Digital Payments and Consumption: Evidence from the 2016 Demonetization in India *

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Abstract

We study how consumer spending responds to digital payments, using the differential switch to digital payments across consumers induced by the sudden 2016 Indian Demonetization for identification. Use of digital payments rose by 2.94 percentage points and monthly spending increased by 2.38% for an additional 10 percentage points in prior cash dependence. Spending remained elevated even when cash availability recovered. Robustness analyses show that the spending response is not driven by purchase substitution, income shocks, credit supply, or price changes. We provide causal evidence that digital payments increase consumer spending due to subdued endowment effects.

JEL Codes: D12, D14, D91, E21.

Keywords: digital payments, financial technology, consumption, India, Demonetization, endowment effect, payment choice, behavioral finance.

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1 Introduction

The increasing digitization of the global economy is changing how products and services are produced, distributed, and sold all around the world. Digital payment instruments such as debit cards, credit cards, and mobile money have gained widespread popularity. Globally, the share of adults using digital payments rose by 11 percentage points from 41% to 52% between 2014 to 2017 (Demirgüç-Kunt et al., 2018, Chapter 4). Motivated by the reduction of operational costs and the improvement of financial inclusion brought by digital payment technologies, several governments have launched official programs to promote digital payments.[1]

In this paper, we study whether and how households' adoption of digital payments affects their spending decisions. Theoretically, digital payments can affect consumption through two channels. First, digital payments reduce transaction costs as they render storing, transporting, and counting paper bills and coins unnecessary. Second, they also evoke subdued endowment effects: Consumers feel less attached to their money with digital transactions.[2] Whereas both mechanisms lead to a prediction that the adoption of digital payments increases spending, the potential welfare implications for consumers are different. Given the rapid pace at which digital payments are displacing cash, understanding and assessing this effect is important.

Identifying the causal impact of digital payments on spending, however, is challenging empirically. The observed use of digital payments is an equilibrium out-

[1] The Prime Minister Jan Dhan Yojana scheme and RuPay cards in India, the Singapore Quick Response (SG QR) code in Singapore, and the Faster Payment System (FPS) in Hong Kong are some examples of government official programs. Relatedly, governments in Mexico, Brazil, South Africa, and Mongolia among others digitize government transfer payments.

[2] The endowment effect whereby individuals feel attached to their own money is closely related to the literature on the endowment effects of financial assets. For example, Anagol et al. (2018) identify a robust endowment effect for stock holdings, a phenomenon that is likely rooted in "warm glow" based explanations. In our setting, digital payments evoke weaker endowment effects than cash payments.

come that is affected by the availability of digital payments as well as both consumers' and merchants' awareness of and willingness to use digital payments. On the one hand, consumers do not have equal access to digital payments. On the other hand, merchants are not uniformly willing to accept digital payments. Small or stand-alone merchants quite often put restrictions for digital payments such as minimum spending.[3] Even in a setting where merchants are willing to accept digital payments and consumers have access, consumers may often choose to pay a small receipt with cash and switch to digital payments for a larger receipt. This leads to a mechanical relationship between receipt size and cash usage, hindering the causal inference of the impact of digital payments on spending.

We exploit a unique nationwide "Demonetization" shock to the digital payment adoption in India, combined with extensive anonymized transaction-level data from a large Indian supermarket chain, to identify the impact of digital payments on consumption. The Demonetization shock refers to the unexpected removal of 86% of the existing currency in circulation by the Indian government from midnight of November 8$^{\text{th}}$, 2016. The policy resulted in a sudden and sharp decline in the availability of cash and a forced uptake of digital payments. As consumers who relied more on cash prior to this policy were more affected by the forced switch to digital payments, we compare changes in spending patterns across individuals with varying degrees of prior cash dependence in a difference-in-differences (DiD) framework. We include individual fixed effects to absorb the impacts of time-invariant individual characteristics and district-by-year-month fixed effects to control for the impacts of underlying economic conditions that can vary by district, such as the district-specific exposure to the Demonetization (Chodorow-Reich et al., 2020; Crouzet et al., 2021).

[3] Consumers' adoption of digital payments can feed back into merchants' adoption choice, and vice versa (e.g., Higgins, 2020).

We verify the validity of the identifying assumption for the DiD empirical design, that is, the group characterized by a given prior cash dependence serves as a good counterfactual for other groups with different prior cash dependence. We show that the difference in spending across individuals with varying levels of prior cash dependence before the Demonetization announcement is economically negligible and statistically insignificant, which confirms the parallel-trends assumption. We also investigate whether observable differences among individuals with varying treatment intensity could drive our results by allowing these observable characteristics to affect the response to Demonetization. The results obtained from this alternative specification resemble those from the main analysis.

Next, we extend the baseline DiD framework to an instrumented DiD design, as in Hudson et al. (2017). We validate that prior cash dependence captures the extent of the forced switch to digital payments in the first stage: Usage of digital payments rose by 2.94 percentage points for an additional 10 percentage points in prior cash dependence following the Demonetization. Moreover, such a forced switch to digital payments is associated with a marked and highly statistically significant increase in spending: Moving from the 25^{th} to the 75^{th} percentile of prior cash dependence is associated with an 11.9% increase in spending. The Wald estimator suggests that a one percentage point increase in the digital payment fraction leads to a 0.81 percent increase in total spending.

The estimated effect reflects the local average treatment effect (LATE) for the sub-group of consumers who "complied" with the Demonetization shock and increased their digital payment usage following the shock, conditional on the exclusion restriction being satisfied (Imbens and Angrist, 1994). We perform a battery of tests to rule out various scenarios under which the exclusion restriction might be violated. For instance, we directly test a possible shift of unobserved purchases to those recorded in our data following the Demonetization. As food and non-durable products are typically more accessible through informal markets, any potential shift of purchases is more likely to affect those items. Hence, we examine whether the observed increased spending responses in supermarket data are concentrated primarily on food items. We find a markedly higher increase in non-food spending and durable goods spending, which contradicts what a shift of purchases from informal markets to supermarkets would predict. We also find that among consumers, a higher level of prior food spending is associated with a stronger spending response, opposite to what the anticipated outcome of shifting food purchases from informal markets to supermarkets.

In addition, we separately examine the responses of informal markets to the Demonetization. Using a different dataset containing detailed records of merchants' mobile payment use, we find that informal markets increased their usage of digital payments following the Demonetization, consistent with the findings of Crouzet et al. (2021). They also extended more informal credit. Both responses alleviate the negative impact of the Demonetization on their business and mitigate the extent to which consumers need to move purchases to the formal market. We also test for and rule out income shocks, credit supply, or price changes as plausible explanations for our findings. While the exclusion restriction cannot be statistically tested directly, these results support its validity.

Having established the causal impact of digital payments on consumption, we explore whether the effect is driven by lower transaction costs or subdued endowment effects. To achieve this focus, we exploit the different extent of the endowment effect associated with online and offline purchases. Online purchases are characterized by a time lag between the purchase decision and the delivery of goods. At the time of the purchase decision, both cash payment (i.e., cash on delivery) and digital payments involve no physical exchange of money between hands. Therefore, paying for an online purchase with cash invokes lower behavioral costs than paying for an offline purchase with cash. Given that the transaction costs associated with cash should apply equally to online and offline shopping, comparing consumer spending responses to digital payments in the supermarket with that in an online grocery store allows us to separate the channel through reduced transaction costs from the subdued endowment effect. We find that the spending responses are much muted in the online setting – it is one-third of the effect found in the propensity score matched supermarket panel. We also document a larger consumption response associated with temptation goods compared to non-temptation goods. The evidence suggests that the behavioral forces are crucial in driving our baseline findings.

This paper contributes to the literature on the economic impacts of digital payments. Jack and Suri (2014) show that digital payments reduce transaction costs and enhance risk sharing and consumption smoothing. Bachas et al. (2021) find that debit cards tied to existing savings accounts enable consumers to build trust and accumulate savings. In contrast, Breza et al. (2020) examine the effect of introducing digital or conventional payroll accounts to workers and find that consumers use mainly conventional but not digital accounts to save. While some experimental studies uncover an increase in consumers' willingness-to-pay associated with cards (Feinberg, 1986; Prelec and Simester, 2001), they do not involve real money transactions comparable to typical households' actual spending. By focusing on a forced uptake of digital payments following the sudden and unexpected 2016 Indian Demonetization and analyzing transaction-based spending data to trace out the effect of digital payment adoption on spending, we overcome the key limitations of the experimental studies and establish that digital payments lead to an increase in actual spending likely through subdued endowment effects. Our paper is also related to the findings by Agarwal et al. (2018), Chodorow-Reich et al. (2020), and Crouzet et al. (2021) that the drying-up of cash due to Demonetization leads to an increase in the adoption of digital payments.

This paper also contributes to the policy debate about the costs and benefits of moving to a cashless economy. Cash poses substantial operational costs to the economy as a whole: The central bank is responsible for manufacturing, quality control, circulation control, and counterfeit detection; banks spend resources in managing their ATMs, branches, teller services as well as deposit collection and handling of coins.[4] Moreover, there are indirect, societal costs of cash such as constraining the effectiveness of monetary policy and facilitating illegal activity and tax evasion (Rogoff, 2017). Moving to digital payments can potentially reduce these direct and indirect costs and therefore promote economic growth and efficiency. Given the heavy use of cash in India and many other emerging economies, such gain could be substantial. In spite of evidence supporting the welfare-enhancing effects of digital payments (Jack and Suri, 2014; Bachas et al., 2021), discussions regarding potential drawbacks are limited. Our paper provides causal evidence that digital payments lead to increased spending and documents that the spending response is likely driven by subdued endowment effects. Despite the caveat that the estimated effects pertain to the group of complying consumers and cannot be directly extrapolated to the average population, this finding suggests that a move from cash towards digital payments could unintentionally encourage some individuals to overspend, which could undermine their sound personal financial planning.

[4] In the primarily cash-based Indian economy, the total currency operation costs is estimated to be 210 billion rupees (3.15 billion dollars) annually (Mazzotta et al., 2014).

2 The 2016 Demonetization in India

On November 8th, 2016, at 8:15pm local time, the Indian Prime Minister Narendra Modi announced a Demonetization scheme in an unscheduled live television address: The two largest denomination notes, the 500 and 1000 rupee notes (7.5 and 15 dollars, respectively), would cease to be legal tender and be replaced by new 500 and 2000 rupee notes. Effective at midnight, holders of the old notes could deposit them at banks but could not use them in transactions. The stated objectives of the policy were to weed out black money, remove fake paper notes, and reduce corruption, tax evasion, and terrorism.[5]

At the time of the announcement, the demonetized 500 and 1000 notes accounted for 86% of currency in circulation. There was prolonged unavailability of new notes due to printing press constraints. Before the November 8th announcement, the government did not print and distribute a large number of new notes to maintain the secrecy of the policy. Total currency declined overnight by 75% and recovered only slowly over the next several months (Chodorow-Reich et al., 2020).

Such a large drop has profound impacts as India was a primarily cash-based economy. Currency in circulation accounts for almost 18% of India's GDP, compared to 3.5% to 8% in the United States and the United Kingdom. About 87% of the value of all transactions in 2012 was in cash (Mazzotta et al., 2014). In 2015, usage of debit cards at purchase transactions (point-of-sales machines) accounted for only around 12% of total volume and 6% of total value of debit card transactions; the remaining transactions are ATM transactions such as cash withdrawals and deposits, which would map into using cash at purchase transactions.[6] The large and sud-

[5]The Indian government had demonetized paper notes on two prior occasions — once in 1946 and once in 1978 — in both cases, the goal was to combat tax evasion and black money.

[6]Source: RBI's Concept Paper on Card Acceptance Infrastructure published on March 8th 2016, available at https://www.rbi.org.in/Scripts/PublicationReportDetails.aspx?UrlPage=&ID=

den Demonetization event in November 2016 represents a forced switch away from using cash for transactions. The economic costs associated with adopting digital payments are small for consumers as the ownership of bank accounts, debit cards, and mobile phones were very common in India by 2016 (Agarwal et al., 2022).[7]

3 Data

We use anonymized transaction-level data from a large Indian supermarket chain. The data comprise all purchases in 171 stores in twenty-one districts of five states from April 2016 to September 2017. For each purchase transaction, we observe the date and address of the store where the purchase was made. We also observe the payment method(s) and their shares if multiple payment methods were used to pay for the purchase. The main payment method categories include cash, debit cards, credit cards, and mobile payments.

We conduct our analysis at the individual consumer level. To this end, we focus on the purchases that involve the use of a loyalty card and therefore can be linked to individual consumers. These purchases account for 80% of all purchases we observe, consistent with the magnitude reported by Hastings and Shapiro (2018). We discuss sample construction in greater details in Online Appendix Section A. To ensure that the household-level changes in payment choice and spending following the Demonetization are well-defined, a necessary requirement for DiD research designs, we restrict the sample to consumers that started shopping at this chain before November 2016 and remained as customers afterwards. The household-level panel data set contains a total of 924,753 individual consumers.

840.

[7] As of November 2nd, 2016, there were 254.5 million new accounts and 194.4 million debit cards issued under the Prime Minister Jan Dhan Yojana (PMJDY) scheme. Source: PMJDY archive reports, available at https://pmjdy.gov.in/.

For each individual in our panel, we aggregate the purchases to the monthly level. Measures we use in our analysis include payment instruments usage, total spending and its composition, and spending variety and shopping intensity. All nominal variables are deflated to December 2015 real Indian rupee (INR) using India's overall CPI.[8] We code observations of flow variables as zero if the individual did not have any corresponding transactions in the given month.[9]

Table 1 presents the summary statistics of usage of payment instruments in the cross-section of individuals. For each individual, we calculate the share of spending paid by cash, debit cards, credit cards, and mobile payments, separately for the seven months prior to the Demonetization (i.e., April to October 2016) and the eleven months following the Demonetization announcement (i.e., November 2016 to September 2017). The average cash usage drops from 70% to 57% following the Demonetization; such a decline is mostly compensated by an increase in debit card usage from 24% to 35%. Usage of mobile payments and credit cards also increases modestly from the respective pre-Demonetization level.

4 Identification and Empirical Strategy

Our goal is to estimate the impact of digital payment use on consumer spending. Several important confounding factors hinder a straightforward causal identification when using an ordinary least squares (OLS) regression of spending on a measure of digital payment usage. One crucial omitted variable is access to digital payments, which is neither evenly distributed nor randomly assigned in the population. Prior research (e.g., Borzekowski and Kiser, 2008) shows that access to digital

[8] We obtain similar results if we do not deflate nominal values.

[9] Admittedly, zero-valued observations would drop out in log-linear regression specifications and may affect the consistency of the estimate. We show that we obtain estimates of similar economic magnitude in both the level and the log specifications in Section B.5.

payments can be influenced by socioeconomic status. Higher-income individuals tend to have better access to digital payment options and spend more relative to lower-income individuals. Moreover, even if we were to equalize access to digital payments across individuals, consumers may often choose to pay a small receipt with cash and switch to digital payments for a larger receipt. Therefore, both the omitted variable and the reverse causality are likely to bias the OLS estimate of the causal parameter of interest – the coefficient of the digital payment usage on an individual's spending – upward.

To tackle this identification challenge, ideally one would randomly assign identical consumers to cash and digital payment methods that are both accepted in the merchant. In this randomized setting, the difference in spending amount between cash users and digital payment users would be orthogonal to all individual characteristics and therefore reflect the impact of payment methods. In practice, we cannot impose the use of digital payments on individuals for real money transactions. However, the sudden dry-up of cash due to the Demonetization effectively compels cash-dependent consumers to adopt digital payment methods. This involuntary switch to digital payments breaks the correlation between the usage of digital payments and unobserved determinants of spending choices. Crucially for causal identification, both cash and digital payments are equally accepted in all supermarket stores in the data throughout the entire sample period, enabling us to sidestep the confounding factor of merchants' adoption choices.

For each individual consumer i, we measure the prior cash dependence as the share of spending paid by cash from April 2016 to October 2016, a continuous variable ranging between 0 and 1. By comparing changes in spending patterns across individuals with varying degrees of prior cash dependence, our empirical approach can be described as a DiD design with continuous treatment intensity.

Admittedly, there are observable and unobservable differences among individuals with differential pre-Demonetization reliance on cash, which could affect their consumption patterns. If our research design is valid, we expect to observe an economically and statistically insignificant difference in consumption across different levels of prior cash dependence during the pre-Demonetization period (i.e., parallel trends in the pre-treatment period).

Table 2 reports the correlation between prior cash dependence, the treatment intensity variable, and various spending characteristics. Although dependence on cash is likely orthogonal to the sudden Demonetization announcement, it may be endogenously related to wealth and other demographic variables. To the extent that the identifying assumption lies in the parallel trends assumption, the difference in spending level across different levels of prior cash dependence is of lesser concern, and we will test explicitly for the parallel trends prior to the Demonetization. In addition, we perform various diagnostic checks and falsification tests on the validity of the DiD strategy.

Our baseline panel regression specification is as follows:

$$y_{i,t} = \mu_i + \pi_{d,t} + \beta \cdot (PriorCashDependence_i \times Post_t) + \varepsilon_{i,t} \tag{1}$$

$y_{i,t}$ is a measure of spending behavior (spending amount, payment pattern) of consumer i in month t. The key variable of interest is the interaction term between $PriorCashDependence_i$ and $Post_t$, an indicator for post-Demonetization months. Its coefficient β measures the forced switch to digital payments.

We include a host of fixed effects to control for confounding factors that are invariant in certain dimensions. Individual fixed effects, μ_i, absorb fixed individual characteristics, whether observed or unobserved, disentangling the Demonetization shock from socioeconomic and demographic sources of omitted variable bias. Time fixed effects, $\pi_{d,t}$, further neutralize the impacts of common trends. The substantial variation in the supply of new paper bills after the Demonetization announcement across districts likely causes the common trends of observed within-individual changes in payment choice and spending to differ across different districts (Chodorow-Reich et al., 2020). To fully control for the impact of district-specific currency supply shocks, we include a separate set of time fixed effects for each district (hence the subscript d) for a cleaner identification. Standard errors in all regression analyses are doubly clustered at individual level and at month level.

In addition, we study the dynamics of the spending response using the following distributed lag model:

$$y_{i,t} = \mu_i + \pi_{d,t} + \sum_{t=-3}^{10} \beta_t \left(PriorCashDependence_i \times \mathbb{1}_t \right) + \varepsilon_{i,t} \tag{2}$$

where $\mathbb{1}_t$ is an indicator variable for each of the months before and after the Demonetization. The first four months in our sample period, April to July 2016, constitute the omitted baseline group. In this dynamic specification, the coefficients β_0 measures the immediate spending response during the Demonetization month. $\beta_1, \cdots, \beta_{10}$ track the spending response one month, two months, $\cdots$, and ten months after the Demonetization, respectively. Similarly, $\beta_{-3}, \cdots, \beta_{-1}$ capture the difference of trends in spending across individuals with varying prior reliance on cash in each of the three months before the Demonetization.

To better interpret our estimates for a causal relationship between digital payments and spending, we augment our baseline DiD framework with an instrumental variables (IV) framework. This approach, often referred to as an instrumented DiD design (Hudson et al., 2017), has been widely used and discussed in the literature, including Duflo (2001), Bhuller et al. (2013), De Chaisemartin and d'Haultfoeuille (2018), Hvide et al. (2022), and others. In this framework, we interpret the impact of the Demonetization on spending as the reduced form estimate and the impact on digital payment usage as the first-stage estimate. By taking the ratio between these two estimates, we obtain an IV estimate of the effect of digital payments on spending, known as the Wald estimator.

In addition to the standard parallel trend assumption for DiD designs, this instrumented DiD framework requires additional identifying assumptions that include a valid first stage and the satisfaction of the exclusion restriction. We use the first-stage coefficient and F-statistic to establish a valid first stage. As for the exclusion restriction, while we acknowledge that it cannot be statistically tested directly, we conduct an extensive battery of analyses to rule out several possible scenarios that may invalidate the exclusion restriction.

5 Evidence of Spending Increase Induced by Digital Payments

5.A Illustration using Two-Group Analyses of the Unconditional Patterns

We illustrate the core idea of our identification strategy in a two-group comparison between consumers with above- and below-median prior cash dependence in Figure 1. In the sample, the median prior cash dependence is 100%, so the two groups correspond to full cash users and mixed cash users prior to the Demonetization.

Panel (a) plots the average share of spending paid by digital payments for the two groups over time. In the seven months before the Demonetization, consumers with above-median prior cash dependence had a 0% use of digital payments by construction and consumers with below-median prior cash dependence had a stable average use of digital payments of 58%. The average use of digital payments during this period likely reflects the equilibrium choice for payment methods in the steady-state absent from a cash shortage such as the Demonetization. In November 2016, when the Demonetization occurred, consumers with above-median prior cash dependence increased their use of digital payments by more than 20 percentage points, whereas consumers with below-median prior cash dependence increased by 11 percentage points. This implies that the Demonetization disproportionately affected the payment choice of more cash-dependent consumers and forced them to switch to digital payments. Panel (b) plots the average level of the natural logarithm of spending amount for the two groups over time. Overall, consumers with above-median prior cash dependence have lower spending than consumers with below-median prior cash dependence, consistent with the notion that wealthier and higher-income individuals have better access to digital payments than less wealthy and lower-income individuals. The average spending of both groups appeared to be stable in the seven months prior to the Demonetization, lending credence to the validity of the parallel trends assumption. In November 2016, consumers with above-median prior cash dependence increased their spending by more than 30%, whereas consumers with below-median prior cash dependence had little change in their spending. In the ten months following the Demonetization, the average digital payment use and spending of consumers with above-median prior cash dependence did not appear to reverse back to pre-Demonetization levels despite replenishment of the demonetized notes.

This graphical analysis of unconditional means demonstrates our main finding qualitatively: Consumers who used to rely on cash for supermarket spending were forced to switch to digital payments by the Demonetization and, at the same time, increased spending significantly. Such a spending response persists despite the gradual replenishment of the demonetized notes.

5.B Forced Switch to Digital Payments and Its Effect on Spending

We estimate equation (1) to examine the relationship between prior dependence on cash and a consumer's payment choice and spending following the Demonetization. We report the results in Table 3.

Column 1 shows the forced switch to digital payments induced by the Demonetization: An increase of 10 percentage points in the prior cash dependence is associated with a 2.94 percentage point increase in digital payments usage, following the Demonetization. Columns 2–4 decompose digital payments into debit cards, mobile payments, and debit cards. The decline in cash usage is mostly compensated by an increase in debit card usage. Adoption of mobile payments also has a statistically significant increase, albeit with a minuscule economic magnitude. On the contrary, high prior cash dependence leads to a small yet significantly lower credit card usage following the Demonetization.

Column 5 reports the result for the natural logarithm of spending amount and shows that an increase of 10 percentage points in the prior cash dependence is associated with a 2.38% increase in monthly spending. An analysis using the interquartile range of prior cash dependence can demonstrate the economic significance of this estimate: The 25$^{\text{th}}$ and 75$^{\text{th}}$ percentiles of prior cash dependence are 50% and 100%. Therefore, a consumer at the 75$^{\text{th}}$ percentile of prior cash dependence increases spending by 11.9% relative to a consumer at the 25$^{\text{th}}$ percentile.[10]

5.C Testing the Identifying Assumptions

The parallel-trends assumption. To explicitly examine the parallel-trends assumption, in Columns 1-2 of Table 4, we additionally control for $PriorCashDependence_i \times$

[10] Table OA.4 directly examines the level of spending by instruments and shows that a decrease in cash spending is mostly compensated by an increase in debit card spending.

$\mathbb{1}(Pre)$ with Pre equal to 1 for the three months prior to the Demonetization announcement (i.e., August to October 2016). The coefficient estimate of $PriorCashDependence_i \times \mathbb{1}(Pre)$ captures the difference among individuals with varying treatment intensity before the policy change. For the parallel-trends assumption to hold, the coefficient of $PriorCashDependence_i \times \mathbb{1}(Pre)$ should be statistically insignificant and economically small, which is what we find. This evidence confirms the validity of the parallel-trends assumption.

Controlling for the observable differences among individuals with varying treatment intensity. One challenge with the current identification is that consumers with varying degrees of prior cash dependence differ significantly along observable dimensions. The pre-Demonetization parallel trends across consumers with different treatment intensity have already mitigated the concern regarding our empirical strategy. Furthermore, the inclusion of individual fixed effects neutralizes the static impact of time-invariant individual characteristics such as demographic features and unobserved consumption preferences. To directly examine whether observable differences lead to differential responses to the Demonetization, we additionally control for $X_i \times \mathbb{1}(Post)$ with X_i corresponding to observable pre-Demonetization characteristics. We consider all observable pre-Demonetization characteristics as in Table 2 Panel B. In this augmented specification, we allow for these observable features to affect an individual's changes in payment choice and spending following the Demonetization. The results, reported in Columns 3-4 of Table 4, show that the coefficients of $PriorCashDependence_i \times \mathbb{1}(Post)$ do not change in any statistically significant sense.

5.D Dynamics of the Spending Response

We also examine the dynamic patterns of payment choice and spending responses. We estimate equation (2) and plot the estimated β_t coefficients in Figure 2.

Panel (a) shows the dynamic pattern of digital payment use. The coefficients correspond to the change in the fraction of spending paid by digital payments relative to the omitted period April to July 2016 (in percentage points) associated with a one percentage point increase in prior cash dependence. The estimates show that the use of digital payments was stable prior to the Demonetization, increased by 0.28 percentage point (for each one percentage point increase in prior cash dependence) in November 2016 when the Demonetization took place, and then remained elevated till the end of our sample period.

Panel (b) shows the dynamic pattern of monthly spending. The coefficients correspond to the proportional change in monthly spending relative to the omitted period April to July 2016 (in percentage points) associated with a one percentage point increase in the prior cash dependence. This analysis provides another test of the parallel trends assumption underlying our research design. Prior to the Demonetization, there is little change in spending across households with differential degrees of cash dependence. In November 2016, previously cash-reliant households increased their spending relative to the less cash-reliant households; the estimated differential change between the households at the 75^{th} and 25^{th} percentiles of prior cash dependence is 6%. The differential change continues to increase till the end of our sample period. The parallel pre-trend implies that spending would have been unlikely to change if not for the Demonetization, reinforcing our claim that the observed increase in spending by previously cash-reliant consumers is likely to capture the causal response to the adoption of digital payments.

5.E Instrumented Difference-in-Differences Results

In the instrumented DiD framework, we interpret the impact of the Demonetization on spending (Column 5 of Table 3) as the reduced form estimate and the impact of the Demonetization on digital payment usage (Column 1 of Table 3) as the first-stage estimate. By taking the ratio between these two estimates, we obtain an IV estimate of the effect of digital payments on spending, known as the Wald estimator. We present the OLS and IV estimates in Table 5. Compared to the OLS estimate of 0.943, the IV estimate of 0.809 is smaller. The Wald estimator suggests that a one percentage point increase in the digital payment fraction leads to a 0.81 percent increase in total spending. The difference in magnitude between the OLS estimate and the IV estimate presents bias correction toward the expected direction: Instrumenting the observed digital payment use with the interaction of prior cash dependence and the post-Demonetization indicator helps mitigate the omitted variable and the reverse causality issues. In the first stage of the IV (2SLS) estimation, the F-statistic exceeds 1,000, indicating a strong and valid first stage.

The impact of digital payment use on spending may vary across different individuals. Following the framework outlined by Imbens and Angrist (1994), under the assumptions of conditional independence, exclusion restriction, first stage, and monotonicity, our IV estimates can be interpreted as the LATE of digital payment use on spending. This refers to the average treatment effect for the subgroup of complying consumers who are induced to use digital payments due to the Demonetization-induced cash shortage.

We have justified the first stage above. Discussions for the exclusion restriction will be provided in Section 6. The conditional independence assumption is likely satisfied given that we observe clear parallel trends for both digital usage and overall spending. The monotonicity assumption requires that the Demonetization leads to households with a higher initial cash dependence to be weakly more likely to adopt digital payments in response to the induced cash shortage, which is plausible in our context. Therefore, our estimates could be interpreted as LATE. This interpretation also helps justify why the estimated elasticity of digital payment usage on spending is relatively large since it focuses on the specific group of individuals who were induced to adopt digital payments due to the Demonetization.

To gain insights into the characteristics of the complying consumers, we partition our sample of consumers based on income proxies and estimate the first stage for different subgroups. In our analysis, we use average spending prior to the Demonetization to proxy for income. Following Imbens and Angrist (1994), we calculate the relative probability that a complying consumer belongs to a particular subgroup which is the ratio of the first-stage estimate for that subgroup to the overall first-stage estimate.

Table 6 shows that complying consumers are more likely to be low-income households. The relative likelihood of a complier belonging to the below-median spending group is 1.34, indicating a higher probability compared to non-compliers. Conversely, the relative likelihood of a complier belonging to the above-median spending group is 0.952, indicating a slightly lower probability compared to the non-compliers. Further partitioning by spending deciles, we observe that the relative likelihood of a complier belonging to the lowest decile spending group is the highest at 2.007 among all decile spending groups. This suggests that the complying consumers are more likely to be the lowest-income households or those with the lowest level of spending. Taken together, consumers who are prompted to use digital payments due to the cash shortage brought about by Demonetization are more likely to be consumers with lower incomes or limited financial resources.

6 Additional Tests to Validate the Exclusion Restriction

While the exclusion restriction cannot be statistically tested directly, we conduct an extensive battery of analyses to explore and rule out various potential scenarios in which the exclusion restriction might be violated. Previously in Section 5.C, we investigate whether observable differences among individuals with varying treatment intensity could drive our results by allowing these observable characteristics to affect the response to Demonetization. The results obtained from this tighter specification resemble those from the main analysis. In this section, we perform additional tests to address additional concerns on consumers' moving to the formal markets, income shocks, credit supply, and price changes.

6.A Addressing the Identifying Concern of Purchase Substitution

A concern for our identification strategy arises from the possible shift from unobserved purchases to purchases recorded in our data. If cash users used to buy groceries from informal markets, such as wet markets and street stalls, and moved their purchases to the supermarket after the Demonetization, they would have a higher spending response as captured by the data.

Our findings are unlikely to be attributable to the purchase shift. First, new consumers that arrived after the Demonetization are excluded from our analysis. Our estimation is not affected by the shift from informal markets to the supermarket in the form of newly arrived consumers. Second, as food and non-durable products are typically more accessible through informal markets, any potential shift of purchases is more likely to affect those items. Hence, we examine whether the observed increased spending responses in supermarket data are concentrated primarily on food items in Table 7. We find a markedly higher increase in non-food spending and durable goods spending, which contradicts what a shift of purchases from informal markets to supermarkets would predict.

We also test for heterogeneous shifts of purchases from informal markets to the supermarket across consumers. We hypothesize that consumers who mainly bought non-food goods in the supermarket chain are likely to be those who are shifting their food purchases and therefore they should exhibit a higher spending response following the Demonetization.

To test this, we divide all individuals into two groups based on whether the share of food spending prior to the Demonetization reaches the median level (88%, Table 2). We examine the fraction of spending paid by digital payments, log spending, and the share of food spending for the two groups separately and report the estimates in Table 8. Although the switch to digital payments is roughly equalized between the two groups, the spending response is higher among individuals with above-the-median prior food spending, opposite of what the heterogeneous shifts of purchase would predict. The increase in the share of food spending observed among individuals with below-the-median prior food spending lends some support for a shift of purchases from informal markets to the supermarket. On the contrary, individuals with above-the-median prior food spending increased their spending but decreased their share of food spending, implying that their spending response is not driven by the shift of purchases from informal markets to the supermarket.

These analyses aim to provide substantial evidence and assure that the exclusion restriction is likely to hold, strengthening the validity of our causal interpretation.

We also examine the responses of informal markets to the Demonetization to further address this concern. The extent to which consumers may move their purchases from informal markets to formal markets is affected by the responses of informal markets to the Demonetization. The need to migrate purchases is strong if the cash-based nature of informal market activities has not changed much. On the other hand, if informal markets increased adoption of digital payments after the Demonetization, their own responses can limit the extent to which consumers move their purchases to formal markets.

For this analysis, we obtain data from a leading provider of mobile payment in India. The data comprise merchant-level weekly records of transaction volume and amount in fifteen major cities in India. To analyze the responses of informal markets, we restrict to the sample to wet markets and street stalls, which are also known as "kirana" stores in India.

We measure the time of adoption for a merchant as the first week that the merchant has positive e-wallet transactions and therefore is included in the data. The number of new kirana stores that adopted the mobile payment, as shown in Figure 3, increased substantially immediately after the Demonetization announcement. In addition, the kirana stores that had already adopted the mobile payment four weeks prior to the Demonetization announcement also experienced substantially more transactions paid with digital payments (Figure 4).

The fast growing usage of digital payments in informal markets is consistent with the findings of Crouzet et al. (2021). Furthermore, the extension of informal credit to regular consumers is a common practice in informal markets, especially in developing economies. Besides adopting digital payments, increasing the supply of informal credit represents another way for kirana stores to counteract the negative impact of the cash shortage on their business. Although formal tests remain difficult as informal credit is difficult to measure systematically by definition, anecdotal evidence does suggest that kirana stores extended more informal credit to their regular consumers in the period immediately after the Demonetization announcement.[11]

[11] This phenomenon has been reported by the Economic Times, Firstpost, and the Indian Express,

In sum, kirana stores increased usage of digital payments and extended more informal credit when faced with the cash shortage. Both behaviors alleviate the negative impact of the Demonetization on their business and make the migration of consumer purchases to the formal market less likely to occur.

To further address concerns about our findings being driven by an increase in supermarket spending at the expense of other types of spending, we obtain and analyze a separate data set to investigate how the forced switch to digital payments affects a different type of spending, spending on online food delivery. The results are presented in in Online Appendix Section D. Crucially, these results also show no decrease in the examined types of spending, making it less plausible that our main findings are driven by an increase in supermarket spending at the expense of other types of spending.

6.B Addressing the Identifying Concern of Income Shocks

One might be concerned about an income shock channel whereby individuals who switch to digital payments due to the Demonetization shock experience positive income shocks and therefore increase their spending. To begin with, the elevated economic uncertainty and reduction in economic activities following the Demonetization render positive income shocks unlikely to occur.[12] The district$\times$year-month fixed effects we include in our regression specifications also directly control for the time-series fluctuation of national and regional economic conditions.

among others.

[12]The *ex-ante* secrecy and the slow and disorderly replenishment of notes associated with the Demonetization increased economic uncertainty. It is also widely believed that such a policy posed a painful disruption to the economy. For instance, the Conversation commented, "The implementation process faced technical disruptions, leading to severe cash shortages, and the overall poor preparation of the policy led the country into chaos for more than three months." (Source: http://theconversation.com/the-shock-of-indian-demonetisation-a-failed-attempt-to-formalise-the-economy-93328). Relatedly, Chodorow-Reich et al. (2020) find that the Demonetization lowered the growth rate of economic activity by at least 2 percentage points in the fourth quarter of 2016.

A more nuanced income shock explanation involves a re-allocation of (relative) income among individuals of varying exposure to the Demonetization shock. Economic activities in the informal sector, including black market activities, take a hit following the Demonetization as evidenced by the near complete returning of demonetized notes to the RBI.[13] Subramaniam (2020) documents that the informal sector experienced a negative income shock following the Demonetization. Black market activities are largely cash-based. Recipients of the black money payments in cash do not deposit into banks, as doing so would force them to justify the source of income and bear tax consequences. Instead, they tend to use cash to pay for their purchases. In our setting, they will exhibit a high level of cash dependence and therefore be classified as individuals with high treatment intensity. The contraction in black market activities implies that the income shock experienced by individuals with a higher prior dependence, if exists, is negative and therefore makes us underestimate the true positive impact of digital payments on spending.

To examine whether this conjecture holds in our data, we contrast the effect on households who were likely to engage in black market activities with that on other households. Since we do not directly observe households' source of income, we proxy for black market income with the behavior of paying large receipts with cash in the pre-Demonetization period. Spending cash on large receipts is a viable way for them to hide their black market income. On the contrary, using cash for large receipts is quite unusual in normal circumstances, given that small receipts tend to be paid by cash as discussed in Section 4.

In the empirical implementation, we define large receipts as receipts whose amount exceeds the 90$^{\text{th}}$ percentile (452 rupees in December 2015 real terms) in the size dis-

[13] According to the RBI's Annual Report 2017-18, 99% of total 500 and 1000 notes in circulation prior to the Demonetization were returned to the RBI, contrary to the earlier expectations that the restrictions on depositing money from unverifiable sources would lead to difficulty in absorbing black money and liquidation of RBI's currency liabilities.

tribution observed from all receipts paid by cash from April 2016 to October 2016.[14] Table 9 reports the estimation results. We find a much muted response by households who were likely to engage in black market activities, consistent with negative income shocks.

6.C Addressing the Identifying Concern of Credit Supply Changes

Credit cards, one of the digital alternatives to cash payment, allow consumers to borrow to spend. Such a feature relaxes the budget constraint and therefore may increase spending. If banks increase their supply of credit card lending, we might also observe an increase in spending.

In the aggregate, bank credit declined by at least 2 percentage points in 2016Q4 despite an inflow of deposits to the banking sector (Chodorow-Reich et al., 2020). In our context of supermarket spending, credit card usage remained low throughout the sample period; the decline in cash usage is mostly compensated by the uptick in debit card usage (Figure OA.1). Given the aggregate credit contraction and the low usage rate in our context, it is unlikely that credit supply is driving our results.

Can banks increase credit supply targeted to consumers who relied primarily on cash and thus relax their budget constraints more relative to other consumers? Drawing on the insights from the literature on credit history and access to credit, we expect banks to increase their supply of consumer credit to existing credit card users, who are not likely to be consumers who relied primarily on cash for supermarket spending prior to the Demonetization. This conjecture is supported by the result in Table 3 that high prior cash dependence leads to a significantly lower credit card usage, albeit small in magnitude, following the Demonetization. A positive relationship between credit history and access to credit, if anything, would lead us to

[14]For the sake of comparison, the 75$^{\text{th}}$ percentile of all receipts in the full sample, regardless of payment method, is 290 rupees in December 2015 real terms.

underestimate the positive effect of digital payments on spending.

To further investigate whether there is a shift in credit supply following the Demonetization and the extent to which this credit supply channel at work affects our results, we re-estimate equation (1) for three subsamples based on credit card usage– existing users, defined as consumers who used credit cards before the Demonetization; new users, defined as consumers who started to use credit cards following the Demonetization; and non-users, defined as consumers who never used any credit card in the sample period. The results are reported in Table 10.

The spending response associated with prior cash dependence has a smaller magnitude in the sample of existing users (column 2) than in the full sample (column 1). Existing users are also characterized by a markedly lower prior cash dependence. Since they had already adopted digital payments to a large extent, it is not surprising that they do not appear to be affected by the Demonetization as much. Among them, the credit card usage prior to the Demonetization can be viewed as a proxy for the strength of the relationship with banks. We add an interaction term of prior credit card usage and the post-Demonetization indicator to the baseline specification in column 3. The coefficient of this additional interaction term is positive, suggesting that an increase in credit supply contributes to the increase in spending for consumers with stronger relationships with banks. Column 4 shows that the spending response associated with prior cash dependence is larger in the sample of new users. Note that the post-Demonetization spending by new users was influenced by their newly obtained credit card borrowing capacity. Therefore, the difference in the spending response of new users relative to that of non-users can be viewed as an estimate of the added effect of credit supply. Column 5 shows that spending response associated with prior cash dependence in the sample of non-users is almost identical to the full-sample estimate. The comparison of sample sizes shows that the majority of consumers in our sample are non-users — 88% in terms of individual-monthly observations.

Taken together, the results show that an increase in credit supply affects a small fraction of consumers, at best, empirically. Our main results are not driven by the potential confounder of credit supply response.

6.D Addressing the Identifying Concern of Supplier's Pricing Response

We next consider if the estimated effect of digital payments on spending can be explained by an increase in product prices. If product suppliers, either the manufacturers or the supermarket chain, anticipate consumers to become less price sensitive following the adoption of digital payments, they could potentially take advantage of this by increasing their mark-up.

To begin with, aggregate fluctuations in price levels do not affect our analysis as we deflate all nominal variables to December 2015 real INR. In addition, the district$\times$year-month fixed effects we include in our regressions further neutralize any district-specific time-series fluctuation of the general price levels.

Thus, for the increase in mark-up to qualify as an explanation for our results, it has to be the case that the product mark-up is somehow larger for consumers with a high prior cash dependence. As suppliers cannot achieve perfect price discrimination, that is, they cannot directly charge different consumers different prices for the same product at the same store and at the same time, this alternative explanation must involve consumers with different prior cash dependence having different spending profiles.

To test this possibility, we construct a measure of exposure to cash-dependent consumers for each product by taking the average of consumer-level reliance on cash, weighted by the spending amount from April 2016 to October 2016. We sort all products into high-exposure and low-exposure groups based on the median exposure. We then examine whether the price of high-exposure products increases faster relative to low-exposure products in Figure 5.[15] We find no evidence that high-exposure products experienced a larger price increase than low-exposure products.

7 Additional Analyses and Discussions

Thus far, we have documented that the usage of digital payments increased sharply following the Demonetization and as a result, households who previously relied more on cash payments increased their supermarket spending. This finding rejects the prediction of monetary neutrality that consumer valuation of products and services is independent of how money is represented.

Payment instruments have distinctive features that can influence consumer behaviors. Our finding is consistent with two channels. The first involves the *transaction costs* associated with using cash, such as the storage cost, the time costs of traveling to a bank branch or an ATM to withdraw cash (Bachas et al., 2018), and the risk of cash theft (Economides and Jeziorski, 2017; Rogoff, 2014). Using digital payment instruments for purchases can save these transaction costs and hence increase consumer spending, especially spending by those mostly affected by the transaction costs.

The second channel encompasses the various behavioral implications associated

[15]We use the following regression:

$$y_{i,j,t} = \mu_i + \pi_j + \sum_{t \neq 0} \beta_t \mathbb{1}_t + \sum_{t \neq 0} \gamma_t \left(\mathbb{1}_t \times \mathbb{1} \left(HighExposure_i \right) \right) + \varepsilon_{i,j,t} \tag{3}$$

The dependent variable $y_{i,j,t}$ is the log of the mean transaction price of product i in store j on day t. $\mathbb{1}_t$ are monthly indicators; month 0 corresponds to November 2016 when the Demonetization took place and is the omitted baseline group. In this log-linear specification, the exponentiated coefficient for the interaction between month t and the high exposure indicator γ_t corresponds to the incremental change in the price level of month t (normalized by the price level in November 2016) of high-exposure products relative to low-exposure products. We plot the exponentiated γ_t in Figure 5.

with cash payment being effortful, instant, and memorable. The behavioral channel can involve several aspects. In one aspect, the effortful and costly cash payment can serve as a decision point for consumers to evaluate their expenses, while card and mobile payments remove the decision point and hence make spending easier. A different aspect is described as *"pain of paying"* or *payment transparency* (Prelec and Loewenstein, 1998; Soman, 2003; Raghubir and Srivastava, 2008). Cash payment is perceived to be painful because the consumer has to physically endure the act of parting with their hard-earned money. On the contrary, card and mobile payments are perceived to be less painful as no money actually exchanges hands. Another aspect concerns the usefulness for budgeting. Cash payment is considered to be useful for budgeting as cash gives a signal of the remaining budget via a glance into one's pocket (von Kalckreuth et al., 2014) or serves as a commitment device to avoid over-spending (Hernandez et al., 2017). Digital payments can be somewhat useful for budgeting but require some extra effort in terms of logging into the bank account or memorizing the pre-set budget. Relatedly, Li (2023) models cash to be more useful for budgeting than debit cards.

7.A A Simple Framework of Endowment Effects

We characterize these different aspects of the behavioral channel collectively as the subdued endowment effect: Consumers feel less attached to their money with digital transactions. More formally, suppose that an individual's expected payoff from a consumption stream $\{c_t\}_{t=0}^{\infty}$ is

$$E\left[\sum_{t=0}^{\infty} \delta u_t U(c_t)\right], \tag{4}$$

where U is a Constant Relative Risk Aversion (CRRA) utility function with a coefficient of relative risk aversion $\sigma > 0$. $U(c_t)$ measures the utility derived from consuming c_t in period t. u_t is a taste shifter. $0 < \delta < 1$ is the exponential discount factor. We assume that before the Demonetization ($t \leq t_0$), $u_t = u_A$, and after the Demonetization when individuals start taking up digital payments ($t > t_0$), $u_t = u_B > u_A$, because of subdued endowment effects as the digital nature of transactions prompts consumers to pay less attention to the same amount of money used for one unit of consumption. Consequently, the marginal utility of consumption increases following the Demonetization shock.

The individual begins with an initial wealth y_0, and subsequent wealth is generated by the returns from savings in the prior period at an interest rate R.[16] The CRRA utility function ensures that the individual's consumption in period t is $c_t = c_P(u_t)y_t$, where $c_P(u) = \frac{u^{1/\sigma}C}{u^{1/\sigma}C+\delta^{1/\sigma}R^{(1-\sigma)/\sigma}}$ and C solves $E\left[(Cu^{1/\sigma} + \delta^{1/\sigma}R^{(1-\sigma)/\sigma})^{\sigma}\right] = 1$. Therefore, given that $u_B > u_A$, consumption would experience a jump after the Demonetization policy. Moreover, in a broader context, the subdued endowment effect could be further amplified by the existence of a present bias (Cohen et al., 2020; Ericson and Laibson, 2019; Cassidy, 2018; Kremer et al., 2019). This can be introduced as a present bias parameter, β, in addition to the exponential parameter, δ.[17] A present bias will further amplify the increase in spending following the Demonetization shock.

7.B Comparing Offline and Online Purchases

With these conceptual ideas in mind, we set out to empirically identify which channel, transaction costs or behavioral factors, qualifies as a more plausible explanation

[16] To ensure that the transversality condition is satisfied, we assume that $\delta R^{1-\sigma} < 1$.

[17] This yields an effective discount factor as the weighted average of the short-run discount factor, $\beta\delta$, and the long-run exponential discount factor, δ.

for our empirical finding. To do so, we exploit the differential endowment effects for offline and online purchases and compare consumer spending behaviors in the supermarket with an online grocery store. Online purchases of physical goods such as grocery products are characterized by a time lag between the purchase decision and the delivery of goods. Paying with cash for online shopping takes the form of cash on delivery, which is not fulfilled until the delivery takes place. At the time of the purchase decision, both cash payment and digital payments involve no physical exchange of money between hands. Therefore, paying for an online purchase with cash invokes either the decision point or the pain of paying to an lesser extent than paying for an offline purchase with cash. Crucially, the transaction costs associated with cash apply equally to online and offline shopping.

We apply our core empirical approach based on the cross-consumer variation in cash dependence prior to the Demonetization to study payment choice and consumer spending in the online grocery setting. We use the data from a large online grocery retailer in India and construct individual-monthly observations as in our main analysis.[18] We estimate equation (1) to examine how payment choice and spending changes for individuals with different levels of prior cash dependence following the Demonetization conditional on the inclusion of individual fixed effects and district×year-month fixed effects. As in our main analysis using the supermarket data, both cash and digital payments are accepted for all orders in the online grocery retailer throughout the entire sample period, enabling us to sidestep the confounding factor of merchants' adoption choices.

Table 11 reports the estimates obtained from the online grocery retailer data. The sample period, April 2016 to September 2017, covers eleven months following the Demonetization same as in our main analysis using the supermarket data. In Col-

[18] Additional details for sample construction and variable definitions can be found in Online Appendix Section C.

umn 1, we find that the forced switch to digital payments by previously cash-reliant individuals is stronger in the online retailer panel. On the contrary, Column 2 shows that the spending response is much muted. The estimated proportional increase in spending is 0.4 percentage points for every ten additional percentage points of prior cash dependence, or one-sixth of the effect found in the supermarket panel (column 5, Table 3). The varying degrees of responsiveness remain large even after we restrict the supermarket data to a subset of consumers sharing similar characteristics as the consumers of the online grocery retailer (columns 3 & 4).[19]

7.C Spending on Temptation Goods

Another important aspect is that the subdued endowment effect is likely amplified by the present bias. It is plausible that consumers who exhibit a strong endowment effect could be subject to a high degree of present bias, leading to their increased consumption being more concentrated on temptation goods. We directly test the implication of this behavioral bias by examining temptation spending (Gul and Pesendorfer, 2001; Banerjee and Mullainathan, 2010). Guided by prior empirical literature (e.g., Banerjee and Duflo, 2007), we define temptation goods to include tobacco, carbonated drinks, sweets, and instant-prepared food. Using this definition, we measure the extent of temptation spending for individual consumers in our sample. We present the findings in Table 12.

We first show the estimated effect of Demonetization on the probability of positive temptation and non-temptation spending, respectively. A coefficient of 0.034 in column 1 implies that an increase of 10 percentage points in the prior cash dependence is associated with a 0.34 percentage point increase in the probability of spending on temptation goods following the Demonetization. The increase is roughly

[19] Detailed matching procedure is presented in Online Appendix Section C.

0.5% of the average probability of 66%. On the contrary, column 2 shows that an increase of 10 percentage points in the prior cash dependence is associated with a 0.08 percentage point increase in the probability of spending on non-temptation goods, which is 0.08% of the pre-period average probability of 98%, following the Demonetization. A comparison of the two coefficients suggests that the increase in temptation spending is more substantial than that of non-temptation spending.

We split the sample of individual consumers into two sub-samples based on whether their pre-Demonetization monthly spending is above or below the median and report the results of the analysis in columns 3 to 6. Our findings indicate the increase in temptation spending is more pronounced among lower-spending consumers, consistent with the notion that low-wealth consumers with a high marginal propensity to consume tend to be more impatient. This evidence is also consistent with what Banerjee and Mullainathan (2010) show that the fraction of the marginal dollar spent on temptation goods decreases with overall consumption.

7.D Relationship with Existing Empirical Findings on the Demonetization

In this subsection, we compare our findings with the other analyses on the economic impacts of the Demonetization.

Chodorow-Reich et al. (2020) document that the Demonetization led to a decrease in output and consumption. We find that previously cash-reliant consumers switched to digital payments and increased spending after the Demonetization. Our identification differs from that of Chodorow-Reich et al. (2020) in that we rely on different sources of variation. Chodorow-Reich et al. (2020) utilize cross-district variation in exposure to the Demonetization and find that districts more affected by the shock experience a larger decline in economic activities. To understand whether it is the difference in the level of the underlying variation that gives rise to different outcomes, we aligned with their approach and re-estimated an alternative specification that exploits similar cross-district variation using our own data. The results of this analysis, presented in Online Appendix Section B.2, confirm that districts more exposed to the Demonetization did witness a larger decrease in consumer spending. To address the importance of cross-district variation in currency supply shocks, we control for district-by-year-month fixed effects in our baseline analysis. As a result, our identification strategy absorbs the cross-district variation that underlies the findings in Chodorow-Reich et al. (2020).

It is also important to note that our estimates reflect the effect of digital payment adoption on spending specifically for the sub-group of consumers who are prompted to use digital payments due to the cash shortage brought about by Demonetization. In other words, the estimates are better interpreted as the LATE among complying consumers. This interpretation also helps justify why the estimated elasticity of digital payment usage on spending is relatively large since it focuses on the specific group of individuals who were induced to adopt digital payments due to the Demonetization.

The complying consumers, disproportionately more likely to be consumers with lower incomes or limited financial resources, may not represent the behaviors of average individuals. We should exercise caution when extrapolating the LATE to the average treatment effect of digital payment usage for the entire population. However, clarifying the description of the population that the estimated effects are relevant does not necessarily undermine the credibility of the empirical work. It is, as emphasized in (Imbens, 2010, p.416), that "we may then wish to extrapolate to other subpopulations, even if only qualitatively, but given that the nature of those extrapolations is often substantially less credible than the inferences for the particular subpopulation, it may be useful to keep these extrapolations separate from the identification of the effect for compliers."

Our analyses highlight the role of subdued endowment effects of digital payments. Using online spending data from two separate sources (a leading online grocery retailer and a leading online food delivery platform), we document a strong forced switch to digital payments by previously cash-reliant consumers following the Demonetization and a smaller increase in online spending in Section 7.B and Online Appendix Sections C and D. These findings are consistent with the notion that paying for online purchases by paying cash on delivery invokes the behavioral costs associated with cash payment being effortful, instant, and memorable to a lesser extent than paying for offline purchases with cash at the time of the purchase decisions. The more muted increases in online spending, coupled with the finding of a larger consumption response associated with temptation goods compared to non-temptation goods in Section 7.C, suggests that behavioral factors rather than transaction costs more likely explain the large spending response we identify in the supermarket panel.

Lastly, the overall effects of the Demonetization on economic outcomes differ between the short term and the medium to long term. Chodorow-Reich et al. (2020) find the Demonetization led to an immediate output contraction in the same quarter and the effects dissipated over the next few months. Chanda and Cook (2022) find poorer regions experienced larger increases in economic activity in the medium term after the Demonetization. Our analysis and Aggarwal et al. (2023) show that the Demonetization-induced switch to digital payments, however, was persistent and did not revert back when cash availability recovered. Driven by the subdued endowment effects associated with digital payments, spending remained elevated even when new currency arrived.

8 Conclusion

Digital payment instruments provide faster and more convenient ways to pay for goods and services. Digital payments can also facilitate better financial intermediation, reduce transaction costs, and enable consumers to better smooth consumption by facilitating transfers within their informal networks. However, digital payments may also increase spending simply due to subdued endowment effects as consumers feel less attached to their money with digital transactions.

We identify the causal effect of digital payments adoption on spending, using the differential switch to digital payments across consumers induced by the sudden 2016 Indian Demonetization for identification. Using an instrumented DiD empirical approach that exploits the cross-consumer variation in pre-Demonetization cash dependence, we find that digital payments lead to a substantial increase in consumer spending. We show that shifting purchases to the formal market, income shocks, credit supply, and price changes are unlikely to explain our results. We also compare offline purchases with online purchases, where cash spending takes the form of cash on delivery and therefore becomes more similar to digital payments. The finding that the spending response is weaker for online purchases implies the key mechanisms underlying the spending response are the subdued endowment effects of digital payments. Together with the additional evidence that the increased spending is more concentrated in temptation goods compared to non-temptation goods, our analyses suggest potential over-spending driven by behavioral forces.

While both reduced transaction costs and subdued endowment effects lead to increases in spending, the two mechanisms of digital payments have different implications. Reductions in transaction costs can lead to lasting relaxation of household budget constraints and therefore facilitate consumption smoothing and enhance welfare. On the other hand, the subdued endowment effects, leading to weakened attachment to money, can be unsustainable within fixed budget constraints. Consequently, consumers may confront potential welfare losses. The net effects of digital payments on consumer welfare are shaped by the relative impacts of reduced transaction costs versus subdued endowment effects.

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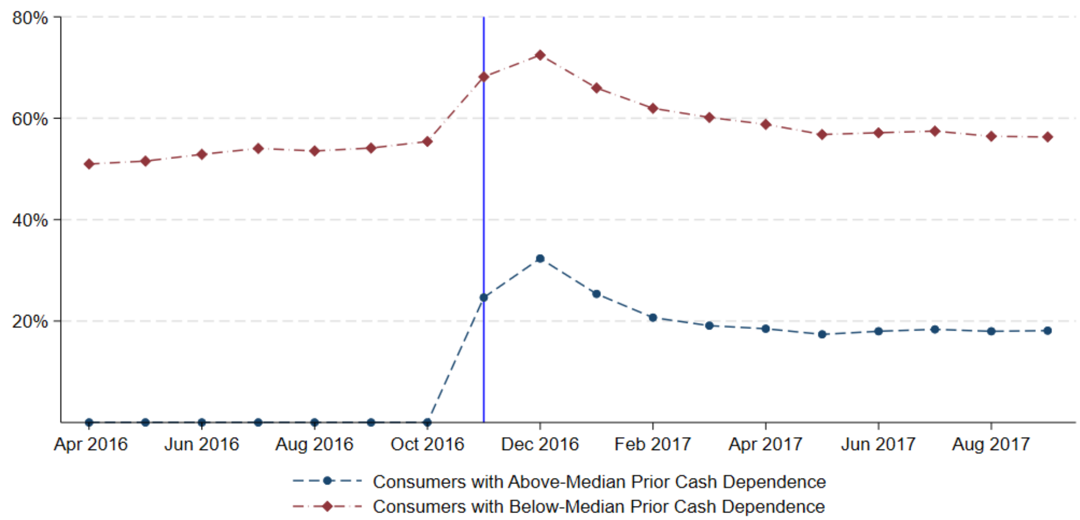
Figure 1: **Cash Usage and Spending Response to Demonetization (Two-Group Comparison)**

This figure plots the average use of digital payments and log spending for consumers with above- and below-median prior cash dependence over time. For each consumer in the sample, the prior cash dependence is calculated as the average share of spending paid by cash from April 2016 to October 2016. In the sample, the median prior cash dependence is 100%, so the two groups correspond to full cash users and mixed cash users prior to the Demonetization.

(a) Use of digital payments over time



(b) Log spending amount over time

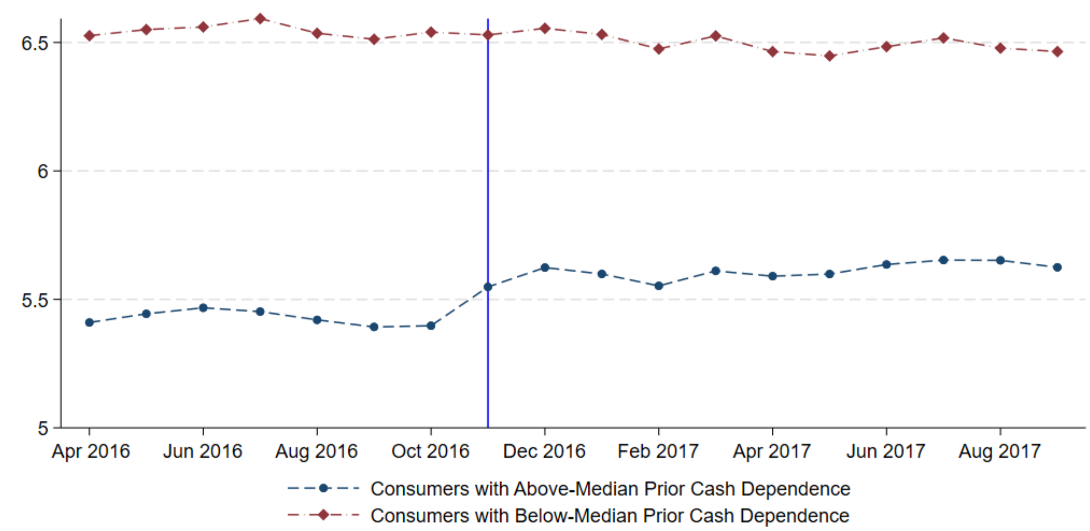
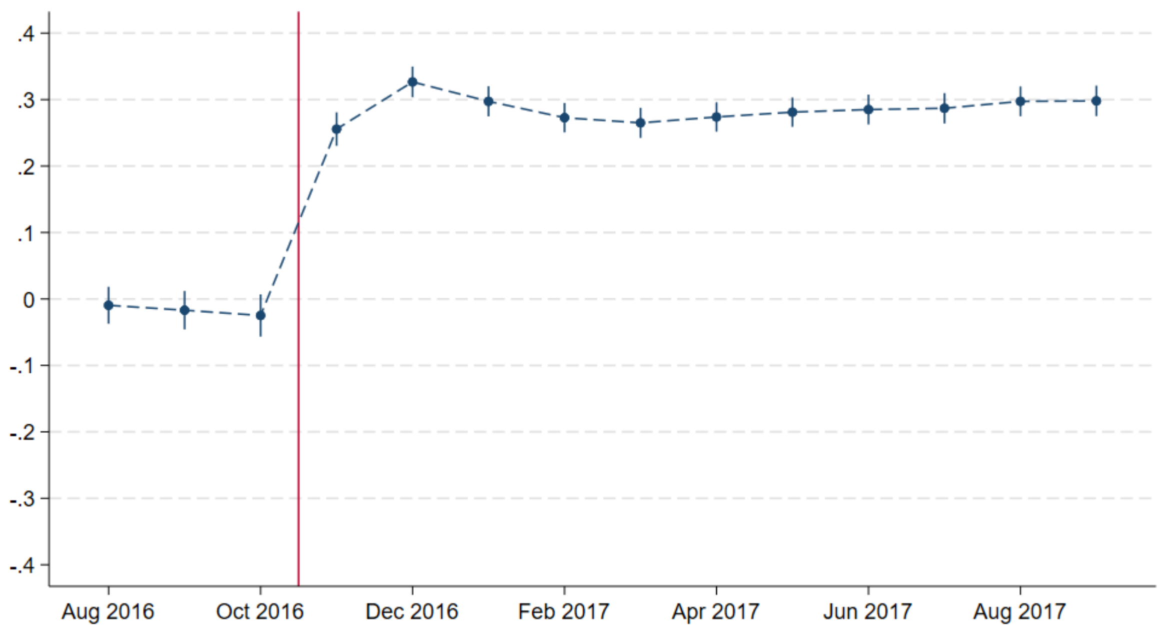


Figure 2: **Dynamic Effects of Digital Payments on Spending**

This figure plots the entire path of coefficients β_t along with their associated 95% confidence intervals of the fraction of spending paid by digital payments and the log level of spending as estimated from equation (2). Standard errors used to construct the confidence intervals in the dynamic regression are doubly clustered at individual level and at month level. The *x-axis* denotes the months (2016:04–2017:09). Demonetization took place in November 2016. In the dynamic specification, April to July 2016 constitute the omitted baseline group. The *y-axis* corresponds to the change in the use of digital payments (the proportional change in spending) relative to the benchmark level measured in the omitted period April to July 2016 in panel a (panel b).

(a) Cash usage



(b) Log spending

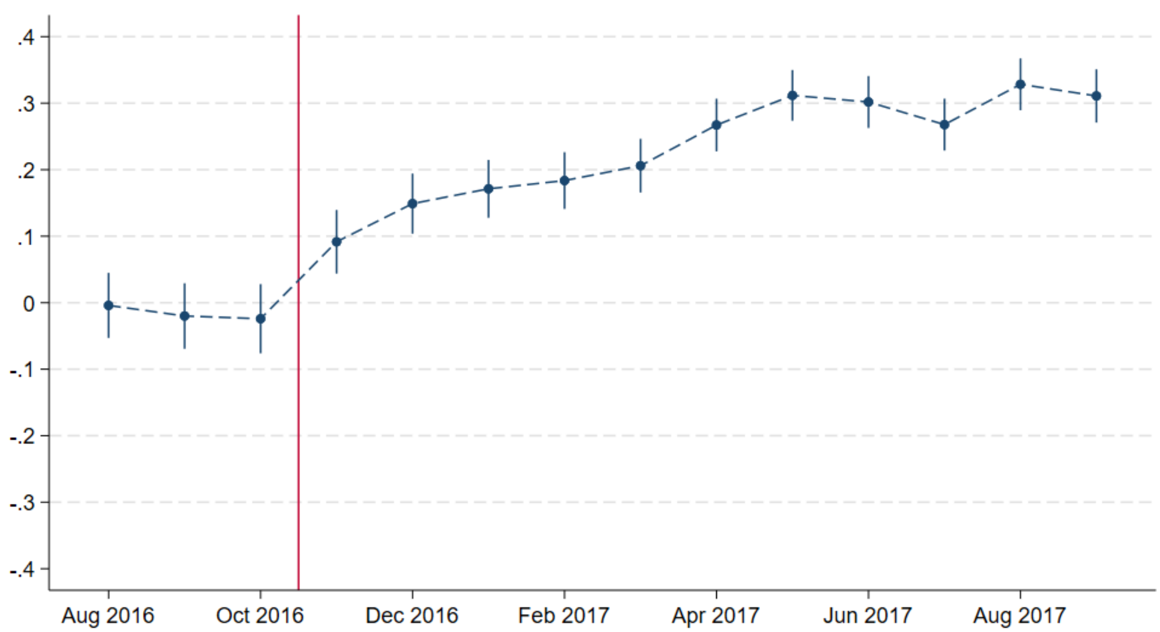


Figure 3: **Informal Markets' Adoption of Mobile Payment**

This figure plots the weekly flow of new kirana stores that newly adopted the mobile payment across fifteen major Indian cities from April 2016 to September 2017.

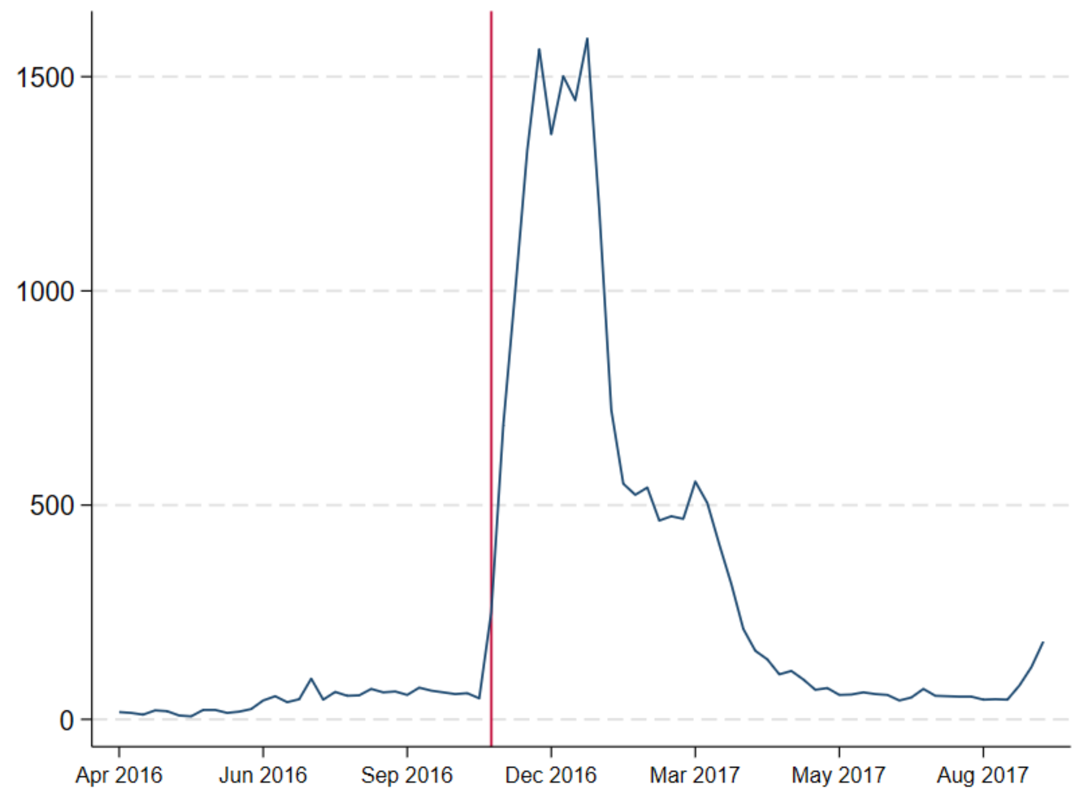
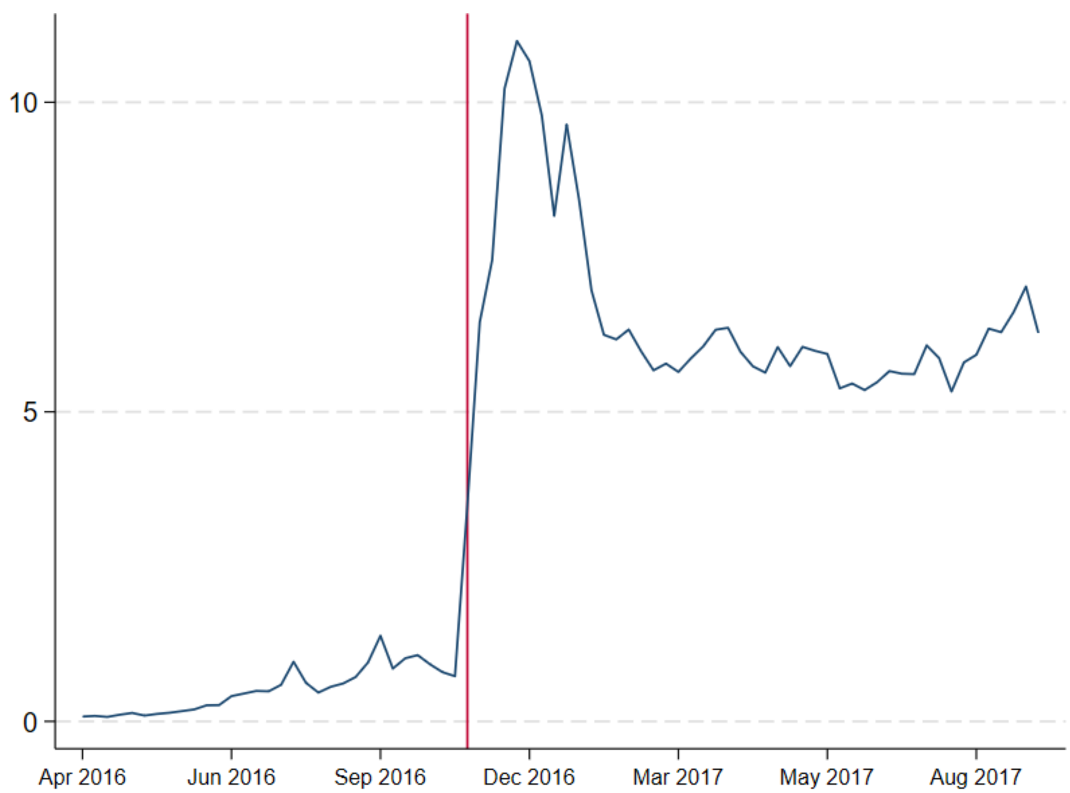


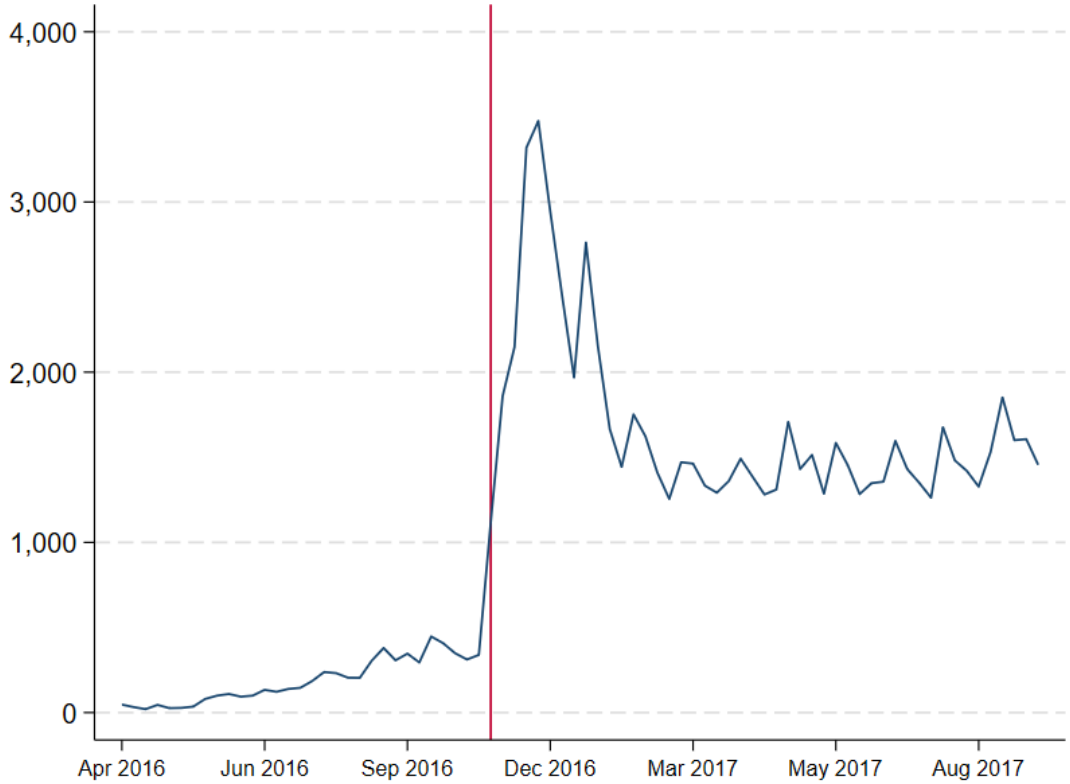
Figure 4: **Digital Payment Transactions in Informal Markets**

This figure plots the weekly average of digital payment transactions for the kirana stores that had already adopted the mobile payment four weeks prior to the Demonetization announcement. Demonetization took place in November 2016. Each panel corresponds to a measure of digital payment transactions, average weekly number of digital payment transactions across merchants in panel (a), average weekly amount of digital payment transactions across merchants in panel (b), and the ratio of merchants with at least one digital payment transaction in panel (c).

(a) Average weekly number of digital payment transactions across merchants



(b) Average weekly amount of digital payment transactions across merchants



(c) Ratio of merchants with at least one digital payment transaction

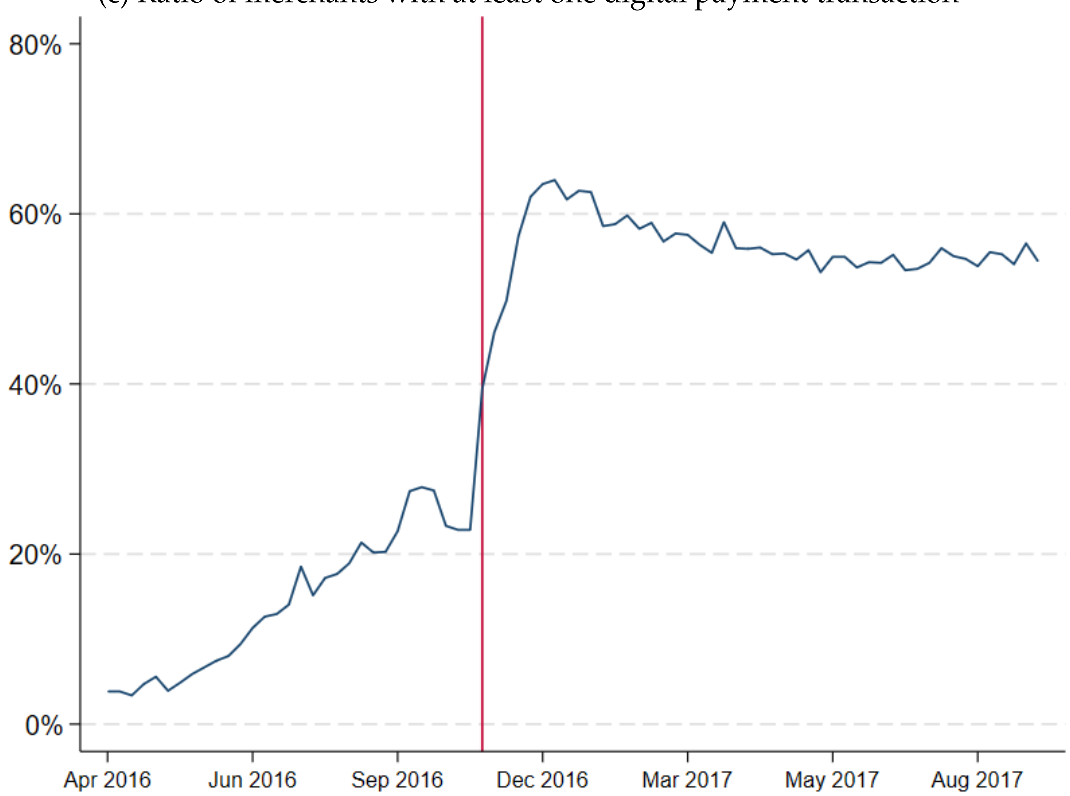


Figure 5: **Price Level by pre-Demonetization Exposure to Cash-Dependent Consumers**

This figure shows the price level of products sold by the supermarket chain, sorted by their pre-Demonetization exposure to cash-dependent individuals, in our sample at a monthly frequency. The figure plots the exponentiated coefficients γ_t and the associated 95% confidence intervals as estimated from equation (3). High (low) exposure products refer to products with above-the-median (below-the-median) exposure to cash-dependent consumers, calculated as the spending-amount-weighted average of consumer-level reliance on cash in the period from April 2016 to October 2016. In this log-linear specification, the exponentiated coefficient for the interaction between month t and the high exposure indicator corresponds to the incremental change in the price level of month t (normalized by the price level in November 2016) of high-exposure products relative to low-exposure products.

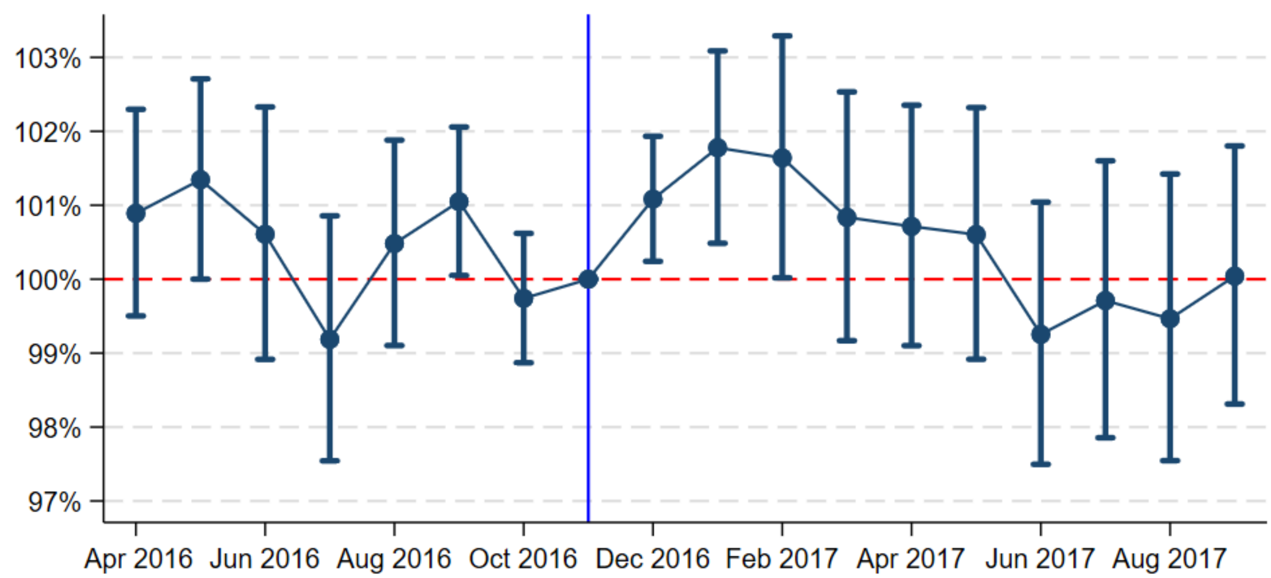


Table 1: **Summary Statistics of Consumer Payment Choice**

This table reports the summary statistics of consumer payment choice in our main analysis sample, which spans the period from April 2016 to September 2017. Additional details for sample construction and variable definitions can be found in Online Appendix Section A. We report the cross-sectional summary statistics of the fraction of each payment method in spending for the pre-Demonetization period (April to October 2016) and post-Demonetization period (November 2016 to September 2017) separately.

	Mean	Std. Dev.	25%	50%	75%
Fraction of payment mode in spending:					
Cash payment (pre-Demonetization)	0.70	0.40	0.29	1	1
Cash payment (post-Demonetization)	0.57	0.42	0.10	0.64	1
Debit cards (pre-Demonetization)	0.24	0.37	0	0	0.47
Debit cards (post-Demonetization)	0.35	0.39	0	0.13	0.74
Mobile payment (pre-Demonetization)	0.0023	0.039	0	0	0
Mobile payment (post-Demonetization)	0.0046	0.051	0	0	0
Credit cards (pre-Demonetization)	0.0075	0.066	0	0	0
Credit cards (post-Demonetization)	0.033	0.14	0	0	0
Number of households	924,753				

Table 2: **Summary Statistics of Consumer Characteristics and Covariate Balance**

This table examines the balance of pre-Demonetization characteristics in our main analysis sample. Additional details for sample construction and variable definitions can be found in Online Appendix Section A. Demonetization took place in November 2016; pre-Demonetization characteristics are measured in the seven months prior to that (April to October 2016). The monetary amount is the local currency Indian rupee (INR), December 2015 real terms, and 1 USD = 66.2 INR as of December 2015.

Panel A: Summary Statistics of Pre-Demonetization Observable Characteristics

	Mean	Std. Dev.	25%	50%	75%
Treatment intensity:					
Prior cash dependence	0.70	0.40	0.29	1	1
Total spending and its composition:					
Monthly spending (in Dec 2015 real INR)	560.3	11872.7	68.8	197.9	553.7
Share of food spending	0.78	0.28	0.65	0.88	1
Share of non-food spending	0.22	0.28	0	0.12	0.35
Share of durable spending	0.0084	0.056	0	0	0
Share of non-durable spending	0.99	0.056	1	1	1
Spending variety and shopping intensity:					
Product variety	10.3	11.8	3	6.33	13
Broad category variety	2.31	1.06	1.50	2	3
Category variety	5.34	4.45	2	4	7
Shop variety	1.02	0.13	1	1	1
Number of shopping trips	1.71	1.55	1	1	2
Number of households	924,753				

Panel B: Correlation between Treatment Intensity and Pre-Demonetization Observable Characteristics

	Correlation
Indicator for registration record containing age	0.00084
Indicator for registration record containing gender	0.0082
Indicator for registration record containing marital status	0.0068
Percentile rank of monthly spending	-0.38
Share of food spending	0.091
Share of durable spending	-0.054
Product variety	-0.33
Broad category variety	-0.36
Category variety	-0.38
Shop variety	-0.040
Number of shopping trips	-0.050

Table 3: **Forced Switch to Digital Payments and Its Effect on Spending**

This table shows the effect of the forced switch to digital payments due to the Demonetization on payment methods and spending (equation (1)). The data are at the individual-month level (April 2016 to September 2017). Outcome variables include the fraction of spending paid by digital payments (and the decomposition of digital payments into debit cards, mobile payments, and debit cards) as well as the log level of spending. Prior cash dependence is the share of spending paid by cash from April 2016 to October 2016 for each consumer. Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	Fraction of payment mode in spending				Spending
	(1) Digital payment fraction	(2) Debit card fraction	(3) Mobile payment fraction	(4) Credit card fraction	(5) Log spending
PriorCashDependence × Post	0.294***	0.260***	0.001	-0.018**	0.238***
	[33.09]	[37.51]	[1.81]	[-3.51]	[9.92]
Individual FEs	Yes	Yes	Yes	Yes	Yes
District × Year-Month FEs	Yes	Yes	Yes	Yes	Yes
R^2	0.620	0.564	0.350	0.403	0.586
No. of Observations	6,561,580	6,561,580	6,561,580	6,561,580	6,561,580

Table 4: **Testing the Identifying Assumptions**

This table presents various diagnostic tests of the identifying assumptions by augmenting equation (1) with additional controls. The data are at the individual-month level (April 2016 to September 2017). Outcome variables include the fraction of spending paid by digital payments and the log level of spending. Prior cash dependence is the share of spending paid by cash from April 2016 to October 2016 for each consumer. Pre is an indicator for the three months immediately before the Demonetization (i.e., August to October 2016). Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. In Columns 3 & 4, we also include the interaction terms of all observable pre-Demonetization characteristics as in Table 2 Panel B with the post indicator. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	(1) Digital payment fraction	(2) Log spending	(3) Digital payment fraction	(4) Log spending
PriorCashDependence × Previous 3 months	-0.017	-0.014	-0.021	-0.039
	[-1.49]	[-0.69]	[-1.73]	[-1.61]
PriorCashDependence × Post	0.286***	0.231***	0.435***	0.234***
	[25.68]	[8.22]	[27.42]	[11.32]
Individual FEs	Yes	Yes	Yes	Yes
District × Year-Month FEs	Yes	Yes	Yes	Yes
Controlling for observables	No	No	Yes	Yes
R^2	0.620	0.586	0.621	0.595
No. of Observations	6,561,580	6,561,580	6,561,580	6,561,580

Table 5: **Effect of Digital Payments on Spending (OLS and IV Estimates)**

This table compares the OLS and IV (2SLS) estimates for the effect of digital payments on spending. The data are at the individual-month level (April 2016 to September 2017). The outcome variable is the log level of spending. The endogenous explanatory variable is the fraction of spending paid by digital payments. The instrument is calculated as an interaction of prior cash dependence, the share of spending paid by cash from April 2016 to October 2016 for each consumer, and a post indicator for post-Demonetization months. Fixed effects are denoted at the bottom. The within R^2 is within the nested fixed effects. For the IV results, we also report the first-stage F statistic for assessing instrument relevance. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	Log spending	
	(1) OLS	(2) IV (2SLS)
Digital payment fraction	0.943***	0.809***
	[125.67]	[10.78]
Individual FEs	Yes	Yes
District $\times$ Year-Month FEs	Yes	Yes
Within R^2	0.0890	0.0872
First-stage F statistic		1,095.1
No. of Observations	6,561,580	6,561,580

Table 6: **Characteristics of Complying Consumers**

This table examines the characteristics of complying consumers, i.e., the consumers who are prompted to use digital payments due to the cash shortage brought about by Demonetization. Following Imbens and Angrist (1994), the relative likelihood of a complying consumer belongs to a particular subgroup in the sample is the ratio of the first-stage estimate for that subgroup to the overall first-stage estimate.

Sub-groups	Subgroup-specific first-stage β	Overall first-stage β	Relative likelihood
Pre-Demonetization spending split by 2 groups			
Above-median spending	0.280	0.294	0.952
Below-median spending	0.394	0.294	1.340
Pre-Demonetization spending split by 10 groups			
Decile 1 spending (lowest decile)	0.590	0.294	2.007
Decile 2 spending	0.490	0.294	1.667
Decile 3 spending	0.420	0.294	1.429
Decile 4 spending	0.374	0.294	1.272
Decile 5 spending	0.332	0.294	1.129
Decile 6 spending	0.309	0.294	1.051
Decile 7 spending	0.290	0.294	0.986
Decile 8 spending	0.284	0.294	0.966
Decile 9 spending	0.281	0.294	0.956
Decile 10 spending (highest decile)	0.296	0.294	1.007

Table 7: **Digital Payments and Different Spending Components**

This table shows the effect of the forced switch to digital payments due to the Demonetization on different components of spending (equation (1)). The data are at the individual-month level (April 2016 to September 2017). Prior cash dependence is the share of spending paid by cash from April 2016 to October 2016 for each consumer. Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	Differentiate food & non-food spending		Differentiate durable & non-durable spending	
	(1)	(2)	(3)	(4)
	$\mathbb{1}$(Food spending > 0)	$\mathbb{1}$(Non-food spending > 0)	$\mathbb{1}$(Durable spending > 0)	$\mathbb{1}$(Non-durable spending > 0)
PriorCashDependence × Post	0.004**	0.050***	0.011***	0.000
	[3.33]	[13.75]	[7.97]	[0.78]
Individual FEs	Yes	Yes	Yes	Yes
District × Year-Month FEs	Yes	Yes	Yes	Yes
R^2	0.345	0.443	0.243	0.251
No. of Observations	6,561,580	6,561,580	6,561,580	6,561,580

Table 8: **Is Increased Spending Driven by the Shift to the Formal Market?**

This table estimates the effect of the forced switch to digital payments due to the Demonetization on payment methods and spending for two subsamples classified by whether the share of food spending prior to the Demonetization is above or below median. The data are at the individual-month level (April 2016 to September 2017). Outcome variables include the fraction of spending paid by digital payments, the log level of spending, and the share of food spending. Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	Pre-Demonetization food spending share is below median (88%)			Pre-Demonetization food spending share is above median (88%)		
	(1) Digital payment fraction	(2) Log spending	(3) Food spending share	(4) Digital payment fraction	(5) Log spending	(6) Food spending share
PriorCashDependence × Post	0.309***	0.133***	0.046***	0.287***	0.284***	-0.018***
	[30.88]	[6.43]	[14.86]	[32.73]	[11.38]	[-21.49]
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes
District × Year-Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.605	0.564	0.366	0.627	0.561	0.322
No. of Observations	3,635,392	3,635,392	3,635,392	2,926,188	2,926,188	2,926,188

Table 9: **Is Increased Spending Driven by Income Shocks?**

This table estimates the effect of the forced switch to digital payments due to the Demonetization on payment methods and spending for two subsamples classified by the behavior of paying large receipts with cash prior to the Demonetization, which can be viewed as a proxy for getting income from black money activities. Large receipts are defined as receipts whose amount exceeds the 90^{th} percentile (467 rupees) in the distribution of receipt size from April 2016 to October 2016. The data are at the individual-month level (April 2016 to September 2017). Outcome variables include the fraction of spending paid by digital payments and the log level of spending. Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	Did not use cash for large bills pre-Demonetization		Used cash for large bills pre-Demonetization	
	(1)	(2)	(3)	(4)
	Digital payment fraction	Log spending	Digital payment fraction	Log spending
PriorCashDependence × Post	0.310***	0.459***	0.200***	-0.025
	[38.50]	[16.18]	[17.01]	[-0.92]
Individual FEs	Yes	Yes	Yes	Yes
District × Year-Month FEs	Yes	Yes	Yes	Yes
R^2	0.656	0.565	0.545	0.486
No. of Observations	3,950,260	3,950,260	2,611,320	2,611,320

Table 10: **Is Increased Spending Driven by Credit Supply Shocks?**

This table estimates the effect of the forced switch to digital payments due to the Demonetization on spending for three subsamples based on credit card usage: existing users, defined as consumers who used credit card before the Demonetization; non-users, defined as consumers who never used any credit card in the sample period; and new users, defined as consumers who started to use credit cards following the Demonetization. The data are at the individual-month level (April 2016 to September 2017). The outcome variable is the log level of monthly spending. Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	Log spending				
	Full	Existing users		New users	Non-users
	(1)	(2)	(3)	(4)	(5)
PriorCashDependence × Post	0.238***	0.086*	0.134**	0.357***	0.243***
	[9.92]	[2.83]	[3.91]	[15.43]	[9.96]
PriorCreditDependence × Post			0.174		
			[1.96]		
Individual FEs	Yes	Yes	Yes	Yes	Yes
District × Year-Month FEs	Yes	Yes	Yes	Yes	Yes
R^2	0.586	0.520	0.520	0.504	0.586
No. of Observations	6,561,580	240,191	240,191	551,031	5,770,358

Table 11: **Forced Switch to Digital Payments and Its Effect on Online Grocery Store Spending**

This table shows the effect of the forced switch to digital payments due to the Demonetization on payment methods and spending (equation (1)) in the online grocery store data. The data are at the individual-month level. Outcome variables include the fraction of spending paid by digital payments and the log level of spending. Prior cash dependence is the average share of spending paid by cash from April 2016 to October 2016 for each consumer. Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	Online grocery sample		Supermarket (matched sample)	
	(1) Digital payment fraction	(2) Log spending	(3) Digital payment fraction	(4) Log spending
PriorCashDependence × Post	0.523***	0.039**	0.304***	0.120***
	[35.17]	[3.23]	[26.23]	[4.14]
Individual FEs	Yes	Yes	Yes	Yes
District × Year-Month FEs	Yes	Yes	Yes	Yes
R^2	0.654	0.570	0.555	0.544
No. of Observations	179,470	179,470	90,720	90,720

Table 12: **Digital Payments and Spending on Temptation Goods**

This table shows the effect of the forced switch to digital payments due to the Demonetization on temptation spending (equation (1)). The data are at the individual-month level (April 2016 to September 2017). Outcome variables include the probability of having positive temptation spending and the probability of having positive non-temptation spending. In columns 1–2, we analyze the full sample of individual consumers. In columns 3–4 (5–6), we analyze the sub-sample of individuals consumers with below-median (above-median) pre-Demonetization monthly spending. Prior cash dependence is the share of spending paid by cash from April 2016 to October 2016 for each consumer. Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	Full Sample		Below-median Spending Pre-Demonetization		Above-median Spending Pre-Demonetization	
	(1)	(2)	(3)	(4)	(5)	(6)
	$\mathbb{1}$(Temptation spending > 0)	$\mathbb{1}$(Non-temptation spending > 0)	$\mathbb{1}$(Temptation spending > 0)	$\mathbb{1}$(Non-temptation spending > 0)	$\mathbb{1}$(Temptation spending > 0)	$\mathbb{1}$(Non-temptation spending > 0)
PriorCashDependence $\times$ Post	0.034***	0.008***	0.022***	0.014***	0.005	-0.002***
	[6.50]	[10.48]	[6.37]	[8.66]	[0.92]	[-3.99]
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes
District $\times$ Year-Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.375	0.304	0.383	0.340	0.334	0.195
No. of Observations	6,561,580	6,561,580	2,233,255	2,233,255	4,328,325	4,328,325

Online appendix

This appendix contains supplementary material, tables, and figures.

A Sample Construction and Variable Definitions

The anonymized transaction-level data from a large Indian supermarket chain used in the main analysis of the paper comprise all purchases in 171 stores in twenty-one districts of five states/union territories from April 2016 to September 2017.

Figure OA.1, which plots the overall shares of different payment methods in the universe of all supermarket transactions over time, demonstrates the rapid switch to digital payments following the Demonetization. The share of cash payment in the total number of transactions (total transaction value) dropped 17 (20) percentage points in November 2016, from 79% (55%) in the previous month. In either measure of payment shares, the majority of this gap is filled by an increase in debit card usage. Usage of other payment methods (e.g., credit cards and mobile payments) remains low. The shift from cash payment to cashless payments is consistent with Agarwal et al. (2018), Chodorow-Reich et al. (2020), and Crouzet et al. (2021), among others.

80% of purchases involve the use of a loyalty card and therefore can be linked to individual consumers, consistent with the magnitude reported by Hastings and Shapiro (2018). There is no cost in obtaining the loyalty card. Consumers can receive cashback for hitting certain spending amount. Crucially for our identification strategy, there is no change in the incentive structure of the loyalty card. We exclude from our analysis the spending transactions that cannot be linked to individual consumers. The percentage of excluded spending transactions is stable over time.

We observe 144.1 million product purchases made on 24.4 million purchase occasions by 4,237,728 households from April 2016 to September 2017. To ensure that the household-level changes in payment choice and spending following the Demonetization are well-defined, we restrict the sample to households that started shopping at this chain before November 2016 and remained as customers afterwards. Our panel contains a total of 924,753 households.

For each product purchased, we observe the quantity, the price (both the listing price and the actual price paid), the product code, a text description of the product, and the product's location within a taxonomy which involves five hierarchical layers of product categories. Using the supermarket's taxonomy, we decompose all products purchased into food products and non-food products. We also consider an alternative dichotomy between durable and non-durable products, based on whether a given product can generally be used for more than one year. The majority of goods sold in the supermarket chain are non-durable, with the exception of furniture, electronics, home appliances, home decor, books & audio and video products, crockery, cooking ware, utensils, sports equipment, and luggage. Non-durable products include all food products as well as health & beauty and household products.

We aggregate the data to the individual-month level. We calculate each individual's total monthly spending, the fraction of spending paid by each of the payment instruments, as well as the share of food, non-food, durable, and non-durable spending in total spending. We also calculate indicators for whether an individual has positive food, non-food, durable, and non-durable spending in a given month, respectively. We measure the variety of supermarket spending by the number of unique products purchased, the number of unique broad categories purchased, the number of unique product categories purchased, and the number of unique stores within the supermarket chain from which a consumer makes purchases. We measure shopping intensity by counting the number of shopping trips in a month, where a trip is defined as a purchase from a given store on a given day.[20]

B Additional Discussion of the Empirical Approach

B.1 Discussions on the Included Fixed Effects

The baseline panel regression equation (1) and its dynamic version equation (2) include individual fixed effects μ_i and district$\times$year-month fixed effects $\pi_{d,t}$. The inclusion of the district$\times$year-month fixed effects ensures that the estimates do not reflect the impact of district-specific exposure to the Demonetization.

To see the effect of these fixed effects, we examine the correlation between district-level exposure to the Demonetization and spending. For each district in our sample, we compute prior cash dependence as the average share of spending paid by cash from April 2016 to October 2016. We also calculate use of digital payments and log spending for each district in each month. Panel (a) of Figure OA.2 plots the change in the use of digital payments from October 2016 to November 2016 against prior

[20] In other words, if a household makes two purchases from two separate stores on a given day, we will count these purchases as two shopping trips. The same applies if this household makes two purchases in the same store on two separate days.

cash dependence. Districts that were previously more dependent on cash experienced a larger increase in digital payments following the Demonetization. Therefore, similar to the more granular individual-level counterpart, the district-level prior cash dependence captures the exposure to the Demonetization.

In panel (b), we plot the change in log spending from October 2016 to November 2016 against prior cash dependence. We find that districts more exposed to the Demonetization experienced a larger decrease in spending. This negative effect is consistent with the disruptive impact of the Demonetization on the overall economy (Chodorow-Reich et al., 2020).

The inclusion of the district×year-month fixed effects in our baseline panel regression equation (1) removes all impacts of district-level time-varying factors including the currency supply shocks.

B.2 The Selection into the Estimation Sample

The sustained lower cash usage in Figure 2 does not contradict with the overall recovery of cash usage months after the Demonetization in Figure OA.1. The district-specific time fixed effects included in the distributed lag model remove the common trends of payment choice. In addition, compositional differences between the two samples further contribute to the divergent patterns. While only existing customers are included in the DiD analysis to ensure that the household-level changes in payment choice and spending following the Demonetization are well-defined, both existing and new customers are present in the universe of supermarket transactions. We measure the time of arrival for a consumer as the time of his/her first spending transaction at the supermarket chain and calculate the share of spending paid by cash in the calendar month of arrival. We plot the average share of spending paid by cash among the new customers over time in the orange solid line in Figure OA.3. In November 2016 when the Demonetization occurred, cash usage of new consumers arriving in that month dropped sharply, similar to the patterns we observed among existing consumers. As cash made a comeback to the economy in 2017, cash dependence of customers newly arriving at the supermarket chain rebounded gradually. Spending transactions that cannot be linked to individual consumers are also excluded from the DiD analysis but retained in the universe of supermarket transactions. Cash usage of these excluded spending transactions exhibits a sharp reduction immediately after the Demonetization announcement but rebounds gradually as the purple dashed line in Figure OA.3 shows. In sum, the spending transactions excluded from the sample for the DiD analysis exhibit a strong recovery of cash usage, driving the full sample patterns in Figure OA.1.

B.3 Subsample Analyses

Columns 1–3 of Table OA.1 show the estimates obtained from the sample excluding full cash users prior to the Demonetization. In this subsample, the effect on the usage of digital payments is quantitatively similar, whereas the effect on log spending is smaller. Columns 4–6 show the results estimated from the sample excluding the first three months following the Demonetization announcement (November 2016, December 2016, and January 2017). These estimates confirm that the spending response is unchanged when cash made a comeback to the economy.

B.4 Characteristics of the Spending Response

In the paper, we have shown that Demonetization induces consumers who were previously heavily cash-reliant to adopt digital alternatives and increase spending. To provide perspectives on the spending response, we exploit the richness of our data to analyze spending variety, and shopping intensity, as well as the quantity and price of goods purchased.

B.4.1 Spending Variety and Shopping Intensity

We also examine how variety of supermarket spending and shopping intensity respond to the forced switch to digital payments in Table OA.2. We measure variety of supermarket spending by the number of unique products purchased (product variety, column 1), the number of unique broad categories purchased (broad category variety, column 2), the number of unique product categories purchased (category variety, column 3), and the number of unique stores within the supermarket chain from which a consumer makes purchases (shop variety, column 4). The estimates show that previously cash-reliant consumers increased the variety of their supermarket spending by a statistically significant margin according to three out of four variety measures as they were forced to switch to digital payments following the Demonetization. We measure shopping intensity by counting the number of shopping trips in a month. In column 5, we find that shopping intensity, as measured by the number of trips, did not change in any statistically significant way.

B.4.2 Quantity and Quality of Products Purchased

Lastly, we examine the quantity and quality of products purchased. The spending data records the name of the products, as well as the product categories. The product name includes the brand and the portion, if applicable. The store classifies all the products into five hierarchical layers of categories. For this analysis, we use the two most granular categorizations. Examples of the second most granular categories include "Cereals - Pulses and Flours," "Fruits," "Vegetables," "Cooking Appliances," and "Infant Underwear & Night Wear." Each of these categories can be further broken down into a few next-level categories. For example, the "Vegetables" category can be broken down into "Local Vegetables" and "Special/Exotic Vegetables." This granular categorization makes the products in the same category more comparable in terms of intrinsic value and therefore makes the quantity purchased and the quality meaningful.

We examine category-level outcome variables by running the following regression for consumer i's spending in category c in month t:

$$y_{i,c,t} = \mu_{i,c} + \pi_{c,d,t} + \beta_c \cdot (PriorCashDependence_i \times Post_t) + \varepsilon_{i,c,t} \tag{5}$$

In this specification, the coefficient β_c measures the impact of Demonetization. The individual×category fixed effects $\mu_{i,c}$ control for the potential differences in spending profiles across consumers; the category×district×year-month fixed effects $\pi_{c,d,t}$ subsume factors such as the seasonality in product demand and supply and the supplier's pricing responses that are allowed to differ across districts.

We conduct the category-level analysis using equation (5) for three outcome variables: the rupee amount spent on the category (Amount), the quantity of goods purchased (Quantity), and the unit price of goods purchased (Quality). The results are reported in Table OA.3. Panel A reports the results using the second most granular definition of categories, and Panel B reports the results using the most granular definition of categories. Under both levels of granularity, we find a positive coefficient for Amount, Quantity, and Quality. The effect is strongest for Quality: Consumers with a higher prior cash dependence buy more expensive products following the Demonetization.

B.5 Alternative Measure of Spending

One may be concerned that the log-linear regression specification forces zero-valued observations to drop out, which might bias the estimate. To mitigate this concern, we also estimate the spending response using the level of total spending as the outcome variable and compare the economic magnitude from the two specifications. Despite using different measurements, the economic magnitude of the estimated spending response remains stable: Moving from the 25th to the 75th percentile of prior cash dependence is associated with a 61.2 INR, or 10.9% increase in monthly spending according to the level specification as shown in Column 1 of Table OA.4; while the corresponding magnitude is a 11.9% increase in spending based on the baseline log specification as shown in Column 5 of Table 3. When we decompose the total monthly spending into different payment instruments in Columns 2–6 of Table OA.4, we also obtain estimates that are quantitatively similar to the baseline results in Columns 1–4 of Table 3. The similarity in the economic magnitudes of alternative estimates underscores the stability of the underlying economic relationship.

C Additional Information on Analyses using the Online Grocery Retailer Data

In Section 7 of the paper, we use the anonymized transaction-level data from a large online grocery retailer to study how the Demonetization affects payment choice and the level of spending in the online grocery setting. This appendix section provides additional details for these analyses.

The data comprise all purchases in six cities in India from January 2016 to April 2019 and contain anonymized consumer identifiers. As in our main analysis using the supermarket data, we restrict the sample to households that started shopping at this online store before November 2016 and remained as customers afterwards.

As in our main analysis using the supermarket data, we exploit the cross-sectional variation in cash dependence prior to the Demonetization at the individual consumer level to estimate the forced switch to digital payments and the associated spending response. For every individual in the online grocery retailer data, we calculate the prior cash dependence by taking the average share of spending paid by cash from January 2016 to October 2016.

Panel A of Table OA.5 reports the summary statistics of consumer characteristics in the online grocery store sample. Consumers in the online grocery retailer data appear to have a lower level of average prior cash dependence (an average of 0.4) than consumers in the supermarket data (an average of 0.7). This is what we would expect for this sample as these consumers are by definition Internet users. Internet users are more likely to adopt digital payments than non-users, as telecommunications access is a key enabler of digital payments (Demirgüç-Kunt et al., 2022). Consumers in the online grocery retailer data also appear to have a lower share of food spending than consumers in the supermarket data. We recognize that these observable differences may affect the spending response to the forced switch to digital payments, and conduct a separate analysis of propensity score matching to remove the impacts of the observable differences.

Panel B of Table OA.5 reports the correlation between prior cash dependence, the treatment intensity variable, and various spending characteristics in the online grocery store data. Importantly, in the online grocery retailer sample, the correlation between our measure of prior cash dependence and percentile rank of monthly spending (both measured prior to the sudden Demonetization) is -0.13, suggesting that in the sample, similar to the supermarket sample, more cash-dependent consumers appear to be lower-income than less dependent consumers.

To further address the concern that selection into the online grocery sample may confound our analysis, we adopt a propensity score matching approach to remove the observable differences between consumers in the two samples. Specifically, we calculate propensity scores based on the logistic regression of an indicator of being in the online grocery sample on prior cash dependence and pre-Demonetization share of food spending. We perform the nearest-neighbor matching without replacement based on the computed propensity scores. Table OA.6 shows that after matching, the difference in prior cash dependence decreases from the pre-matching difference of 0.3 to zero. Differences in other observable covariates between the two samples also reduce substantially from the pre-matching levels.

D Analyses using the Food Delivery Platform Data

We also analyze a separate data set of transaction records from a different spending occasion, food delivery, to address concerns about our findings being driven by an increase in supermarket spending at the expense of other types of spending. To do so, we obtain anonymized transaction-level records from a leading food delivery platform in India. The firm aggregates information on restaurants on an online platform and provides food ordering and delivery services from partner restaurants in select cities. An advantage of this setting compared to other spending occasions is that both cash and digital payments are accepted for all orders in the food delivery platform throughout the entire sample period, enabling us to sidestep the confounding factor of merchants' adoption choices as in our main analyses.

The dataset contains micro-level information about food ordering and delivery on the platform in four major cities in India from April 2016 to December 2017 and contains anonymized consumer identifiers. Consumers can pay by cash (i.e., cash on delivery) or digital payments.

Similar to the online grocery retailer setting, ordering for food delivery on the platform is characterized by a time lag between the purchase decision and the delivery of goods. At the time of the purchase decision, both cash payment and digital payments involve no physical exchange of money between hands. Therefore, paying for food delivery on the platform with cash invokes the behavioral costs associated with cash payment being effortful, instant, and memorable to a lesser extent than paying for food delivery offline with cash.

As in our main analysis using the supermarket data, we exploit the cross-sectional variation in cash dependence prior to the Demonetization at the individual consumer level to estimate the forced switch to digital payments and the associated spending response. For every individual in the online grocery retailer data, we calculate the prior cash dependence by taking the average share of spending paid by cash from April 2016 to October 2016. As in our main analysis, we restrict the sample to consumers that started using the platform's service before November 2016 and remained as customers afterwards.

Table OA.7 reports the estimates obtained from the food delivery platform data. In Column 1, we find that the forced switch to digital payments by previously cash-reliant individuals is stronger in this sample than in the supermarket sample (column 1, Table 3) and is very similar to the corresponding coefficient in the online grocery retailer sample (column 1, Table 11). Column 2 shows that the spending response is much muted compared to the estimate obtained in the supermarket sample (column 5, Table 3) and is very similar to the corresponding coefficient in the online grocery retailer sample (column 2, Table 11). In Columns 3 & 4, we control for a tighter set of location-specific time fixed effects to further sweep potential within-city variations in cash availability. Specifically, we include city-subzone×year-month fixed effects.[21] With the more granular city-subzone×year-

[21]In this sample, each city has 159.5 subzones on average. Compared to existing studies that mainly use district-level variation to measure accessibility of cash in India, our approach of includ-

month fixed effects, the point estimates and statistical significance remain similar.

The stability of empirical estimates obtained from independent separate samples underscores the stability of the economic relationships and bolsters the validity of our findings.

ing city-subzone fixed effects controls for more granular geographic differences, as the entire India includes approximately 600 districts while the four large cities in our sample alone include more than 600 subzones.

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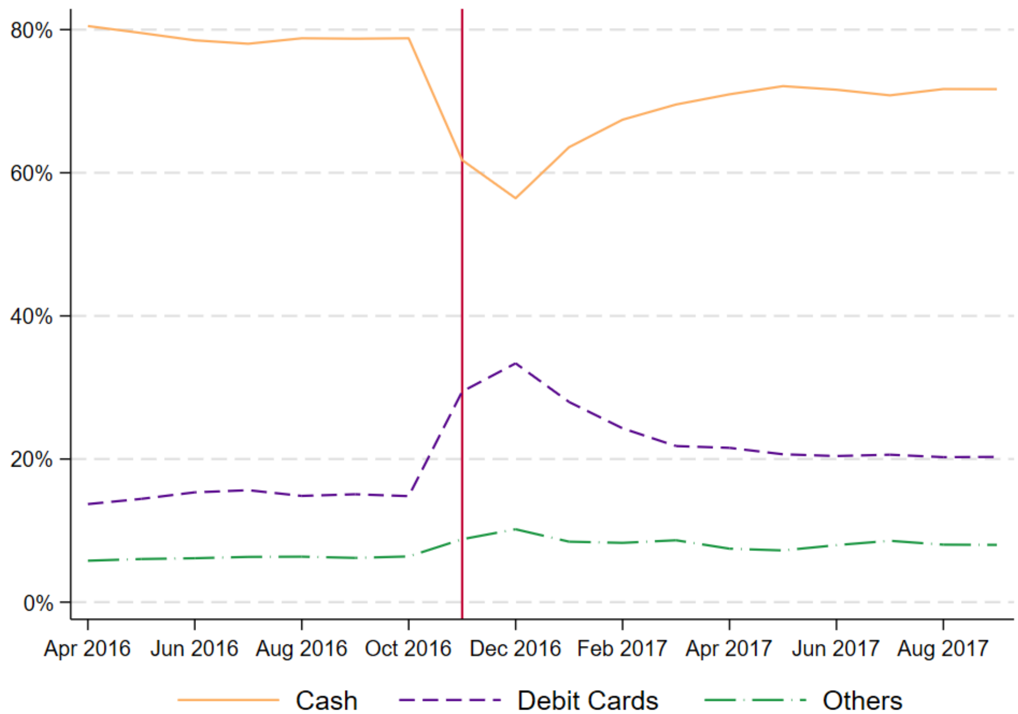
Figure OA.1: **Demonetization and Payment Modes**

This figure plots overall shares of different payment methods in the universe of all supermarket transactions over time. The vertical bar indicates the Demonetization (November 2016). In panel (a), we calculate the share of the number of total transactions. In panel (b), we calculate and plot the share of the total nominal value of transactions.

(a) Shares of the number of total transactions



(b) Shares of the total nominal value of transactions

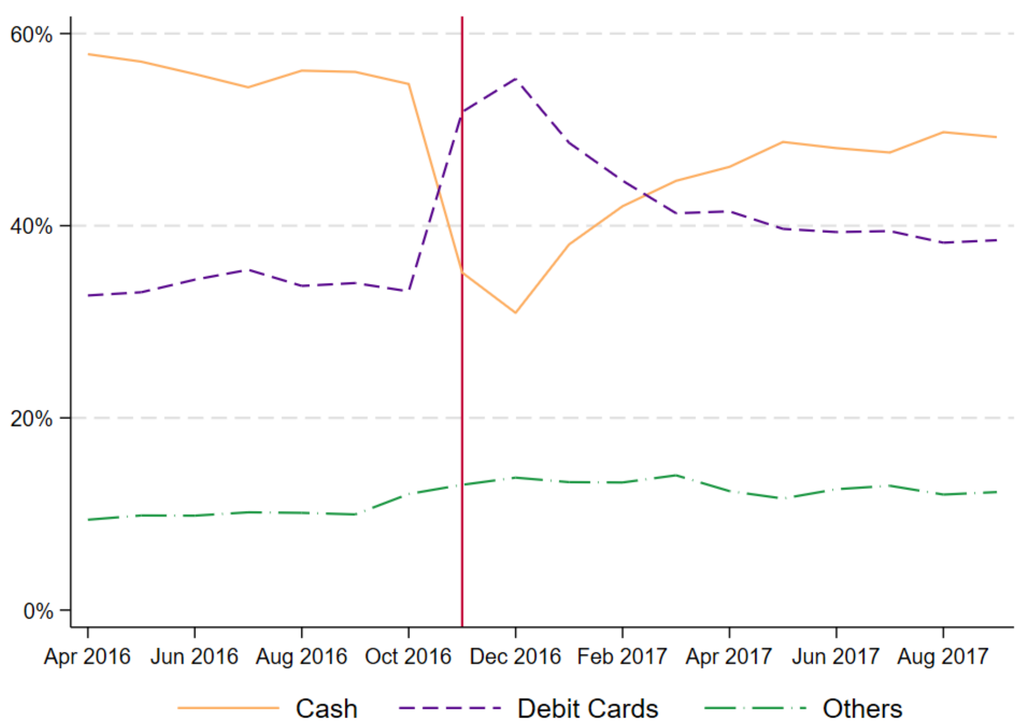
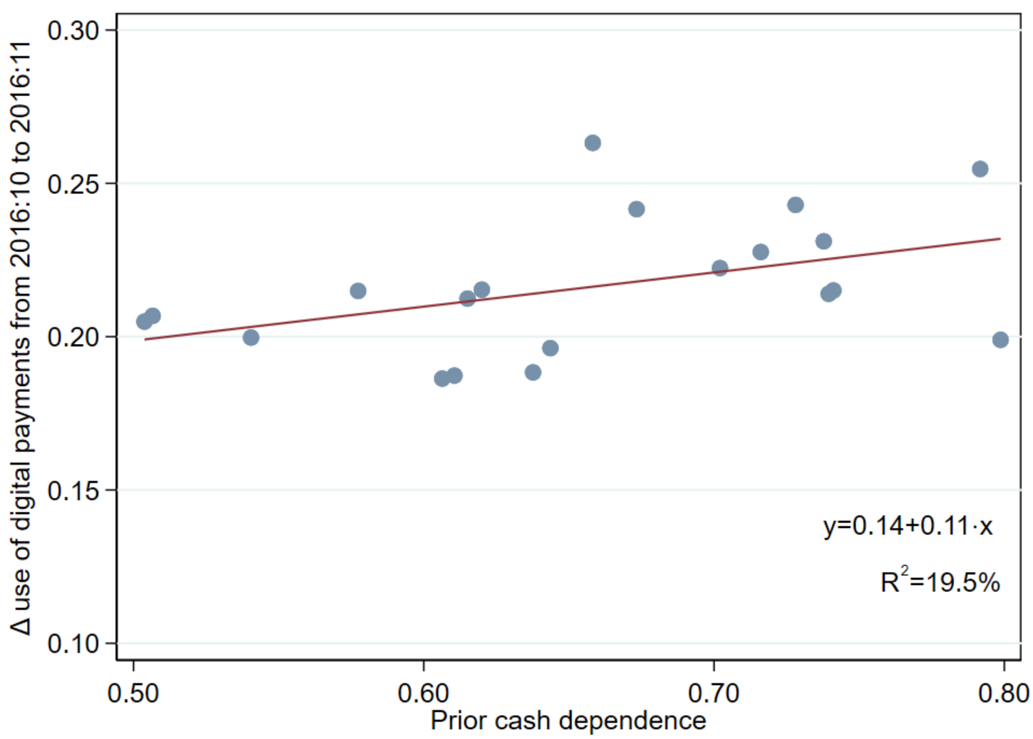


Figure OA.2: **District-level Exposure to the Demonetization and Spending**

This figure shows the correlation correlation between district-level exposure to the Demonetization and spending. For each district in our sample, we compute prior cash dependence as the average share of spending paid by cash from April 2016 to October 2016. We also calculate use of digital payments and log spending for each district in each month. Panel (a) presents a scatterplot of the change in the use of digital payments from October 2016 to November 2016 and prior cash dependence. The red line gives the best-fit line. Panel (b) presents the scatterplot of changes in log spending from October 2016 to November 2016 and prior cash dependence as well as the best-fit line.

(a) District-level prior cash dependence and change in the use of digital payments



(b) District-level prior cash dependence and change in log spending

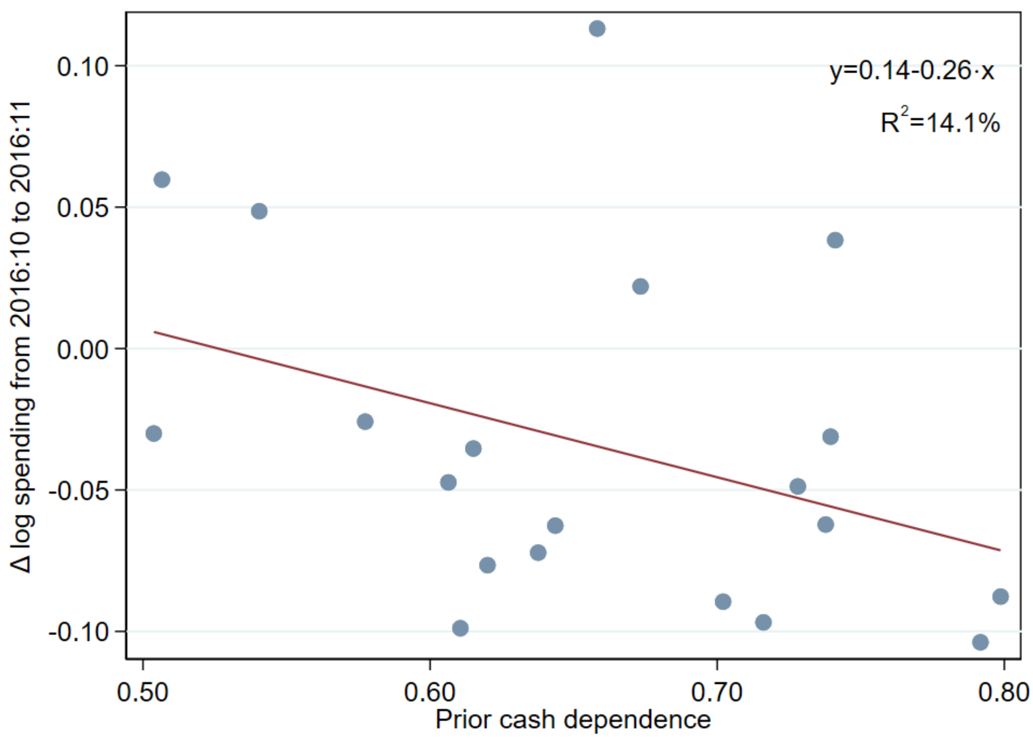


Figure OA.3: **Cash Usage of the Spending Transactions Excluded from the DiD Analysis**

This figure plots the share of spending paid by cash among the new customers who arrive in a month and the share of spending paid by cash among spending transactions that cannot be linked to individual consumers. The vertical bar indicates the Demonetization (November 2016).

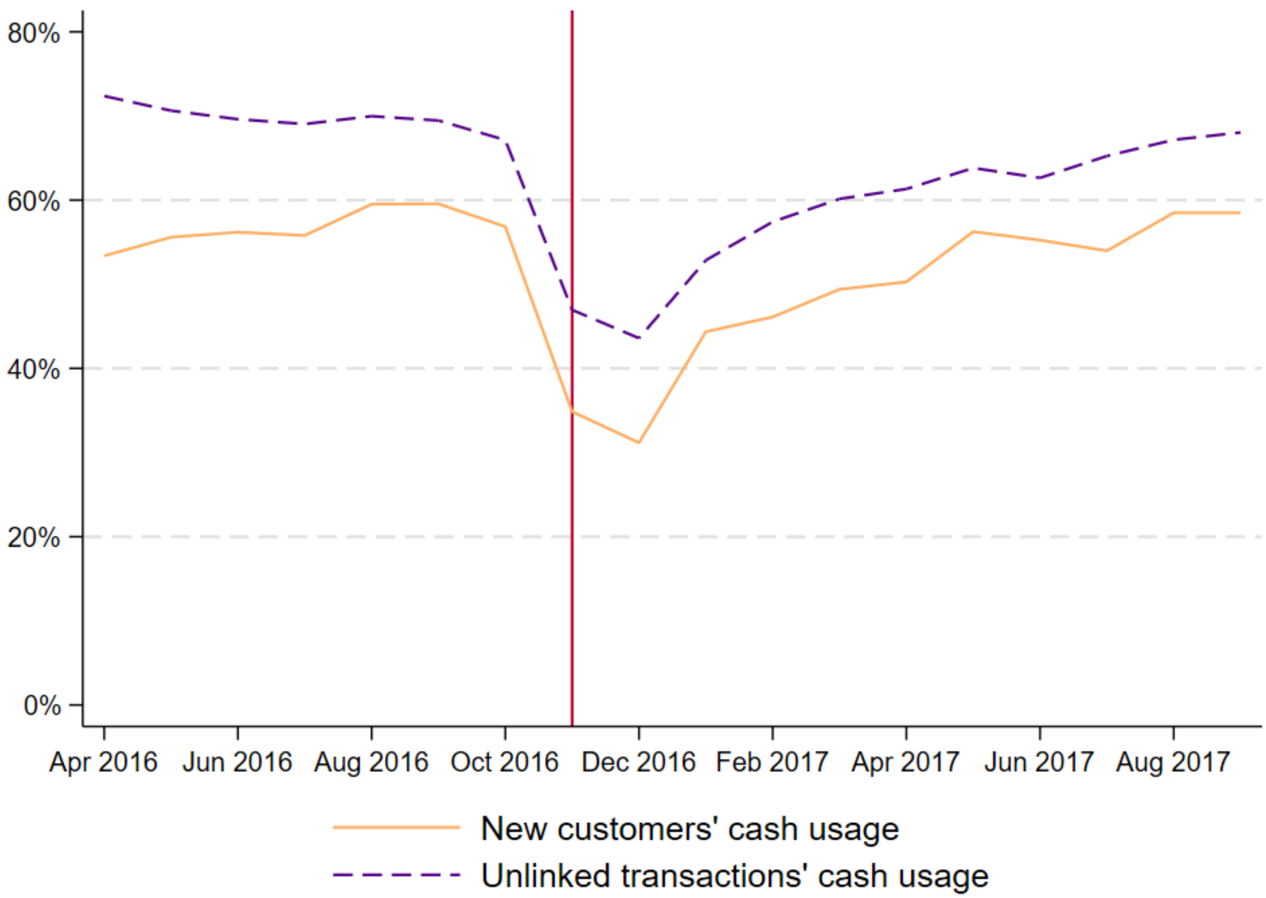


Table OA.1: **Digital Payments and Spending (Subsample Analyses)**

This table shows the subsample analyses for the effect of the forced switch to digital payments due to the Demonetization on payment methods and spending (equation (1)). The data are at the individual-month level (April 2016 to September 2017). In the first subsample analysis (columns 1–3), we exclude full cash users prior to the Demonetization. In the second subsample analysis (columns 4–6), we exclude the first three months following the Demonetization announcement (November 2016, December 2016, and January 2017). Outcome variables include the fraction of spending paid by digital payments and the log level of spending. Prior cash dependence is the share of spending paid by cash from April 2016 to October 2016 for each consumer. Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	Excluding full cash users		Excluding Nov 2016 to Jan 2017	
	(1)	(2)	(3)	(4)
	Digital payment fraction	Log spending	Digital payment fraction	Log spending
PriorCashDependence × Post	0.360***	0.052*	0.288***	0.275***
	[31.70]	[2.57]	[34.82]	[14.23]
Individual FEs	Yes	Yes	Yes	Yes
District × Year-Month FEs	Yes	Yes	Yes	Yes
R^2	0.514	0.539	0.634	0.595
No. of Observations	3,728,609	3,728,609	5,427,290	5,427,290

Table OA.2: **Digital Payments and Shopping Variety and Intensity**

This table shows the effect of the forced switch to digital payments due to the Demonetization on shopping variety and intensity measures (equation (1)). The data are at the individual-month level (April 2016 to September 2017). Product/broad category/category/shop variety is the number of unique products/broad category/categories/shops that a household purchases in the given month. Number of trips is the number of shopping trips, defined as unique shop-date pairs, a household engages in a given month. Prior cash dependence is the share of spending paid by cash from April 2016 to October 2016 for each consumer. Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	(1) Product variety	(2) Broad category variety	(3) Category variety	(4) Shop variety	(5) No. of trips
PriorCashDependence × Post	1.588***	0.170***	0.795***	0.001	-0.005
	[5.96]	[11.62]	[10.08]	[1.46]	[-0.16]
Individual FEs	Yes	Yes	Yes	Yes	Yes
District × Year-Month FEs	Yes	Yes	Yes	Yes	Yes
R^2	0.650	0.531	0.634	0.523	0.594
No. of Observations	6,561,580	6,561,580	6,561,580	6,561,580	6,561,580

Table OA.3: **Digital Payments and Spending Behaviors in Granular Product Categories**

This table estimates the effect of the forced switch to digital payments due to the Demonetization on category-level spending (equation (5)). Panel A reports the results using the second most granular definition of categories and Panel B reports the results using the most granular definition of categories. The data are at the individual-product category-month level (2016:04–2017:09). Amount, Quantity, and Quality are the spending amount in rupees, the quantity of goods purchased, and the unit price of goods purchased by a given consumer on a given category in a given month, respectively. Prior cash dependence is the share of spending paid by cash from April 2016 to October 2016 for each consumer. Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

Panel A: Results using the second most granular definition of categories

	(1) Amount	(2) Quantity	(3) Quality
PriorCashDependence × Post	27.272	0.494	1.412***
	[1.36]	[1.21]	[14.35]
Individual × Category FEs	Yes	Yes	Yes
District × Category × Year-Month FEs	Yes	Yes	Yes
R^2	0.410	0.355	0.680
No. of Observations	42,858,979	42,858,979	42,858,979

Panel B: Results using the most granular definition of categories

	(1) Amount	(2) Quantity	(3) Quality
PriorCashDependence × Post	19.602	0.367	0.847***
	[1.26]	[1.16]	[8.87]
Individual × Category FEs	Yes	Yes	Yes
District × Category × Year-Month FEs	Yes	Yes	Yes
R^2	0.398	0.385	0.771
No. of Observations	54,603,502	54,603,502	54,603,502

Table OA.4: **Heterogeneous Forced Switch to Digital Payments**

This table estimates the effect of the forced switch to digital payments due to the Demonetization on payment methods and spending. The data are at the individual-month level from April 2016 to September 2017. Outcome variables include the level of total monthly spending and its decomposition into different payment instruments. Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	(1) Total spending (INR)	(2) Cash spending (INR)	(3) Digital spending (INR)	(4) Debit card spending (INR)	(5) Mobile payment spending (INR)	(6) Credit card spending (INR)
PriorCashDependence $\times$ Post	122.4***	-111.3***	233.7***	204.8***	1.890	-18.31**
	[3.98]	[-5.00]	[6.66]	[7.09]	[1.71]	[-3.06]
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes
District $\times$ Year-Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.436	0.425	0.444	0.446	0.326	0.398
No. of Observations	12,319,533	12,319,533	12,319,533	12,319,533	12,319,533	12,319,533

Table OA.5: **Summary Statistics of Consumer Characteristics and Covariate Balance in the Online Grocery Store Sample**

This table examines the balance of pre-Demonetization characteristics in the online grocery store data. Additional details for sample construction and variable definitions can be found in Online Appendix Section C. Demonetization took place in November 2016; pre-Demonetization characteristics are measured in the seven months prior to that (April to October 2016). The monetary amount is the local currency Indian rupee (INR), December 2015 real terms, and 1 USD = 66.2 INR as of December 2015.

Panel A: Summary Statistics of Pre-Demonetization Observable Characteristics

	Mean	Std. Dev.	25%	50%	75%
Treatment intensity:					
Prior cash dependence	0.39	0.36	0.062	0.26	0.73
Other characteristics:					
Share of food spending	0.80	0.11	0.75	0.82	0.88
Share of non-food spending	0.20	0.11	0.12	0.18	0.25
Share of durable spending	0.0078	0.022	0	0.00064	0.0078
Share of non-durable spending	0.99	0.022	0.99	1.00	1
Quantity share of food spending	0.86	0.091	0.82	0.88	0.92
Quantity share of non-food spending	0.14	0.091	0.082	0.12	0.18
Quantity share of durable spending	0.0046	0.016	0	0.0015	0.0042
Quantity share of non-durable spending	1.00	0.016	1.00	1.00	1
Product variety	59.1	26.2	40.1	55.7	74.4
Broad category variety	7.30	1.38	6.43	7.43	8.29
Number of shopping trips	6.54	4.46	3.71	5.29	8
Number of households	9,974				

Panel B: Correlation between Treatment Intensity and Pre-Demonetization Observable Characteristics

	Correlation
Percentile rank of monthly spending	-0.13
Share of food spending	0.015
Share of durable spending	-0.017
Product variety	-0.084
Broad category variety	-0.15
Number of shopping trips	-0.064

Table OA.6: **Comparison between the Two Samples**

This table compares the pre-Demonetization characteristics in the online grocery sample and the supermarket sample. Additional details for sample construction and variable definitions can be found in Online Appendix Section A. Demonetization took place in November 2016; pre-Demonetization characteristics are measured in the seven months prior to that (April to October 2016).

	Online grocery sample		Supermarket (full sample)		Supermarket (matched sample)	
	Mean	Median	Mean	Median	Mean	Median
Prior cash dependence	0.39	0.26	0.70	1.00	0.38	0.23
Share of food spending	0.80	0.82	0.78	0.88	0.77	0.81
Share of non-food spending	0.20	0.18	0.22	0.12	0.23	0.19
Share of durable spending	0.01	0.00	0.01	0.00	0.01	0.00
Share of non-durable spending	0.99	1.00	0.99	1.00	0.99	1.00
Number of households	9,974	9,974	924,753	924,753	9,974	9,974

Table OA.7: **Forced Switch to Digital Payments and Its Effect on Online Food Delivery Spending**

This table shows the effect of the forced switch to digital payments due to the Demonetization on payment methods and spending (equation (1)) in the online food delivery data. The data are at the individual-month level. Outcome variables include the fraction of spending paid by digital payments and the log level of spending. Prior cash dependence is the average share of spending paid by cash from April 2016 to October 2016 for each consumer. Post is an indicator for post-Demonetization months. Fixed effects are denoted at the bottom. Standard errors are doubly clustered at individual level and at month level; the corresponding t-statistics are reported in brackets. We use ***, ** and * to denote significance at 0.1%, 1% and 5% level (two-sided), respectively.

	(1) Digital payment fraction	(2) Log spending	(3) Digital payment fraction	(4) Log spending
PriorCashDependence × Post	0.575***	0.041*	0.589***	0.043*
	[36.21]	[2.60]	[38.12]	[2.75]
Individual FEs	Yes	Yes	Yes	Yes
City × Year-Month FEs	Yes	Yes	No	No
City-Subzone × Year-Month FEs	No	No	Yes	Yes
R^2	0.567	0.484	0.573	0.493
No. of Observations	4,447,700	4,447,700	4,447,700	4,447,700