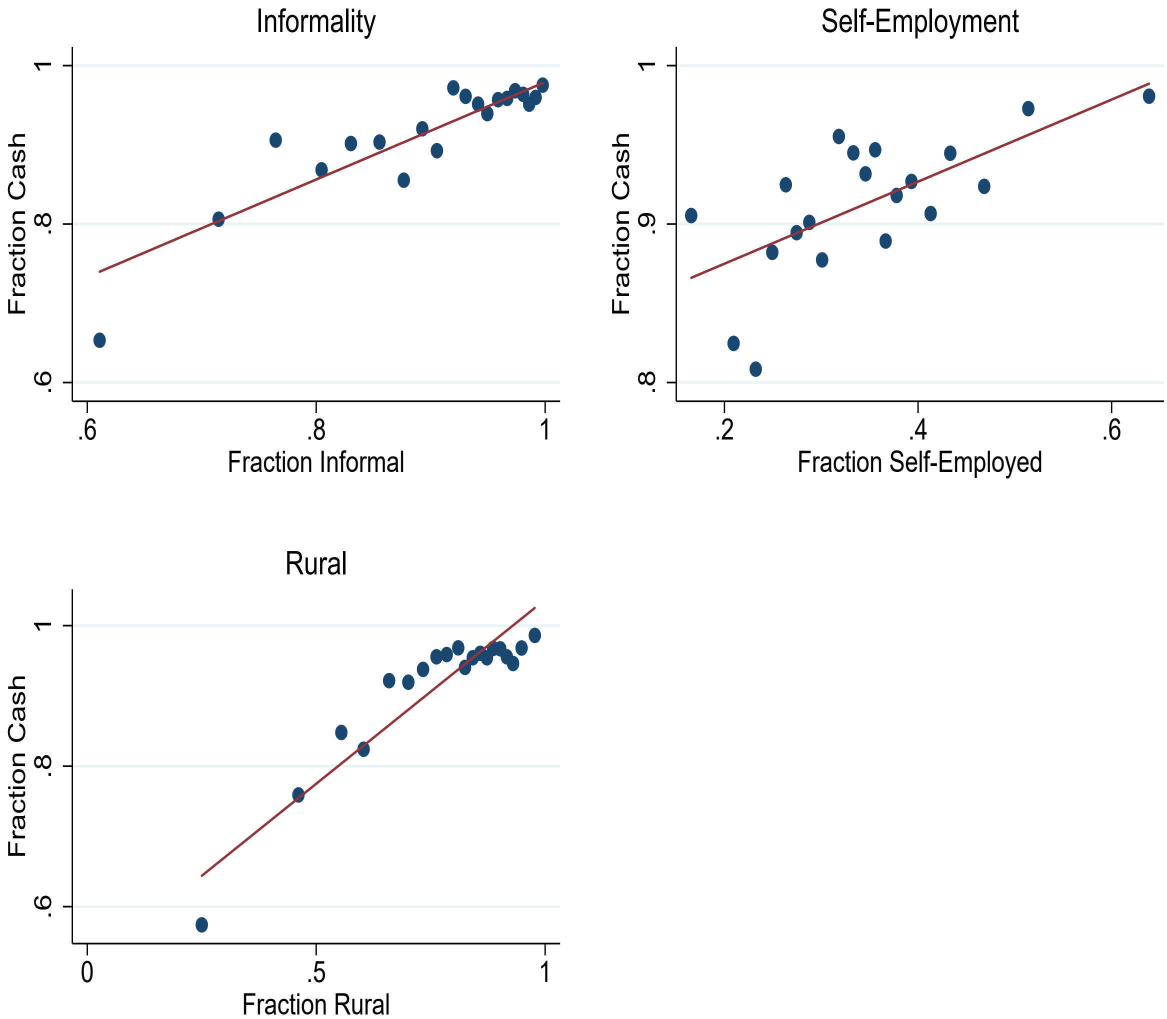
Notes: The above figures show the correlation between district-level informality and other district-characteristics. The y-axis in all figures is regional cash dependency in the district. *Higher Education* refers to adults having completed secondary or higher education. Demographic and labour force characteristics are from the NSS employment-unemployment survey, 2011-12.

Figure A2
Correlates of Regional Cash Dependency



Notes: The above figures show the correlation between district-level informality and other district-characteristics. The y-axis in all figures is regional cash dependency in the district. *Education* refers to adults having completed secondary education or better.

Figure A3
Urbanization, Informality and Regional Cash Dependency



Notes: The above figures show the correlation between district-level informality and other district-characteristics. The y-axis in all figures is regional cash dependency. *Informality* is measured as the share of workers in district working in establishments hiring less than 10 (20) workers if (not) using electricity. The district characteristics are computed using data from the NSS employment-unemployment survey, 2011-12.

B1 Standardized Indices

We describe here the individual components used to construct the indices used in Section 4 to identify heterogeneity in the adoption of digital payments. All indices are defined as the equally weighted average of individual standardized sub-indices:

$$StdIndex_d^k = \frac{1}{n}\sum_{j=1}^{n} StdIndex_d^j \tag{9}$$

where k denotes the characteristic of interest, and j, the individual components comprising the index. d is the district (pincode) for which the index is being constructed. n is the number of individual components used for constructing $StdIndex_d^j$. $StdIndex_d^j$ is defined as:

$$StdIndex_d^j = \frac{x_d^j - \mu^j}{\sigma^j} \tag{10}$$

where x is the district's (pincode's) score corresponding to characteristic k, μ is the nationwide average, and σ, the standard deviation. As each $StdIndex_d^j$ is a standardized index with mean 0 and standard deviation 1, $StdIndex^k$ is also mean 0.

B1.1 Financial Infrastructure Index

This is the only index computed at the pincode level and comprises of three sub-indices: namely the standardized index for per capita PoS terminals, per capita bank branches, and per capita ATM terminals. While the data for PoS and ATM terminals are sourced from the NPCI, the local bank branch density is obtained by mapping bank branches to pincodes using the publicly available MOF file containing addresses of bank branches in operation in 2018.

B1.2 Household Financial Inclusion Index

This index aims to capture the extent of financial inclusion amongst households and is comprised of three sub-indices: namely the fraction of households in a district who have a credit card; the fraction of households who have an employee provident fund account; and the fraction of households with a life insurance. Employee provident funds are defined contribution retirement accounts where employers can make matching contributions. These accounts are typically available to workers in salaried occupations. The data for constructing this index is obtained from the CPHS.

B1.3 Enterprise Financial Inclusion Index

This index measures the extent of financial inclusion amongst unincorporated micro-enterprises in the district. The data is sourced from the Survey of Unincorporated Enterprises conducted by the NSS in 2015-16. The survey excludes from its sampling design any formally registered manufacturing or service enterprise. We specifically choose this survey as registered enterprises have a higher likelihood of maintaining formal accounts, bank accounts, or using the internet for their activities. To this effect, we contend that financial inclusion for informal enterprises serves as a lower bound for regional financial inclusion. This index has four sub-indices: namely whether the enterprise or the enterprise owner has a bank account; whether the enterprise maintains formal accounts; whether the enterprise uses computers or the internet for its operations; and whether the enterprise is registered with local authorities.

B1.4 Education Infrastructure Index

This index tracks the local higher educational inputs in the form of schools and colleges, which can contribute towards regional financial literacy. The data is obtained from the Census 2011, and we obtain the per capita measures of high schools, colleges and vocational training institutions in a district.

B1.5 Physical Infrastructure Index

This index quantifies the level of local physical infrastructure and is comprised of 8 sub-indices – namely, the fraction of villages in the district with a) a bus service; b) a taxi service; c) a post office; d) a sub-post office; e) cell phone coverage; f) internet connectivity; g) major road connectivity. We also include the average hours of power supply received during the summer. The raw data for each variable except for power supply is reported as a dummy equaling 1 if the village has the facility noted above. We consider villages as most urban centres would report a value of 1 along the extensive margin, and there's no corresponding information along the intensive margin for any of these variables. All the variables used to construct this index are sourced from the 2011 population Census.

B1.6 Awareness Index

The awareness index provides a measure of overall household awareness in a region. We use three measures of awareness: the fraction of households where someone regularly reads the newspaper; the fraction of households where a household member regularly listens to the radio; and the fraction of households where someone watches television in excess of 2 hours per day. The data is obtained from the Indian Human Development Survey, 2011-12.

B1.7 Trust Index

The trust index captures regional trust in public institutions. There are four components to this index: namely, a) the fraction of households in a district reporting a high level of confidence in politicians; b) the fraction of households reporting a high level of confidence in the police; c) the fraction of households reporting a high level of confidence in the government; d) the fraction of households reporting a high level of confidence in banks. The data is obtained from the Indian Human Development Survey, 2011-12.

B1.8 Network Index

The networks index captures the strength of social networks. We use 7 sub-indices to caputure this. Namely: a) the fraction of households where at least one member was part of a self-help group; b) the fraction of households where at least one member was part of a religious group; c) the fraction of households where at least one member was part of a caste-based group; d) the fraction of households where at least one member had participated in a political rally in the past year; e) the fraction of households where at least one member was part of a local women's group; f) the fraction of households where at least one member was part of a local savings or credit group; g) the fraction of households where at least one member attended a public event. The data is obtained from the Indian Human Development Survey, 2011-12.

B1.9 Informality Index

The informality index captures the degree of informality in the area using three measures of informality. We use the fraction of rural population in a district; the fraction of self-employed workers in a district; and the fraction of workers who report working in establishments hiring less than 20 workers (less than 10 if using electricity). The data is obtained from the NSS household employment-unemployment survey conducted in 2011-12.

Table B1
Standardized Indices: Summary Statistics

	N	Mean	Std.Dev.	Max.	Min.
	(1)	(2)	(3)	(4)	(5)
Fin. Infra. Index	12961	-.000	.816	20.674	-.486
Informality Index	570	.000	.756	2.084	-3.017
Phy. Infra. Index	558	.000	.681	2.085	-2.001
Educ. Infra. Index	558	.000	.648	5.468	-.872
Ent. Fin. Incl. Index	577	.000	.729	3.314	-1.253
HH. Fin. Incl. Index	379	.000	.750	2.971	-1.300
Awareness Index	352	-.000	.683	2.370	-1.510
Trust Index	352	.000	.541	2.189	-1.671
Network Index	352	.000	.628	2.731	-0.759

Notes: This table shows the summary statistics pertaining to the indices used in the paper. *Fin. Infra.* is the financial infrastructure index. *HH. Fin. Incl.* is the financial inclusion index for households; *Ent. Fin. Incl.* is the financial inclusion index for informal enterprises; *Phy. Infra.* is the physical infrastructure index; *Educ.* is the local education index; *Aware* is the awareness index; *Trust* is the trust index; *Network* is the network index; and *Informality* is the informality index.

Table B2
Unconditional Correlation Between Indices

	Fin. Infra.	HH. Fin. Incl.	Ent. Fin. Incl.	Phy. Infra.	Educ.	Awareness	Trust	Networks	Informality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Fin. Infra.	1.000								
HH. Fin. Incl.	.203	1.000							
Ent. Fin. Incl.	.210	.369	1.000						
Phy. Infra.	.130	.015	.260	1.000					
Educ.	.246	.410	.480	.528	1.000				
Awareness	.139	.163	.220	.309	.412	1.000			
Trust	-.039	-.120	-.025	-.149	.052	.033	1.000		
Networks	.086	.098	.182	.647	.412	.195	-.071	1.000	
Informality	-.248	-.463	-.390	-.211	-.466	-.277	.143	-.064	1.000
Observations	77252	77252	77252	77252	77252	77252	77252	77252	77252

Notes: This table presents the unconditional correlations across the 9 indices in our data. *Fin. Infra.* is the financial infrastructure index. *HH. Fin. Incl.* is the financial inclusion index for households; *Ent. Fin. Incl.* is the financial inclusion index for informal enterprises; *Phy. Infra.* is the physical infrastructure index; *Educ.* is the local education index. The unit of observation is the pincode.

C1 Cash Supply Shock, Household Portfolio Choices, Borrowings, and Consumption

This section rules out that the increase in digital payments in response to the cash shortage induced by demonetization was not driven by changes in households' portfolio choices, improved access to credit, or changes in household consumption. At the outset, it is worth noting that in the absence of transaction costs and equal access to technology, the temporary unavailability of cash should not have affected economic outcomes if firms and households were able to seamlessly switch to non-cash modes of payments. The presence of transaction costs and barriers to accessing alternate payment systems can affect households' economic activity, especially in areas with high cash dependency and low penetration of alternate technologies. This can dampen economic activity and *reduce* households' likelihood to undertake digital payments. Thus, the increase in digital payments identified in Section 3.1 is possibly an underestimate of the treatment effect if demonetization negatively affected households' income or consumption. We provide empirical evidence in this section that changes in households' portfolio choices, borrowings or consumption could not have explained the increase in digital payments in response to demonetization.

We first examine whether the policy intervention affected household balance sheets, which could have affected incomes and consumption. Households could replace their existing currency with new currency through bank visits. A lower pace of currency replenishment in areas located farther from CCs could have warranted a larger number of bank visits. If these visits increased households' familiarity with savings instruments, either through interactions with individual bankers or general information disseminated in bank branches, it could have affected households' portfolio choices. A reallocation of household portfolios or repeated interactions with bank officials can increase digital payments, either through higher incomes and consumption, or improved financial literacy. Similarly, increased bank visits could also have improved households' access to credit, which could also have boosted consumption, and positively affected digital payments.

In this regard, we use the CPHS data to identify the impact of demonetization on household portfolio choices and borrowings. The CPHS enquires whether households undertook any investment across both risky and risk-free financial assets in the four months prior to the survey. The survey also enquires about any outstanding debt held by households and disaggregates debt across bank and non-bank sources. As the CPHS does not provide pincode identifiers, we aggregate $Dist_{CC}$ to the level of the district by computing the median pincode-CC distance across all pincodes in a district. We use this aggregated distance measure to identify the treatment's impact on household outcomes. As the CPHS only reports household portfolio choices and borrowings along the extensive margin, we estimate linear probability models with household and state-survey wave fixed effects. Household weights provided by the CPHS are included in all the regressions, and the standard errors are clustered by district.

The results in Appendix Table C1 find no evidence suggesting that demonetization increased households' participation in financial assets or improved their access to credit. If anything, the coefficients suggest a reduction in households' propensity to invest in risk-free assets [columns (1), (3) and (4)] in areas with high exposure to the cash supply shock. This is consistent with the policy of demonetization being equivalent to a liquidity shock and negatively affecting household investments. There is also no impact on households' likelihood of outstanding borrowings, either from bank or non-bank sources.

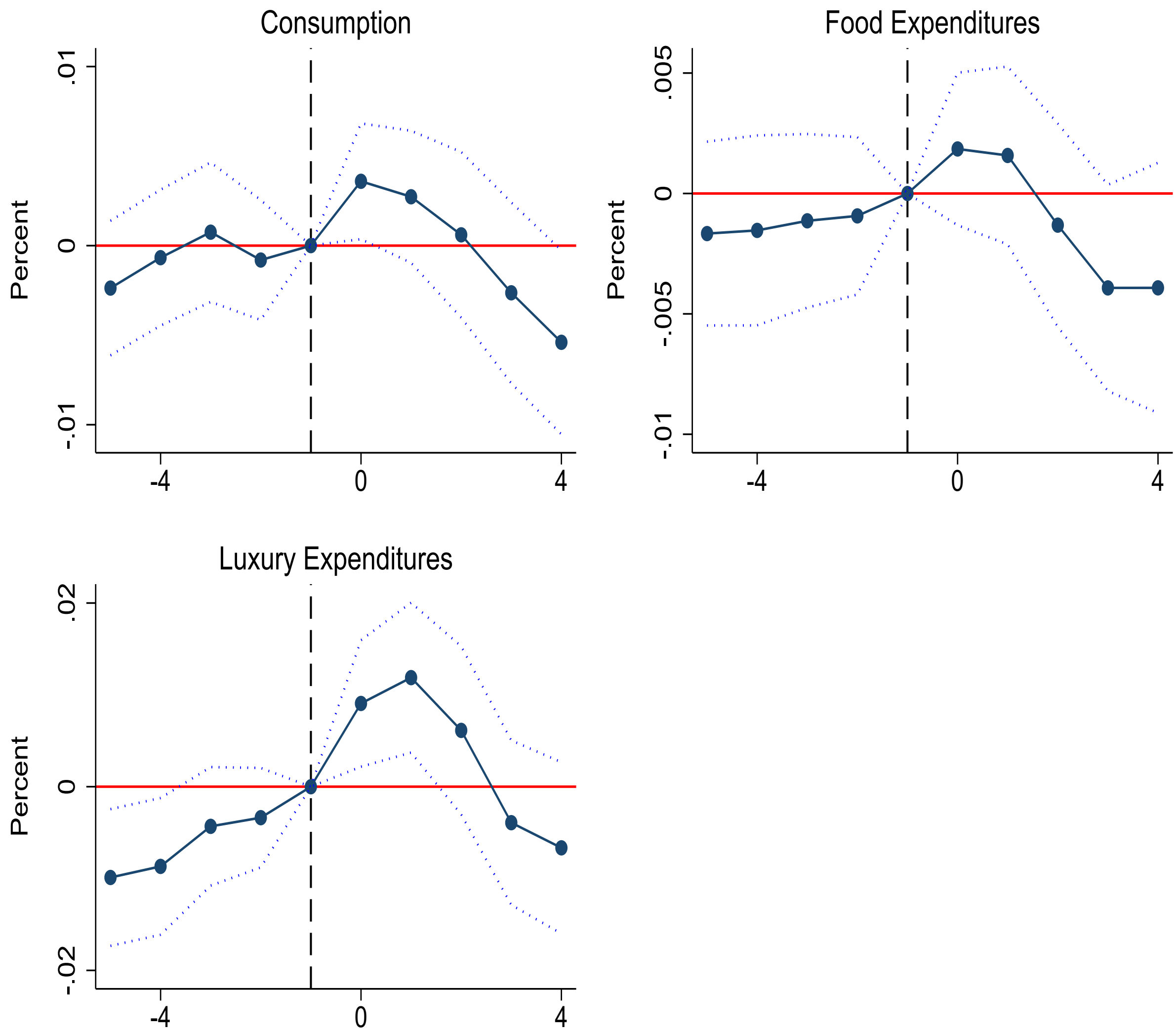
Next, we use information on households' monthly per capita consumption from the CPHS to directly identify the impact of the cash supply shock on household consumption. A possible confounder to our findings is that the government could have anticipated the economic disruptions wrought about by delays in cash replenishment and provided temporary income support to affected households. While no explicit income support policy was effected during this period, the government did expand the rural workfare scheme, guaranteeing low-skilled work opportunities to rural households in exchange for a daily wage.[52] If the government correctly identified areas where cash replenishment was slower – namely areas located farther from centres of cash – and targeted workfare programs to these areas as a form of income support, it could have led to increased consumption, which in turn could have affected digital payments.

Appendix Table C2, however, offers limited evidence of a positive treatment effect on household consumption. On the contrary, consistent with the disruptive nature of demonetization, we see that the treatment negatively affected household consumption over the long term, driven by a reduction in spending on essential items [columns (1)-(4)]. Columns (5)-(6) finds an increase in household spending of luxury items during the near term, which subsequently dissipates over the long term.

The results in Appendix Table C2 are confirmed by event study plots in Appendix Figure C1. Here, we identify an increase in monthly per capita expenditure of households in the quarter of demonetization, followed by a monotonic decline across all subsequent quarters. Consistent with the DiD coefficient, the event-study figure identifies a negative and statistically significant coefficient in the final CPHS survey wave. Compared to the event study plots identifying the treatment effect on cash withdrawals and digital payments (Figure 5), we see that for at least two quarters – namely the first and second quarters succeeding the treatment – cash withdrawals were significantly lower in pincodes located farther from CCs, while digital payments continued to remain significantly higher. However, households' consumption did not vary in these two quarters as a function of their distance to CCs, suggesting that an aggregate increase in household consumption is unlikely to explain the increase in digital payments for areas with higher exposure to the cash supply shock.

[52]This is the Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS), which provides an employment guarantee of 100 days of work per calendar year for each rural household. The MGNREGS wages often serve as the wage floor in rural India.

Figure C1
Cash Supply Shock and Household Consumption: Event-Study Plots



Notes: The above figure shows the impact of the cash supply shock on households' monthly per capita consumption. The unit of observation is the household, the outcome of interest (logged) per capita monthly consumption. The top-left panel considers all expenditures; the top-right panel considers essential spending; the bottom-left panel considers luxury expenditures. The dashed vertical line denotes the survey wave conducted between May and August 2016 – the period prior to demonetization. The solid line shows the coefficients; the dashed lines the 95% confidence intervals. The regression includes household and state-survey wave fixed effects, in addition to district and household covariates. Household weights are included; standard errors are clustered by district.

Table C1
Cash Supply Shock and Household Investment and Borrowing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. Variable	Any Investment In						Any Borrowing From		
	Risk-Free Asset	Risky Asset	Any Fin. Asset	Multiple Fin. Asset	Gold Real Estate	Durable Asset	Any Source	Banks	Informal Sources
Dist_{CC} × Near Term	-.005**	-.000	-.005**	-.003***	-.001	-.000	-.000	.001	-.001
	(.002)	(.000)	(.002)	(.001)	(.001)	(.001)	(.002)	(.001)	(.001)
Dist_{CC} × Long Term	-.006**	-.000	-.006**	-.003***	.000	-.001	-.001	.000	.001
	(.003)	(.000)	(.003)	(.001)	(.002)	(.001)	(.002)	(.001)	(.002)
Observations	1089151	1089151	1089151	1089151	1089151	1089151	1089151	1089151	1089151
R^2	.44	.20	.44	.41	.34	.21	.48	.34	.39
Dep Var Mean	.21	.00	.21	.06	.08	.04	.09	.03	.05

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

Notes: This table estimates the impact of a shock to cash supply on households' investment and borrowings along the extensive margin. The unit of observation is the household. The outcome variable in each instance is a binary. The outcome variables in columns (1)-(6) pertain to household investments; in columns (7)-(9), household borrowings. Multiple financial assets is a dummy which equals 1 if the household invested across multiple financial instruments. Investment in durable asset is a dummy equaling 1 if the household has purchased any durable asset in the 4 months preceding the survey. $Dist_{CC}$ is measured at the level of the district, as the median value of $Dist_{CC}$ across all pincodes located in the district. All specifications include household and state-time fixed effects, along with household and district-specific covariates. All specifications are weighted using household-specific weights. Standard errors are clustered by district.

Table C2
Cash Supply Shock and Household Consumption

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable	Per Capita Monthly Household Consumption (Log)					
	Total		Essential		Luxury	
Dist_{CC} × Near Term	.002	.001	-.001	-.000	.012***	.013***
	(.002)	(.002)	(.002)	(.002)	(.004)	(.004)
Dist_{CC} × Long Term	-.004*	-.005**	-.005**	-.005**	-.002	-.001
	(.002)	(.002)	(.002)	(.002)	(.004)	(.004)
Observations	1089151	1089151	1089151	1089151	1089151	1089151
R^2	.71	.72	.71	.72	.59	.60
Dep Var Mean	2037.36	2037.36	866.32	866.32	296.14	296.14
Covariates	N	Y	N	Y	N	Y

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

Notes: This table estimates the impact of the cash supply shock on households' per capita monthly consumption. The unit of observation is the household. The outcome variable in each instance is logged. Consumption on essential commodities include food expenditures, expenditures on education, health and clothing. Luxury expenditures include food eaten outside home, spending on alcohol, tobacco and packaged foods. $Dist_{CC}$ is measured at the level of the district, as the median value of $Dist_{CC}$ across all pincodes located in the district. All specifications include household and state-time fixed effects. Columns (2), (4) and (6) also include household and district-specific covariates. All specifications are weighted using household-specific weights. Standard errors are clustered by district.

D1 Adoption in Areas with High Informality

Section 1.1 documented an unconditional positive correlation between regional cash dependency and informality while Section 2.3 showed that areas located farther from centres of cash were also likely to have higher informality. This suggests that regions with high informality – namely low urbanization, a high share of self-employment, and a high share of informal workers – are likely to be adversely affected by the cash supply shock. As the informal sector is characterized by low-skilled workers (see Appendix Table E1), limited social networks and incomplete credit and insurance markets, we would expect households and enterprises operating in the informal sector to have limited shock coping and consumption smoothing abilities. Examining treatment heterogeneity across areas with high informality thus allows us to explore the distributional implications of demonetization, particularly considering that almost a fourth of India's labour force is employed in non-farm informal enterprises.[53]

We begin by examining treatment heterogeneity across areas with relatively high ex-ante informality. In the absence of pincode-level measures of informality, we begin with urbanization as a marker of informality, noting the strong district-level correlation between the fraction of rural households, self-employment, and the share of workers in informal enterprises.[54] Column (1) of Appendix Table D1 identifies a statistically significant negative coefficient, implying that the adoption of digital payments was significantly lower in rural pincodes, conditional on cash shortage. The uninteracted double-difference coefficient estimating the treatment effect for urban pincodes remains positive and significant, indicating that the treatment effect on digital payments was driven by urban pincodes. Over the long-term, digital transactions in an urban pincode at the 75th percentile of the $Dist_{CC}$ distribution continued to be 50 percent higher relative to an urban pincode at the 25th percentile. In contrast, moving from a rural pincode at the 25th percentile of the $Dist_{CC}$ distribution to one at the 75th percentile increased digital transactions by 8 percent. Effectively, the treatment effect in rural pincodes was dampened by a factor of 6.

Columns (2)-(4) of Table D1 show consistency to district-level measures of informality. Column (2) replicates the pincode-level results using the share of the rural population in a district, while columns (3) and (4) use the fraction of informal and self-employed workers as measures of regional informality. The coefficients uniformly confirm that the treatment had a muted impact on digital transactions in regions with high (above median) informality. Additionally, Appendix Table D2 rules out the alternate explanation of faster cash replenishment to such areas – if anything, cash withdrawals were significantly lower in rural pincodes, conditional on their distance from CCs.[55]

Collectively, these show that while the sharp reduction in cash supply generated a substantial and persistent increase in digital payments in areas located farther from centres of cash, this was concentrated in regions with relatively high urbanization and low ex-ante informality. Appendix D1 shows that the relatively lower adoption of digital payments in areas with higher informality dissipates in pincodes with high pre-treatment financial infrastructure. Combined with Appendix Table C2, this points to important distributional implications of the unanticipated reduction in cash supply. Barriers to accessing financial infrastructure served as a key

[53]The nationally representative survey on unincorporated micro-enterprises, conducted by the NSS in 2015-16, estimates in excess of 50 million micro-enterprises operating in India, employing over 100 million workers.

[54]In the absence of accurate Census data for pincodes, we use RBI's classification of bank branches as urban or rural, described in Section 2.2.1 to determine whether a pincode is urban or rural.

[55]The absence of pincode-level data on cash withdrawals from banks mean that we are unable to rule out that the limited treatment effect in rural areas is due to larger cash withdrawals from banks (as opposed to ATMs). However, anecdotal evidence during this period suggests that overall cash replenishment was slower in rural areas (see for instance Indian Express, 2017; Business Standard, 2019).

constraint in adopting digital payments in areas with relatively high economic vulnerability and cash dependency. This provides empirical support to concerns voiced by Lahiri (2020) that the negative effects of demonetization were likely to have been disproportionately borne by the cash-dependent informal sector.

We finish by exploring potential factors which can explain the muted adoption of digital payments in areas with high informality. Given the strong negative correlation between financial infrastructure and informality, a key mechanism of interest is identifying whether treatment heterogeneity exists across areas with high informality, conditional on access to financial infrastructure. We also consider whether alternative channels, such as corruption and differential reductions in rural consumption, can explain the lack of adoption of digital payments in response to the cash shortage for areas with higher informality.

We first consider the corruption channel and examine whether the strong anti-corruption pitch surrounding demonetization dampened participation in digital payments. For instance, if areas with higher ex-ante informality also had higher levels of corruption, it is possible that income-hiding motives could have prevented households and retailers from adopting digital payments. Panel A of Appendix Table D3 however rules out that higher levels of corruption can explain the limited adoption of digital payments in areas with high informality. Irrespective of regional corruption levels, demonetization continued to have a negative differential impact on the adoption of digital payments in areas with relatively higher informality.

The descriptive statistics in Section 1.1 noted that areas with relatively high informality had higher cash dependence and lower household consumption. It is possible that the policy differentially affected household consumption in areas with high informality, leading to a mechanical reduction in digital payments. Columns (1) and (2) of Appendix Table D4 however, rules out any heterogeneity in household consumption across rural households or households most likely to be operating in the informal sector. Consequently, the limited adoption of digital payments in these areas cannot be attributed to a differential reduction in household consumption.

The final channel explored is financial infrastructure. We draw from our findings in Sections 3.3 and Figure 8, which confirmed financial infrastructure as a necessary condition for adopting digital payments. We now identify heterogeneity in the adoption of digital payments across regional measures of informality, conditional on financial infrastructure. The intuition is to examine whether the adoption of digital payments in areas with high informality could have been boosted through the targeted rollout of financial infrastructure or whether there existed other barriers to adoption, precluding households residing in these areas from switching to digital payments in the absence of cash.

We test this by re-estimating heterogeneity across regional informality, conditional on the level of financial infrastructure. If factors other than financial infrastructure drive the limited adoption of digital payments in rural areas, we should continue to identify a negative sign on the triple interaction coefficient, even in areas with relatively high financial infrastructure. We split our sample into 3 bins corresponding to the bottom quartile, the middle 2 quartiles, and the top quartile of financial infrastructure. Column (1) of Appendix D5 shows that in areas with low financial infrastructure, there is no impact of the treatment on the adoption of digital payments, confirming again the necessity of financial infrastructure for the adoption of digital payments using debit and credit cards. In areas with intermediate financial infrastructure [column (2)], while the average pincode sees an increase in digital payments, especially over the long term, we identify a large negative coefficient on the triple interaction term corresponding to informality. Finally, consistent with our expectations, column (3) shows that in areas with relatively high financial infrastructure, there is no longer any differential negative effect across areas with high informality. The double-difference coefficient remains positive across both the near and the long term, and the sum of the coefficients too is positive and statistically significant. Thus, the availability of financial infrastructure did induce households to switch to adopting digital payments, even in areas with high informality and, arguably, higher cash dependence.

Table D1
Impact on digital financial inclusion:
Heterogeneity by degree of informality

	(1)	(2)	(3)	(4)
Dep. Variable	Log(Digital Transactions)			
	Pincode Heterogeneity	District Heterogeneity		
Dist_{CC} × Near Term	.031***	.014***	.015***	.010***
	(.004)	(.002)	(.002)	(.002)
Dist_{CC} × Long Term	.040***	.020***	.021***	.015***
	(.005)	(.003)	(.003)	(.003)
Dist_{CC} × Rural × Near Term	-.030***			
	(.004)			
Dist_{CC} × Rural × Long Term	-.034***			
	(.005)			
Dist_{CC} × Rural × Near Term		-.018***		
		(.003)		
Dist_{CC} × Rural × Long Term		-.015***		
		(.004)		
Dist_{CC} × Informal × Near Term			-.020***	
			(.003)	
Dist_{CC} × Informal × Long Term			-.017***	
			(.004)	
Dist_{CC} × Self Emp. × Near Term				-.009***
				(.003)
Dist_{CC} × Self Emp. × Long Term				-.005
				(.004)
Observations	101803	101803	101803	101803
R^2	.92	.92	.92	.92

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

Notes: This table estimates treatment heterogeneity across pincode and district measures of informality. The unit of observation is the pincode. The outcome of interest is (logged) average volume of monthly digital payments undertaken through POS terminals. The sample is restricted to pincodes within 40 km of a currency chest (CC). *Near-Term* refers to 4 quarters between October 2016 and September 2017; *Long-Term* refers to 2 quarters between October 2017 and March 2018. Column (1) considers pincode-level heterogeneity; *Rural* is a dummy equal to 1 if a pincode is classified as rural. Pincodes are considered rural if no bank branch in the pincode is classified as "metropolitan" or "urban". Columns (2)-(5) consider district-level heterogeneity. *Rural*, *Informal* and *Self-Employed* represent dummies equaling 1 if the district has a relatively high share of rural households (informal/self-employed workers), relative to the national median in 2011-12. All specifications include pincode and district-time fixed effects along with pincode-specific time-varying covariates. Standard errors in parentheses, clustered by pincode.

Table D2
Heterogeneity in Cash Withdrawals Across Local Measures of Informality

	(1)	(2)	(3)	(4)
Dep. Variable	Log(Cash Withdrawals)			
	Pincode Heterogeneity	District Heterogeneity		
Dist_{CC} × Near Term	-.001	-.005***	-.004***	-.005***
	(.001)	(.001)	(.001)	(.001)
Dist_{CC} × Long Term	.004***	-.000	.000	-.001
	(.001)	(.001)	(.001)	(.001)
Dist_{CC} × Rural × Near Term	-.004***			
	(.001)			
Dist_{CC} × Rural × Long Term	-.006***			
	(.001)			
Dist_{CC} × Rural × Near Term		.000		
		(.001)		
Dist_{CC} × Rural × Long Term		-.001		
		(.001)		
Dist_{CC} × Informal × Near Term			-.001	
			(.001)	
Dist_{CC} × Informal × Long Term			-.002**	
			(.001)	
Dist_{CC} × Self Emp. × Near Term				.001
				(.001)
Dist_{CC} × Self Emp. × Long Term				.000
				(.001)
Observations	104789	104789	104789	104789
R^2	.96	.96	.96	.96

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

Notes: This table estimates treatment heterogeneity across pincode and district measures of informality. The unit of observation is the pincode. The outcome of interest is (logged) average volume of monthly cash withdrawals through ATM terminals. The sample is restricted to pincodes within 40 km of a currency chest (CC). *Near-Term* refers to 4 quarters between October 2016 and September 2017; *Long-Term* refers to 2 quarters between October 2017 and March 2018. Column (1) considers pincode-level heterogeneity; *Rural* is a dummy equal to 1 if a pincode is classified as rural. Pincodes are considered rural if no bank branch in the pincode is classified as "metropolitan" or "urban". Columns (2)-(4) consider district-level heterogeneity. *Rural*, *Informal* and *Self-Employed* represent dummies equaling 1 if the district has a relatively high share of rural households (informal/self-employed workers), relative to the national median in 2011-12. All specifications include pincode and district-time fixed effects along with pincode-specific time-varying covariates. Standard errors in parentheses, clustered by pincode.

Table D3
Heterogeneity in Digital Transactions and Cash Withdrawals Across Rural Pincodes, Conditional on Regional Corruption

Panel A: Digital Transactions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable	Log(Digital Transactions)							
	Overall Corruption		Corruption in Police		Corruption in Banks		Corruption in Land and Housing	
	High	Low	High	Low	High	Low	High	Low
Dist_{CC} × Near Term	.000	.008***	.001	.008***	.002	.007***	.003	.006*
	(.003)	(.002)	(.003)	(.002)	(.003)	(.002)	(.002)	(.003)
Dist_{CC} × Long Term	.007**	.016***	.009**	.016***	.010***	.014***	.009***	.018***
	(.003)	(.003)	(.004)	(.003)	(.003)	(.003)	(.003)	(.004)
Dist_{CC} × Std. Informal × Near Term	-.022***	-.010***	-.020***	-.012***	-.023***	-.008***	-.017***	-.015***
	(.003)	(.003)	(.004)	(.003)	(.003)	(.003)	(.002)	(.004)
Dist_{CC} × Std. Informal × Long Term	-.019***	-.007*	-.021***	-.008***	-.020***	-.006	-.014***	-.019***
	(.003)	(.004)	(.005)	(.003)	(.003)	(.004)	(.003)	(.005)
Observations	44310	57458	27325	74434	40521	61256	68080	33688
R^2	.92	.93	.90	.93	.91	.93	.93	.92

Panel B: Cash Withdrawals

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable	Log(Cash Withdrawals)							
	Overall Corruption		Corruption in Police		Corruption in Banks		Corruption in Land and Housing	
	High	Low	High	Low	High	Low	High	Low
Dist_{CC} × Near Term	-.005***	-.005***	-.004***	-.005***	-.004***	-.006***	-.005***	-.002**
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Dist_{CC} × Long Term	-.001	-.001	-.001	-.001	-.000	-.002*	-.002**	.002*
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Dist_{CC} × Std. Informal × Near Term	-.000	-.003***	-.000	-.002***	.000	-.003***	-.001**	-.002*
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Dist_{CC} × Std. Informal × Long Term	-.002***	-.002	-.001	-.003	-.002*	-.003	-.002	-.004***
	(.001)	(.002)	(.001)	(.002)	(.001)	(.002)	(.002)	(.002)
Observations	45799	58954	28389	76355	41934	62828	70178	34575
R^2	.96	.96	.95	.96	.96	.96	.96	.96

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

Notes: This table estimates treatment heterogeneity across regional levels of informality, conditional on states' pre-treatment levels of corruption. The unit of observation is the pincode. The outcome of interest in Panel A is (logged) average volume of monthly digital payments undertaken through POS terminals; in Panel B, average volume of cash withdrawals undertaken through ATM terminals. The sample is restricted to pincodes within 40 km of a currency chest (CC). *Near-Term* refers to 4 quarters between October 2016 and September 2017; *Long-Term* refers to 2 quarters between October 2017 and March 2018. *Std. Informal* is the informality index, described in Appendix B1. Across both panels, columns (1)-(2) splits the sample by states' overall corruption levels; columns (3)-(4) splits the sample by states' level of corruption in the police; columns (5)-(6) splits the sample by states' level of corruption in banks; columns (7)-(8) splits the sample by states' level of corruption in real estate transactions. States have "high" corruption levels if their corruption score exceeds the median score across all states. All specifications include pincode and district-time fixed effects along with pincode-specific time-varying covariates. Standard errors in parentheses, clustered by pincode.

Table D4
Cash Supply Shock and Household Consumption: Heterogeneity by Household Characteristics

	(1)	(2)	(3)	(4)	(5)
Dep. Variable	Per Capita Monthly Household Consumption (Log)				
$Dist_{CC}$ × Near Term	-.000	.002	.001	.001	.001
	(.002)	(.002)	(.002)	(.002)	(.002)
$Dist_{CC}$ × Near Term	-.006***	-.005**	-.004*	-.006***	-.005**
	(.002)	(.002)	(.002)	(.002)	(.002)
$Dist_{CC}$ × Rural × Near Term	.002**				
	(.001)				
$Dist_{CC}$ × Rural × Long Term	.003**				
	(.001)				
$Dist_{CC}$ × Informal × Near Term		-.001			
		(.002)			
$Dist_{CC}$ × Informal × Long Term		.001			
		(.002)			
$Dist_{CC}$ × White-Collar × Near Term			.001		
			(.002)		
$Dist_{CC}$ × White-Collar × Long Term			.001		
			(.002)		
$Dist_{CC}$ × Ins. × Near Term				.000	
				(.001)	
$Dist_{CC}$ × Ins. × Near Term				.003***	
				(.001)	
$Dist_{CC}$ × Ret. Save × Near Term					.002**
					(.001)
$Dist_{CC}$ × Ret. Save × Near Term					.003***
					(.001)
Observations	1089151	938129	938129	1088320	1088320
R^2	.72	.71	.71	.72	.72
Dep Var Mean	2037.36	2037.36	2037.36	2037.36	2037.36

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

Notes: This table estimates the heterogeneity in the impact of the cash supply shock on households' per capita monthly consumption across household characteristics. The unit of observation is the household. The outcome variable is logged household per capita monthly expenditure. $Dist_{CC}$ is measured at the level of the district, as the median value of $Dist_{CC}$ across all pincodes located in the district. All specifications include household, state-time fixed effects and household covariates. *Rural* is a dummy equaling 1 if the household resided in a rural area; *Informality* is a dummy equaling 1 if some household member was employed in an informal occupation prior to demonetization; *White Collar* is a dummy equaling 1 if any household member was occupied in a white-collar position prior to demonetization; *Ins.* is a dummy equaling 1 if any member of the household had a life insurance policy in the pre-demonetization period; *Ret. Save* is a dummy equaling 1 if any member of the household had any retirement savings in the pre-demonetization period. All specifications are weighted using household-specific weights. Standard errors are clustered by district.

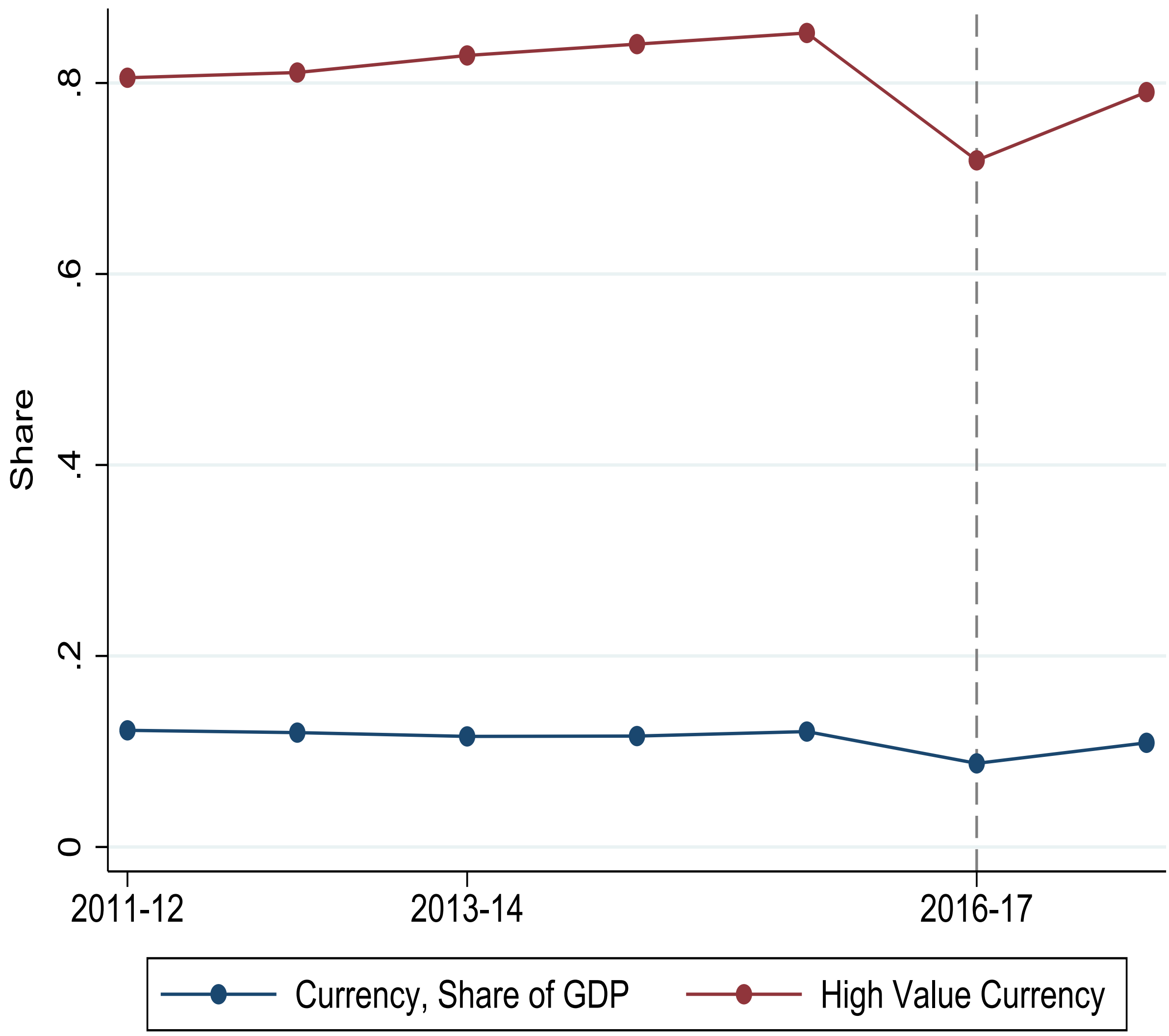
Table D5
Cash Supply Shock, Cash Withdrawals and Digital Transactions: Heterogeneity by Regional Informality, Conditional on Financial Infrastructure

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable	Log(Digital Transactions)			Log(Cash Withdrawals)		
	Fin. Infra. Bottom Quartile	Fin. Infra. Middle Quartiles	Fin. Infra. Top Quartile	Fin. Infra. Bottom Quartile	Fin. Infra. Middle Quartiles	Fin. Infra. Top Quartile
Dist_{CC} × Near Term	-.004	.005	.032***	-.004**	-.003***	-.006***
	(.006)	(.004)	(.010)	(.002)	(.001)	(.002)
Dist_{CC} × Long Term	-.005	.016***	.036***	-.004	.001	-.002
	(.007)	(.004)	(.012)	(.004)	(.001)	(.003)
Dist_{CC} × Std. Informal × Near Term	-.004	-.013***	.002	-.000	-.000	-.001
	(.008)	(.005)	(.008)	(.002)	(.001)	(.002)
Dist_{CC} × Std. Informal × Long Term	.001	-.011**	.003	.006	-.001	-.002
	(.010)	(.005)	(.008)	(.006)	(.001)	(.002)
Observations	12538	35748	19667	13041	36741	19953
R^2	.89	.93	.95	.94	.97	.98

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

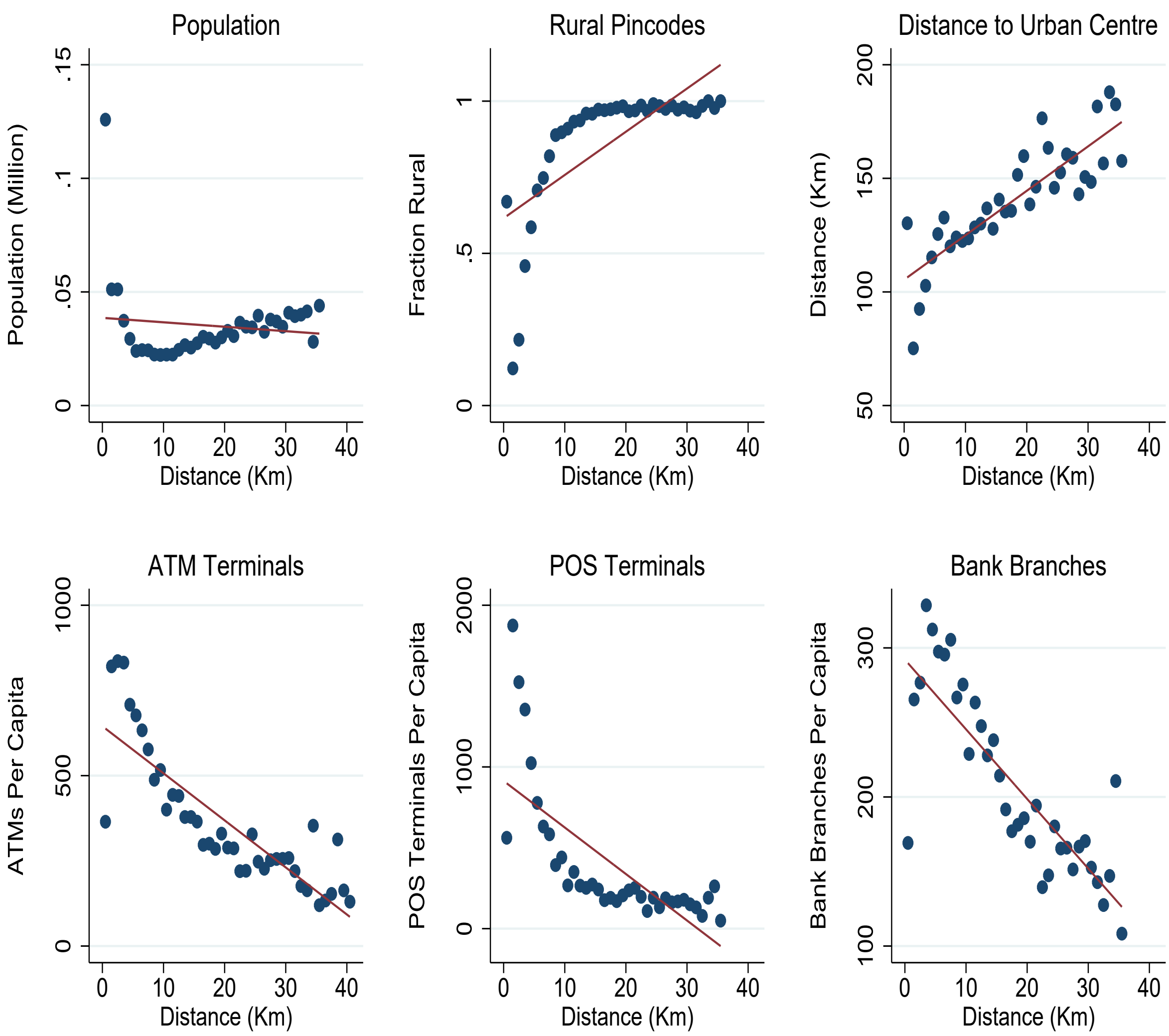
Notes: This table identifies heterogeneity in the impact of the cash supply shock on digital payments across regional informality, conditional on regional financial infrastructure. The unit of observation is the pincode. The outcome of interest is the (logged) average volume of monthly cash withdrawals from ATMs in a quarter. The sample is restricted to pincodes within 40 km of a currency chest (CC). *Near-Term* refers to 4 quarters between October 2016 and September 2017; *Long-Term* refers to 2 quarters between October 2017 and March 2018. *Std. Informal* is the informality index, described in Appendix B1. The sample in columns (1) and (2) is restricted to pincodes where the pre-treatment financial infrastructure index score falls in the bottom quartile; columns (3) and (4) restrict the sample to pincodes where the pre-treatment financial infrastructure index score falls in the middle quartiles; columns (5) and (6) restrict the sample to pincodes where the pre-treatment financial infrastructure index score falls in the top quartile. Standard errors in parentheses, clustered by pincode.

Figure E1
Currency in Circulation as a Fraction of GDP



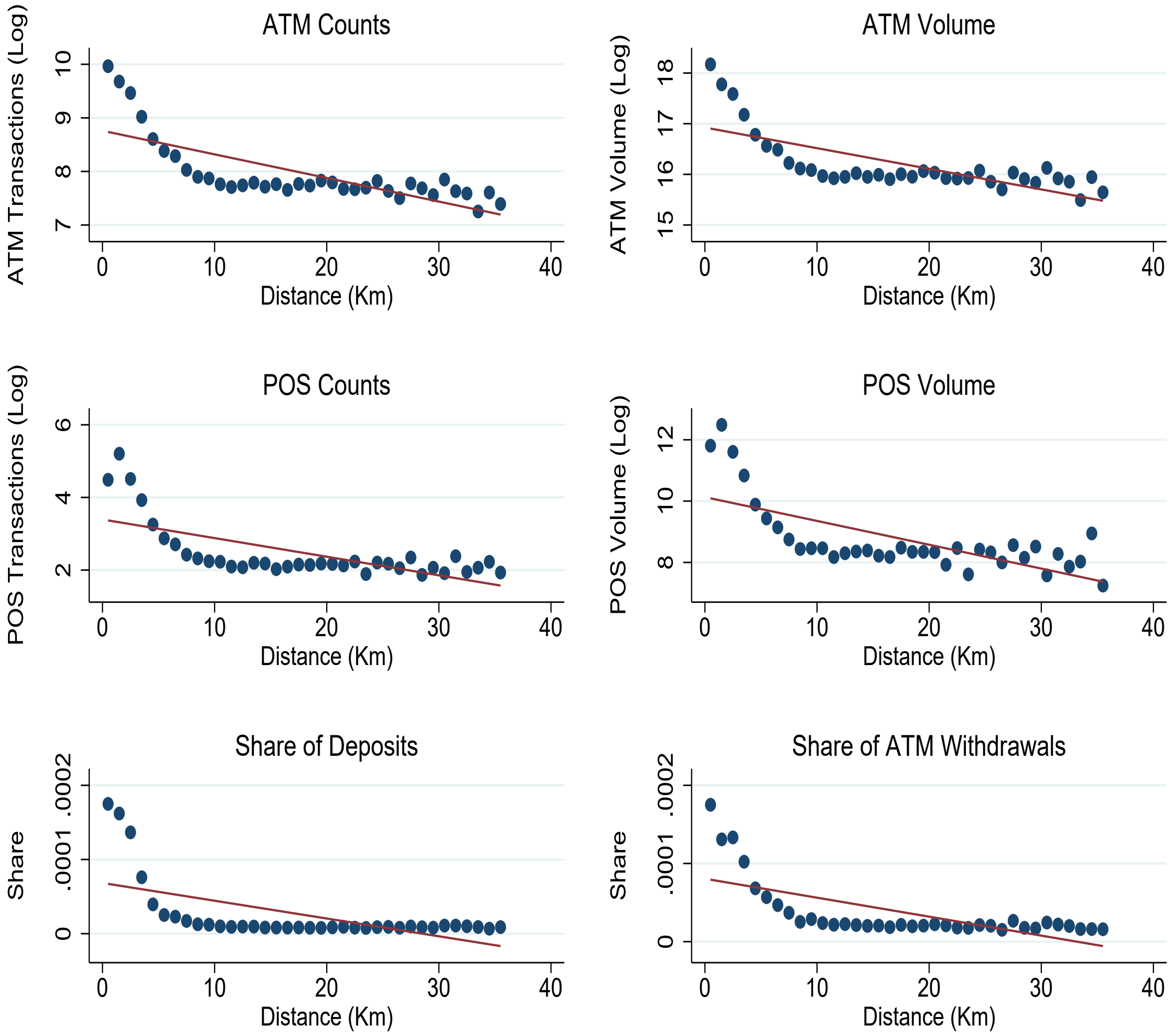
Notes: The above figure plots the annual trends in currency in circulation as a share of GDP and high value currency as a fraction of total currency in circulation. "High value currency" denotes currency in INR 500, 1,000 and 2,000 denominations. The vertical dashed line denotes the year of the demonetization intervention.

Figure E2
Distance to Currency Chests and Pre-Treatment Pincode Characteristics



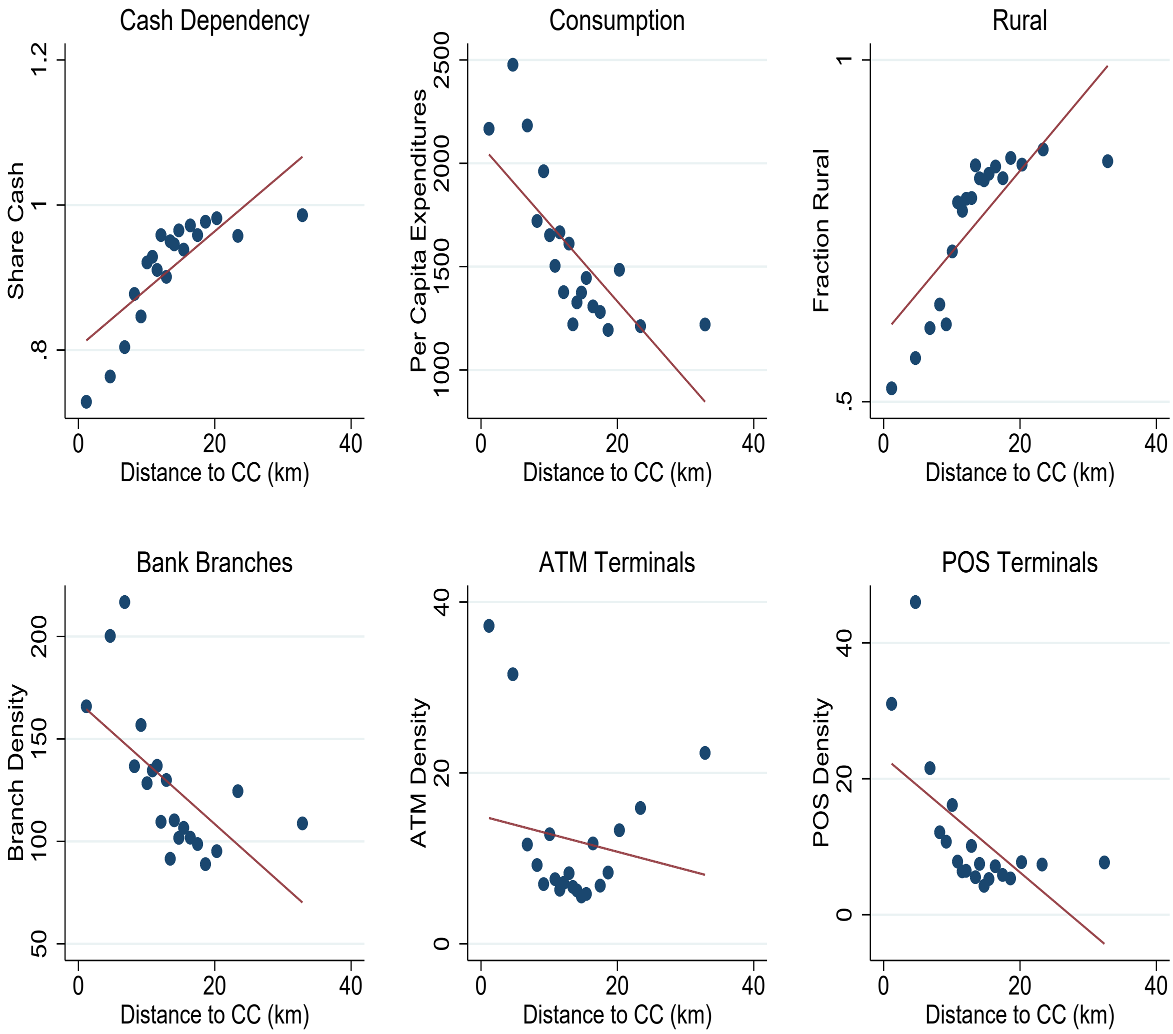
Notes: The above figures depict the correlation between pincodes' distance to the nearest currency chest and pincode characteristics. The horizontal axis in each figure is divided into 40 1 km bins of pincodes' distance to the nearest currency chest. Rural pincodes are pincodes in which no bank branch is classified as an "urban" or "metropolitan" branch.

Figure E3
Distance to Currency Chests and Pre-Treatment Pincode Economic Characteristics



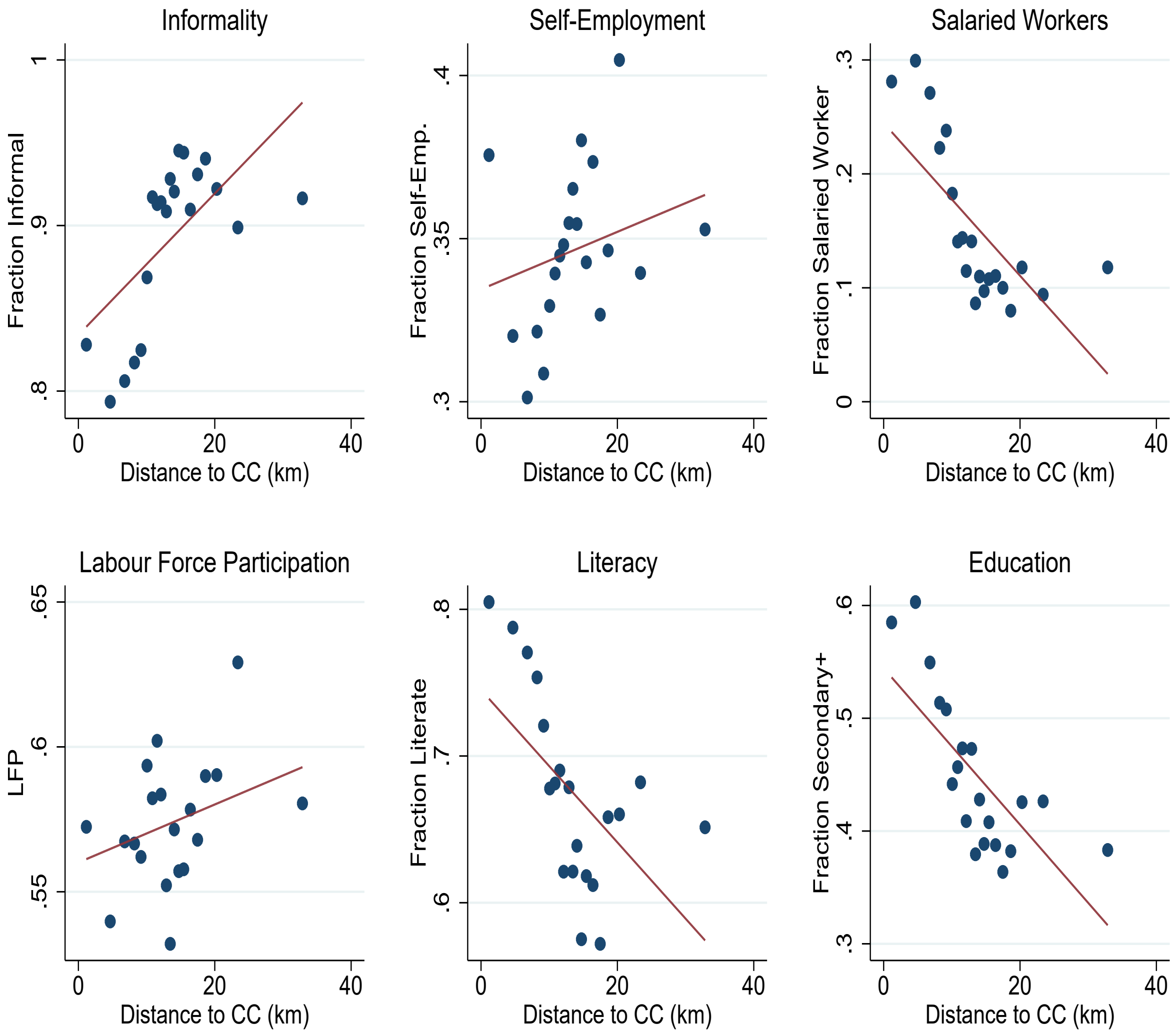
Notes: The above figures depict the correlation between pincodes' distance to the nearest currency chest and pre-demonetization pincode characteristics. The horizontal axis in each figure is divided into 40 1 km bins of pincodes' distance to the nearest currency chest. ATM/POS counts (volumes) reflect the number (volume) of transactions undertaken through ATM/POS terminals. Share of deposits/ATM withdrawals is the volume of bank deposits/ ATM withdrawals attributable to the pincode. Each point reflects the unconditional mean of the pincode characteristic of interest corresponding to the distance to currency chest bin.

Figure E4
Distance to Currency Chests and Pre-Treatment District Economic Characteristics



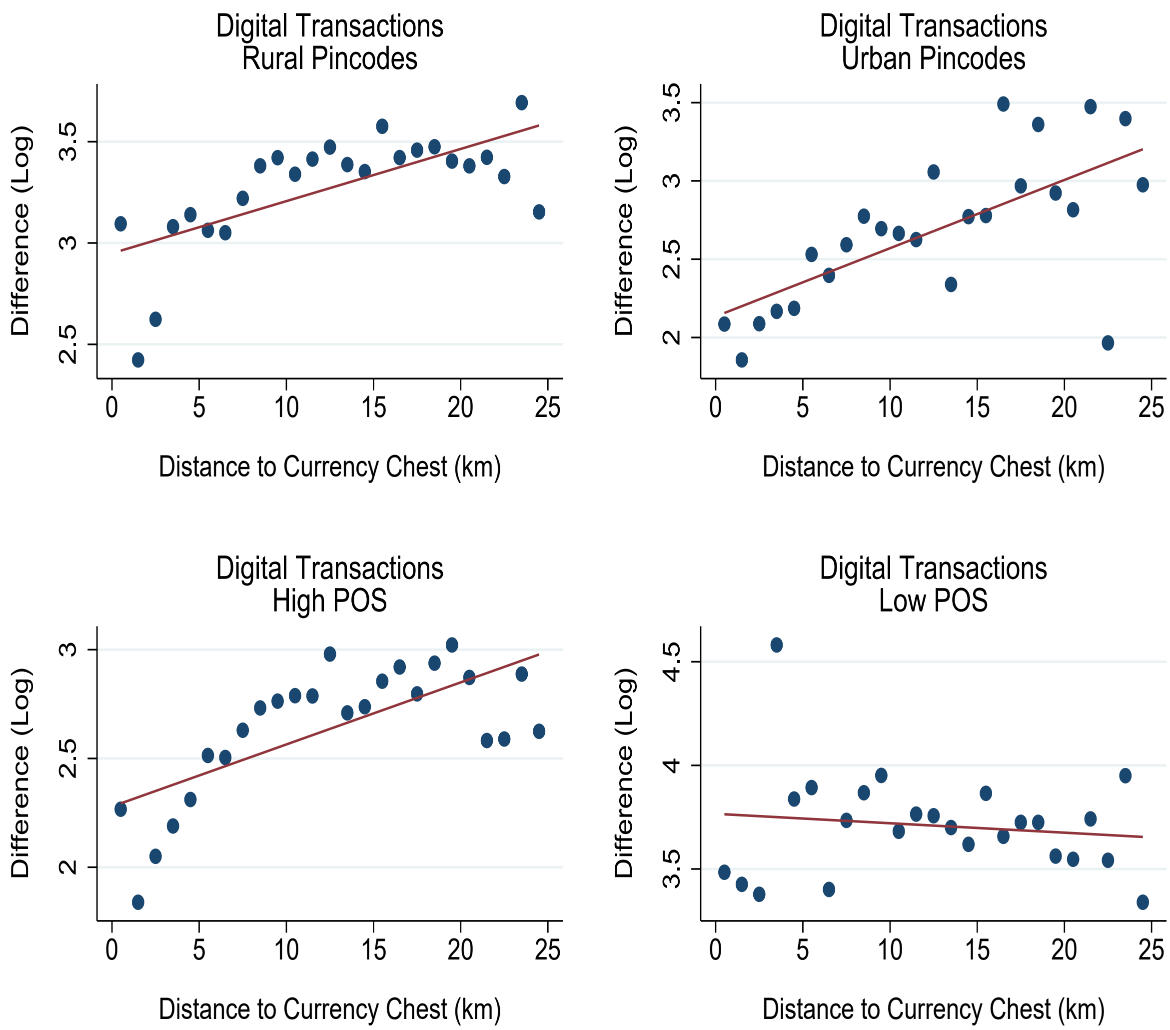
Notes: The above figures depict the correlation between a district's median pincode's distance to the nearest currency chest and pre-demonetization district characteristics. The horizontal axis in each figure is divided into 40 1 km bins of pincodes' distance to the nearest currency chest. Bank branches (ATM/POS terminals) are the number of bank branches (ATM/POS) in the district, scaled by the district population. Cash dependency is the fraction of district per capita monthly expenditures not accounted for by digital transactions undertaken through POS terminals. Consumption refers to the district's average per capita monthly expenditures.

Figure E5
Distance to Currency Chests and Pre-Treatment District Workforce Characteristics



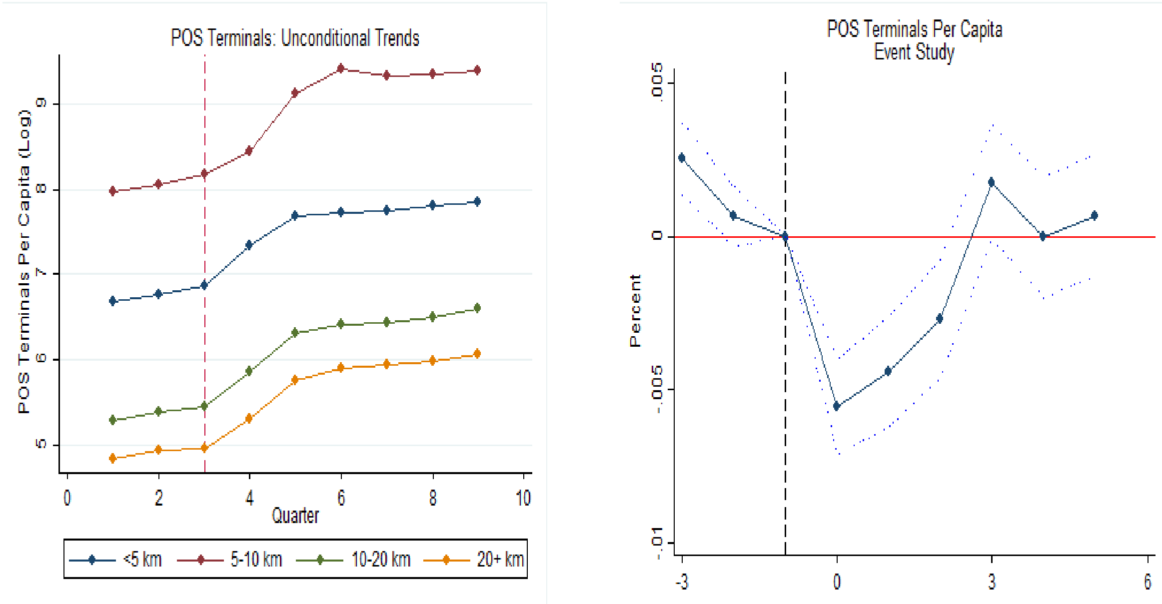
Notes: The above figures depict the correlation between a district's median pincode's distance to the nearest currency chest and pre-demonetization district characteristics. The horizontal axis in each figure is divided into 40 1 km bins of pincodes' distance to the nearest currency chest. Informality refers to the fraction of workers in the district not employed in the formal sector. Education refers to the fraction of adults in the district who have completed secondary education or better.

Figure E6
Cash Supply Shock, Cash Withdrawals and Digital Transactions: Event Study Plots



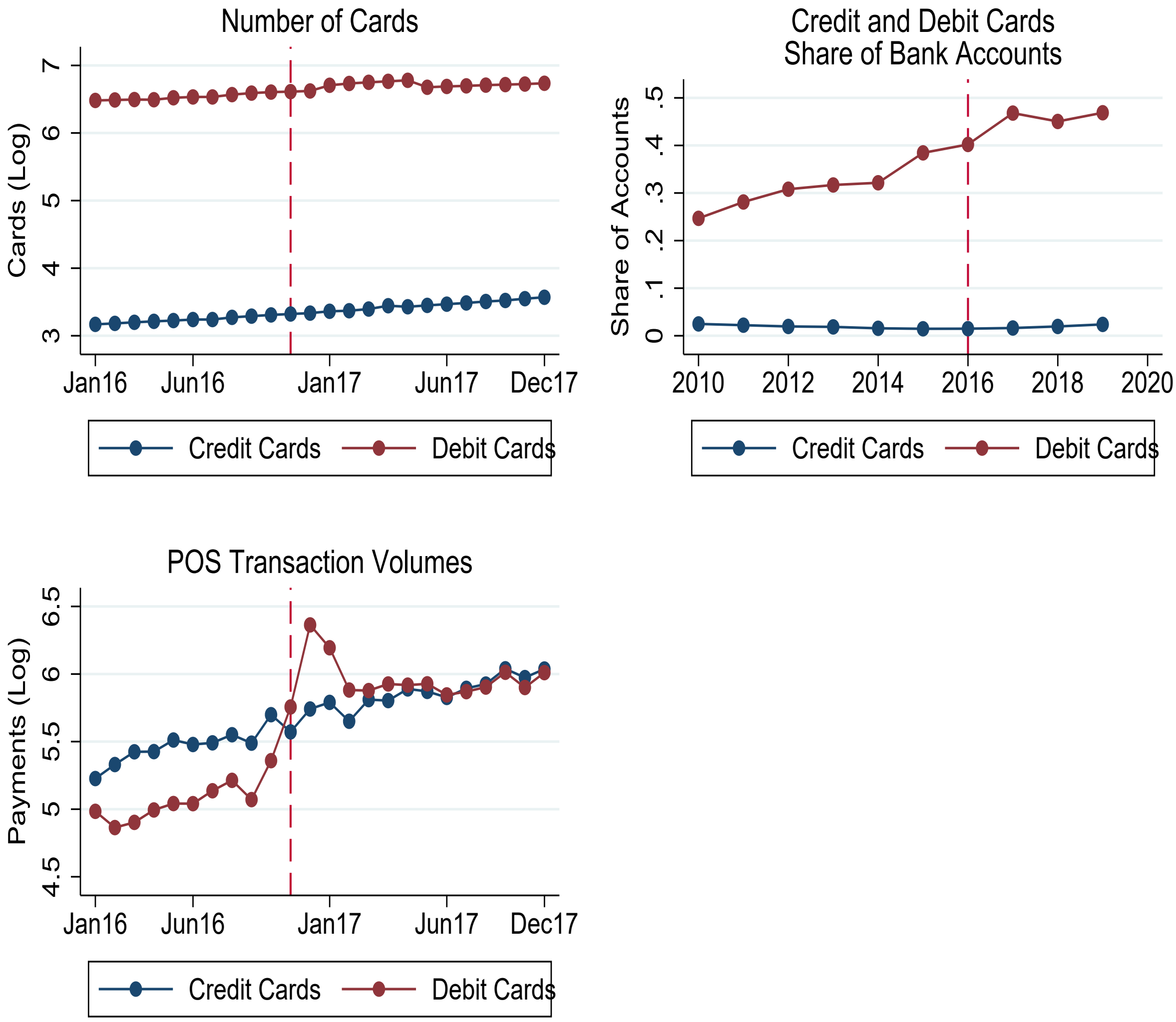
Notes: The above figures plot the pincode-level long-differences in cash withdrawals and digital transactions by distance to currency chest, conditional on pincode characteristics. The horizontal axis is divided into 40 1 km bins of pincodes' distance to the nearest currency chest. Each point on the figure represent the unconditional average difference (log) between the post-treatment value, and the pre-treatment value, corresponding to pincodes located within that distance bin. Near-term refers to the logged difference between average pincode outcome values between September 2017 and January 2016; long-term refers to the logged difference in outcome values between March 2018 and January 2016. The top-left panel restricts the sample to rural pincodes; the top-right panel restricts the sample to urban pincodes; the bottom-left panel restricts the sample to pincodes with relatively high (above median) POS terminals per capita; the bottom-right panel restricts the sample to pincodes with relatively low (above median) POS terminals per capita.

Figure E7
Cash Supply Shock and Per Capita POS Terminals



Notes: This figure shows the evolution of POS terminals across pincodes across quarters. The left panel shows the growth in per capita POS terminals (logged) across 4 bins of pincodes' distance to currency chests. The right panel shows the corresponding event-study plot estimated using specification (5). The outcome of interest is per capita POS terminals (logged). The unit of observation is the pincode. The quarter prior to the demonetization intervention (June-September 2016, shown as the vertical dashed line) is taken as the reference period. The vertical line plots the coefficient values while the dashed lines show the 95% confidence intervals. All specifications include pincode and district-quarter fixed effects, along with pincode covariates. Standard errors are clustered by pincode.

Figure E8
Aggregate Trends in Debit and Credit Cards



Notes: The above figures shows aggregate trends in debit and credit cards around the period of demonetization. The top left figure shows the (logged) number of credit and debit cards for each month; the top right panel shows the number of credit and debit cards as a share of bank accounts for each year; the bottom panel shows the (logged) volume of transactions undertaken using credit and debit cards at POS terminals for each month. The vertical dashed line in each figure denotes the month/year of demonetization.

Table E1
Formal and Informal Sector Worker Characteristics

	Formal Sector	Informal Sector
	(1)	(2)
Fraction Rural	.367	.552
Fraction Secondary+	.717	.565
Fraction Salaried Work	.758	.230
Fraction Casual Work	.206	.256
Daily Wage, Rural (INR)	279.466	204.910
Daily Wage, Urban (INR)	535.849	272.626
Observations	19,390	93,435

Notes: The above table presents summary statistics across formal and informal sector workers. Formal sector workers are workers who are a) employed in enterprises which uses electricity and employs 10 or more workers; or b) employed in enterprises which employs 20 or more workers. Secondary+ refers to the fraction of workers who have completed secondary education or better. The data is from the NSS Employment-Unemployment survey undertaken in 2011-12.

Table E2
Does Distance to Currency Chests Predict Pre-Treatment Cash Withdrawals and Digital Transactions?

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable	Log(Cash Withdrawals)			Log(Digital Transactions)		
$Dist_{CC}$	-.078***	-.071***	-.000	-.142***	-.131***	-.001
	(.004)	(.002)	(.001)	(.008)	(.005)	(.002)
Observations	16755	16755	11780	14039	14039	11780
R^2	.16	.34	.87	.12	.30	.89
Dep Var Mean	53.01	53.01	53.01	0.47	0.47	0.47
District FE	N	Y	Y	N	Y	Y
Pincode Covariates	N	N	Y	N	N	Y

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

Notes: This table estimates whether pincodes' distance to the nearest currency chest affects pre-treatment cash withdrawals and digital payments. The unit of observation is the pincode. The outcome of interest in columns (1)-(3) is the (logged) average volume of monthly cash withdrawals from ATMs in the pre-treatment period; in columns (5)-(8), the (logged) average volume of monthly digital payments undertaken through POS terminals. The sample is restricted to pincodes within 40 km of a currency chest (CC). Columns (1) and (4) include no covariates; columns (2)-(3) and (4)-(5) include district fixed effects; columns (3) and (6) include pincode-specific covariates. Standard errors in parentheses, clustered by district.

Table E3
Cash Supply Shock, Cash Withdrawals and Digital Transactions: Heterogeneity by District Connectivity

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable	Log(Cash Withdrawals)			Log(Digital Transactions)		
Dist_{CC} × Near Term	-.004***	-.003***	-.003***	.003	.000	.003
	(.001)	(.001)	(.001)	(.002)	(.003)	(.003)
Dist_{CC} × Long Term	-.001	.001	.001	.012***	.011***	.009***
	(.001)	(.001)	(.001)	(.003)	(.003)	(.003)
Dist_{CC} × High Roads × Near Term	-.001			.007**		
	(.001)			(.003)		
Dist_{CC} × High Roads × Long Term	.002			.002		
	(.001)			(.004)		
Dist_{CC} × High Bus × Near Term		-.003***			.010***	
		(.001)			(.003)	
Dist_{CC} × High Bus × Long Term		-.002*			.003	
		(.001)			(.004)	
Dist_{CC} × High Taxi × Near Term			-.002**			.005
			(.001)			(.003)
Dist_{CC} × High Taxi × Long Term			-.002*			.007*
			(.001)			(.004)
Observations	103424	103424	103424	100479	100479	100479
R^2	.96	.96	.96	.92	.92	.92

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

Notes: This table tests for heterogeneity in the impact of the cash supply shock across districts' transport connectivity. The unit of observation is the pincode. The outcome of interest in columns (1)-(3) is the (logged) average volume of monthly cash withdrawals from ATMs in a quarter; in columns (4)-(6), the (logged) average volume of monthly digital payments undertaken through POS terminals. All specifications include pincode and district-time fixed effects, along with pincode-specific time-varying covariates. *Near-Term* refers to 4 quarters between October 2016 and September 2017; *Long-Term* refers to 2 quarters between October 2017 and March 2018. *High Road* is a dummy for districts where a relatively high (above median) share of villages are connected by a national or state highway, or an all weather district road; *High Bus* is a dummy equaling 1 if a relatively high (above median) fraction of villages in the district are connected by a bus service; *High Taxi* is a dummy equaling 1 if a relatively high (above median) fraction of villages in the district are connected by taxi services. Standard errors in parentheses, clustered by pincode.

Table E4
Cash Supply Shock, Cash Withdrawals and Digital Transactions: Robustness to Alternate Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable	Log(Cash Withdrawals)			Log(Digital Transactions)		
	Control Initial Values	District Cluster	Alternate Distance Breaks	Control Initial Values	District Cluster	Alternate Distance Breaks
Dist_{CC} × Near Term	-.003***	-.004***		.010***	.006***	
	(.001)	(.001)		(.002)	(.002)	
Dist_{CC} × Long Term	-.003***	-.001		.001	.013***	
	(.001)	(.001)		(.002)	(.002)	
$\text{Dist}_{CC:Bottom10pc}$ × Near Term			-.075***			-.176***
			(.012)			(.038)
$\text{Dist}_{CC:Bottom10pc}$ × Long Term			-.053***			-.126***
			(.014)			(.044)
$\text{Dist}_{CC:10-25pc}$ × Near Term			-.095***			-.012
			(.012)			(.038)
$\text{Dist}_{CC:10-25pc}$ × Long Term			-.028**			.082*
			(.014)			(.043)
$\text{Dist}_{CC:50-75pc}$ × Near Term			-.123***			.090**
			(.013)			(.042)
$\text{Dist}_{CC:50-75pc}$ × Long Term			-.033**			.229***
			(.014)			(.047)
$\text{Dist}_{CC:75-90pc}$ × Near Term			-.138***			.135***
			(.014)			(.048)
$\text{Dist}_{CC:75-90pc}$ × Long Term			-.043***			.290***
			(.016)			(.056)
$\text{Dist}_{CC:Top10pc}$ × Near Term			-.115***			.009
			(.018)			(.061)
$\text{Dist}_{CC:Top10pc}$ × Long Term			-.028			.181***
			(.025)			(.070)
Observations	104285	104789	104789	92662	101803	101803
R^2	.96	.96	.96	.93	.92	.92

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

Notes: This table shows the robustness of the baseline results to alternate specification choices. The unit of observation is the pincode. The outcome of interest in columns (1)-(3) is the (logged) average volume of monthly cash withdrawals from ATMs in a quarter; in columns (4)-(6), the (logged) average volume of monthly digital payments undertaken through POS terminals. The sample is restricted to pincodes within 40 km of a currency chest (CC). *Near-Term* refers to 4 quarters between October 2016 and September 2017; *Long-Term* refers to 2 quarters between October 2017 and March 2018. Columns (1) and (4) control for a linear time-trend in the initial values of the outcome variable; columns (2) and (5) cluster the standard errors at the level of district; columns (3) and (6) test for non-linearities in the treatment across select percentiles of the $Dist_{CC}$ distribution. All specifications include pincode and district-time fixed effects, along with pincode-specific time-varying covariates. Standard errors in parentheses, clustered by pincode in all specifications except columns (2) and (5).

Table E5
Robustness to Alternate Dependent Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Per Capita Volumes		Inverse Hyperbolic Sine Transformation		Transaction Counts	
Dep. Variable	Cash Withdrawals	Digital Payments	Cash Withdrawals	Digital Payments	Cash Withdrawals	Digital Payments
Dist_{CC} × Near Term	-.006***	.014***	.000***	.000***	-.004***	.011***
	(.000)	(.002)	(.000)	(.000)	(.001)	(.002)
Dist_{CC} × Long Term	-.002***	.023***	-.001***	.000*	.004***	.014***
	(.001)	(.002)	(.000)	(.000)	(.001)	(.002)
Observations	104789	101803	104789	101803	104789	101803
R^2	.94	.89	.98	.86	.97	.93

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

Notes: This table shows the robustness of the baseline results to alternate functional forms of the outcome variable. The unit of observation is the pincode. The outcome of interest in the odd-numbered columns is average monthly cash withdrawals from ATMs in a quarter; in the even-numbered columns, average volume of monthly digital payments undertaken through POS terminals. Columns (1)-(2) measure the outcome variables in levels; columns (3)-(4) consider an inverse hypoerbolic sine transformation. The sample is restricted to pincodes within 40 km of a currency chest (CC). *Near-Term* refers to 4 quarters between October 2016 and September 2017; *Long-Term* refers to 2 quarters between October 2017 and March 2018. All specifications include pincode and district-time fixed effects, along with pincode-specific time-varying covariates. Standard errors in parentheses, clustered by pincode.

Table E6
Cash Supply Shock, Cash Withdrawals and Digital Transactions:
Robustness to Collapsed Sample

	(1)	(2)	(3)	(4)
Dep. Variable	Log(Cash Withdrawals)		Log(Digital Transactions)	
Dist_{CC} × Near Term	-.006***	-.005***	.018***	.004*
	(.001)	(.001)	(.002)	(.002)
Dist_{CC} × Long Term	-.003***	-.002***	.022***	.005**
	(.001)	(.001)	(.002)	(.002)
Observations	49901	35038	47226	34976
R^2	.96	.98	.93	.94

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

Notes: This table shows the robustness of the baseline results to collapsing the time-variation in the sample to a single pre-treatment period, and 2 post-treatment periods. The unit of observation is the pincode. The outcome of interest in columns (1)-(2) is the (logged) average volume of monthly cash withdrawals from ATMs; in columns (3)-(4), the (logged) average volume of monthly digital payments undertaken through POS terminals. *Near-Term* refers to 4 quarters between October 2016 and September 2017; *Long-Term* refers to 2 quarters between October 2017 and March 2018. The sample is restricted to pincodes within 40 km of a currency chest (CC). All specifications include pincode and district-time fixed effects. Columns (2) and (4) also include pincode-specific time-varying covariates. Standard errors in parentheses, clustered by pincode.

Table E7
Heterogeneity in Cash Withdrawals Across Local Financial Infrastructure

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable	Log(Cash Withdrawals)					
	Pincode Measure			District Measure		
Dist_{CC} × Near Term	-.005***	-.005***	-.005***	-.005***	-.005***	-.005***
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Dist_{CC} × Long Term	-.002**	-.001	-.002**	-.001*	-.001	-.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Dist_{CC} × High POS × Near Term	.002*					
	(.001)					
Dist_{CC} × High POS × Long Term	.003*					
	(.002)					
Dist_{CC} × High Branch × Near Term		.000				
		(.001)				
Dist_{CC} × High Branch × Long Term		.000				
		(.001)				
Dist_{CC} × High ATM × Near Term			.001			
			(.001)			
Dist_{CC} × High ATM × Long Term			.003***			
			(.001)			
Dist_{CC} × High POS × Near Term				.001		
				(.001)		
Dist_{CC} × High POS × Long Term				.001		
				(.001)		
Dist_{CC} × High Branch × Near Term					.001	
					(.001)	
Dist_{CC} × High Branch × Long Term					.000	
					(.001)	
Dist_{CC} × High ATM × Near Term						.002**
						(.001)
Dist_{CC} × High ATM × Long Term						.001
						(.001)
Observations	104789	104789	104789	104780	104789	104780
R^2	.96	.96	.96	.96	.96	.96

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

Notes: This table estimates treatment heterogeneity across pincode and district measures of financial infrastructure. The unit of observation is the pincode. The outcome of interest is (logged) average volume of monthly cash withdrawals undertaken through ATM terminals. The sample is restricted to pincodes within 40 km of a currency chest (CC). *Near-Term* refers to 4 quarters between October 2016 and September 2017; *Long-Term* refers to 2 quarters between October 2017 and March 2018. Columns (1)-(3) considers pincode-level heterogeneity; columns (4)-(6) consider district-level heterogeneity. *High POS* is a dummy equaling 1 for pincodes/districts where the per capita POS terminals in the pincode/district exceed the national median. *High Branch* is a dummy equaling 1 for pincodes/districts where the per capita bank branches in the pincode/district exceed the national median. *High ATM* is a dummy equaling 1 for pincodes/districts where the per capita ATM terminals in the pincode/district exceed the national median. All specifications include pincode and district-time fixed effects along with pincode-specific time-varying covariates. Standard errors in parentheses, clustered by pincode.