

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

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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. Digital payment use 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* D12, D14, D91, E21)

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The increasing digitization of the global economy is changing how products and services are produced, distributed, and sold all around the world. Digital payments, 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 and 2017 (Demirgüç-Kunt et al. 2018, chap. 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 household 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 outcome 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 in which 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

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, Balasubramaniam, and Ramadorai (2018) identify a robust endowment effect for stock holdings, a phenomenon that is likely rooted in a “warm-glow”-based explanations. In our setting, digital payments evoke weaker endowment effects than do cash payments.

³ Consumer adoption of digital payments can feed back into merchant adoption, and vice versa (e.g., Higgins 2020).

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, 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, Gupta, and Mezzanotti 2023).

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, Hull, and Liebersohn (2017). We validate that prior cash dependence captures the extent of the forced switch to digital payments in the first stage: digital payment use 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 25th percentile to the 75th 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% increase in total spending.

The estimated effect reflects the local average treatment effect (LATE) for the subgroup 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 nondurable 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 nonfood 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 data set 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, Gupta, and Mezzanotti \(2023\)](#). 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 has 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 more 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 nontemptation 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, Kanz, and Klapper \(2020\)](#) 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, Gupta, and Mezzanotti (2023) 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, and 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 toward digital payments could unintentionally encourage some individuals to overspend, which could undermine their sound personal financial planning.

1. The 2016 Demonetization in India

On November 8, 2016, at 8:15 p.m. local time, Indian Prime Minister Narendra Modi announced a Demonetization scheme in an unscheduled live television address: The two largest denomination notes, the 500 and 1,000 rupee notes

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

(\$7.5 and \$15, respectively), would cease to be legal tender and be replaced by new 500 and 2,000 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 gross domestic product (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 the total volume and 6% of the 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 sudden 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

2. Data

We use anonymized transaction-level data from a large Indian supermarket chain. The data comprise all purchases in 171 stores in 21 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.

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 8, 2016, available at <https://www.rbi.org.in/Scripts/PublicationReportDetails.aspx?UrlPage=&ID=840>.

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

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 detail in [Internet 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 who started shopping at this chain before November 2016 and remained as customers afterward. The household-level panel data set contains a total of 924,753 individual consumers.

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 the 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 7 months prior to the Demonetization (i.e., April to October 2016) and the 11 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.

3. 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](#)) has shown that access to digital 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

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 [Internet Appendix Section B.5](#).

Table 1
Summary statistics of consumer payment choice

	Mean	SD	25%	50%	75%
<i>Fraction of payment mode in spending:</i>					
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				

This table reports the summary statistics of consumer payment choices 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 [Internet 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.

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 merchant adoption.

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, the observable and unobservable differences among individuals with differential pre-Demonetization reliance on cash could affect their consumption patterns. If our research design is valid, we expect to observe an economically and statistically insignificant difference in consumption across

Table 2
Summary statistics of consumer characteristics and covariate balance

(A) Summary statistics of pre-Demonetization observable characteristics					
	Mean	SD	25%	50%	75%
<i>Treatment intensity:</i>					
Prior cash dependence	0.70	0.40	0.29	1	1
<i>Total spending and its composition:</i>					
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 nonfood spending	0.22	0.28	0	0.12	0.35
Share of durable spending	0.0084	0.056	0	0	0
Share of nondurable spending	0.99	0.056	1	1	1
<i>Spending variety and shopping intensity:</i>					
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				
(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				

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 [Internet Appendix Section A](#). Demonetization took place in November 2016; pre-Demonetization characteristics are measured in the 7 months prior to that (April to October 2016). The monetary amount is the local currency Indian rupee (INR), December 2015 real terms, and US\$1 = 66.2 INR as of December 2015.

different levels of prior cash dependence during the pre-Demonetization period (i.e., parallel trends in the pretreatment 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 i 's prior cash dependence and 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 the individual level and at the month level.

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

$$y_{i,t} = \mu_i + \pi_{d,t} + \sum_{t=-3}^{10} \beta_t (\text{PriorCashDependence}_i \times \mathbb{1}_t) + \varepsilon_{i,t}, \quad (2)$$

where $\mathbb{1}_t$ is an indicator variable for each of the months before and after the Demonetization. The first 4 months in our sample period, April to July 2016, constitute the omitted baseline group. In this dynamic specification, the coefficient β_0 measures the immediate spending response during the Demonetization month. $\beta_1, \dots, \beta_{10}$ track the spending response 1 month, 2 months, \dots , and 10 months after the Demonetization, respectively. Similarly, $\beta_{-3}, \dots, \beta_{-1}$ capture the difference of trends in spending across individuals with varying prior reliance on cash in each of the 3 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, Hull, and Liebersohn 2017), has been widely used and discussed in the literature, including Duflo (2001), Bhuller et al. (2013), De Chaisemartin and d'Haultfoeuille (2018), and Hvide et al. (2022). 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.

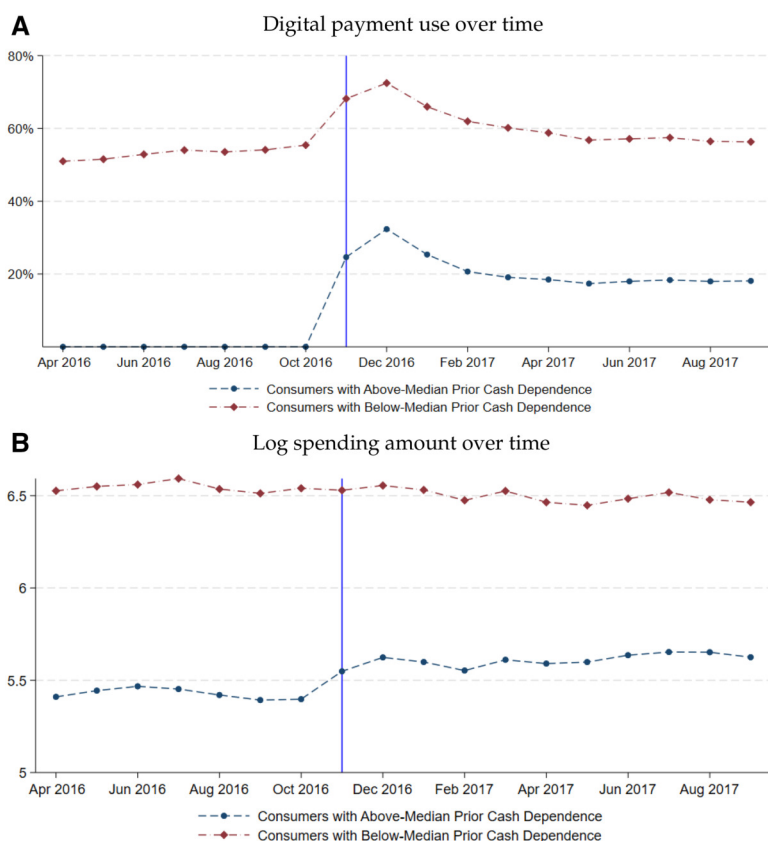
4. Evidence of Spending Increase Induced by Digital Payments

4.1 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 7 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 appear to be stable in the 7 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 10 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,

**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.

at the same time, increased spending significantly. Such a spending response persists despite the gradual replenishment of the demonetized notes.

4.2 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

Table 3
Forced switch to digital payments and Its effect on spending

	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 \times Post	0.294*** [33.09]	0.260*** [37.51]	0.001 [1.81]	-0.018** [-3.51]	0.238*** [9.92]
Individual FEs	Yes	Yes	Yes	Yes	Yes
District \times Year-month FEs	Yes	Yes	Yes	Yes	Yes
R^2	.620	.564	.350	.403	.586
No. of observations	6,561,580	6,561,580	6,561,580	6,561,580	6,561,580

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 reported at the bottom. Standard errors are doubly clustered at the individual level and at the month level; the corresponding t -statistics are reported in brackets. * $p < .1$; ** $p < .05$; *** $p < .01$ (two sided).

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 inter-quartile range of prior cash dependence can demonstrate the economic significance of this estimate: The 25th and 75th percentiles of prior cash dependence are 50% and 100%. Therefore, a consumer at the 75th percentile of prior cash dependence increases spending by 11.9% relative to a consumer at the 25th percentile.¹⁰

4.3 Testing the identifying assumptions

The parallel-trends assumption. To explicitly examine the parallel-trends assumption, in columns 1 and 2 of Table 4, we additionally control for $PriorCashDependence_i \times \mathbb{1}(Pre)$ with Pre equal to 1 for the 3 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

¹⁰ Internet Appendix Table IA.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.

Table 4
Testing the identifying assumptions

	(1) Digital payment fraction	(2) Log spending	(3) Digital payment fraction	(4) Log spending
PriorCashDependence \times Previous 3 months	−0.017 [−1.49]	−0.014 [−0.69]	−0.021 [−1.73]	−0.039 [−1.61]
PriorCashDependence \times Post	0.286*** [25.68]	0.231*** [8.22]	0.435*** [27.42]	0.234*** [11.32]
Individual FEs	Yes	Yes	Yes	Yes
District \times Year-month FEs	Yes	Yes	Yes	Yes
Controlling for observables	No	No	Yes	Yes
R^2	.620	.586	.621	.595
No. of observations	6,561,580	6,561,580	6,561,580	6,561,580

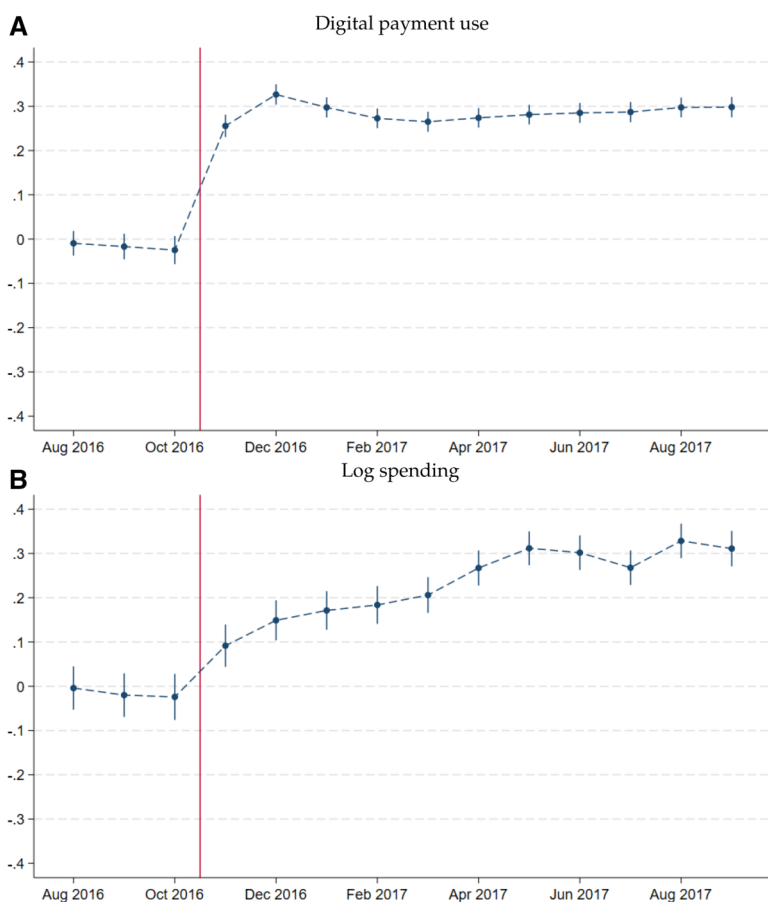
This table presents various diagnostic tests of the identifying assumptions by augmenting Equation (1) with additional controls. The data are at the individual-month level (April 2016 to September 2017). Outcome variables include the fraction of spending paid by digital payments and the log level of 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 3 months immediately before the Demonetization (i.e., August to October 2016). Post is an indicator for post-Demonetization months. Fixed effects are reported at the bottom. In columns 3 and 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 the individual level and at the month level; the corresponding t -statistics are reported in brackets. * $p < .1$; ** $p < .05$; *** $p < .01$ (two sided).

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 intensities 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 and 4 of Table 4, show that the coefficients of $PriorCashDependence_i \times \mathbb{1}(Post)$ do not change in any statistically significant sense.

4.4 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.

**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 the individual level and at the 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 constitutes 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).

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 1-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 points (for each 1-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 1-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, spending across consumers changes little 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 75th and 25th 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.

4.5 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 1-percentage-point increase in the digital payment fraction leads to a 0.81% 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 sub-group of complying consumers who are induced to use digital payments due to the Demonetization-induced cash shortage.

Table 5
Effect of digital payments on spending (OLS and IV estimates)

	Log spending	
	(1) OLS	(2) IV (2SLS)
Digital payment fraction	0.943*** [125.67]	0.809*** [10.78]
Individual FEs	Yes	Yes
District × Year-month FEs	Yes	Yes
Within R^2	.0890	.0872
First-stage F-statistic		1,095.1
No. of observations	6,561,580	6,561,580

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 reported 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 the individual level and at the month level; the corresponding t -statistics are reported in brackets. * $p < .1$; ** $p < .05$; *** $p < .01$ (two sided).

We have justified the first stage above. Discussions for the exclusion restriction will be provided in Section 5. 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 noncompliers. 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

Table 6
Characteristics of complying consumers

Subgroups	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

This table examines the characteristics of complying consumers, that is, the consumers who are prompted to use digital payments due to the cash shortage brought about by Demonetization. We follow [Imbens and Angrist \(1994\)](#) to calculate the relative likelihood of a complying consumer belonging to a particular subgroup in the sample as the ratio of the first-stage estimate for that subgroup to the overall first-stage estimate.

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.

5. 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 4.3](#), 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.

5.1 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 who arrived after the Demonetization are excluded from our analysis. Our estimation is not affected by the shift from informal markets to

Table 7
Digital payments and different spending components

	Differentiate food & nonfood spending		Differentiate durable & nondurable spending	
	(1) 1(Food spending > 0)	(2) 1(Nonfood spending > 0)	(3) 1(Durable spending > 0)	(4) 1(Non- durable spending > 0)
PriorCashDependence \times Post	0.004** [3.33]	0.050*** [13.75]	0.011*** [7.97]	0.000 [0.78]
Individual FEs	Yes	Yes	Yes	Yes
District \times Year-month FEs	Yes	Yes	Yes	Yes
R^2	.345	.443	.243	.251
No. of observations	6,561,580	6,561,580	6,561,580	6,561,580

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 reported at the bottom. Standard errors are doubly clustered at the individual level and at the month level; the corresponding t -statistics are reported in brackets. * $p < .1$; ** $p < .05$; *** $p < .01$ (two sided).

the supermarket in the form of newly arrived consumers. Second, as food and nondurable products are typically more accessible through informal markets, any potential shift in 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 nonfood 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 nonfood 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

Table 8
Is increased spending driven by the shift to the formal market?

	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 \times Post	0.309*** [30.88]	0.133*** [6.43]	0.046*** [14.86]	0.287*** [32.73]	0.284*** [11.38]	−0.018*** [−21.49]
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes
District \times Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes
R^2	.605	.564	.366	.627	.561	.322
No. of observations	3,635,392	3,635,392	3,635,392	2,926,188	2,926,188	2,926,188

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 the 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 reported at the bottom. Standard errors are doubly clustered at the individual level and at the month level; the corresponding t -statistics are reported in brackets. * $p < .1$; ** $p < .05$; *** $p < .01$ (two sided).

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 the 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 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 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).

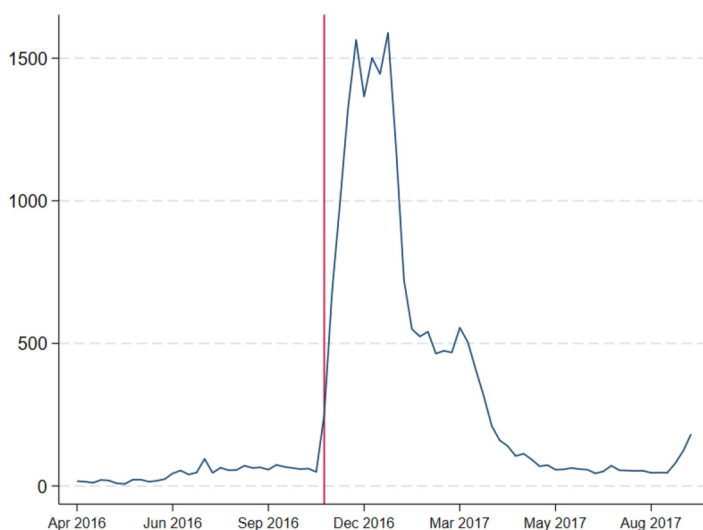


Figure 3
Informal market adoption of mobile payment

This figure plots the weekly flow of new kirana stores across 15 major Indian cities from that newly adopted mobile payment April 2016 to September 2017.

The fast-growing usage of digital payments in informal markets is consistent with the findings of [Crouzet, Gupta, and Mezzanotti \(2023\)](#). 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

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 [Internet Appendix Section D](#). Crucially, these results also show no decrease in the examined types of spending, making it

¹¹ This phenomenon has been reported by [the Economic Times](#), [Firstpost](#), and [the Indian Express](#), among others.

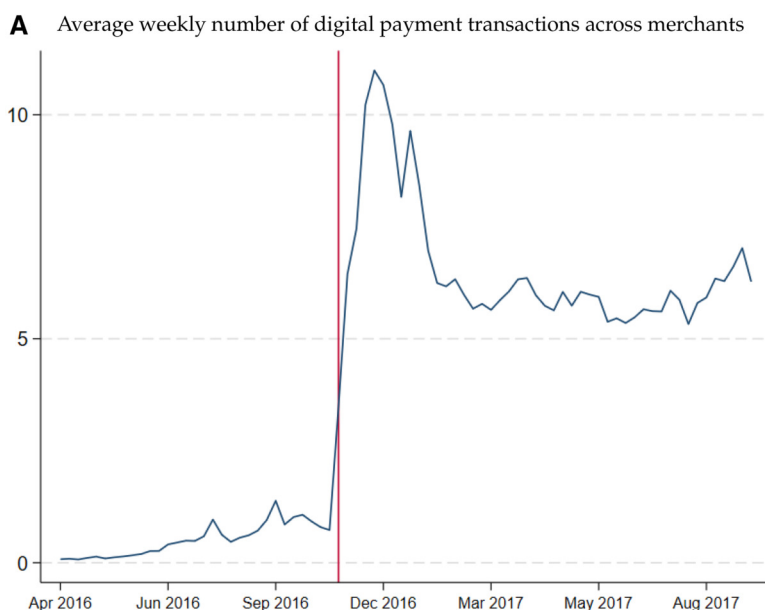


Figure 4
Digital payment transactions in informal markets

This figure plots the weekly average of digital payment transactions for the kirana stores that had already adopted the mobile payment 4 weeks prior to the Demonetization announcement. Demonetization took place in November 2016. Each panel corresponds to a measure of digital payment transactions, the average weekly number of digital payment transactions across merchants in panel A, the 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.

less plausible that our main findings are driven by an increase in supermarket spending at the expense of other types of spending.

5.2 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

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.

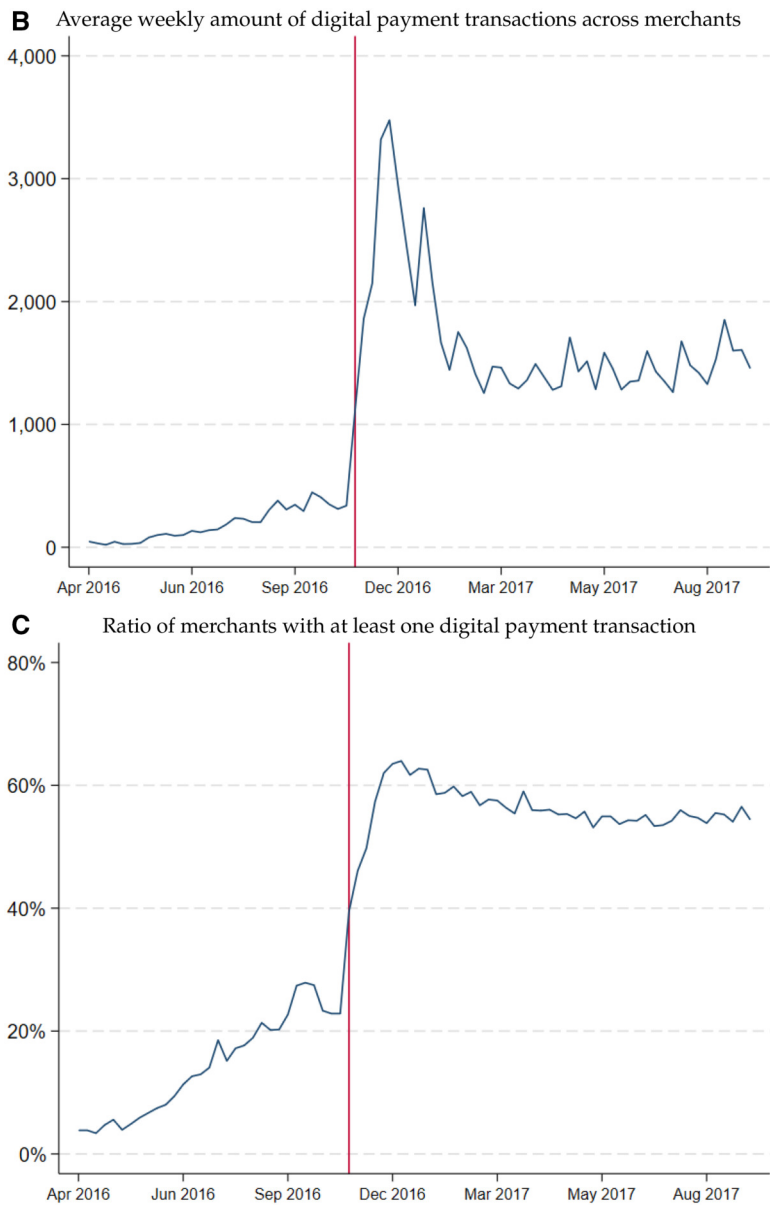


Figure 4
(Continued)

specifications also directly control for the time-series fluctuation of national and regional economic conditions.

A more nuanced income shock explanation involves a reallocation 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 return 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 3.

In the empirical implementation, we define large receipts as receipts whose amount exceeds the 90th percentile (452 rupees in December 2015 real terms) in the size distribution 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.

5.3 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.

¹³ According to the RBI's Annual Report 2017–18, 99% of total 500 and 1,000 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.

¹⁴ For the sake of comparison, the 75th percentile of all receipts in the full sample, regardless of payment method, is 290 rupees in December 2015 real terms.

Table 9
Is increased spending driven by income shocks?

	Did not use cash for large bills pre-Demonetization		Used cash for large bills pre-Demonetization	
	(1) Digital payment fraction	(2) Log spending	(3) Digital payment fraction	(4) Log spending
PriorCashDependence \times Post	0.310*** [38.50]	0.459*** [16.18]	0.200*** [17.01]	−0.025 [−0.92]
Individual FEs	Yes	Yes	Yes	Yes
District \times Year-month FEs	Yes	Yes	Yes	Yes
R^2	.656	.565	.545	.486
No. of observations	3,950,260	3,950,260	2,611,320	2,611,320

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 90th 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 reported at the bottom. Standard errors are doubly clustered at the individual level and at the month level; the corresponding *t*-statistics are reported in brackets.
* $p < .1$; ** $p < .05$; *** $p < .01$ (two sided).

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 (Internet Appendix Figure IA.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 rely 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 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 reestimate 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 nonusers, defined as consumers

Table 10
Is increased spending driven by credit supply shocks?

	Log spending				
	Full	Existing users		New users	Nonusers
	(1)	(2)	(3)	(4)	(5)
PriorCashDependence × Post	0.238*** [9.92]	0.086* [2.83]	0.134** [3.91]	0.357*** [15.43]	0.243*** [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	.586	.520	.520	.504	.586
No. of observations	6,561,580	240,191	240,191	551,031	5,770,358

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 cards before the Demonetization; nonusers, 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 reported at the bottom. Standard errors are doubly clustered at the individual level and at the month level; the corresponding *t*-statistics are reported in brackets. * $p < .1$; ** $p < .05$; *** $p < .01$ (two sided).

who never used any credit card in the sample period. Table 10 reports the results.

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, 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 nonusers can be viewed as an estimate of the added effect of credit supply. Column 5 shows that the spending response associated with prior cash dependence in the sample of nonusers is almost identical to the full-sample estimate. The comparison of sample sizes shows that the majority of consumers in our sample are nonusers — 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.

5.4 Addressing the identifying concern of supplier's pricing response

Next, we consider whether 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 will become less price sensitive following the adoption of digital payments, they could potentially take advantage of consumer behaviors and increase their mark-up.

To begin, 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.

6. 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

¹⁵ We use the following regression:

$$y_{i,j,t} = \mu_i + \pi_j + \sum_{t \neq 0} \beta_t \mathbb{1}_t + \sum_{t \neq 0} \gamma_t (\mathbb{1}_t \times \mathbb{1}(\text{HighExposure}_i)) + \varepsilon_{i,j,t}. \quad (3)$$

The dependent variable $y_{i,j,t}$ is the logarithm of the mean transaction price of product i in store j on day t . $\mathbb{1}_t$ are monthly indicators, and 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.

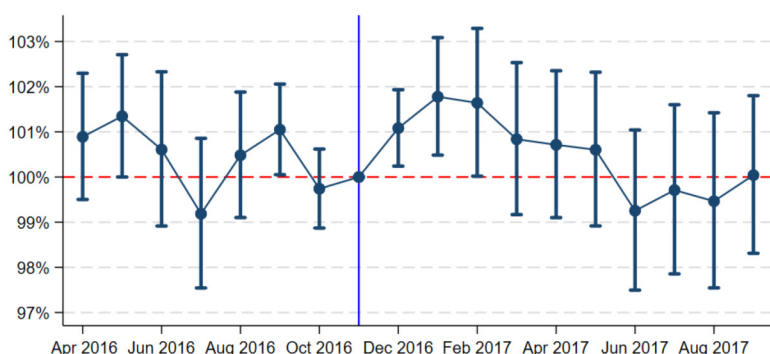


Figure 5

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.

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 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; Raghuram 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, Schmidt, and Stix 2014) or serves as a commitment device to avoid overspending (Hernandez, Jonker, and Kosse 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 preset budget. Relatedly, Li (2023) models cash to be more useful for budgeting than debit cards.

6.1 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

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

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 $E[(Cu^{1/\sigma} + \delta^{1/\sigma} R^{(1-\sigma)/\sigma})^\sigma] = 1$. 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, Rao, and Schilbach 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.

6.2 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

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 .

explanation 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, a consumer will find making an online purchase with cash on delivery less painful than making an in-person 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 \times 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 merchant adoption.

Table 11 reports the estimates obtained from the online grocery retailer data. The sample period, April 2016 to September 2017, covers 11 months following the Demonetization same as in our main analysis using the supermarket data. In Column 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 10 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 and 4).¹⁹

6.3 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

¹⁸ Additional details for sample construction and variable definitions can be found in [Internet Appendix Section C](#).

¹⁹ Detailed matching procedure is presented in [Internet Appendix Section C](#).

Table 11
Forced switch to digital payments and its effect on online grocery store spending

	Online grocery sample		Supermarket (matched sample)	
	(1) Digital payment fraction	(2) Log spending	(3) Digital payment fraction	(4) Log spending
PriorCashDependence \times Post	0.523*** [35.17]	0.039** [3.23]	0.304*** [26.23]	0.120*** [4.14]
Individual FEs	Yes	Yes	Yes	Yes
District \times Year-month FEs	Yes	Yes	Yes	Yes
R^2	.654	.570	.555	.544
No. of observations	179,470	179,470	90,720	90,720

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 reported at the bottom. Standard errors are doubly clustered at the individual level and at the month level; the corresponding t -statistics are reported in brackets. * $p < .1$; ** $p < .05$; *** $p < .01$ (two sided).

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 nontemptation 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 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 nontemptation 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 nontemptation spending.

We split the sample of individual consumers into two subsamples 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

Table 12
Digital payments and spending on temptation goods

	Full sample		Below-median spending pre-Demonetization		Above-median spending pre-Demonetization	
	(1) 1(Temptation spending > 0)	(2) 1(Non-temptation spending > 0)	(3) 1(Temptation spending > 0)	(4) 1(Non-temptation spending > 0)	(5) 1(Temptation spending > 0)	(6) 1(Non-temptation spending > 0)
PriorCashDependence × Post	0.034*** [6.50]	0.008*** [10.48]	0.022*** [6.37]	0.014*** [8.66]	0.005 [0.92]	−0.002*** [−3.99]
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes
District × Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes
R ²	.375	.304	.383	.340	.334	.195
No. of observations	6,561,580	6,561,580	2,233,255	2,233,255	4,328,325	4,328,325

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 nontemptation spending. In columns 1 and 2, we analyze the full sample of individual consumers. In columns 3 and 4 (5 and 6), we analyze the subsample 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 reported at the bottom. Standard errors are doubly clustered at the individual level and at the month level; the corresponding *t*-statistics are reported in brackets. * $p < .1$; ** $p < .05$; *** $p < .01$ (two sided).

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.

6.4 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 reestimated an alternative specification that exploits similar cross-district variation using our own data. The results of this analysis, presented in Internet 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 subgroup 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 6.2](#) and [Internet Appendix Sections C and D](#). These findings are consistent with the behavioral costs associated with cash payment: when consumers pay for online purchases with cash on delivery, the experience feels less memorable than if they were to pay in-person with cash. The more muted increases in online spending, coupled with the finding of a larger consumption response associated with temptation goods compared to nontemptation goods in [Section 6.3](#), 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, Kulkarni, and Ritadhi \(2023\)](#) 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.

7. 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 because of 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 nontemptation goods, our analyses suggest potential overspending 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.

Code Availability: The replication code is available in the Harvard Dataverse at [doi:10.7910/DVN/B1WQOX](https://doi.org/10.7910/DVN/B1WQOX).

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