Can Cashless Payments Spur Economic Growth?*

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Abstract

After the introduction of a nationwide Unified Payment Interface (UPI) in 2016, India has become one of the world's leading economies for cashless transactions. We use the heterogeneity in the intensity of the adoption of digital payments across districts to show that economic outcomes, as measured by household income and small business activities, increased significantly in districts with a higher intensity of cashless transactions. We exploit the differences in the timing of participation on the UPI platform by different banks to establish a causal link between digital payments and economic outcomes. Our results are stronger in financially less developed regions of the country and for financially weaker households such as small traders. Relaxation of borrowing constraints and reduction in the transaction cost of payments are two principal mechanisms behind our results. These findings have important implications for theories of intermediation and for other economies that are considering the adoption of digital payment systems.

Keywords: Cashless, FinTech, Digital Payments, UPI
JEL Classification: G21, G28

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1 Introduction

Can the means of payments affect economic growth? While the debate on whether monetary variables, such as cash, can affect economic outcomes is not new (Lucas and Stokey, 1987; Woodford, 2003), recent technological advancements in cashless payments has reinvigorated this debate. In a frictionless economy, the means of payments act simply as a medium to settle claims across transacting parties, leaving no role for it to directly influence real economic outcomes. However, in the presence of transaction costs and information asymmetry between transacting parties, some forms of payments can be more effective than others in minimizing these frictions. As a result, the medium of payment can affect real outcomes and economic growth. As countries around the world are experimenting with digital payments, a careful empirical examination of the effect of cashless payments on economic outcome can help shape the policy debates as well as shed light on economic frictions at play.[1] Our paper provides one of the first empirical evidence on this question using the large scale adoption of cashless payments across India in the past few years.

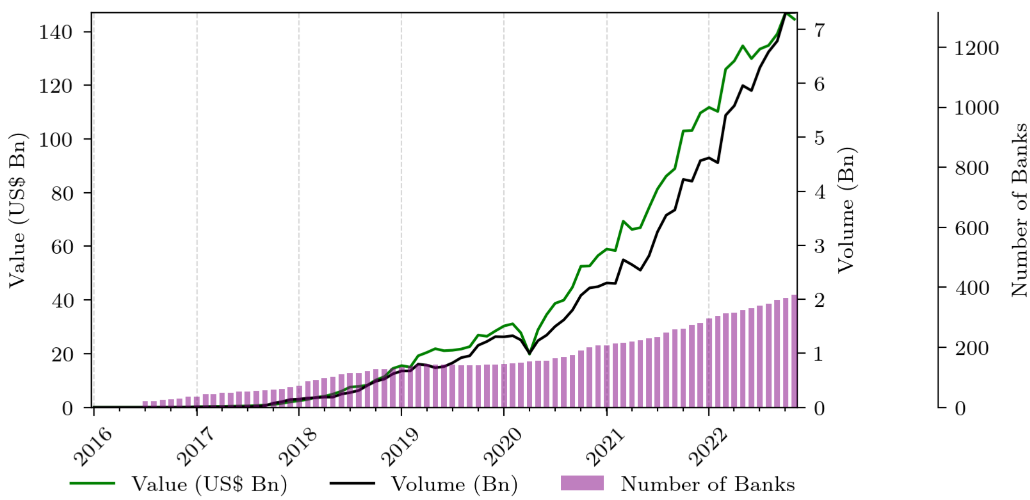
The adoption of digital payments in India presents a unique and attractive empirical setting for three principal reasons. First, the economic magnitude of the adoption is large. Digital payments in India accelerated after the nationwide launch of the Unified Payments Interface (UPI) on August 25, 2016,[2] an initiative of the Government of India, that facilitated a quick and seamless settlement of payments across the entire banking network in the country without any cost to the consumers and merchants. Second, the extent of cashless transactions in the early years varied greatly across districts depending on a host of factors, including whether the main public sector bank in the district was an early adopter of the UPI platform or not. We exploit these differences to estimate a causal effect. Finally, we are able to obtain a very high-frequency and granular household level panel data, which allows us to measure

[1] The Federal Reserve system in the U.S. launched "FedNow", an instant payment system, in July 2023. Brazil launched its own fast payment system "Pix" in 2020.

[2] https://www.npci.org.in/what-we-do/upi/product-overview

economic outcomes at a micro-level. The richness of our data facilitates a causal analysis. In particular, we exploit variations in the benefits of cashless payments within a specific district-quarter across different households based on the likely benefit of digital transactions to them. Such a within-district-quarter empirical approach minimizes concerns about omitted time-varying latent characteristics of districts from affecting our results.

Figure 1: Growth in Digital Transactions on the UPI Platform



Source: National Payments Council of India (NPCI)

Figure 1 shows the evolution of digital payments on the UPI platform over time on a monthly basis since its launch in late 2016. Average monthly volume of digital transactions increased rapidly from a level of less than \$1 billion in early 2017 to almost \$150 billion by the end of 2022. In the early months of its launch, less than 30 banks joined the platform, allowing their customers to use the UPI by linking their bank accounts to the UPI mobile Apps. The number of participating banks increased steadily over time, covering practically the entire banking sector by the end of 2022. Three critical factors were responsible for the successful launch and adoption of the UPI platform. First, every Indian resident was provided with a unique identification card, called the Aadhaar Card, through a nationwide initiative that started in 2010.[3] Second, the central government launched a universal banking program

[3]Aadhaar is a Hindi word for 'foundation'.

in 2014, called the Pradhan Mantri JanDhan Yojna (JDY), to provide a bank account to every household in the country (Agarwal, Alok, Ghosh, Ghosh, Piskorski, and Seru, 2017; Chopra, Prabhala, and Tantri, 2017). Third, the country invested significant resources in developing the digital infrastructure needed for such a secure and fast payments architecture that operates across platforms (Acharya, 2023). Importantly, the digital and biometric-based Aadhaar card made the verification of a banking transaction instant and secured. After the launch, several government sponsored incentive schemes and promotional campaigns were launched across the nation. Furthermore, two additional factors - the demonetization of high denomination currency notes in November 2016, and the COVID-19 pandemic - provided additional boost to the adoption of digital payments in the country.

Our empirical work is motivated by two principal economic frictions that a mass adoption of instant digital payment system can alleviate to foster economic growth. First, it can minimize transaction costs of payments, which in turn can facilitate higher level of economic activities. For example, street vendors and small shopkeepers can easily accept payments for their goods and services through a digital wallet after the launch of the UPI system.[4] The benefits of lower transaction cost can be especially high in areas with lower availability of formal financial institutions. Second, a digital payments economy can alleviate financing frictions by improving the flow of information to the lenders for credit decisions (Berg, Burg, Gombović, and Puri, 2020; Balyuk, 2023; Parlour, Rajan, and Walden, 2022), improving the processing time for credit decisions (Fuster, Plosser, Schnabl, and Vickery, 2019), or increasing the ability of lenders to enforce the repayment contracts (Brunnermeier and Payne, 2022; Dai, Han, Shi, and Zhang, 2022). Indeed, several FinTech firms around the globe use digital payments information to provide financing, especially to small businesses who face greater limitations in gaining access to financing opportunities (Ghosh, Vallee, and Zeng, 2021).

[4] For example, see the IMF's report on India's digital stack: https://www.imf.org/external/pubs/ft/fandd/2021/07/india-stack-financial-access-and-digital-inclusion.htm

We use a panel data covering 200,000 unique households spread across over 500 districts in India from 2014 to 2022 for our empirical work.[5] Our dataset has information on their overall income, income from business activities, credit market outcomes, and a host of characteristics such as their occupation and whether they reside in a rural or urban area. Our focus on household level outcomes is especially suited for the task at hand since the mass adoption of digital payments is more likely to alleviate the transaction cost and credit constraint frictions for these households and small businesses operated by them. We focus on three key measures of real economic activities: (a) overall income of these households, (b) creation of new businesses by them, and (c) their business income. We measure the extent of digital payments in a district by the volume of transactions reported by PhonePe, the leading provider of UPI App in the country, on a district-quarter level. The UPI payments data starts in 2018, i.e., a year after the launch of UPI, when the transaction volume started to become significant.

In our first analysis, we relate the extent of digital payments in a district in a quarter to the next quarter's economic outcome of the households living in that district. Since our model includes household and year-quarter fixed effects, our estimates are not contaminated by time-specific shocks to the aggregate economy, or time-invariant district and household characteristics. We show that the level of digital payments forecasts next quarter's economic outcome in an economically and statistically significant manner. The elasticity of income to digital payments is 0.09. In terms of business activities, districts with twice the level of digital payment have 0.84% higher number of households engaged in business activities, which is economically large since only 17% of households in our sample are engaged in business activities. In line with these findings, the level of income from business activities increased considerably for households residing in high digital payment districts, with an elasticity of business income to digital payments of 0.16.

[5] The dataset comes from the Center for Monitoring Indian Economy (CMIE). It provides a representative sample of households across the country covering various income, age, education, and occupation group. See Gupta, Malani, and Woda (2021) for a detailed discussion of the database.

Our base case specification shows that the level of digital payments affect next quarter's economic outcome. We obtain similar results if we relate economic outcomes at the annual level to the level of digital payments in the district in the previous year. Our results, therefore, are less susceptible to unobserved shocks that affect the adoption of digital payments and economic outcomes at the same time. Yet, there can be endogeneity concerns that can arise from a correlation between digital payment adoption and expectations of future economic outcome in an area. In general, the key threat to our identification comes from time-varying changes in unobserved factors that might affect the adoption of digital payments and economic growth in a district. We use two complementary identification strategies to establish a causal link.

Our first empirical strategy exploits the difference in the timing of participation on the UPI platform by different banks, and its impact on the adoption of digital payments by consumers who reside in different districts of the country. To use the UPI system, a customer needs to link her bank account to an UPI App that can be provided by either the same bank or third parties. Therefore, whether a customer's bank participates in the UPI platform or not becomes a key driver of her willingness and ability to begin using digital payments. While practically the entire banking sector has joined the platform by now, in the initial months of the UPI's launch less than 30 banks participated on the platform. Consequently, the UPI adoption rate across districts varied by the timing of a bank's participation on the platform and the presence of the bank in a given district. We exploit an institutional setting that provides quasi-random variation in the bank's presence across districts to achieve a causal link between digital payments and economic outcomes.

The Government of India nationalized all the large private sector banks of the country in two waves of nationalization in 1969 and 1980. Soon after the first wave of nationalization in 1969, the Reserve Bank of India (RBI) established a system of "lead bank", under which every district in the country was assigned a government owned bank as its main bank. The lead bank was primarily responsible for expanding access to financial services to underserved communities, especially in rural and semi-urban areas. While there have been several tweaks to the initial structure of the lead bank system over the decades, the basic structure has remained intact. Every district in the country still has a government owned lead bank. Due to a long historical nature of this system, the lead bank remains a dominant source of banking infrastructure in the district, especially in rural and semi-urban areas.

At the time of the launch of the UPI in August, 2016, 29 banks had indicated their interests in joining the platform. 21 of these banks joined the platform immediately, and the rest of them did so by the end of November, 2016. Within the set of government owned bank, some large banks were notably missing from this list, creating a significant hurdle for their customer's desire to switch to digital payments. We divide districts into two categories based on whether their lead bank was an early or a late adopter of the UPI, i.e., whether it was part of the first set of 29 banks or not.[6] The late adopter banks joined the platform by May, 2017. Since the adoption of digital payments rely heavily on the network effects of its users, a delay of 6-9 months can have a large impact on the use of cashless payments even in normal times. But, the month of November, 2016 has a special significance in the history of India's monetary policy. During this month, the country went through an unexpected demonetization shock, making the availability of digital means of payments especially valuable. While the customers of the early adopter banks could easily switch to digital payments, those of the late adopter banks could not, leaving a long lasting difference in the adoption of digital payments across these districts. The combination of the difference in the timing of adoption, the history of the lead bank system in the country, and the occurrence of demonetization shock during this period present us with a quasi-exogenous variation in the adoption of digital payments across different districts of the country.

We create a matched sample of early and late adopter districts that are in the same state

[6]The late adopter lead banks are: Indian Bank, Indian Overseas Bank, Bank of India, Syndicate Bank, Corporation Bank, Punjab & Sind Bank, and Dena Bank. The early adopter lead banks are: Andhra Bank, Bank of Maharashtra, Canara Bank, Punjab National Bank, United Bank of India, UCO Bank, Union Bank of India, Vijaya Bank, Oriental Bank of Commerce, Allahabad Bank, State Bank of India, Bank of Baroda, and Central Bank of India.

with very similar bank branch penetration in 2016, literacy rate, and population. Further, we ensure that the lead banks across the early and late adopter districts are comparable in terms of their organizational status, namely whether they were an acquisition target during the sample period or not, to rule out any effect that can arise from their differential ability and focus.[7] Therefore, we obtain two sets of very similar districts that differ in terms of their lead bank's participation on the UPI system. We document a significantly higher level of digital payments in early districts, about 15-25% depending on the quarter, compared to their late counterparts. The difference persisted for a long time after the launch of the UPI platform, consistent with the presence of strong network externality in the adoption of these methods of payments and financial technology in general (Crouzet, Gupta, and Mezzanotti, 2023; Higgins, 2020).

We employ a standard difference-in-differences empirical design to compare economic outcomes for households who live in the early versus late adopter districts. Both groups exhibit parallel trend in their income before the UPI shock, but households in the early adopter districts have 7.85% higher income in the post-UPI period. These households have 1.03% higher level of business ownership and consequently significantly higher business income in the post-UPI period in the difference-in-differences specification.

Our specifications include fixed effects for $state \times year - quarter$ to alleviate concerns for time-varying state level policies from impacting our results. We also include fixed effects for the interaction between year-quarter and an indicator for whether the household lives in an urban or a rural area to soak away the differential effect of government policies that target rural areas. Finally, we obtain a measure of district-level demonetization shock from the study by Chodorow-Reich, Gopinath, Mishra, and Narayanan (2020). We include fixed effects for the interaction between year-quarter and the level of demonetization shock to soak

[7]Specifically, the Indian banking sector underwent a wave of bank consolidation in 2019-2020. Some of the weak banks were acquired by other banks that were relatively stronger in financial and organization capabilities. We ensure that a late adopter district with the lead bank that was the target (not a target) of these consolidations is matched with an early adopter district with a lead bank that was also a target (not a target) of consolidation.

away the differential impact of demonetization shock on economic outcomes across districts.

We also exploit the variation in the intensity of early UPI participation by all banks in the district, and not just its lead bank, in an additional empirical specification. In this specification, we are able to capture the variation that arises from the branch network of the private sector banks as well as all the non-lead public sector banks in the district. Districts with a higher fraction of branches of the early adopter banks have a significantly higher level of digital payments on a per capita basis. Such districts have higher income and higher levels of business activities, consistent with our earlier results.

Our second identification strategy relies on a different assumption, and in turn, provides some novel economic insights about the economic mechanism as well. We exploit differences across households within a district-year-quarter to soak away time-varying differences across districts. Specifically, we estimate the differential effect of digital payments on outcomes for self-employed versus other households. The key idea behind our identification strategy is that the self-employed households are more likely to benefit from the adoption of digital payments as it allows entrepreneurs to start their own businesses or expand the scale of their business due to lower transactions costs and improved access to business credit. While other households also benefit from faster and cheaper payments processing, by definition they are relatively less likely to benefit from the channels that underpin business growth. Using a within-district-year-quarter variation, we show that self-employed households experience a significantly higher increase in their income compared to other households in higher digital payments districts.

In a supplementary test, we focus exclusively on a smaller set of self-employed households: 'street vendors and hawkers'. This category of self-employed households often operate with little-to-no collateral, and therefore face large credit constraints. We show that this group of entrepreneurs experience a significant increase in income compared to the other self-employed households as the level of digital payments increase.

Consistent with the idea that digital payments can alleviate frictions generated by the lack of access to brick-and-mortar banks, we find that the effect of digital payment on economic outcomes is stronger for districts with fewer bank branches on a per capita basis. In terms of credit outcomes, we find that a significantly higher fraction of households borrow for business purposes after an increase in digital payments in their districts. Their overall borrowing goes up and the composition of their borrowing changes as well: borrowings from formal sources such as banks increase, whereas borrowings from informal sources such as money-lenders and other individuals come down. Further, we find that marginal borrowers, namely the street vendors and hawkers, increase their borrowing from formal sources such as banks and non-bank financial companies (NBFCs), whereas they significantly decrease their borrowing from informal sources such as money lenders and family and friends. These findings are consistent with a relaxation of credit constraints, both in terms of quantity and quality of credit.

Our main results are based on the panel of household survey by the CMIE. One may be concerned about some noise in our data, as is typical in most household panel survey data of this nature. Our empirical strategy, especially the one with a difference-in-differences design across early and late adopter districts, is unlikely to be affected by these concerns since these variations are unlikely to be correlated with the identity of lead banks in a district, and even less so in a time-varying manner. Another concern about our study could arise due to a "reporting" bias. If digital payment adoption makes households more likely to accurately report their income, then our empirical design presents no challenge. Some households are likely to underreport in the absence of digital information, whereas some overreport, making it unlikely to affect our main results. However, if households in lower digital payment districts systematically underreport their income to avoid taxes, then a part of our effect can be attributed to the underreporting bias. Our collective findings make this channel unlikely to explain all our results since there is no incentive to underreport credit outcomes or the source of credit for tax-avoidance reasons. Further, our results show that marginal entrepreneurs exhibit higher income after the adoption of digital payments. In our sample, most of these households are below the tax exemption limit of annual income in India, which provides very little incentive to distort income reporting in a systematic manner.

Yet, to address the issue more directly, we conduct three additional tests. In our first test, we measure economic activity with the night time lights data at the district-quarter level and show that districts with higher digital payment activity have higher income and business activities. Since the night lights data carefully captures economic activities from formal and informal sources, and is free from the survey reporting biases, our results are unlikely to be driven by either the misreporting incentives or other issues with a survey based methodology. Second, we use the district-quarter level data on the amount of credit extended by all commercial banks in the country that the RBI reports on a quarterly basis. We show that the level of credit increases in a district following an increase in digital payments. Since credit creation is correlated with economic growth, the finding helps us rule out the reporting bias channel. In addition, the result lends further support to the credit constraints channel behind our findings. Finally, we document an increase in the purchase of durable goods such as generator sets and computers in districts with higher digital payments. These measures of economic activity are less susceptible to reporting errors and incentives to hide income.

In sum, we show that digital payments impact real economic outcomes, especially for marginal households and for households who live in financially less developed districts. The relaxation of credit constraints is a key mechanism behind our findings. While there is a large and growing literature on the role of digital payments on borrowing outcomes, to the best of our knowledge our paper is one of the first to tease out a causal link between digital means of payment and real activities at a national level. Our work complements the body of work on mobile money, most notably on M-Pesa in Kenya, that are used predominantly for remittances. Our work is of independent interest not only due to our focus on a different economy, but also due to some key differences in the operations of mobile money (M-Pesa) and a pure digital payments system (the UPI). These differences allow us to uncover the effect of means of faster payments on economic outcomes that is independent of the effect that arise due to better access to a storage-of-value technology or the dependence on an agent-based network that the mobile money relies on. We discuss these differences in detail in the literature review section.

In Section 2, we discuss the contribution of our work to the existing literature. Section 3 discusses the institutional setting of the UPI platform in more detail. Section 4 describes the data that we use and presents descriptive statistics. In Section 5, we discuss our empirical strategy and show our results, before we conclude in Section 6.

2 Literature Review

Our paper contributes to several strands of literature in economics and finance. It is most closely related to the growing literature that studies the effect of cashless payments on borrowing constraints faced by various agents in an economy. The main idea here is that digital payments can alleviate credit rationing due to information frictions in an economy (Stiglitz and Weiss, 1981). Recent studies such as Ghosh et al. (2021) and Brunnermeier and Payne (2022) indicate that electronic payments generate a verifiable digital transactions history which help reduce information asymmetry between lenders and borrowers. Furthermore when used for online retail purchases, cashless payments enhance the digital footprint of consumers in an economy. This improves access to credit for potential borrowers as suggested by Berg et al. (2020) and Agarwal, Alok, Ghosh, and Gupta (2021), as well as increase the repayment likelihood of borrowers as shown by Dai et al. (2022). Moreover, improved digital footprint also helps lenders to price their loans better, as suggested by Di Maggio and Yao (2021). In general, there is a fast growing literature on the effect of FinTech on credit outcomes (Chava, Ganduri, Paradkar, and Zhang, 2021). While we build on this literature, our paper is distinct on a key dimension – it provides one of the first pieces of evidence on the impact of digital payments on real economic output. It is not clear ex ante whether and to what extent a switch to cashless payment can impact real economic activities. For example, if FinTech lenders simply substitute traditional forms of credit (Gopal and Schnabl, 2022), then it may not have any meaningful impact on real output.

Our work relates to an important literature on mobile money, most notably on M-Pesa in Kenya.[8] Jack and Suri (2014) show that the use of M-Pesa in Kenya allows households to smooth their consumption since they are able to receive payments through remittances from a wider network of family and friends. Further, Suri and Jack (2016) show that the use of M-Pesa improved the allocation of consumption and labor in the economy, which in turn resulted in a reduction in poverty. While there are several similarities between our setting and mobile money such as M-Pesa, there are fundamental differences across the two systems. The mobile money sits outside the banking system since the accounts are only linked to a phone number, and not to a bank account or credit card (Suri, 2017). Therefore, a consumer needs to store her money with the mobile company as she begins to use the system. As a result, mobile money combines means of payments and store-of-value functions, unlike the digital payment system such as UPI that is purely a means of payment system.

Related, the availability of mobile money in Kenya effectively provided previously unbanked population to banking services as they could deposit and withdraw cash using the mobile money system. Therefore, it is hard to separate financial inclusion effect from means of payments effect using the setting of M-Pesa. In addition, mobile money system such as M-Pesa works as an agent-based model, i.e., the consumer needs to have access to a network of mobile phone agents to send or receive payments. The UPI system doesn't rely on a network of agents, and therefore it does not face any trust, agency, information, or transaction cost frictions that an agent based system can create. Though not exclusively, a mobile money system is predominantly a person-to-person system, where people receive remittances from their family and friends, whereas the UPI system covers both peer-to-peer transfers and person-to-merchant transfers. The distinction is economically important as it allows the

[8]See Suri (2017) provides an excellent survey of this literature.

creation of new businesses at a much lower transaction cost under the UPI system. In sum, while our study complements the literature on M-Pesa, our exercise provides a relatively cleaner setting to tease out the effect of cashless means of payments that is independent of the implications of changes in the store of value, availability of an agent network, or remittances from family and friends.

The literature on fast payment system is still in its early stages. Sarkisyan (2023) studies the impact of Pix, the fast payment system of Brazil, on the concentration in deposit markets. He shows that instant payment system can result in increased competition in the banking market. Ouyang (2021) shows that BigTech firms such as Alibaba can use the additional data generated by the firm-sponsored payment system, Alipay, to improve credit access for marginal borrowers. Such a firm-sponsored system is related but inherently different from payment systems such as FedNow, Pix, of UPI, i.e., an interoperable nationwide system of fast payments where customers can transact freely across financial institutions. These papers do not study the implication of fast payments on real economic outcomes, the focus of our study. Collectively, our findings document complementary but different aspects of the positive effects of cashless payments on financial and real outcomes.

Our work complements two recent papers on network externality of financial technological adoption. Crouzet et al. (2023) document significant complementarity in the adoption of digital wallets by consumers in India after the demonetization shocks. Higgins (2020) shows that a program of the Mexican government that distributed debit cards to cash transfer recipients led to increased adoption of point of sale terminal by small retail shops, which in turn led to an increase in their sales and profits. Since these sales came at the expense of large super markets, the paper is silent on overall economic impact of debit card adoption. Our paper's identification strategy relies on the presence of strong network externality in the adoption of UPI payments, consistent with the core idea of both of these papers.

At a broader level, our work relates to the literature on the role of financial development on economic growth, an idea first made prominent by Schumpeter (1911). Using data from over 80 countries, King and Levine (1993) show that high level of financial development is positively related to improvement in economic efficiency, capital accumulation and increase in present and future rates of economic growth. Rajan and Zingales (1998) and Demirgüç-Kunt and Maksimovic (1998) show that financial development promotes economic growth by reducing the cost of external financing for firms. Beck, Demirgüç-Kunt, and Maksimovic (2008) use survey data in 48 countries to show that financial development is significantly correlated with availability of external financing for firms, especially smaller firms who may find it more difficult to access financial services. Claessens and Laeven (2003) also find increase in economic growth with financial development due to improved access to financing. Cetorelli and Strahan (2006) also explore the role of financial development on real economic activity and show that concentrated local US banking markets result in increased difficulties in access to credit for newer, smaller firms. Using data from Italy, Guiso, Sapienza, and Zingales (2004) report that financial development facilitates economic growth by increasing business creation. In the Indian context, there is a rich literature on the role of rural banks and micro-financial institutions on economic growth and consumer welfare (Burgess and Pande, 2005; Banerjee, Duflo, Glennerster, and Kinnan, 2015).

Lastly, we contribute to the literature that captures drivers of economic growth in India. Using the demonetization shock in India, Chodorow-Reich et al. (2020) study the role of cash crunch on economic output across districts that were hit differentially by the shock. They document a decline in the output in the affected districts in the immediate aftermath of the demonetization shock. Gupta et al. (2021) study the impact of the COVID-19 pandemic on income and consumption. Balakrishnan and Parameswaran (2007) identify the various growth regimes in India and find that in the last two decades, services have led economic growth. Basu and Maertens (2007) also study the trends and patterns of economic growth in India and conclude that structural drawbacks such as paucity of infrastructure are a main hinderance to economic growth.

3 Institutional Details

The Unified Payments Interface or UPI is a real-time payment solution that has standardized and automated India's multiple traditional payment platforms. It facilitates instant fund transfer between bank accounts via mobile phones. Using a set of Application Programming Interfaces (APIs), UPI currently facilitates 'peer-to-peer' and 'peer-to-merchant' pay and collection requests for in-person, online, and in-app purchases. The system also allows users to set up recurring payments of up to ₹2,000 ($\sim$US$25) at any frequency, using RuPay debit and credit cards, for their utility bill payments. The pilot program was launched on April 11, 2016 with 21 participating banks and UPI-enabled applications were available for download on Google Play store starting August 25, 2016.

The participants of the UPI ecosystem include payer and payee Payment Service Providers (PSPs), remitter bank, beneficiary bank, the National Payments Corporation of India (NPCI), bank account holders and merchants. As of February 2023, the UPI platform hosts 385 banks in India, of which 60 are PSPs and have their own applications on the UPI platform, whereas the remaining 325 banks are Issuers alone, i.e., they do not have their own applications on the UPI platform. However, account holders in these Issuer banks can access the platform through any UPI-enabled application they are registered on. UPI-enabled applications are provided by either banks directly, as discussed above, or by Third Party Application Providers (TPAPs) such as PhonePe, Google Pay and Amazon Pay. The UPI platform allows for full interoperability across all UPI-based payment applications and participating institutions.

In the UPI ecosystem, the mobile phone is the primary device for payment authorization. A bank account holder who banks with any UPI member bank can register themselves on a UPI-enabled application using their AADHAR ID, a 12-digit individual identification number issued by the Unique Identification Authority of India (UIDAI) on behalf of Government of India, and generate their UPI ID, also known as a Virtual Address (VA). Registered UPI users can then use the user-friendly, one-click, two-factor authentication based UPI platform that allows for push and pull payment requests. Moreover, the platform provides a lot of flexibility to merchants and developers to customize their UPI-based applications to their business requirements. Registered UPI users who do not have a smartphone or internet connection can also access UPI via the UPI PIN option.

4 Data & Descriptive Statistics

We obtain data from multiple sources. The data on the measure of digital payment adoption at the district-level comes from PhonePe, one of the leading firms in the industry. We obtain district-quarter-level UPI transaction amount data from 2018 Q1 to 2022 Q1. Founded in December 2015, PhonePe is a leading digital payments and financial technology company in India that facilitates e-commerce payments, utility bill payments, mobile recharge and offline payments. It also provides investment services. PhonePe is owned by the Flipkart Group (87% holding in PhonePe), a subsidiary of Walmart Inc. In 2022, PhonePe had a market share of about 50% by value.[9]

Our main data for measuring economic outcomes comes from a survey data of a large panel of households covering approximately 500 districts of the country: the Consumer Pyramids Household Survey (CPHS) by the Centre for Monitoring Indian Economy (CMIE). The CMIE is a private organization that conducts CPHS, a continuous survey administered on a panel of nationally representative sample of over 170,000 households three times a year.[10] In every wave the participants provide information on their monthly outcome variables for the last four months. We use the household-level income, business activity, borrowings, and a host of other characteristics of the households from the CPHS database for our analyses. For our analysis, we collapse the data at the quarterly level to match it with the frequency of our explanatory variable, the level of digital payments in a district in a quarter. More

[9] See https://www.npci.org.in/what-we-do/upi/upi-ecosystem-statistics#innerTabTwoJan23.

[10] Each cohort of survey is called a "wave" by the CMIE. Each wave has about 170,000 households. The number of unique households across the entire sample period is over 200,000.

information on this survey data, including the variables used in the study, is provided in the Appendix.

In order to develop a metric of financial development in a district, we use the data on the number of bank branches at district-level provided by the Reserve Bank of India (RBI) for the end of year 2016. We also use the district-level bank credit data provided by the RBI in order to analyze the impact of cashless payments on aggregate credit in a district. This data is available at quarterly frequency. We use population estimates for 736 districts in India in 2020 provided by Wang, Kim, and Subramanian (2021). These estimates were arrived at by summing the population count using the WorldPop raster data.[11] We scale the number of branches per district by its population to arrive at our measure of financial development across the country.

We obtain data on the lead bank of a district from the RBI's website, and we ensure that we measure the lead bank as of 2016. We also obtain a dataset from the RBI on the number of bank branches for every bank in every district of the country as of 2019. We remove branches opened after 2016 to capture the number of branches per bank per district as of the launch of the UPI. We need this variable to construct the intensity of early adopters of the UPI platform for one of our empirical tests.

Our night light data comes from Beyer, Hu, and Yao (2022) who provide the average luminous intensity at the district-month level. The data comes from the VIIRS Day Night Band (DNB) onboard the Joint Polar Orbiting Satellite System. Beyer et al. (2022) provide a detailed decription of this database and provide evidence on its usefulness in measuring economic activities at a high frequency, granular level.

Descriptive Statistics: As shown earlier in the paper, Figure 1 presents a graphical summary of the evolution of digital payments in the country since 2016. The amount of digital transaction increased from a negligible amount in 2016 to over $140 billion per month

[11]https://hub.worldpop.org/geodata/summary?id=6527

in 2022. The number of transaction reached a level of 7 billion transactions per month. Figure A1 shows the geographical dispersion in the adoption rate across districts. We compute the average amount of digital transaction per person over all the quarters in the post-UPI period for each district and report these averages graphically in the map. We also present the geographical dispersion in financial development measure, i.e., per capita bank branches, alongside the digital payment adoption map. As we can see, there is a rich heterogeneity across the country on both these measures. We exploit these differences across the districts in our empirical work.

For our outcome variables from the CPHS database, we first aggregate the information for each household at the quarterly level. Thus, our analysis is based on about 200,000 unique households over a 35 quarters from 2014Q1 to 2022Q3, providing us with over 4.9 million observations. Depending on the specific test, we use different parts of this broad sample. Table 1 presents the summary statistics of the main variables used in our study based on the pooled observations of households and quarters. On average, a district has about ₹3,400 (∼US$42.50) of digital payment transaction per person per quarter in our sample. There is a wide cross-sectional variation in this measure across districts as indicated by the standard deviation of ₹4,900 (∼US$60).

Our primary outcome variable is the income of the household in a given quarter. We subtract any government transfers, such as direct benefit programs of the Government of India, from our income measure to ensure that our results are driven by real economic outcomes and not merely through transfers. We winsorize the income variable from 1% from both tails to ensure our results are not driven by outliers. As shown in Table 1, households in the sample have an average monthly income of ₹20,000 (∼US$250), representing an annual income of ₹2,40,000 (∼US$3000). These numbers are representative of the entire population of the country since the CPHS sampling is a reasonable representation of the country's population.

We focus on two variables for business activities: (a) the number households who are

Table 1: Summary Statistics

Table 1 presents the descriptive statistics of key variables used in the analysis. Cashless Transaction (bil.) measures the average quarterly value of UPI transaction over the 2018-2022 period in billions of Indian Rupee. Cashless Transaction per Person measures the average quarterly value of digital payment transaction in a district scaled by the population of the district. These variables are available only from 2018. Remaining variables are computed based on the entire sample of the CMIE data, i.e., over 2014-2022. Monthly Income and Monthly Business Income are computed at the household level, and are reported in local currency (Indian Rupee). Further details on variable construction are provided in the Appendix.

	Mean	SD	P25	P50	P75	N
Cashless Transaction (bil.)	15.21	52.80	0.76	2.63	9.09	2,361,213
Cashless Transaction/Person	3378.58	4915.96	1041.60	1833.04	3507.12	2,361,213
Monthly Income	19780.55	16061.60	9400.00	15000.00	24666.67	4,985,092
Monthly Business Income	3605.90	10283.76	0.00	0.00	0.00	4,985,092
% with business income	16.55	37.16	0.00	0.00	0.00	4,985,092
% with borrowing	29.90	45.78	0.00	0.00	100.00	3,583,735
% borrowing for business	3.50	18.38	0.00	0.00	0.00	3,583,735
% with bank borrowing	7.59	26.49	0.00	0.00	0.00	3,583,735
% with borrowing NBFC	1.68	12.84	0.00	0.00	0.00	3,583,735
% with borrowing informal	20.82	40.60	0.00	0.00	0.00	3,583,735
% entrepreneur	25.28	43.46	0.00	0.00	100.00	4,985,092
% hawkers	3.29	17.83	0.00	0.00	0.00	4,985,092
% farmers	13.10	33.74	0.00	0.00	0.00	4,985,092
% salaried	21.32	40.95	0.00	0.00	0.00	4,985,092

engaged in business activities in the district, and (b) the value of business income earned during the quarter. We obtain these information using two complementary approaches. In the first approach, we classify a household as a business owner if they report positive income from business activities in the given quarter. 16.55% of households in our sample are classified as business owners as per this definition. Their monthly business income is approximately ₹3,600 ($\sim$US$45). We winsorize the business income variable from 1% from both tails to ensure our results are not driven by outliers. In the second approach, we classify a household as a business owner if they identify their occupation as an entrepreneur. Compared to our earlier definition, this classification also includes households who have started a business but they do not yet have any income. It is also possible that some households do not report their income separately under the business income category; instead, their business income is simply reflected in their total income in the database. The second definition avoids these measurement related issues. About 25% of households are classified as entrepreneurs in our sample. After presenting our main results with both these definitions of business ownership, we focus mainly on the income based definition in the rest of the paper.

In terms of credit outcomes, our dataset identifies whether a household has borrowed during the quarter, the source of such borrowing, and the purpose of borrowing.[12] Since we do not have information on the amount of borrowing except for our analysis using the RBI data, all our other analyses on borrowings are based on the binary outcome variables for the relevant credit outcomes. 29.90% of the sample households reports some form of borrowing, and 7.59% reports borrowing from a bank. 1.68% of observations have borrowings from the NBFC, i.e., Non-Bank Finance Companies. These are non-bank institutions in the formal lending market. 20.82% of borrowers have debt outstanding from informal sources. These are borrowings from sources such as money lenders, family and friends, employers, or local businesses. Therefore, a relatively smaller fraction of borrowers borrow from formal financial

[12] This data is available at a frequency of once every four months, unlike the income data that is available for every month. Hence, on the quarterly basis number of observations (approximately 3.6 millions) with credit outcome is approximately 3/4th that of income variables (approximately 4.9 million).

institutions in the sample. In our empirical analysis, we analyze whether digital payments alter the level as well as the source of borrowings. Our database also has information on the purpose of borrowing. 3.50% of observations has some form of outstanding borrowing for businesses purposes.

Finally, the Table provides the breakdown of occupation across households: entrepreneurs (25.28%), farmers (13.10%), and salaried employees (21.32%). Within the category of entrepreneurs, 3.29% identify themselves as hawker and small traders. These households run businesses such as fruit stalls and street food out of push-carts, roadside stalls, or other similar arrangements. Compared to more established entrepreneurs, they lack formal collateral in the form of a brick-and-mortar shop. They are likely to be relatively more credit constrained and they are likely to face a higher transaction cost friction in accepting payments from their customers in the pre-digital payment economy. Other occupation categories include retirees, unemployed, social workers, wage earners, laborers, and miscellaneous.

5 Empirical Strategy & Results

5.1 Baseline Results

We begin our study by estimating the following panel regression model at the household-quarter level:

$$y_{idt} = h_i + yq_t + u_i \times yq_t + \beta \times log(digital)_{d,t-1} + \epsilon_{idt} \tag{1}$$

y_{idt} measures economic outcomes of household i in district d in quarter t. We focus on two key economic outcomes: (a) total income of the household that captures their income from both business activities and labor, and (b) business creation and business income of the household. Total income is measured as the log of quarterly household income after excluding government transfers. We have two different approaches to capture the creation of businesses and income derived from business activities. In the first approach we consider a household as a business owner if they report positive income from business activities, whereas in the second approach we classify them as a business owner if they self-report their occupation as an entrepreneur.

The key explanatory variable is the log of the digital payments volume in the district of the household in quarter $t-1$. Our model include household (h_i) and year-quarter (yq_t) fixed effects. Hence we exploit the within household variation in these outcomes after soaking away the effect of aggregate economic shocks during the year-quarter. The inclusion of household fixed effects obviates the need for the inclusion of district fixed effects.[13] We also include a set of fixed effects, $u_i \times yq_t$, by interacting whether the household lives in an urban or rural area with the year-quarter dummies. We do so to account for time varying differences in government welfare schemes or COVID-19 shock that can potentially have different implications for these households over time in rural versus urban areas of the country.[14] All standard errors are clustered at the district-quarter level since our key explanatory variable, digital payments, vary at this level of aggregation. Our results are statistically stronger if we cluster the standard errors at the household level.

The digital payments data starts in the first quarter of 2018. Therefore, we estimate the above regression model with economic outcomes measured from the second quarter of 2018 to the second quarter of 2022, as we need the first quarter of data for the construction of the lagged value of digital payments. Our empirical setting estimates the effect of digital payment on next quarter's economic outcomes. Therefore, our model does not suffer from any bias that my arise due to unobserved shocks to a district that can simultaneously increase both digital transactions and economic activity. The real threat to our identification strategy in this model comes from any forward-looking, i.e., an increase in digital transaction in a

[13]Further, due to the inclusion of granular household fixed effects that is a unit within the district, our estimates are identical if we use population adjusted measure of digital payments since our population data does not vary over time for a district.

[14]Our results remain similar without the inclusion of $u_i \times yq_t$.

district in period t in anticipation of improved economic outcomes in period $t+1$. We first present the result of this model to establish some baseline results before discussing alternative identification strategies that we employ later in the paper.

Table 2: Cashless Payments and Outcomes: Panel Data

Table 2 presents the regression estimate of equation 1. The model is estimated with district-quarter level observations. The dependent variable is the log of income in Column (1), whether the household reports positive business income or not in Column (2), and business income in Column (3). The dependent variable in Column (4) measures business ownership based on the occupation of the household, i.e., whether they self-report themselves as entrepreneurs or not. Column (5) uses the log of income as the dependent variable and presents the results only for the sub-sample of self-reported entrepreneurs. Lagged Cashless Payment measures the log of the amount of cashless transaction in the previous quarter. All standard errors are clustered at the district-quarter level.

	(1) Income	(2) Bus(Y/N)	(3) Bus Inc	(4) Entr(Y/N)	(5) Income
Lagged Cashless Payments	0.0868***	0.0122**	0.1688***	0.0299***	0.0943***
	(0.0112)	(0.0049)	(0.0540)	(0.0030)	(0.0105)
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year-Qtr Fixed Effects	Yes	Yes	Yes	Yes	Yes
Urban x Year-Qtr Fixed Effects	Yes	Yes	Yes	Yes	Yes
Sample	All	All	All	All	Entrepreneurs
Nobs	2,209,164	2,209,164	2,209,164	2,209,164	592,443
Adjusted R-squared	0.558	0.499	0.511	0.494	0.662

standard error in parentheses

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

Table 2 presents the regression results. Column (1) provides an elasticity estimate of 0.0868 for the effect of digital transaction on income. The estimate is statistically significant at the 1% level. The estimate is economically important as well: a doubling of digital payment is associated with 6.2% higher income based on these estimates. The interpretation of the economic magnitude has an important caveat for external validity: our estimates come from a transition period as an economy moves from a cash based system to a cashless payment system. In the steady state, the effects are likely to be smaller.

Column (2) presents the estimation results for the ownership of business as the dependent variable based on whether the household reports positive business income or not. The model estimates the effect of digital payment on the extensive margin of business activities. We find a statistically significant coefficient of 0.0122 on the lagged cashless payment variable. Based on these estimate, a doubling of cashless intensity translates into approximately 0.84% higher business ownership in the area. The unconditional mean of business ownership is 16.55% in our sample. Thus the economic effect is reasonably large: an increase of about 5% of the sample mean with a doubling of the digital payment volume.

Column (3) presents the results on the effect of digital payments on the intensive margin of business income. Since business income, by definition, is only reported for households who own a business, majority of outcomes have zero value for the dependent variable. We use the methodology suggested by Chen and Roth (2023) to focus on the estimates of extensive margin while working with observations with a lot of zero values. Specifically, we transform the business income variable Y into $m(Y)$ that is defined as $log(Y/Y_{min})$ for all $Y > 0$ and $m(Y) = 0$ for all $Y = 0$. Y_{min} is the minimum value of business income for all positive values of the observations. Thus, the Chen-Roth transformation sets the value of zero business income households to those of the minimum positive income, allowing us to focus on the extensive margin estimates.[15] We find a significant increase in business income for households who reside in districts with higher volume of digital payments. The estimated coefficients approximately translates into an economic magnitude of 0.16% higher business income for every 1% increase in digital transaction.

Column (4) documents a statistically significant impact of digital payments on business creation based on the self-reported occupation of the household. The estimated coefficient of 0.0299 means that a doubling of cashless payments translates into approximately 2% higher business ownership. As expected, the estimate based on self-reported occupation suggests a higher impact of cashless payments on business creation compared to the stricter definition based on positive business income that we used earlier in the analysis. Finally, Column (5)

[15] Notably, the transformation avoids the potential pitfall of an alternative empirical strategy of using log(1+Y) where the estimates can be scale dependent. Our base case result, however, do not change if we use log of one plus business income as the dependent variable.

of the Table limits the sample to only the self-reported entrepreneurs and uses the log of their total income as the dependent variable. A key advantage of this specification is that we are able to capture the income of entrepreneurs even if they combine it with their regular income, instead of reporting it under business income. We find an elasticity of 0.0943 for this subsample, which translates into a 6.7% higher income for households that reside in a district with double the intensity of cashless payments.

Overall, these findings establish our baseline results: digital payment in a district is related to the next period's economic outcomes. A key question is whether these effects are causal in nature or do they simply reflect the endogenous nature of their relationship? Our baseline empirical model is less susceptible to endogeneity concerns that arise from unobserved shocks that affect both the adoption of digital payment and economic activities in a district at the same time since we use the lagged value of digital payment as the explanatory variable. The key threat to our identification come from the possibility of a forward looking expectation of growth and the adoption of digital payments. In order to establish a more concrete causal link between the variables, we use two complementary identification strategies, one exploits the difference in the timing of UPI participation by banks, and the other exploits within-district-year variation in outcomes across households. These empirical designs complement each other and they provide some novel insights into the underlying economic mechanism as well.

5.2 Identification using differential timing of UPI participation

In two waves of nationalization in years 1969 and 1980, all the large private sector banks of the country were nationalized by the government of India. Soon after the first wave of nationalization, the government launched a system of "lead banks" in the country based on the recommendation of Gadgil committee formed by the Reserve Bank of India.[16] Under this

[16] See a brief history of this system at the Reserve Bank of India's website: https://m.rbi.org.in/scripts/PublicationDraftReports.aspx?ID=552

system, one of the government owned banks (also called the PSU Banks, PSBs, or nationalized banks) was designated as the lead bank for each district. The lead bank had the primary responsibility for expanding access to banking and credit to underserved communities in the district. While there have been several tweaks to the roles and responsibilities of the lead banks since its inception, the basic structure remains intact. Every district in the country has a lead bank responsible for carrying out several government-led credit and banking schemes, and expanding access to banking services in general. Due to a long history of this system in the country, lead banks still have significant presence in their districts in terms of branch network and customer base, especially in the rural and semi-urban areas.

Table A1 in the Appendix of the paper formally establishes this assertion. We use the number of branches of a bank in a district as the dependent variable and whether the bank is the lead bank or not as the explanatory variable in the regression model. We include district fixed effects in the model; hence the estimates provide us with the additional number of branches of the lead bank compared to other banks in the same district. As shown in Column (1) of the Table, lead banks have about 30 more branches in a district compared to all other banks of the country.[17] We obtain similar results for alternative empirical specifications, such as a Poisson regression or a regression with log transformed values of the dependent variable.

Interestingly for our identification purposes, banks differed in the timing of their participation on the UPI platform, as shown earlier in Figure 1. A customer needs her bank to participate on the UPI platform before she can link it with an UPI App on her mobile phone. Therefore, customers of banks that participated earlier in the program have earlier access to cashless payment infrastructure, providing us with a reasonably exogenous variation in the adoption of cashless payments in the early years of the UPI's launch. Early adoption of a product can have a long lasting impact on wider adoption if it comes with positive network externality, i.e., complementarity in usage across consumers. Consistent with this view of

[17]We limit our attention of PSBs and the Private Sector Banks only for this analysis, leaving rural and cooperative banks out. We do so because these institutions represent only a small fraction of banking activities in India compared to the PSBs and the Private Sector Banks.

technology adoption, Crouzet et al. (2023) show that the use of digital wallet adoption in an area increased persistently in response to demonetization shock in India.

At the time of the UPI's launch, 21 banks participated in the program. Some of these banks were part of a pilot program that the RBI conducted before the launch of the UPI. A handful of other banks, 8 of them to be precise, had indicated their desire to join the platform at the time of launch, but delayed the joining by a few months to sort out some technical glitches. Within the next few months all of these banks joined the platform and the number of participating banks increased to 30 by the end of November, 2016. Within the set of public sector banks, seven banks were notably missing from this list: Indian Bank, Indian Overseas Bank, Bank of India, Syndicate Bank, Corporation Bank, Punjab & Sind Bank, and Dena Bank. All other public sector banks that had lead bank responsibilities anywhere in the country were the early participants.[18] Banks that joined the UPI platform in the earlier phase (i.e., by November 2016) are defined as the "early adopter" banks, whereas the remaining ones are defined as "late adopters". We refer to them as simply "early" and "late" banks or districts in the paper for expositional simplicity. We compare households who reside in early versus late districts to obtain a causal link from digital payments to economic outcomes in a difference-in-differences setting.

The bank's decision to join the UPI platform is reasonably exogenous to the household's hidden characteristics, i.e., our unit of analysis, and the hidden investment opportunity set of their districts. Mishra, Prabhala, and Rajan (2022) document significant stickiness in technological adoption by the Indian public sector banks, i.e., for our sample of banks. They argue that "stickiness of past bank structures and managerial practices" are key impediments to the adoption of new technologies in these banks, lending credence to our argument that the difference in the timing of participation across banks is unlikely to be driven by unobserved time-varying economic potential of the districts in which they are the lead banks.

[18]These banks are: Andhra Bank, Bank of Maharashtra, Canara Bank, Punjab National Bank, United Bank of India, UCO Bank, Union Bank of India, Vijaya Bank, Oriental Bank of Commerce, Allahabad Bank, State Bank of India, Bank of Baroda, and Central Bank of India.

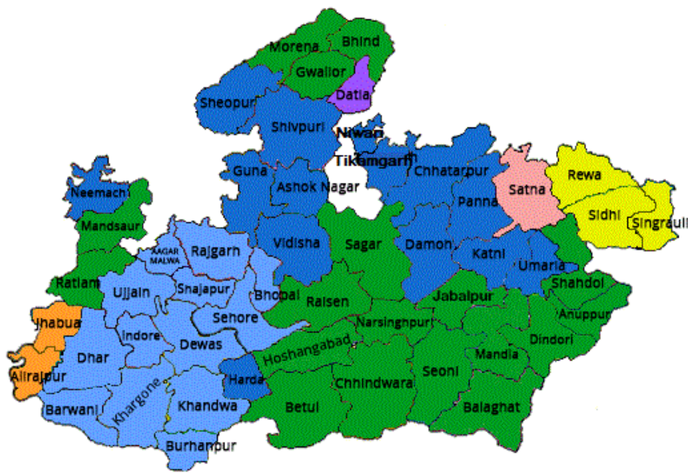
Early banks joined the platform between August and November, 2016, whereas their late adopter counterparts did so between December, 2016 and May, 2017. The customers of the early banks, therefore, had access to the UPI platform anywhere between one to nine months ahead of those of the late banks. Given the network externality involved in the adoption of a novel payment system, even a small delay in the starting point can lead to large differences in the adoption rate in the immediate aftermath of the launch. But the month of November 2016 has a special significance in the history of India's macroeconomy. It was during this month that the government launched a nationwide demonetization program, where high denomination currency notes were withdrawn from the circulation. As a result, the benefit of digital transactions went up in a disproportionate manner. While the customers of the early districts could switch to digital methods of payments immediately, those in the late districts had to face delay. All these features of our setting make the timing of treatment reasonably exogenous for our analysis.

Since the lead bank assignment happened decades before the UPI shock and since we have a number of districts in the country that are otherwise identical on various socio-economic dimensions, our empirical setting is attractive. We illustrate the implementation of this empirical design with an example of the state of Madhya Pradesh (MP) in Figure 2, obtained from the State Level Bankers' Committee (SLBC) of the state. Districts with different colors have different lead banks assigned to them. For example, the district of Betul, colored in green, has Central Bank of India as its lead bank, whereas Burhampur, colored light blue in the map, is led by Bank of India. Central Bank of India was an early adopter of the UPI platform, whereas Bank of India joined it later.

Betul and Burhampur are very similar districts in terms of their economic opportunities due to their geographical proximity and the fact that they are in the same state. They are also similar in terms of measures of financial development (69 vs. 68 bank branches per million people for Betul and Burhanpur, respectively) and literacy rate (69% vs. 64%). Yet, Betul has a significantly higher level of digital transactions on a per person basis compared to

Burhanpur, as shown in Figure 3. In the first quarter of 2018, Betul (the early district) has ₹66 of digital transaction per person compared to Burhanpur's ₹39. By 2022, the two districts have ₹4760 and ₹4460 of digital payments per person, repectively. The initial difference persisted over time, consistent with the idea of a long-lasting effect of the early adoption of a product with strong network externality in usage. Our empirical strategy compares households residing in these districts, before and after the launch of UPI.

Figure 2: Lead Banks in MP

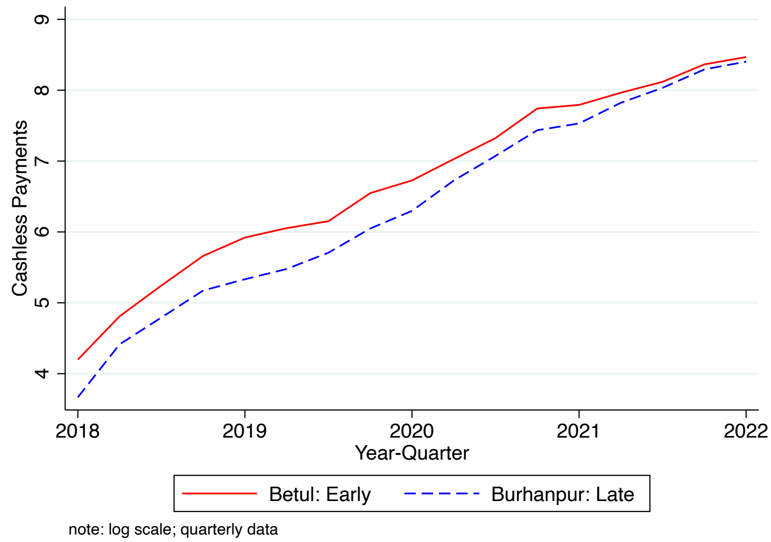


Source: Reproduced from http://www.slbcmadhyapradesh.in/lead-banks.aspx

As mentioned above, the lead bank role is assigned only to the government owned banks. Private banks, such as the ICICI Bank, also have branch networks across the country but they do not act as lead banks. Our identifying strategy exploits the variation in UPI adoption due to the incremental role played by the government owned lead banks. However, to avoid any potential bias due to differences in the presence of other banking institutions across districts with early versus late adopter banks, we conduct our matched sample analysis across districts with similar level of overall bank branch penetration based on the per capita number of brick-and-mortar bank branches in 2016.

With these institutional details and idea behind our identification strategy, we conduct a standard difference-in-differences test comparing households who reside in an early adopter

Figure 3: Case Study of Two Districts in MP



The figure plots the log value of the total amount of digital transaction in Rupees scaled by the population of the district.

district with households who reside in a comparable late district on a matched sample. We have 404 unique districts in the sample that have coverage both on the CMIE database as of 2016, the year of our matching criteria, and the identity of the lead district. For our matched sample analysis, we begin with the sample of all the late districts (92 districts in the sample) and find a comparable early district in the same state. We match within a state to ensure that our results are not driven by difference in state-specific policies, either in terms of the incentives provided to adopt the digital modes of payments or in terms of other economic policies. Further, we require the two districts to be comparable in terms of per capita bank branches before the launch of the UPI, the literacy rate, and population. We match on the level of brick-and-mortar branches to control for the supply of financial services. Matching on literacy rate ensures that our results are not driven by any difference in the the customer's knowledge and willingness to use an electronic medium of payment.

The banking sector in India went through a significant wave of consolidation in 2019-2020, where relatively weaker public sector banks were acquired by the stronger ones. A potential concern in teasing out the effect of digital payments on economic outcome using the timing of the lead bank participation as a source of variation is that the acquired banks may not be focussed on providing access to digital platform to their customers in the same manner as the relatively stronger surviving banks. The banks that were acquired are: Syndicate Bank, Oriental Bank of Commerce, United Bank of India, Allahabad Bank, Andhra Bank, Corporation Bank, Vijaya Bank and Dena Bank.[19] Some of them were early adopters, whereas some late. Therefore, we have variation within the set of acquired banks across early and late adopters. In our final matching criteria, we ensure that we match districts within the set of whether their lead bank was acquired or not, allowing us to separate out the effect of differential focus of these banks.

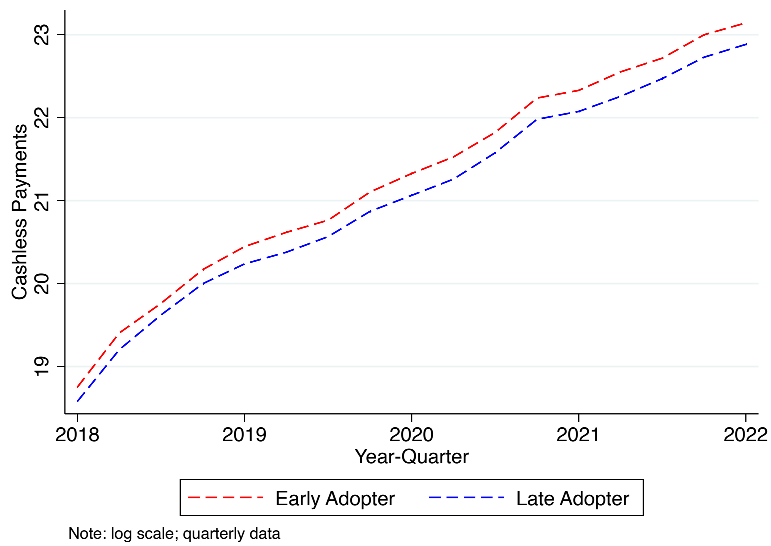
We require the matched districts to be in the same state and then within 50% of the standard deviation of each of the three quantitative dimensions of matching. Within the set of all early districts that meet these criteria for a late district, we pick up to three early districts with closest value of per capital bank branch penetration. At the end of the process, we collect all the unique late and early adopter districts as our matched sample. In some cases, an early adopter district gets matched with more than one late adopter districts; therefore, the unique number of districts that enter our sample is smaller than a matching algorithm without replacement. Our results are not sensitive to these choices. For example, our results remain similar if we match with or without replacement or whether we change the matching criteria by changing the bandwidth within which we find the matched districts.

We obtain a sample of 49 early and 42 late adopter districts in the matched sample, spread across 10 large states in the country. Figure A2 plots the kernel densities of bank branch penetration, literacy rate, population and income across the early and late adopter districts. As expected the two densities have a nearly identical shape for the three dimensions that we match on: bank branch penetration, literacy rates, and population. At the same time, they are also well balanced on income as measured in 2016. Figure 4 shows the average value of digital transactions across the early and late districts over time. The early districts started

[19] In addition, various affiliates of the State Bank of India were also consolidated into the parent company.

at a higher level of digital payments soon after the launch of the UPI, and the difference persisted over the entire sample period. The difference in digital payments across the two group is about 15-25% in a given quarter. This finding is consistent with a model of product diffusion with network externality, where the effect of initial condition can have a long-lasting impact as in Crouzet et al. (2023). In addition, Higgins (2020) documents evidence in support of network externality across merchants and consumers in the adoption of debit cards in Mexico.

Figure 4: Digital Payments: Early vs. Late District



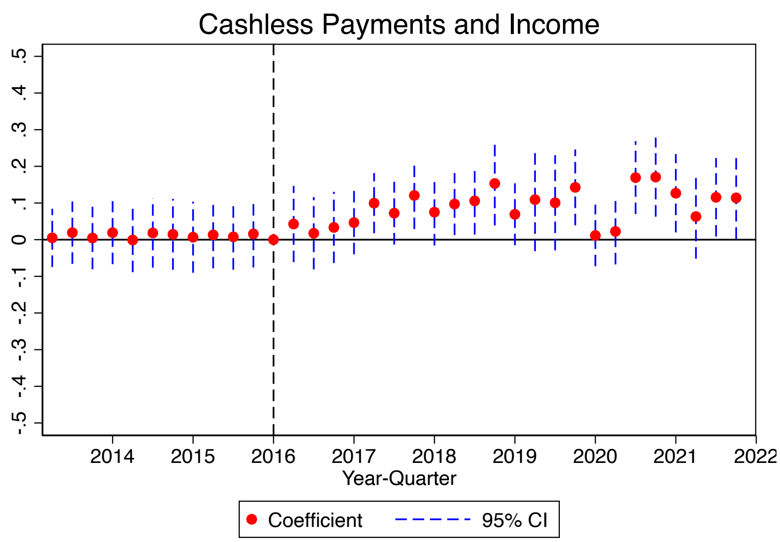
The figure plots the log value of the total amount of digital transaction in Rupees for the early and late districts.

After establishing the importance of the lead bank's adoption of the UPI platform on the level of cashless payments in the district, we estimate the following model:

$$y_{idst} = h_i + yq_t + u_i \times yq_t + s \times yq_t + demo_d \times yq_t + \sum_{\tau}(yq = \tau) \times \beta_\tau \times early_d + \epsilon_{idst} \quad (2)$$

y_{idst} are the outcome as defined earlier for household i in district d in state s at time t; h_i stands for household fixed effects; yq_t is the year-quarter fixed effects. We include fixed effects for the interaction of the year-quarter dummies with state s of the household, the extent of demonetization shock experienced by their district ($demo_d$), and whether they live in urban or rural areas (u_i). The demonetization shock variable comes from Chodorow-Reich et al. (2020), who sort districts on a scale of 1 to 7 based on the intensity of cash shortage in November, 2016. We include the interaction of the demonetization shock levels with year-quarter fixed effect to separate out the differential effects of demonetization across these districts. The coefficients β_τ provide the estimate of the difference in outcome for households in early versus late districts over time. We focus on our primary measure, the income earned by the household for this analysis. Estimation results are provided in Figure 5.

Figure 5: Effect of Digital Payments on Household Income



As shown in the figure, prior to the adoption of the UPI in 2016, households in early versus late districts have a parallel trend in their income. The coefficient becomes positive and significant in the early districts approximately four to five quarters after the launch. The delayed response of cashless payment on real outcomes is expected for two primary reasons. First, there is likely to be a delay in the adoption of digital payments and creation of new entrepreneurial activities due to learning and set-up time. Second, higher access to credit that can potentially come as a result of digital footprint is likely to take some time. By 2018, households in the early adopter districts have almost 10% higher income. The difference between the two groups persist over time, albeit with higher volatility in later periods. Specifically, our coefficients are weaker during the quarters for which the CPHS data collection period coincided with the second COVID wave in India in early 2021. Difficulty in data collection as well as a complete lockdown of economic activities that affected street vendors could be potential explanations behind this finding.

Our main interest lies in estimating the average effect of digital payments across these districts in the post-UPI period compared to before. We estimate the following model with observations from all district-quarters in the sample:

$$y_{idst} = h_i + yq_t + u_i \times yq_t + s_d \times yq_t + demo_d \times yq_t + \beta \times post_t \times early_d + \epsilon_{idst} \tag{3}$$

$post_t$ equals one for observations after 2016, and zero otherwise. The estimation results are provided in Table 3. Model 1 of the Table shows that households in the early adopter districts have significantly higher income of 7.85% in the post period. These households have 1.03% higher business ownership rate based on whether they have positive business income or not, and the level of their business income is approximately 13% higher. Based on the occupational definition of business ownership, early districts have 2.43% higher level of entrepreneurs compared to the late districts, and their overall income is about 5% higher. All these estimates are economically large and statistically significant.

Finally, we estimate a two stage least squares regression model with the interaction of post-2016 dummy variable and early district as an instrument for the adoption of digital payments to obtain an estimate of the effect of digital payment on outcomes in an instrumental variable framework. Since all districts have precisely zero value of digital payments in the pre-UPI period, we take a log transform of the cashless payments using the Chen and Roth (2023) methodology for each district-quarter observation. We exclude observations from

Table 3: Cashless Payments and Outcomes: Early vs. Late Adopters

Table 3 presents the regression estimate of the regression model in equation 3. The model is estimated with household-quarter level observations from 2014-2022. The dependent variable is the log of income in Column (1), whether the household reports positive business income or not in Column (2), and business income in Column (3). The dependent variable in Column (4) measures business ownership based on the occupation of the household, i.e., whether they self-report themselves as entrepreneurs or not. Column (5) uses the log of income as the dependent variable and presents the results only for the sub-sample of self-reported entrepreneurs. Lagged Cashless Payment measures the log of the amount of cashless transaction in the previous quarter. All standard errors are clustered at the district-quarter level.

	(1)	(2)	(3)	(4)	(5)
	Income	Bus(Y/N)	Bus Inc	Entr(Y/N)	Income
Early x Post	0.0785***	0.0103*	0.1265**	0.0243***	0.0497***
	(0.0141)	(0.0055)	(0.0544)	(0.0035)	(0.0152)
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes
State x Yr-Qtr Fixed Effects	Yes	Yes	Yes	Yes	Yes
Urban x Yr-Qtr Fixed Effects	Yes	Yes	Yes	Yes	Yes
Demonetization x Yr-Qtr Fixed Effects	Yes	Yes	Yes	Yes	Yes
Sample	All	All	All	All	Entrepreneurs
Nobs	936,842	936,842	936,842	936,842	219,788
Adjusted R-squared	0.506	0.353	0.370	0.407	0.637

standard error in parentheses

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

2017 from this model since we do not have data on digital payments during this year, even though these quarters are from post-2016 period. Estimation results are provided in Table 4. Column (1) presents the estimate of the first-stage regression result. Consistent with the results so far, we obtain an economically meaningful and statistically significant coefficient on the instrument, $early \times post$. The t-statistics of the instrument is 8.11, indicating a strong instrument. The adjusted R^2 of the model is high both due to the strength of the instrument and the inclusion of year-quarter fixed effects that captures a large part of variation in digital payment intensity. Columns (2)-(4) present the second stage regression result with the instrumented values of cashless payment as the explanatory variable. All our coefficients are statistically significant at 1% level.

Table 4: Early vs. Late Adopters: 2SLS Regression

Table 4 presents the regression estimate of the two-stage least squares regression model. The dependent variable in the first stage regression is the log of one plus cashless payment in the district. The instrument is the interaction of early district with a dummy variable *post* that takes a value of one for quarters after 2016, and zero otherwise. Columns (2)-(4) use the instrumented value of cashless payment as the explanatory variable. The dependent variables are: log of total income, whether a household owns a business or not, and the log of one plus business income in Columns (2), (3), and (4), respectively. The model is estimated with household-quarter level observations from 2014-2022. All standard errors are clustered at the district-quarter level.

	(1) Cashless Payment	(2) Income	(3) Owns Business	(4) Business Income
Early x Post	0.3243***			
	(0.0400)			
Digital Payments		0.2763***	0.0455**	0.5451***
		(0.0637)	(0.0194)	(0.1963)
Household FE	Yes	Yes	Yes	Yes
State x Year-Quarter FE	Yes	Yes	Yes	Yes
Urban x Year-Quarter FE	Yes	Yes	Yes	Yes
Demon. x Year-Quarter FE	Yes	Yes	Yes	Yes
Nobs	748,641	748,641	748,641	748,641
Adjusted R-squared	0.987	.	.	.

standard error in parentheses

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

5.2.1 Intensity of Early Adoption

In our analysis so far, we focus on the variation in early adoption of the digital payment system that comes from the status of the lead bank of the district. We do so to obtain a potentially exogenous variation that is unlikely to be contaminated by the endogenous entry of a bank in a district. However, we miss out on the variation that comes from the early adoption of the UPI platform by the non-lead banks, both private and public sector ones. In the earlier analysis, we match district based on the number of bank branches per million population to account for this effect. In our next empirical specification, we extend our analysis by exploiting the variation in the intensity of bank branches that were linked to the UPI platform during the early periods. An additional advantage of this specification is that we are able to estimate the model for every district in the country that enter our sample, and not just the matched sample.

We compute the fraction of all UPI-enabled branches in a district as a measure of the intensity of the early adoption. To be precise, we count the number of branches of all the early adopter banks in a district and divide it by the total number of branches in a district to obtain our explanatory variable called the *Early Adopter Fraction*. There is wide variation in this measure across different districts of the country as shown in Figure A3 in the Appendix. Districts with higher intensity of early adopter banks have significantly higher level of digital payments per person as shown in Figure A4 in the Appendix. For this figure, we estimate a cross-sectional regression with per capital digital payment as the dependent variable and the *Early Adopter Fraction* as the explanatory variable and report the coefficient estimate and 95% confidence interval in the Figure. Therefore, early adoption of the UPI platform matters for the long run adoption rate, consistent with our earlier results where we solely focused on the lead bank status.

We estimate the difference-in-differences regression model of equation 3 by replacing *lead* with *Early Adopter Fraction* as the key explanatory variable. Results are provided in Table 5.

Higher early adoption rate is associated with significantly higher income, business creation, and business income. Since the explanatory variable is the fraction of early adopter branches, the estimated coefficients represent the effect of moving from a district with no branches with digital payment access to a district with 100% access. In terms of sample variation, these estimates show that one standard deviation higher fraction (0.12) of early adopters is associated with approximately 2% higher income, 3% higher business ownership, and 33% higher business income.

Table 5: Cashless Payments and Outcomes: Intensity of Early Adopter Banks

Table 5 presents the regression estimate of the regression model in equation 3 with *Early Adopter Fraction* as the key explanatory variable instead of *lead*. The model is estimated with household-quarter level observations from 2014-2022. The dependent variable is the log of income in Column (1), whether the household reports positive business income or not in Column (2), and business income in Column (3). Lagged Cashless Payment measures the log of the amount of cashless transaction in the previous quarter. All standard errors are clustered at the district-quarter level.

	(1) Income	(2) Owns Business	(3) Business Income
Early Adopter Fraction x Post 2016	0.1680***	0.2479***	2.7843***
	(0.0341)	(0.0182)	(0.1961)
Household Fixed Effects	Yes	Yes	Yes
Urban x Year-Quarter Fixed Effects	Yes	Yes	Yes
Demonetization x Year-Quarter Fixed Effects	Yes	Yes	Yes
Sample	All	All	All
Nobs	4,323,660	4,323,660	4,323,660
Adjusted R-squared	0.536	0.373	0.387

standard error in parentheses

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

Overall, our results establish a causal link between cashless payment and economic outcome under the assumption of exogeneity of the timing of the adoption of UPI by the lead bank of the district. Further, our results based on the intensity of digital payment adoption by bank branches in a district lend support to the claim that as the access of digital payments expanded so did the improvement in economic outcomes.

5.3 Identification using variation across occupation

We now exploit the variation in outcomes across different types of households within the same district and same year-quarter to identify our effect using a different empirical strategy that also allows us to make some progress on the economic channels underlying our key results. Motivated by economic frictions that connect digital payments to economic outcomes, our identification strategy in this section rests on the assumption that the benefit of digital payments accrues at a disproportionately higher rate to self-employed households (such as shop-keepers, hawkers, and other entrepreneurs) compared to salaried households at any given point in time.

We argue that digital payments benefit self-employed households at a higher rate on account of both the key economic channels we have in mind: (a) lower transaction costs help them with higher volume of business transactions, and (b) better information availability via digital transactions improves their access to external financing. Our empirical setting is especially powerful because these households often have very limited access to financing from traditional institutions. On the other hand, in recent years there has been significant growth in Fintech companies that use information contained in digital payments to lend to these small borrowers. FinTech companies use a variety of tools of expand access to credit for such households. Our discussion with some of the industry leaders suggest at least three such potential channels: (a) improvement in information availability due to digital footprints, (b) the ability to tailor a borrower's repayment schedule based on the pattern of their cashflows, and (c) enhanced ability to collect the repayments. For examples, some FinTech lenders are able to obtain their repayments from small shopkeepers by directly accessing their payments through the digital platform. In addition, some small business owners prefer a tailored repayment contract. Collectively, these channels improve a borrower's access to financing, which in turn with their ability to start or expand their business. And these economic forces are likely to be stronger for self-employed households, and especially for marginal businesses that have limited availability of collateral in the form or building, land and machine.

We estimate the following regression model with the inclusion of district-year-quarter fixed effects that soak away time-varying unobserved shocks across districts:

$$y_{idt} = h_i + dyq_{dt} + \beta \times self_{i,pre} + \theta \times self_{i,pre} \times log(digital)_{d,t-1} + \epsilon_{idt} \tag{4}$$

y_{idt} measures the log income of household i in district d in year t. dyq_{dt} are district-year-quarter fixed effects. The inclusion of district-year-quarter fixed effects means that we identify our effect through the variation in the effect of digital payments across occupation categories, and our results cannot be explained away by time varying district characteristics. $self_{i,pre}$ measures whether the household is self-employed or not in the "pre" period, i.e., before we measure economic outcomes. Specifically, we classify households under different categories based on their occupation status as of 2016, 2017, and 2018, i.e., before we measure the economic outcomes. A household gets classified under an occupation category if it is in the same category for at least 2 of these 3 years. We consider the following categories of occupation in the CPHS database as self-employed: Entrepreneurs, Self-employed Entrepreneurs, Self-employed Professionals, and Small Traders/Hawkers. We label them all as "Entrepreneurs" for expositional convenience. We compare their outcomes with households who are salaried employees and workers. The comparison groups cover both the government employees and private sector employees, for example a salaried employee at a doctor's office or local business falls under this category. We estimate the model on the same sample as our base case analysis with equation 1, i.e., on the sample from 2018-2022 for which the digital payment data is available at the district-quarter level.

Results are presented in Table 6. Column (1) considers all the entrepreneurs as self-employed and compares them with salaried households. The elasticity of income to digital payments is higher by 0.0507 for the self-employed group compared to salaried households in this specification. Thus, when digital payments increase in a district, it leads to higher

Table 6: Effects For Self-Employed Households

Table 6 presents the regression estimate of the regression model in equation 4. The model is estimated with household-year-quarter level observations. The dependent variable is the log of average monthly income of a household in a given year-quarter. Cashless Payment measures the log of the amount of cashless transaction in the previous year-quarter. All standard errors are clustered at the district-quarter level.

	(1)	(2)	(3)	(4)
	Income	Income	Income	Income
Self-Employed X Lagged Cashless Payments	0.0507^{***}	0.0700^{***}	0.0538^{***}	0.0208^{***}
	(0.0015)	(0.0027)	(0.0065)	(0.0033)
Household Fixed Effects	Yes	Yes	Yes	Yes
District-Year-Qtr Fixed Effects	Yes	Yes	Yes	Yes
Self-Employed Group	Entr.	Hawkers	Farmers	Hawkers
Comparison Group	Salaried	Salaried	Salaried	Other Entr.
Nobs	866,453	449,597	666,029	476,370
Adjusted R-squared	0.678	0.690	0.599	0.655

standard error in parentheses
* $p < .10$, ** $p < .05$, *** $p < .01$

income for self-employed households compared to all the salaried households in the same district after accounting for unobserved district-year-quarter shocks.

In Column (2), we compare a subset of entrepreneurs who are more likely to benefit from digital payments, namely the hawkers, to the salaried households. As discussed earlier, this sub-category of entrepreneurs are relatively poor and their business establishment is typically "unbankable". Often their business is run out of a fruit stall or a temporary location, with very limited availability of collateral. The effect of digital payments is especially higher for this sub-group: an increase in elasticity of 0.07. In Column (3), we consider a different occupation category as self-employed: the farmers. The group of farmers are also likely to benefit more from the relaxation of credit constraints and transaction costs as is the case with the entrepreneurs. Our results confirm this hypothesis.

Finally, in Column (4) of the Table, we compare hawkers to other entrepreneurs. Our motivation behind this specification is two-fold. First, it allows us to exploit the variation in outcome across relatively marginal versus more established entrepreneurs. Second, one may be concerned that our estimates simply capture the "reporting" effect and not real economic effects. To be precise, if digital payments deter tax avoidance, then a move towards cashless society can result in higher reported income even if the actual income has not changed. Since salaried households are less likely to be affected by this channel, comparing them with entrepreneurs can lead to a bias due to tax avoidance incentives. When we compare income across hawkers and more established entrepreneurs, such a bias is likely to disappear for two reasons. First, both these group of households generate income from their own business. Second, the tax avoidance incentive should be lower for marginal entrepreneurs since they earn significantly lower income on average, compared to other entrepreneurs. In our sample, hawkers have about 35% lower annual income than other entrepreneurs. With an average annual income of less than ₹200,000, majority of hawkers in our sample fall under the tax exemption limit as per the Indian tax rule. Hence, they have little incentive to hide income in a systematic manner. Therefore, comparing hawkers with other entrepreneurs provides a setting when benefits of digital payments are higher for the hawkers but the tax avoidance incentives lower. Our results show that the income increased significantly more for hawkers compared to the other entrepreneurs.

5.3.1 Economic Channels

Our results showing increased benefit of digital payments to entrepreneurs and specially to marginal entrepreneur is consistent with the economic channels of lower transaction costs and credit market frictions we have in mind. We now present some additional analysis in support of these channels. The benefit of a mobile-based digital payment system should be especially high in districts where physical bank branches are scarce. In these areas, both the transaction costs of payments and the borrowing frictions are likely to be higher. We sort districts into percentiles based on the number of bank branches on a per capita basis as of 2016, and create a variable *Lower Fin Dev* that measures one minus the percentile ranking. In other words, *Lower Fin Dev* measures lower financial development on the dimension of traditional banking before the launch of UPI. We estimate the following regression model:

$$y_{i,t} = h_i + yq_t + u_i \times yq_t + \beta \times log(digital)_{i,t-1} + \gamma log(digital)_{i,t-1} \times LowFinDev_i + \epsilon_{i,t} \quad (5)$$

The coefficient on the interaction term, γ, measures the incremental effect of digital payments on outcomes in districts with relatively lower financial development before the launch of the UPI platform. Table 7 presents the results. Across three measures of economic outcome, we find a positive and significant coefficient on the interaction term. In other words, the impact of digital payment on household income and business activity is higher for financially less developed districts.

Table 7: Effects Across Financial Development

Table 7 presents the regression estimate of the regression model in equation 5. The model is estimated with household-quarter level observations. The dependent variable is log of income in Columns (1), a binary variable indicating whether the household owns a business or not in Columns (2), and the log of one plus average monthly business income in Columns (3). Cashless Payment measures the log of the amount of cashless transaction in the previous quarter. All standard errors are clustered at the district-quarter level.

	(1) Income	(2) Bus(Y/N)	(3) Bus Inc
Lagged Cashless Payment	0.061***	0.005	0.085
	(0.012)	(0.006)	(0.061)
Lagged Cashless Payment x Lower Fin Dev	0.033***	0.016***	0.177***
	(0.007)	(0.003)	(0.038)
Household Fixed Effects	Yes	Yes	Yes
Year-Qtr Fixed Effects	Yes	Yes	Yes
Urban x Year-Qtr Fixed Effects	Yes	Yes	Yes
Sample	All	All	All
Nobs	2,156,732	2,156,732	2,156,732
Adjusted R-squared	0.557	0.502	0.513

standard error in parentheses

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

The level of financial development affects both the transactions cost of payments, for example by increasing the distance between an average household and a bank branch, and the access to credit. In our next test, we investigate whether digital payments alleviate credit constraints of the affected households.

Our database allows us to observe both the source and the use of borrowing. Using the same regression specification as in our base model of equation 1, Column (1) of Table 8 shows that higher cashless payments is associated with higher borrowing for business purposes in the following period. In economic terms, a doubling of digital payments is associated with approximately 0.82% higher borrowing for business purposes. This is an economically meaningful impact since the unconditional mean of "borrowing for business purposes" in our sample is 3.50%.

Columns (2) and (3) focus on the source of borrowings. Households in our sample borrow from multiple sources, including banks, Non-Bank Finance Companies (NBFC), local money lenders, family and friends, and others. In Column (2), we consider borrowings from formal sources, namely banks and NBFCs, and find a significant increase in such borrowings for districts with higher digital payment transactions. A household residing in a district with twice as high the digital payments as another district has a 1% higher probability of borrowing from a formal source such as a bank or an NBFC. In contrast, their borrowings from informal sources declined significantly as shown in Column (3): A household residing in a district with twice as high the digital payments as another district has a 2.11% lower probability of borrowing from such sources. Therefore, our estimates suggest that the increase in borrowing comes mainly from formal sources of debt, and the households change the composition of debt away from informal borrowings towards formal borrowings. Therefore, the relaxation of credit constraints works via both the quantity and the quality of borrowing, under the reasonable assumption that informal sources of borrowings are relatively inefficient compared to borrowing from formal sources.

The variation across occupation categories provides us with an additional useful setting to test whether digital payments alleviates credit market friction for households who face higher

Table 8: Borrowings and Cashless Payments

Table 8 presents the regression estimate of the regression model in equation 1 with credit outcomes as the dependent variables. The model is estimated with household-quarter level observations. The dependent variable is a binary variable indicating whether the household borrowed for business purposes, whether the household has borrowing outstanding from a formal source or whether the borrower has any borrowings from informal sources, as defined by borrowings from sources other than banks and non-bank finance companies (NBFC). Cashless Payment measures the log of the amount of cashless transaction in the previous quarter. All standard errors are clustered at the district-quarter level.

	(1) Business	(2) Formal	(3) Informal
Lagged Cashless Payments	0.0122***	0.0144**	-0.0312**
	(0.0034)	(0.0064)	(0.0148)
Household Fixed Effects	Yes	Yes	Yes
Year-Qtr Fixed Effects	Yes	Yes	Yes
Urban x Year-Qtr Fixed Effects	Yes	Yes	Yes
Nobs	1,433,159	1,433,159	1,433,159
Adjusted R-squared	0.369	0.302	0.324

standard error in parentheses
* $p < .10$, ** $p < .05$, *** $p < .01$

constraints such as hawkers compared to other self-employed households. Hawkers have very little collateral, and therefore they face relatively larger credit constraints. We use the same within-district-year empirical strategy as in the earlier section (section 5.3) and present the estimation results comparing borrowing outcomes for hawkers with those of more established entrepreneurs in Table 9. As shown earlier, we document an increase in borrowing for all households in higher digital payment districts. While we do not find strong evidence of a further increase in borrowing by the hawkers compared to other entrepreneurs (Column (1)), there is a remarkable change in their source of borrowing. As shown in Column (2), their borrowings from formal sources increased significantly with the adoption of digital payments. In contrast, Column (3) shows that their borrowing from informal sources of debt came down significantly after the rise in digital payments. Such a compositional shift can be especially valuable for marginal agents of the economy.

Table 9: Borrowings and Cashless Payments Across Occupation

Table 9 presents the regression estimate of the regression model in equation 4. The model is estimated with household-year-quarter level observations. The dependent variable is a binary variable indicating whether the household borrowed for business purposes, whether the household has borrowing outstanding from a formal source or whether the borrower has any borrowings from informal sources, as defined by borrowings from sources other than banks and non-bank finance companies (NBFC). Cashless Payment measures the log of the amount of cashless transaction in the previous quarter. All standard errors are clustered at the district-quarter level.

	(1)	(2)	(3)
	Business	Formal	Informal
Hawkers X Lagged Cashless Payments	0.0005	0.0078***	-0.0061**
	(0.0018)	(0.0019)	(0.0029)
Household Fixed Effects	Yes	Yes	Yes
District x Year-Qtr Fixed Effects	Yes	Yes	Yes
Nobs	306,818	306,818	306,818
Adjusted R-squared	0.452	0.389	0.473

standard error in parentheses
* $p < .10$, ** $p < .05$, *** $p < .01$

5.4 Additional Analyses

5.5 Night Light Data

A potential concern with our analysis is related to the issue of "reporting bias". If digital payment adoption allows households to keep a better record of their financial transactions, then the quality of their reporting is likely to improve. There are two related issues on this dimension. First, if digital payment simply improves the accuracy of information, then our empirical strategy remains valid. Some households are likely to underestimate their income in the absence of digital information, whereas some others overestimate. The noise creates measurement error without generating any bias in our estimates. The resulting measurement error should make it harder for us to find the results that we document in the paper. The second concern is more critical for us: are households hiding information in the CPHS survey, our data source, in a systematic manner to avoid taxes? Under this scenario, digital payments simply makes it hard to underreport income, and our findings can be attributed to a reduction in hiding behavior, rather than an improvement in economic outcomes.

There are several reasons that our analysis is unlikely to be driven by this effect. First, our information does not come from tax records, rather from a detailed survey by the CMIE. Therefore the underreporting incentive is less severe for our data. Second, we document improvement not only in income, but also on business activity and credit outcomes. These economic measures are less likely to be affected by a desire to avoid taxes. In fact, some form of credit can lower the tax burden. Therefore, underreporting of borrowings is not incentive-compatible with tax avoidance. Third, we show that even within the class of self-employed household, it is the set of hawkers who show higher improvement after the adoption of digital payments. These households' income is typically below the level of tax exemption limit in the country. Therefore, the incentive to hide income is absent. Finally, our results documenting an increase in formal sources of credit by marginal entrepreneurs and a corresponding decrease in informal credit cannot be explained away by the hiding behavior.

Yet, we address this issue more directly by utilizing night light data as a measure of economic activity that captures both formal and informal economic sector, and is free from the survey reporting biases. A disadvantage of the data is that we miss individual specific variation in this dataset as it is aggregated at the district-quarter level. We obtain the district level night light data from the VIIRS nighttime lights dataset of Beyer et al. (2022) and aggregate it at the district-quarter level to construct the measure of economic outcome. We estimate the following regression model to estimate the effect of digital payments on night lights:

$$nl_{dt} = district_d + yq_t + \beta \times log(digital)_{d,t-1} + \epsilon_{dt} \tag{6}$$

Table 10 presents the estimation results. Column (1) of the Table uses district and year-quarter fixed effects and shows a positive elasticity of 0.0307 between cashless payments and night lights. Column (2) controls for the time-varying effect of demonetization shock by including the $Demonetization \times Year - Quarter$ fixed effects in the model and obtain similar results. Columns (3) and (4) include additional fixed effects to soak away the effect of time varying differences in night lights across states, as well as across districts with different levels of financial development, literacy rate, and population. Our results remain the same. In sum, our results relating digital payments to economic activities using the CPHS data is unlikely to be an artifact of reporting biases.

Table 10: Cashless Payments and Night Lights

Table 10 presents the results of a panel regression model with district-quarter level night light data from over the sample period. The dependent variable is the log of night lights intensity in a given district in a quarter as in Beyer et al. (2022). Cashless Payment measures the log of the amount of cashless transaction in the previous quarters. All standard errors are clustered at the district level.

	(1) Night Light	(2) Night Light	(3) Night Light	(4) Night Light
Lagged Cashless Payments	0.0307***	0.0321***	0.0419***	0.0305***
	(0.0086)	(0.0106)	(0.0106)	(0.0098)
District Fixed Effects	Yes	Yes	Yes	Yes
Year-Qtr Fixed Effects	Yes	Yes	Yes	Yes
Demonetization x Year-Qtr Fixed Effects	No	Yes	Yes	Yes
State x Year-Qtr Fixed Effects	No	No	Yes	Yes
FinDev x Year-Qtr Fixed Effects	No	No	No	Yes
Literacy x Year-Qtr Fixed Effects	No	No	No	Yes
Population x Year-Qtr Fixed Effects	No	No	No	Yes
Nobs	7,792	6,912	6,912	6,912
Adjusted R-squared	0.952	0.950	0.973	0.974

standard error in parentheses
* $p < .10$, ** $p < .05$, *** $p < .01$

5.6 RBI Credit

As a supplementary test to the district-level night time light data analysis, we also analyze the level of credit creation in a district in a quarter based on the data provided by the Reserve Bank of India. The database is naturally free from any reporting bias. In addition, the analysis provides further support for the credit constraint channel behind the effect of digital payments on economic outcome. Similar to the analysis with night light data, a disadvantage of the data is that we miss individual specific variation in this dataset as it is aggregated at the district-quarter level. We estimate a panel data regression with district and year-quarter fixed effects, and log of total credit as the dependent variable. The lagged value of the log quarterly cashless payments in the district is the explanatory variable.

Results are provided in Table 11. Column (1) uses the first lag of cashless payment in the quarter as the explanatory variable, and reports an elasticity of 0.0243. Columns (2), (3), and (4) use the second, third, and fourth lag of cashless payment as the explanatory variable. Across the specifications, we find a strong position relation between cashless payments and credit. In Column (5), we use a distributed lag model to jointly exploit the information in lagged values of cashless payments across all four quarters. The sum of the coefficients on all the lagged explanatory variables is 0.0396, and the the p-value for the relevant F-test for their joint significant is 0.005. Therefore, as districts see increased level of cashless payments, the level of credit creation increases.These findings corroborate the evidence from micro-level data supporting a real effect of cashless payments on economic outcomes.

5.7 Durable Consumption

We now present some additional evidence against the "reporting bias" interpretation of our results by focusing on economic outcomes that depend on real income of the households, and not on just the reported income: the level of their consumption. Our database allows us to observe whether a household has purchased assets such as a car, a generator set, or a television set in the survey period. Using these consumption items as a measure of economic growth, we estimate our base model and present the results in Table 12.

Households residing in districts with higher digital payment intensity are more likely to buy a generator set, car, television, air-conditioner or computer than those in lower digital payment districts. Our estimates are significant in all cases expect one. Thee findings alleviate concern that we are simply capturing the reporting bias in our study.

Table 11: Cashless Payments and RBI Credit: Panel Data

Table 11 presents the results of a panel regression model with district-quarter level data from 2018-2022. The dependent variable is the log of credit extended by all banks in a given district in a quarter as reported by the RBI. Cashless Payment measures the log of the amount of cashless transaction in the previous quarters. All standard errors are clustered at the district level.

	(1)	(2)	(3)	(4)	(5)
Cashless Payment.L1	0.0243***				0.0316***
	(0.0086)				(0.0059)
Cashless Payment.L2		0.0233***			-0.0100***
		(0.0083)			(0.0030)
Cashless Payment.L3			0.0233***		0.0024
			(0.0085)		(0.0031)
Cashless Payment.L4				0.0220***	0.0156***
				(0.0082)	(0.0060)
District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year-Qtr Fixed Effects	Yes	Yes	Yes	Yes	Yes
Nobs	8,064	7,560	7,056	6,552	6,552
Adjusted R-squared	0.9976	0.9977	0.9979	0.9980	0.9981
Number of Districts	504	504	504	504	504
p-value (F-test)					0.0005

standard error in parentheses

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

Table 12: Asset Purchase

Table 12 presents the regression estimate of equation 1 to analyze the effect of digital payments on durable goods purchase. The model is estimated with district-quarter level observations. The dependent variable measures whether the hiusehold purchased a given item, identified at the top of the Column, in a given quarter or not. Lagged Cashless Payment measures the log of the amount of cashless transaction in the previous quarter. All standard errors are clustered at the district-quarter level.

	(1)	(2)	(3)	(4)	(5)
	Generator	Car	Television	Air-Cond.	Computer
Lagged Cashless Payments	0.0025**	0.0018**	0.0016	0.0025***	0.0037***
	(0.0011)	(0.0007)	(0.0028)	(0.0007)	(0.0008)
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes
Urban x Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes
Nobs	1,433,159	1,433,159	1,433,159	1,433,159	1,433,159
Adjusted R-squared	0.033	0.045	0.056	0.019	0.042

standard error in parentheses

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

5.8 PMJDY Scheme

As discussed earlier, the Government of India launched a nationwide campaign to provide a bank account to every individual in the country in 2014. Having a bank account is a necessary condition for the use of UPI account. However, if the lead banks of late versus early adopter districts opened bank accounts under this scheme at a different rate, then our difference-in-differences strategy would not be able to differentiate between the stand alone effect of cashless payments from that of access to bank accounts.

We obtain data on number of bank accounts opened under the PMJDY scheme as of January 31, 2015, i.e., soon after the launch of the scheme. We compute the number of new accounts opened under the PMJDY scheme on a per branch basis for every public sector bank in the country. Figure A5 plots the average number of new bank accounts across early and late adopter districts. As shown in the figure, there is no clear pattern across the two groups. The average number of new accounts is 1041 and 991 for the early and late adopter districts, respectively. The difference is within 0.25 standard deviation of the variable, and statistically insignificant. Therefore, our results are unlikely to be explained by differential effects of PMJDY. Further, as discussed earlier, several banks underwent a merger about a couple of years after the launch of the UPI. Our results are unlikely to be explained away by differences in the acquirer and target banks' involvement in the PMJDY scheme over time as we ensure that we match an early adopter district that was acquired with a later adopter district that was also acquired in this phase of consolidation.

We are able to provide more direct evidence against this alternative using detailed district level data on the number is PMJDY accounts opened by the end of 2022 in two large states of the country for which we have such a granular dataset available.[20] Even though we only have this data for Maharashtra and Tamilnadu, they cover 31 districts in our matched sample analysis, covering over 40% of households in our matched sample analysis. As shown in

[20]We are able to obtain this level of granular data for Maharashtra and Tamilnadu in response to a query in the Rajya Sabha, the upper house of the Indian Parliament.

Appendix Table A2, the early and late adopter districts are almost identical in terms of PMJDY accounts opened on a per capita basis: the average (s.d.) number of per capital PMJDY account is 0.243 (0.076) for the early district as compared to 0.225 (0.099) for the late districts. The difference is statistically insignificant and within one-fourth of the standard deviation. Since our matched sample ensures that the early and late districts are similar in 2016 in terms of their bank branch density, this is not a surprising result. We obtain similar results using the log of number of new accounts opened under the PMJDY scheme. Again, the result is not surprising because we have matched our districts not only on per capita bank branch penetration in 2016 but also in terms of their population. Therefore, on both metric, namely the total number of new accounts under the PMJDY and the per capita accounts, the treated and control districts look similar.

In Table A3, we estimate the difference-in-differences regression model for the matched sample of districts in Maharashtra and Tamilnadu after including fixed effects with the interaction of the log of number of new accounts opened under the PMJDY with year-quarter dummy variables. Such a specification allows us to directly soak away the effect of PMJDY account from the effect of UPI adoption. As shown in the Table, our results remain robust to the inclusion of these fixed effects.

6 Conclusion

We document evidence in support of a positive impact of digital payments on economic outcomes as measured by household income and business activities. Our empirical setting from India is especially attractive since the country has become one of the leading economies of the world in adopting digital payments at mass scale. Further, we study the economic outcomes at the household level. Since these economic agents face significant frictions in accessing traditional credit markets and payment systems, the adoption of digital payments is especially valuable to them.

We use the difference in the timing of participation by a bank on the UPI system and the variation in their presence across different districts of the country as a source of quasi-exogenous variation in the adoption of digital payments after the launch of UPI. Our empirical setting allows us to draw a causal inference by investigating economic outcomes for households who reside in districts of early adopter versus late adopter banks. In a complementary identification strategy, we exploit the within-district-year-quarter variation in outcomes across self-employed and salaried households to soak away the effect of common economic shocks at the district-quarter level. We find that self-employed households, especially marginal entrepreneurs such as hawkers and vendors, benefitted more from the adoption of digital payments.

We provide several pieces of evidence to support the claim that credit constraints and transaction cost frictions are some of the key drivers of our findings. Economic outcomes are especially better in districts where traditional banking infrastructure is weak, suggesting that digital payments alleviate frictions created by the lack of brick-and-mortar institutions. Within the occupation categories, results are stronger for marginal self-employed households such as hawkers and small traders. These agents face significant frictions in accessing traditional financial markets that the digital payment infrastructure alleviates. We show that marginal entrepreneurs' borrowing from formal sources of financing goes up with digital payments, whereas their borrowing from informal sources come down at the same time. These findings provide insights into economic mechanism behind our findings.

Countries around the world are considering a move towards digital payment in various forms. Our study provides valuable inputs to policymakers: a move towards digital payment system affects economic outcomes in a positive manner. Therefore, the impact of digital payments go beyond a simple change in the means of payment. It alleviates economic frictions that can benefit marginal agents of the economy in a meaningful way.

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Appendices

A Data & Variables

Variable	Data Source	Variable Construction
Household income	CMIE	`Total_Income` excluding Government Transfers from the CMIE Income Pyramids.
Owns Business	CMIE	`Income_Of_Household_From_Business_Profit` from the CMIE Income Pyramids. We construct a binary variable which is 1 if a household reports business income and 0 otherwise
Business income	CMIE	`Income_Of_Household_From_Business_Profit` from the CMIE Income Pyramids. It reports the total business income reported by a household in Indian Rupees
Cashless intensity	PhonePe, WKS	We first use the amount of cashless transactions in a district in a year provided by PhonePe and divide it by the population estimate of that district as provided by WKS. We then estimate the percentile ranking of this value to arrive at cashless intensity of a district in a year
Post		This is a binary variable which is 1 for all years after 2016 and is 0 for all years before and including 2016. Since UPI was launched in India in the third quarter of 2016, this variable helps to record the nationwide shock to cashless payments
Urban District	CMIE	We use the indicator `Region_Type` from the CMIE database and construct this binary variable which is 1 for all urban districts and is 0 for all rural districts
SE	CMIE	SE refers to 'Self-employed'. We use the indicator `Nature_Of_Occupation` from the CMIE Income Pyramids and construct this binary variable which is 1 if occupation is reported as Entrepreneurs, Self-employed Entrepreneurs, Self-employed Professionals, Small Traders/Hawkers, Organized Farmers, and Small/Marginal Farmers and is 0 otherwise
LowFinPctl	RBI, WKS	We use district-level bank branches data provided by the Reserve Bank of India (RBI) for December 2016 and district-level India population estimates provided by WKS. We construct this variable by dividing number of bank branches in a district by its population, estimating its percentile rank, or `Dist_FinDev_Percentile` and finally arriving at LowFinPctl = 1 - `Dist_FinDev_Percentile`, a measure of low financial development in a district

Note: WKS refers to India district-level population estimates provided by Wang et al. (2021) for the year 2020

Variable	Data Source	Variable Construction
LowFin	RBI, WKS	This is a binary variable which is 1 if a district falls in bottom 33 percentile of `Dist_FinDev_Percentile` calculated above, and 0 otherwise
Bank Borrowing Outstanding	CMIE	`Has_Outstanding_Borrowing` from CMIE's Aspirational dataset. It is a binary variable which is 1 if a household has an outstanding borrowing and is 0 otherwise
Borrowing for Business	CMIE	`Borrowed_For_Business` from CMIE's Aspirational dataset. It is a binary variable which is 1 if a household has an outstanding borrowing for business and is 0 otherwise

Note: WKS refers to India district-level population estimates provided by Wang et al. (2021) for the year 2020

Table A1: Presence of Lead Bank in a District

Table A1 presents the estimate of cross-sectional regression models that use the number of branches of a bank in a district as the dependent variable and whether the bank is the lead bank or not as the explanatory variable. The unit of observation is the district-bank pair. The model uses district fixed effects, providing us with an estimate of the incremental number of branches of the lead bank compared to all other banks in the same district. Model (1) is estimated with an OLS model with the raw number of bank branches as the dependent variable. Model (2) uses a Poisson regression model to account for the fact that the dependent variable is count data. Models (3) and (4) are based on log transformed values. All standard errors are clustered at the district level.

	(1)	(2)	(3)	(4)
	Branches	Branches	Log(Branches)	Log(1+Branches)
Lead Bank	30.2337***	2.1291***	2.2295***	2.5605***
	(1.1124)	(0.0379)	(0.0283)	(0.0285)
District Fixed Effects	Yes	Yes	Yes	Yes
Nobs	20,370	20,370	12,554	20,370
Adjusted R-squared	0.3540		0.3212	0.3450
Number of Districts	485	485	485	485
Model	OLS	Poisson	OLS	OLS

standard error in parentheses

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

Table A2: Summary Statistics: PMJDY Accounts

Table A2 presents the summary statistics of the number of new accounts opened under the Prime Minister Jan Dhan Yojna (PMJDY) by the end of 2022. The sample covers all districts in the states of Maharashtra and Tamilnadu that enter the matched sample analysis. Panel A pools early and late adopter districts, whereas Panels B and C provide the corresponding statistics for the two groups separately.

Panel A: All Districts in MH & TN	Mean	SD	Min	P50	Max	N
Per Capita Accounts	0.233	0.089	0.072	0.243	0.482	31
log (no. of accounts)	13.294	0.589	12.336	13.214	14.353	31
Panel B: Late Districts						
Per Capita Accounts	0.225	0.099	0.072	0.234	0.482	17
log (no. of accounts)	13.173	0.527	12.336	13.171	14.263	17
Panel C: Early Districts						
Per Capita Accounts	0.243	0.076	0.130	0.267	0.366	14
log (no. of accounts)	13.441	0.646	12.371	13.522	14.353	14

Table A3: Cashless Payments and Outcomes: Controlling for PMJDY Accounts

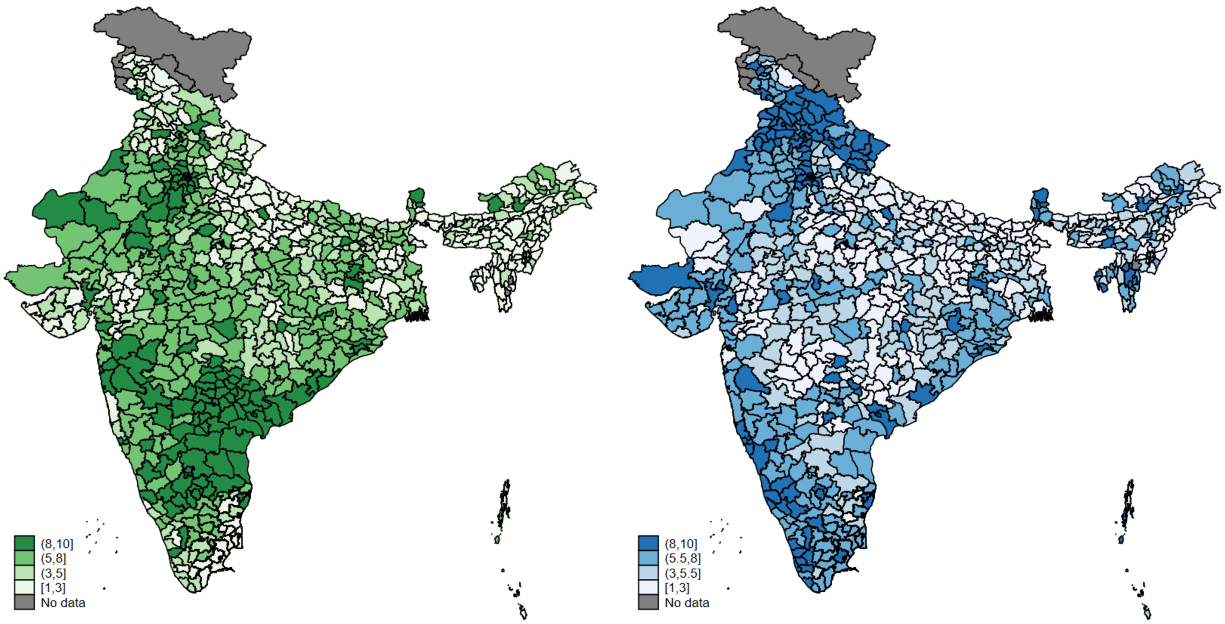
Table A3 presents the estimate of regression model of equation 3 with the inclusion of fixed effects for the interaction of the log number of PMJDY accounts and year-quarter. The model is estimated with the sample of matched districts in Maharashtra (MH) and Tamilnadu (TN). All standard errors are clustered at the district-quarter level.

	(1) Income	(2) Bus(Y/N)	(3) Bus Inc	(4) Entr(Y/N)	(5) Income
Early x Post	0.0962***	0.0374***	0.4039***	0.0504***	0.1033***
	(0.0211)	(0.0060)	(0.0633)	(0.0052)	(0.0216)
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes
State x Yr-Qtr Fixed Effects	Yes	Yes	Yes	Yes	Yes
Urban x Yr-Qtr Fixed Effects	Yes	Yes	Yes	Yes	Yes
Demonetization x Yr-Qtr Fixed Effects	Yes	Yes	Yes	Yes	Yes
PMJDY x Year-Qtr Fixed Effects	Yes	Yes	Yes	Yes	Yes
Sample	MH+TN	MH+TN	MH+TN	MH+TN	MH+TN
Nobs	431,137	431,137	431,137	431,137	84,124
Adjusted R-squared	0.511	0.327	0.347	0.382	0.661

standard error in parentheses

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

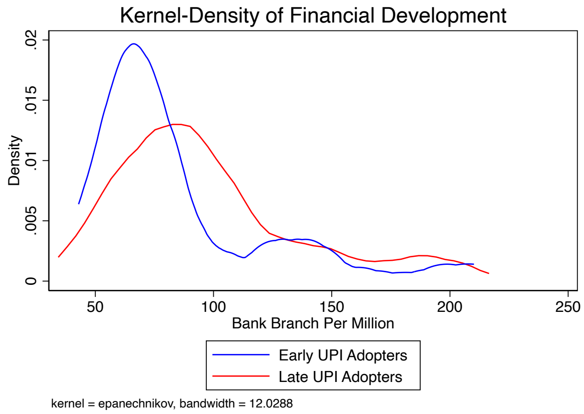
Figure A1: District-level Intensities



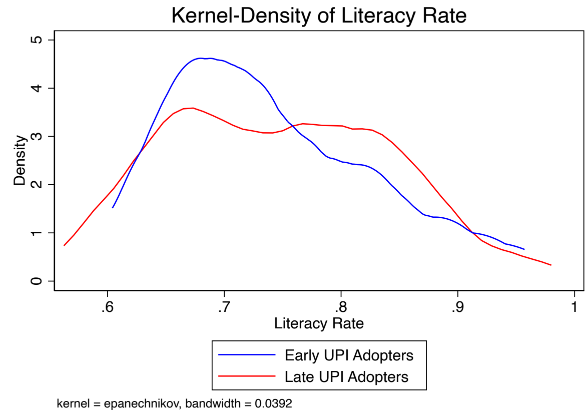
(a) Digital Payments Intensity

(b) Financial Development Intensity

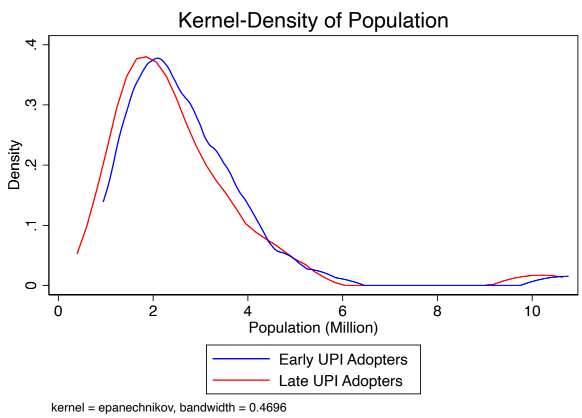
Figure A2: Kernel Densities Across Early and Late Adopter Matched Districts



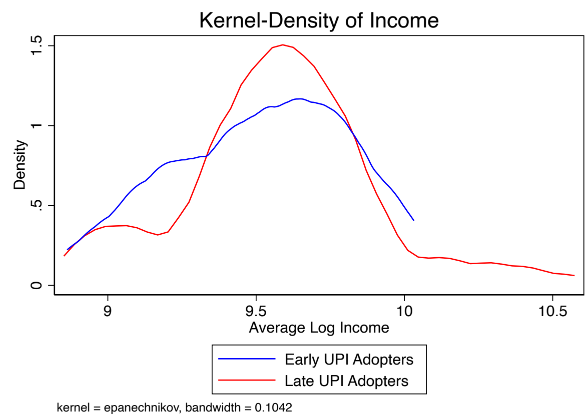
(a) Bank Branch Penetration



(b) Literacy Rates



(c) Population



(d) Average Income, 2016

Figure A3: Early Adopter Bank Branch Fraction

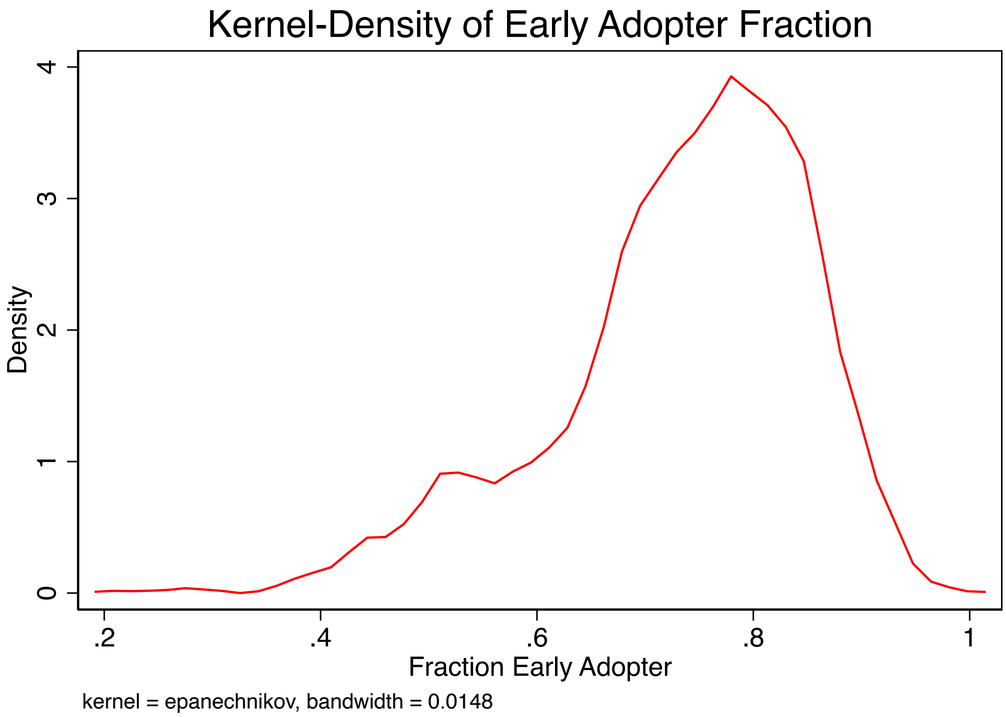


Figure A4: Effect of Early Participation on Cashless Adoption Per Capita

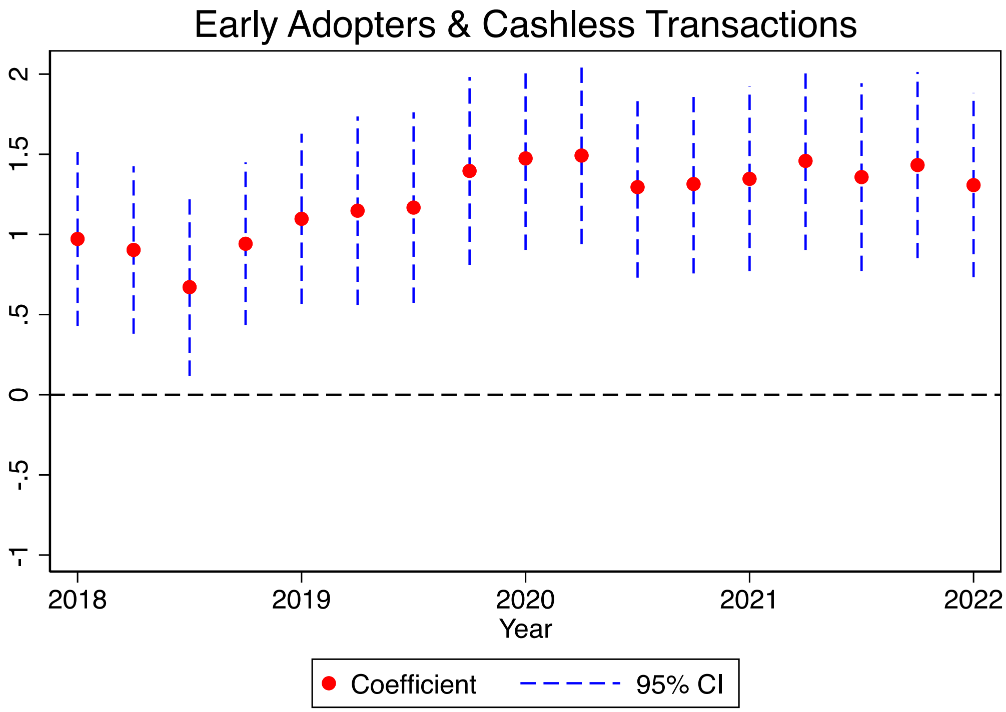


Figure A5: New Accounts Across Early (Blue) and Late (Red) Adopter Banks