NBER WORKING PAPER SERIES

BREAKING BARRIERS TO FINANCIAL ACCESS:
CROSS-PLATFORM DIGITAL PAYMENTS AND CREDIT MARKETS

Shashwat Alok
Pulak Ghosh
Nirupama Kulkarni
Manju Puri

Working Paper 33259
http://www.nber.org/papers/w33259

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2024, Revised December 2025

We thank seminar participants at ABFER, American University, Bank of International Settlements, BIS-CEPR-Gerzensee-SFI Conference on Financial Intermediation, George Mason University, Federal Deposit Insurance Corporation, Federal Reserve Board of Governors, Fed Philadelphia/ Univ of Delaware Fintech conference, Fordham University, Henry Thornston memorial lecture, Bayes University, Imperial College, Indian Institute of Management, Bangalore, Institute for Economic Growth, Delhi, London Business School, National University of Singapore, RIETI International Workshop, Tokyo, Taiwan Symposium on Innovation Economics and Entrepreneurship, Taipei, University of California, Berkeley, UMIF seminar, UNC Conference on wealth and inequality, University of Texas, Austin, University of Texas, Dallas, University of Tokyo, University of Zurich, Vanderbilt University, WEFIDEV-RFS-CEPR Conference in Finance and Development, WFA conference, and the World Bank for providing useful feedback. We also thank Advait Moharir, Amartya Rajamalla, Debargha Som, and Nruhari Viswanath for excellent research assistance. All opinions expressed in this paper reflect those of the authors and not necessarily those of the RBI, CAFRAL, or the National Bureau of Economic Research.

Breaking Barriers to Financial Access: Cross-Platform Digital Payments and Credit Markets
Shashwat Alok, Pulak Ghosh, Nirupama Kulkarni, and Manju Puri
NBER Working Paper No. 33259
December 2024, Revised December 2025
JEL No. D14, G24, G28, G51, J15, R21, R23, R31

ABSTRACT

Can cross-platform digital payments improve credit access at scale? We study this question using India's launch of the Unified Payments Interface (UPI), a nationwide cross-platform payment system. Using rarely available data on the universe of consumer loans, we show that digital payments increased financial deepening, allowing new borrowers to access formal credit. Banks mainly expanded lending to prime borrowers, while fintechs served all borrower segments—but only fintechs extended credit to new-to-credit customers. Credit growth was strongest in regions with affordable internet and recent bank account penetration. We find no evidence that this expansion increased default rates.

Shashwat Alok
Indian School of Business (ISB)
shashwat_alok@isb.edu

Pulak Ghosh
India Institute of Management Bangalore
pulak.ghosh@iimb.ac.in

Nirupama Kulkarni
CAFRAL
nirupama.kulkarni@gmail.com

Manju Puri
Duke University
Fuqua School of Business
and NBER
mpuri@duke.edu

Financial inclusion remains a central challenge for policymakers worldwide. While household access to savings accounts has improved substantially in the past decade, access to credit continues to elude newly banked households due to insufficient or non-existent credit histories (Bachas et al., 2018; Chioda et al., 2024; D'Andrea and Limodio, 2024).[1] Fintechs potentially provide a possible solution: by using alternate data and through novel credit scoring models, they can expand credit to underserved markets. However, evidence to date suggests that fintechs primarily cater to already included borrowers with limited evidence of broad-based financial inclusion.

In this study, we examine whether establishing digital infrastructure that enables seamless and costless payments across apps and banks can expand credit access. The cross-platform payment infrastructure allows prospective borrowers to create alternate data in the form of verifiable digital transaction trails that lenders can use to assess borrower creditworthiness, potentially improving financial inclusion. Such infrastructure could reduce information asymmetries, lower loan origination costs, and expand access to underserved households.

We use India's 2016 launch of the Unified Payments Interface (UPI) — a public digital payment infrastructure — as a natural experiment to study these questions. UPI is one of the world's earliest large-scale public digital payment systems, designed as a zero-cost platform enabling real-time, interoperable payments across banks and applications, that generates real-time digital financial footprints. Today, UPI has become the dominant retail payment rail in India, with over 300 million individuals and 50 million merchants using UPI. As of October 2023, nearly 75% of all retail digital payment transactions were through UPI.[2] A defining feature of UPI is that it enables cross-platform digital payments, and customers can seamlessly transact across any banking or third-party applications. Any third-party digital payment app, for example, Google Pay, would build its UPI application free of cost on the UPI platform. Now consider a customer who uses "Google Pay", a popular UPI application. She can transfer funds from Bank A to a customer or merchant account at Bank B without needing to log on to the respective banks' native

[1] TransUnion estimates that about 82% of the adult population (840 million individuals) in India remained credit unserved or underserved in 2022. This is not just an emerging-market phenomenon: a 2022 TransUnion Study finds that even in developed countries like Canada, the unserved/underserved population is 31% of the adult population, while nearly 4.5% and 14.1% of U.S. households remain unbanked and underbanked as of 2021 (Federal Deposit Insurance Corporation, 2021).

[2] See GOI Press Release, 2023 and GOI Website, 2023

applications.[3] Unlike Venmo/PayPal in the US, users are not locked to a single app and can make payments across platforms using a unified interface. By allowing consumers to generate digital financial footprints across institutions, UPI represents a unique policy experiment in digital inclusion.

Such a payment infrastructure has the potential to transform credit markets by allowing consumers to generate digital financial footprints across institutions. UPI reduces lending costs by creating alternate data through digital payment footprints that lenders can use to assess borrowers' cash flows and creditworthiness. While previous work (Berg et al., 2020; Agarwal et al., 2020; Chioda et al., 2024) has highlighted that fintechs can expand credit through the use of customers' digital footprints when credit bureau scores may not exist, the crucial question is how to facilitate the creation of digital footprints in the first place. Digital payment infrastructure, such as UPI, provides a possible answer. Studying the link between UPI and credit is important as it can highlight a distinct macro-linkage: can payment infrastructure improve overall financial deepening? One cannot answer this question by extrapolating findings from studies based on a single fintech. Apart from the fact that the fintech may not be representative of all fintechs (e.g., offering a platform to sell goods), one cannot separate whether observed patterns reflect firm-level expertise or strategies or generalizable market outcomes that extend to consumers not on the fintech's platform. By contrast, we analyze the universe of consumer loans across all intermediaries, enabling us to capture the aggregate effects of digital infrastructure on credit markets. Our data allow us to measure how both incumbent banks and new fintech entrants respond, and to identify which types of borrowers—prime, subprime, or new-to-credit each intermediary serves.

This paper provides the first economy-wide evidence on the impact of such infrastructure on credit in aggregate, by credit-risk categories, and by intermediary type. Using proprietary regulatory datasets that cover the universe of consumer loans alongside payments, internet rollout,

[3]In the absence of UPI, users could use RTGS/NEFT by logging on to the bank's website which is both costly due to additional fees for fund transfers between banks, time consuming and in some cases not instantaneous. One can also use the bank's native app (though these are a more recent phenomenon), such as YONO for State Bank of India (SBI), but can initiate only one-way transfers from SBI to other accounts of SBI. With UPI, users can transfer funds across any bank.

and bank account penetration data, we examine how UPI adoption influenced credit supply by banks (incumbents) and fintechs (new entrants). We document four main findings. (i) UPI adoption substantially increased financial deepening, measured by both the total number of loans and the number of loans per capita, particularly for subprime and new-to-credit borrowers. (ii) Banks and fintechs responded differently: banks mainly increased lending to prime borrowers, whereas fintechs expanded across all segments—and only fintechs extended credit to new-to-credit customers. (iii) Complementarities matter: UPI's impact was strongest in regions with affordable internet access (following Reliance Jio's low-cost 4G rollout) and where households had been recently onboarded into the banking system through the Jan Dhan Yojana (JDY) program. (iv) Importantly, increased credit access did not come at the cost of higher defaults, suggesting that UPI-enabled digital footprints helped lenders identify underserved but creditworthy borrowers.

We obtain and merge multiple proprietary datasets that are rarely available to researchers. The primary dataset comprises the universe of consumer loans from TransUnion CIBIL at the pincode-month level, spanning lenders (including banks and fintechs) and borrower types (prime, subprime, and new-to-credit). We combine the credit registry data with regulatory data on deposits from India's central bank, the Reserve Bank of India (RBI), to measure ex-ante UPI exposure, and pincode-level payments data from one of India's top five digital payment service providers (State Bank of India). Regulatory data on the rollout of 4G broadband towers and on previously unbanked accounts (Jan Dhan Yojana/JDY accounts) allow us to evaluate the role of technology complementarities in financial inclusion.

To establish causality, we exploit the staggered adoption of UPI by banks (Dubey and Purnanandam, 2024) in our empirical design and rely on two key insights. First, a bank account is necessary to use the full functionality of UPI. Hence, depositors in regions served by early adopter banks were likely to adopt UPI early on. Second, network externalities in the adoption of digital payments (Higgins, 2024) induce regions served by early adopter banks into further UPI uptake. We exploit these ex-ante neighborhood-level differences in UPI exposure and generate exogenous variation within narrow neighborhoods (pincodes). UPI exposure is defined as the ex-ante fraction of deposit accounts (as of March 2016) of early adopter banks in a pincode.[4] Using the exposure measure, we construct a difference-in-differences (DiD) empirical design that compares high-exposure pincodes (treatment group) — neighborhoods exposed to early adopter banks — to low-exposure pincodes (control group) post-UPI to examine credit outcomes. Since government policies operate at administrative-level geographies (such as districts), the pincode-level data allows us to compare within districts, by including district-by-time fixed effects. Pincode fixed effects absorb time-invariant differences between treatment and control groups. The identifying assumption in this canonical DiD setup requires that treated and control pincodes exhibit similar trends absent treatment, conditional on district-by-month fixed effects. Importantly, balance tests show no statistically significant differences in pre-UPI growth in economic activity proxied using nightlights, credit, banking penetration, and employment rate, assuring us that pincodes do not vary with exposure along demographic or economic characteristics. Finally, high-exposure pincodes exhibit high UPI usage, validating our exposure measure.

We conduct robustness tests using our granular pincode-level data that allow us to strengthen the empirical strategy by controlling for time-varying aggregate shocks and trends at a more granular level. We construct granular grids (similar to Moscona et al. (2020)) by dividing the Indian map into rectangular units of size 0.4×0.4 degrees. Adding grid-by-month fixed effects, then controls for time-varying factors within very narrow geographies (grids) compared to districts. We also go further and take an alternate approach that compares only neighboring pincodes that abut each other (neighboring 'pairs' similar to Beerli et al. 2021) and compare outcomes within-neighborhood pairs, controlling for pair-by-month fixed effects. By construction, these results are from a narrower set of pincodes but yield very similar results.

Between 2015 and 2019, credit increased rapidly, and fintech loans in the subprime and new-to-credit segment witnessed a 10-fold increase. In the aggregate, a 1% increase in UPI transactions is associated with a striking 0.73% increase in credit. To answer how UPI affects financial deepening, we use our preferred DiD setting and examine the UPI impact on the number of loans originated. To account for baseline level variation in credit across pincodes, we also

[4] Pincodes are geographic units used by India Post and are similar to zip codes in the US. Districts are a more aggregated geographic unit, similar to counties in the US.

use population-normalized measures (number of new loans per capita). To measure financial deepening, we focus on the extensive margin—the total and per capita number of new loans extended to new-to-credit borrowers. This captures the expansion of credit access, especially for individuals entering formal credit markets for the first time. Results are robust to alternative measures using the rupee value of loans (scaled or unscaled, see Section 5). While loan growth can reflect either the intensive margin (more borrowing by existing clients) or the extensive margin (new borrowers), our analysis emphasizes new-to-credit individuals. These borrowers lack prior histories or credit scores, making them a central focus for financial inclusion. Since these borrowers did not have access to formal credit earlier, the growth in the number of new accounts helps identify the extensive margin or new borrowers accessing credit directly, reflecting financial deepening. Credit to new-to-credit borrowers is entirely driven by fintech lenders, and grows 45x from a low base in the pre-period.[5] These results are quantitatively and qualitatively similar using the total number of loans per capita. Bank lending to the new-to-credit segment shows no increase, highlighting the role of fintechs in financial inclusion. As against this stark contrast between bank and fintech lending on the extensive margin, both fintechs and banks increase credit to subprime and prime borrowers. These results are qualitatively similar when we include grid-by-month fixed effects. Similarly, restricting to pincode pairs yields similar results and strengthens the causal interpretation of our results.

In principle, both banks and fintechs can use payment data to lend to households. UPI reduced lending costs in two ways. First, lenders could assess borrower creditworthiness by verifying digital transaction histories. Second, UPI improved speed and convenience by allowing lenders and borrowers to use third-party applications such as Google Pay and Paytm, lowering processing costs. While the first mechanism expands credit to new borrowers, the second enhances efficiency for existing ones. The distinct lending patterns we observe suggest both mechanisms were at play. The rise in prime and subprime lending, especially by banks, likely reflects improved speed and

[5]The speed at which fintechs reached marginal borrowers is striking. By 2019, fintechs were originating nearly three-quarters (72%) as many loans to new-to-credit customers as incumbent banks. When including subprime borrowers, the number of marginal customers served by fintechs rose to 84% of the banks' total. This rapid convergence is particularly noteworthy, as these new entrants achieved it in just a few years, competing against banks with a decades-long presence.

convenience, consistent with evidence from developed countries where fintechs primarily serve prime borrowers (Fuster et al., 2019; Seru, 2020). Yet prime borrowers already had the greatest credit access. The more transformative development is the rapid expansion of fintech credit to marginal borrowers—particularly new-to-credit households—an extensive-margin effect driven entirely by fintechs.

The creation of digital payment footprints through UPI enabled fintechs to serve new-to-credit borrowers in a way banks did not. Traditional banks tend to be slower to adopt new technologies (Mishra et al., 2022), whereas fintechs are more agile and inclined to incorporate alternative data into their credit models. Consistent with this, the surge in fintech lending to new-to-credit borrowers appears with a lag, as their in-house models began incorporating UPI data.[6]

Financial inclusion is supported by two complementarities to UPI usage. First, UPI usage requires access to fast, reliable, and low-cost internet. In 2016, Reliance Jio launched 4G services, improving network coverage and lowering the cost of internet access. We exploit the entry of a Jio tower across pincodes as a source of exogenous variation in cheap and reliable internet access. While Jio's entry into a pincode is likely not random, credit supply and economic activity are not quantitatively important factors in determining entry to a pincode, ensuring a credible identification strategy. Fintech loans are higher in pincodes with high-UPI exposure with early Jio entry, and as before, only fintechs cater to new-to-credit borrowers. Bank lending to this segment continues to remain muted. These results highlight the complementarity between digital inclusion through low-cost internet access and UPI in financial inclusion.

A second crucial element in the credit uptake was the previously launched "Jan Dhan Yojana" (JDY) scheme. Introduced in 2014 as part of India's financial inclusion mission to facilitate savings accounts for the unbanked, JDY boosted bank account access (Agarwal et al., 2017). Since users need a bank account to operate UPI, JDY ensured that the environment was primed for UPI take-off. Fintech loans to new-to-credit borrowers are higher in regions with ex-ante more JDY

[6]Using loan-level data from one fintech, we show that UPI transaction activity correlates positively with internal credit scores, larger loan amounts, and lower interest rates. While UPI expanded credit for both prime and subprime borrowers, its most novel contribution was enabling fintechs to reach households without any prior credit history, hence, we place particular importance on this finding. This finding addresses a core question of financial inclusion: how to bring first-time borrowers into formal markets at scale. UPI demonstrates how public digital infrastructure can fill this gap, offering a potential blueprint for policymakers.

account holders, that is, regions with ex-ante more new-to-banking customers with no/thin credit history. These results highlight how UPI enabled financial deepening and complements savings bank account-oriented financial inclusion programs in expanding credit access.

Credit increases represent an economically significant 22% increase in high-exposure pincodes, showing that the amount of credit supplied by both banks and fintechs was not simply a reallocation of existing credit into smaller loans for more borrowers. Back-of-the-envelope calculations show that the new fintech loans were economically significant for the underserved borrowers receiving them, and are nearly 2.8 (4.7) times the urban (rural) monthly per capita expenditure.

Though financial deepening accompanies aggregate credit increases, it does not translate to higher default rates (controlling for borrower risk). Thus, UPI-based information allowed lenders to cater to underserved, creditworthy borrowers without taking on additional default risk.

Our contribution is threefold. First, we show how digital public infrastructure can foster financial inclusion, moving beyond the limited reach of private fintech initiatives. Second, we distinguish the roles of banks and fintechs in expanding credit, highlighting how incumbents and entrants serve different borrower segments. Third, we provide rare evidence from large-scale, regulatory data on how open payment systems can reshape credit markets.

Related literature. We contribute to several strands of literature. First, our paper relates to the growing literature on fintech's impact on credit markets. While technology-driven cost savings can in principle foster financial inclusion, direct evidence remains limited (e.g., Fuster et al. (2019); Bartlett et al. (2022); Balyuk et al. (2022); Gopal and Schnabl (2022); Chioda et al. (2024); Kalda and Neshat (2024)).[7] We emphasize a key precondition for inclusion: the ability of underserved households to generate verifiable digital payment histories that lenders can use to assess cash flows. By providing a zero-cost, cross-platform payment system, UPI solved this cold-start problem, enabling fintechs to expand credit to new-to-credit borrowers.

Second, our work intersects with research on the macroeconomic and credit implications of

[7] See Berg et al. (2022) for a survey.

digital or cashless payment systems (Agarwal et al., 2020; Ouyang, 2022; Sarkisyan, 2023; Dubey and Purnanandam, 2024; Ghosh et al., 2024). Survey data used by Dubey and Purnanandam (2024) show that UPI adoption increased economic output. By contrast, our comprehensive credit bureau data make it possible to dissect the credit channel, showing how UPI enabled lending to new-to-credit borrowers and documenting the distinct roles of banks and fintechs. This establishes a key macro-level linkage: a public, cross-platform payment system can generate broad credit deepening across risk profiles, with fintechs driving expansion into new-to-credit segments.

Note that our paper is distinct from other papers on fintech and cashless payments. These studies typically use a single fintech and focus on single or closed platforms with proprietary data to examine payments and the linkage to credit. However, findings from a single, closed fintech platform may not generalize to other settings. Such platforms often hold a monopoly over proprietary data, can leverage synergies from cross-selling other financial products, and may possess firm/segment-specific expertise, making it unclear if similar effects would manifest in a public cross-platform digital payment system accessible to all intermediaries. By analyzing the universe of consumer loans across all intermediaries, our study complements this work by examining a different, crucial question: can an open and interoperable public digital payment infrastructure expand credit access at scale, especially for the unserved? Using granular credit bureau data, we directly identify new-to-credit borrowers and show that UPI's open nature allowed new-entrant fintechs to serve this segment, while incumbent banks focused on prime customers.

Finally, our paper connects to the literature on basic savings account access (Dupas et al., 2018; Bachas et al., 2021; Breza et al., 2024). While providing bank accounts is a critical first step, many newly banked individuals still lack a pathway to credit (Agarwal et al., 2017). Our findings show how a digital payment layer like UPI bridges this gap, turning accounts into active sources of verifiable data and lowering transaction costs that unlock formal credit. Our work is also related to research on information frictions in credit markets: Kanz (2016) shows how credit bureaus reshaped lending once histories existed, while we show how digital payments generate those histories for new-to-credit borrowers.

Our paper contributes not just to the academic literature but also helps inform policymakers. India's experiment with public investment in digital infrastructure (UPI) has attracted significant attention from policymakers worldwide, drawing comments from Fed policymakers (Yadav, 2024), the World Bank (The Economic Times, 2023), and Bill Gates (The Indian Express, 2020) as a model with potential lessons for other countries. Despite the significant attention on UPI among policymakers globally, research on the impact of this initiative on credit markets is lacking. Our study fills the gap and provides the first comprehensive analysis of how a large-scale public digital payment experiment can drive financial inclusion through first-time access to formal credit markets.

2 Institutional Details

UPI In November 2016, the National Payments Corporation of India (NPCI) officially rolled out the Unified Payments Interface (UPI) all over India for public use. Through UPI, customers and merchants can securely transfer money between bank accounts. Customers can link their bank accounts to mobile applications and transact safely, instantly, securely, and across digital payment platforms (also termed as interoperability). Transactions are protected with end-to-end encryption, which ensures that personal data remains confidential both at the time of the transaction and after its successful completion.

After its launch, UPI transactions rose from 1 million transactions in October 2016 to nearly 10 billion transactions in October 2023. UPI transactions accounted for nearly 75 percent of all retail transaction volume in 2022-23 (Rao, 2023). As per GlobalData research, cash transactions declined from 90 percent of the total volume in 2017 to less than 60 percent in 2021, with UPI and other digital transaction systems accounting for the remaining. A large impetus to UPI uptake was the 2016 demonetization episode, which overnight discontinued 86 percent of cash in circulation. The sudden shortage of cash pushed people into using digital payments as a mode of payment. By the end of 2017, UPI transactions had grown by 900 percent compared to pre-demonetization levels.

Several factors are responsible for the widespread adoption of UPI. UPI facilitates e-commerce, as businesses, merchants, and vendors can seamlessly integrate into the UPI network and accept cashless payments. UPI has also bridged the gap between traditional banking and technology, enabling financial access. Any customer with a bank account can make payments via UPI. Importantly, UPI allows users to create a digital footprint of money flow, which lenders can access, enabling financial inclusion. This last feature has transformed the fintech industry. Several innovations, such as digital wallets, investment platforms, lending apps, expense trackers, and more, have effectively used UPI to provide add-on services. Internet Appendix Figure IA1 describes how UPI allows users to create digital footprints that lenders can use in deciding to lend. Even in the aggregate, state-level UPI is strongly correlated with credit. A 1% increase in UPI is associated with a 0.73% increase in credit (Figure 1). The rise in UPI allowed lenders, primarily fintech lenders that operate within the digital realm, to access payment transaction data to determine creditworthiness. Figure IA2 shows the loan application interface for a UPI user.

UPI infrastructure The underlying technical infrastructure for UPI is complex and costly to build. Internet Appendix Figure IA3 explains the flow of how UPI works. While end-users interact with the consumer-facing interface of the UPI network, only regulated financial institutions can connect to the UPI network. Regulated entities include banking apps and third-party apps — called Third Party Application Providers (TPAPs) — who can partner with multiple banks. Examples include CRED, backed by Axis Bank; Google Pay, backed by multiple banks; and BHIM, the official app released by NPCI.[8] The connected banks are called Payment Service Providers (PSP) and are responsible for the onboarding of users, authentication, and registration, and for ensuring that the TPAPs are compliant and secure. They also act as a grievance redressal mechanism for resolving complaints. PSPs that have onboarded the Payer are called Payer PSPs, and PSPs that have onboarded the Payee are called the Payee PSP. Each person on the UPI network has a unique address to identify them. When a UPI transaction is initiated, the UPI Switch finds the Payee PSP using the unique address and routes the transaction to the Payee's corresponding PSP. After validation, the transfer of money occurs in real time, unlike card transactions, in which the

[8]See the NPCI website, for the list of approved apps and their connected banks here.

money moves at the end of the day. UPI can also be used to pay merchants and follows a similar process. Shops have a static QR that can be scanned with the UPI app, and payments can be settled in real-time. Importantly, for the period of our analysis, there was no payment charge for the consumer or the merchant, unlike credit or debit cards, which charge 1-2% as interchange fees. UPI, thus, is a unique experiment — a payment infrastructure designed for mass retail adoption. Its simplicity, cross-platform interoperability, and zero cost made it scale across the entire economy

Jio rollout Reliance Jio Infocomm Limited, popularly known as Jio, is an Indian mobile network operator with national coverage across all 22 Telecom circles and provides 4G services. In September 2016, Jio made its formal entry into the market with a unique proposition: focusing on high-speed data rather than voice and messaging services. The launch of Reliance Jio transformed the Telecom industry. Jio offered customers 4G internet with data plans amounting to 1 GB per day. In comparison, its competitors offered only 1 GB of data per month. In addition, initial prices were at just ₹5 per GB compared to ₹250-300 per GB for competitors. Low costs and attractive discounts allowed Jio to expand its market share quickly. By February 2017, Jio had crossed 100 million subscribers. As per the Telecom Regulatory Authority of India (TRAI), there were 1.17 billion mobile phone subscriptions in India by February 2019. The growth was especially pronounced in rural areas, with over 500 million wireless subscriptions, roughly 100 million more than before Jio formally began its operations.

The Jan Dhan Yojana (JDY) scheme In August 2014, a large-scale universal banking program, the Pradhan Mantri Jan-Dhan Yojana (JDY) was launched with the mission of financial inclusion. The stated goal was to ensure that essential financial services, such as savings and deposit accounts and remittances, were made affordable, especially to previously financially excluded individuals. While previous programs had targeted inclusion based on village-level metrics of banking access, JDY explicitly aimed to provide access to each household. JDY served as a precursor to the UPI infrastructure as users need a bank account to use UPI. JDY ensured that previously financially excluded parts of the population had a bank account with over 280 million new bank accounts opened under the JDY scheme (Agarwal et al., 2017). By July 2016, nearly 99% of Indian households had a bank account due to the JDY schemes, ensuring that the preconditions for UPI take-off were in place.

3 Data and Empirical Strategy

3.1 Data

Our study combines several unique regulatory and proprietary data, rarely made available to researchers. Table A1 describes the main variables and common terms used in our paper.

Credit bureau data Our primary data is from TransUnion CIBIL, India's largest and oldest credit registry amongst 4 bureaus.[9] The 2005 Credit Information Companies Regulation Act was effective on December 14, 2007, and requires financial institutions to submit monthly data on all new loans granted and loan repayments to credit bureaus.

To study how UPI affects financial deepening, we need data on the universe of loans and across financial intermediaries. We are fortunate to have access to data from TransUnion CIBIL that covers the universe of loans across lenders in India. This loan-level data is aggregated to the pincode level[10] at the monthly frequency for October 2015–January 2019. Our analysis focuses on the consumer loan segment, where alternative data on digital transactions is expected to have the greatest impact. To measure financial deepening, we use the total number of, and the number of loans per capita, defined as the number of new loans granted normalized by population as of 2016, using population data from Meta and winsorized at the 1% level. We also obtain data on the total Rupee amount of credit, which we use to estimate aggregate effects. We also observe the fraction of loans that defaulted, where default for a loan is defined as a loan that is 90 days past due within 12 months of issuance. Data is available for different lender types and borrower

[9] The remaining three credit information companies bureaus are Equifax, Experian, and CRIF-Highmark.

[10] Pincodes refer to six-digit codes in the Indian postal code system used by India Post and correspond to zip codes in the US. We also use a higher level of aggregation, districts, in our empirical specification, which correspond to geographic administrative units similar to counties in the USA.

categories.

Three features of the data make it uniquely suited for our purposes.

First, we observe all retail lending across lender types, namely, banks and fintechs. Fintech (classified by TransUnion CIBIL) refers to non-banking financial corporations that use new-age technologies, such as mobile applications, to deliver financial services. Previous studies typically have used data from single firms. Relying on single-firm data might not be representative of all fintechs, as the individual fintech's lending decisions might be driven by unique fintech-specific funding and other idiosyncrasies. We observe lending across fintechs in India, allowing us to estimate the aggregate effects of UPI on credit access. Additionally, recent research suggests that technological shifts are likely to affect banks and fintechs differently (Seru, 2020) and are important determinants of who gets access to credit. Thus, we need the disaggregation between fintechs and banks to truly look at the impact of digital infrastructure on aggregate credit access, which the CIBIL data provides.

Second, we observe new-to-credit borrowers who represent those borrowers for whom the credit bureau does not have a formal credit history. Given our central focus on examining questions of financial deepening, this segment is the most relevant for our analysis. This category captures borrowers with the highest information asymmetry between lenders and borrowers and helps us focus on borrowers who did not previously have access to formal credit. The remaining borrower segments based on their credit risk profiles (as indicated by their credit scores ranging from 300 to 900) include: subprime (300 to 680), near-prime (681 to 730), prime (731 to 770), prime-plus (771 to 790), and super-prime (791 and above) borrowers.

Third, our data covers the universe of consumer loans. Since our primary focus is on financial inclusion, universal coverage ensures we capture access to marginal and underserved or unserved households. The sheer scale of our data stands out in stark contrast to previous studies using Credit Bureau data (such as in the US) that typically are able to access only a small (5%) sample and often lack the level of lender and borrower detail on loans that we have.

We benchmark the aggregate CIBIL data to publicly available data from the RBI. Data on the gross flow of new credit (new loan originations) is not available from any public source, even at the aggregate level. However, the RBI provides aggregate statistics on total outstanding loans, that is, the stock of credit. We use this data to estimate annualized net credit flow, which equals new consumer loans granted less consumer loans repaid. Reassuringly, we find an economically meaningful 83% correlation between annualized gross credit flows estimated from our data and the net credit flow estimates from RBI data.[11]

Banks' deposits data Our second dataset is on total bank deposits from the regulator, RBI. This unique and proprietary data is crucial to constructing our pincode-level UPI exposure measure. Our empirical strategy combines two key ingredients: (i) some banks were early to adopt UPI relative to others, and (ii) users need a bank account to make UPI transactions. We leave the details of how the measure is constructed to Section 3.2 and describe the underlying data here. Information on bank-wise UPI adoption is publicly available.[12] Annual branch-level deposit data is from Basic Statistical Returns (BSR) maintained by RBI and is as of March 31st, the end of the fiscal year. We map the branches to the pincode and aggregate deposit accounts to the bank-pincode level using data as of March 31st, 2016, the latest data available before widespread UPI adoption in November 2016. Of particular importance to us is the granularity of the deposit data, which allows us to define pincode-level exposure to UPI adoption and is instrumental in setting up an empirical strategy that controls for unobservable factors within very narrow geographies.

Data on payment transactions Our third dataset on branch-level UPI transactions (value and volume) is from India's largest public sector bank (State Bank of India/SBI), which ranks among the top five banks in terms of UPI market share. Since a bank account is needed to make a UPI transaction, this data captures all UPI transactions made by the depositors of SBI. We aggregate UPI transactions to the pincode-month level for the period January 2017 to January 2019 to validate our measure of UPI exposure. We verify that our proprietary data accurately represent the broader economic trends in UPI usage by comparing it to publicly available national aggregates from NPCI.

[11] Internet Appendix Table IA1 reports these correlations.

[12] Available from Government of India website: http://cashlessindia.gov.in/upi_services.html.

Reassuringly, the two data series are 97% correlated (Internet Appendix Table IA1), assuring us that we accurately capture UPI take-up across time.

Data on Jan Dhan Yojana (JDY) accounts We obtain regulatory data on the number of Jan Dhan Yojana (JDY) accounts opened at the pincode-month level from the Department of Financial Services, Government of India. This data covers the period July 2014-November 2016.

Jio 4G tower data We obtain proprietary data from the Telecom and Regulatory Authority of India (TRAI) on the location and date of setting up of all Base Transceiver Stations (BTS) in India. A BTS, which we refer to as a tower, acts as a communication link between the network and user devices (e.g., mobile phones). We restrict to 4G technology towers. We know the service providers, namely, Jio, Airtel, BSNL, and Vodaphone. Data on Jio towers for November 2016–January 2019 is used for the baseline analysis. Data on non-Jio towers is used in robustness tests.

3.2 Exposure measure

The main empirical strategy relies on the staggered adoption of UPI by participating banks. The Government of India lists the early adopter banks that were live on the UPI platform as of 2016 Q3.[13] We generate regional variation in exposure to UPI following the approach in Dubey and Purnanandam (2024) with an important distinction. We access proprietary data on bank deposits at the branch level, provided by RBI, that allows us to measure UPI exposure at a more granular pincode level. The regional UPI exposure measure relies on two key insights. First, a bank account is necessary to use the full functionality of UPI. Thus, depositors in regions served by early adopter banks were likely to adopt UPI early on. Second, there are significant network externalities in the adoption of digital payments (Higgins, 2024). As depositors in regions served by early adopter banks increased UPI uptake, it further catalyzed broader regional adoption through these network externalities.

Together, these two insights suggest that the fraction of depositors at early adopter banks in a pincode predicts UPI usage. To construct the exposure measure, we use the data on bank-wise

[13]Available on http://cashlessindia.gov.in/upi_services.html Government of India website.

deposits at the pincode-bank level. We take the deposit data as of March 2016, the latest data available before widespread UPI adoption in November 2016. We classify banks that were live on UPI as of 2016 Q3 as 'early' adopter banks since the GoI makes this data publicly available [14].

Formally, we compute the UPI exposure for pincode p as follows:

$$\text{Exposure}_p = \frac{\text{Total deposit accounts of Early Adopter Banks}_p}{\text{Total deposit accounts of all Banks}_p} \tag{1}$$

Importantly, relying on granular pincode-level variation allows us to strengthen our empirical identification on two fronts. First, one concern might be that early adopter banks differ in significant ways from late adopter banks. Alternatively, early adopter banks may choose to be so, anticipating greater adoption or larger peer effects. By focusing on pincode-level variation, we ensure that local differences in high-exposure pincodes, such as local economic conditions or aggregate peer effects that drive UPI adoption or pincode-level characteristics, are not driving the bank-level decision to adopt UPI. Second, a higher level of aggregation, such as at the district level, does not have the same advantage. For example, several social welfare mandates, such as branching regulations and priority sector lending, operate at the district level. If such mandates were particularly binding for certain types of banks (Cramer, 2025), district-level exposure variation may be contaminated by these bank-level differences. Instead, the granularity of our data allows us to compare pincodes within a district, assuaging such concerns.

However, one could still argue that time-varying factors differentially affect high- and low-exposure pincodes. For instance, high-exposure pincodes could have higher ex-ante economic growth prior to UPI, which may result in higher ex-post UPI transactions and credit outcomes. These concerns are allayed to a large extent as all our empirical specifications rely on within-district comparisons using district$\times$time fixed effects that control for time-varying factors at the district level in a non-parametric way. In supplemental tests, we also control for time-varying factors within narrow geographies *within* districts and construct grids by dividing the Indian map into rectangular units of size 0.4×0.4 degrees. These grids allow us to build artificial geographies that

[14] Our data on UPI transactions is from SBI, and hence, we exclude SBI from the exposure measure to avoid a mechanical correlation between the two. Our results are robust to including SBI in the exposure measure calculation.

represent narrow geographies within districts. We are also able to compare within neighboring pincodes. These tests assuage endogeneity concerns and are possible because we have access to pincode-level measures.

In addition, in Appendix Table A2 we also show the balance tests. We examine whether exposure measures are correlated with ex-ante differences in economic activity or credit. Using nightlight intensity at the pincode level as a measure of economic activity, we find that the growth in economic activity does not vary with UPI exposure prior to the launch of UPI. These regions do not differ in terms of the number of credit accounts or population, either. Since financial inclusion is of particular interest to us, we also examine the heterogeneity in credit to the new-to-credit segment and see no difference. Similarly, there is no difference in the prime or subprime segments. To ensure that these pincodes do not differ in any other observable economic or financial characteristic, we collect additional pincode-level data from the Consumer Pyramids Household Survey data from CMIE. Reassuringly, high exposure pincodes do not vary in the fraction of households with access to bank accounts, credit card holdings, or health insurance. Pincodes do not differ in households with mobile phones, which is important given our focus is on digital transactions. The fraction of households with health insurance also does not vary with UPI exposure. UPI exposure across pincodes is also uncorrelated with unemployment rates, consistent with no differences in night-light intensity growth. In addition to parallel pre-trends, these balance tests assure us that observable differences in high-exposure pincodes do not drive our results.

Does the UPI exposure measure capture UPI usage? We examine whether our exposure measure captures variation in UPI usage. Internet Appendix Figure IA4 compares UPI transaction value and volume in low- and high-exposure locations. Consistent with our premise, high-exposure (defined as above median exposure) pincodes have persistently greater UPI usage throughout our analysis period. More formally, we estimate the effect of exposure on UPI transactions using the specification:

$$Y_{pt} = \delta_{gt} + \alpha_{d(p)t} + \beta \times \text{UPI Exposure}_p + \epsilon_{pt} \tag{2}$$

for pincode p in district $d(p)$ in month-year t. Observations are at the pincode-month level for January 2017 to January 2019. Y_{pt} is the UPI transaction volume and value. UPI Exposure$_p$ is defined in Equation (1). The coefficient of interest, β, measures the impact on UPI take-up for areas more exposed to early adopter banks. Standard errors are clustered by pincode. This specification is analogous to examining the first-stage effect relating our exposure measure to UPI transactions.

In line with Internet Appendix Figure IA4, Appendix Table A3 column 1 shows that moving from a UPI exposure of 0 to a UPI exposure of 1 is associated with a 42% higher (36% in per 1000 capita terms) number of UPI transactions. These results validate the UPI exposure measure of treatment intensity.

3.3 Descriptive statistics

Table 1 presents the summary statistics for key variables. Early adopter banks have a median (mean) deposit account market share of 47% (49%) across pincodes, as indicated by the UPI exposure measure with significant geographic distribution (Panel A, Figure 2) across pincodes. The frequency distribution shows some bunching at extreme values: out of 12,576 pincodes in our sample, 2,602 pincodes have zero exposure, and 1,049 pincodes have 100% exposure (Panel B, Figure 2). UPI transactions have been exponentially increasing with growth in high-exposure pincodes outpacing low-exposure pincodes (Internet Appendix Figure IA4). At the pincode-month level, the UPI transaction value stood at ₹8.35 million (₹156 on a per capita basis). The transaction amount at the median is much smaller at ₹2.13 million, translating into a median transaction size of ₹1990.65 ($=\frac{₹2.13 \text{ million}}{1070}$), indicating that UPI was predominantly used for small-value transactions.

Along the credit dimension, pincodes received approximately 83 loans in a month, highlighting low loan penetration in general. While this number captures both the extensive (loans to new borrowers) and intensive margin (loans to borrowers with previous credit history), the new-to-credit loans show an even lower penetration, with 20 loans, on average, in a pincode-month. Subprime borrowers are the least served. While this category represents marginal borrowers, it includes borrowers who have a credit history and hence an available credit score. In contrast, the prime borrower category shows a relatively higher access to credit with approximately 45 loans, higher than both the new-to-credit and the subprime categories.

There is heterogeneity in which segments banks and fintechs cater to. For fintechs, new-to-credit loans, subprime, and prime borrowers are comparable at 1.31, 0.44, and 1.53 loans, on average, respectively. In contrast, bank lending is disproportionately higher for prime borrowers (42.94) compared to new-to-credit (18.76) and subprime borrowers (4.29). Per capita loans paint a similar picture.

More important to us is the relative growth in fintech and bank lending and the compositional differences in new-to-credit lending. Figure 3 shows the loan composition for banks and fintechs across borrower creditworthiness between 2015 and 2019. Total credit increases across the board, but fintechs grow significantly faster over the four years. New-to-credit and subprime loans form a more significant fraction of fintechs' loan portfolio, suggesting market segmentation with fintechs catering to underserved borrowers. As of 2019, approximately 30.0% (20.81%) of fintechs' (banks') overall lending is to new-to-credit and subprime borrowers. In the aggregate, however, fintechs remain a small fraction relative to banks (Table IA2).

The time series shows similar trends. Prime loans grew for both fintechs and banks (Figure 3). In contrast, banks exhibit a muted growth for the new-to-credit and subprime segment, while fintechs exhibit a considerable uptick in these underserved segments. The speed at which fintech companies reached marginal borrowers is striking. By 2019, new-entrant fintechs had achieved significant scale, originating 72% as many loans to new-to-credit customers as incumbent banks. When including subprime borrowers, fintechs' reach extended to 84% of the volume served by banks. This rapid convergence is especially notable, as these new entrants achieved this feat in just a few years, competing against banks with decades-long presence.

3.4 Main empirical strategy

Our main analysis assesses the impact of UPI exposure on financial deepening using the following difference-in-differences specification:

$$Y_{pt} = \alpha_{d(p)t} + \delta_{gt} + \theta_p + \beta \times \text{Post}_t \times \text{UPI Exposure}_p + \epsilon_{pt} \tag{3}$$

for pincode p belonging to district $d(p)$ in month-year t. Observations are at the pincode-month-year level from October 2015 to January 2019. Post_t takes a value of 1 from November 2016. The dependent variable, Y_{pt}, is the number of loan originations, which precisely captures financial deepening. We also use loans per 1000 capita, defined as the number of loans originated, scaled by population. The new loans originated capture both the extensive margin (new borrowers with access to loans) and the intensive margin (existing borrowers with prior access to loans). The loans to the new-to-credit borrowers explicitly capture the extensive margin. θ_p refers to pincode fixed effects, δ_{gt} refers to grid $\times$ month fixed effects, and $\alpha_{d(p)t}$ refers to the district$\times$month fixed effects. Standard errors are clustered at the pincode level. The coefficient of interest, β, measures the impact on credit for pincodes with higher UPI exposure — specifically, change in credit for pincodes associated with 1% higher share of early adopter bank deposit accounts — relative to the pre-period.

We control for time-invariant factors within very narrow geographies with pincode fixed effects. In addition, the district$\times$month-year fixed effect allows us to control for time-invariant and time-varying factors at the district-month level. Importantly, the treatment effects are identified within district$\times$month across pincodes with varying exposure to early adopter banks. Several bank mandates and social welfare mandates, such as bank branching regulations and priority sector lending requirements, operate at the district level. Since such mandates can be particularly binding for certain types of banks (Cramer, 2025), district$\times$month fixed effect allows us to compare across pincodes within the same district, holding constant the district-level differences in lending due to such regulations. In addition, since the district-level aggregation captures economically integrated units, we are also able to control for time-varying local economic conditions.

Our key identification assumption follows the canonical difference-in-differences strategy that requires that, conditional on district$\times$month-year fixed effects, treated and control pincodes exhibit parallel trends in the counterfactual, in the absence of treatment. While this assumption is fundamentally untestable, we provide support by examining the pre-trends in an event study analysis. To this end, we introduce indicator variables that identify months in relative event-time interacted with UPI Exposure_p analogous to the specification in Equation (3):

$$Y_{pt} = \delta_{gt} + \alpha_{d(p)t} + \theta_p + \beta_\tau \times \sum_\tau \mathbb{1}_\tau \times \text{UPI Exposure}_p + \epsilon_{pt} \tag{4}$$

for pincode p belonging to district $d(p)$ in month t. Observations are also at the pincode-month-year level, and τ is an indicator for each month between October 2015 and January 2019. Y_{pt} is the number of loan originations and loans per 1000 capita. δ_{gt}, $\alpha_{d(p)t}$, and θ_p are district×month-year and pincode fixed effects as in Equation (2). β_τ captures the difference in outcomes for each of the dependent variables with treatment intensity at time τ relative to October 2015.

4 The Effect of UPI on Financial Deepening

Previous work has shed light on how lenders have developed better credit risk models through the use of alternative data based on cashless payments (Berg et al., 2020; Agarwal et al., 2020; Ghosh et al., 2024; Chioda et al., 2024). Potentially, lender reliance on alternate data can help new borrowers get access to credit. However, the challenge still remains in how to facilitate the creation of digital footprints in the first place. UPI is a digital public infrastructure that allows users to generate financial transaction data, eliminating the need for lenders to invest in generating consumer data. UPI can facilitate credit provision for borrowers who face the dual challenge of no credit bureau scores due to their limited credit history, combined with a lack of digital transaction history. By providing potential borrowers with the digital infrastructure to generate transaction footprints, UPI can potentially address longstanding questions of how to assess the creditworthiness of underserved borrowers and improve financial inclusion.

UPI reduced lending costs in two distinct ways. First, lenders could incorporate alternate payment data into their credit risk models, improving their assessment of borrower cash flows and creditworthiness. While this alternate data can augment lenders' risk assessment of both financially-included and financially-excluded borrowers, it has the potential to revolutionize credit markets by opening credit access for previously excluded borrowers. Second, consistent with prior findings, technology can improve the speed and convenience of lending (BIS, 2019; Fuster et al., 2019; Seru, 2020; Mishra et al., 2022). UPI allowed lenders to tap into new and existing borrowers accessing UPI through third-party applications such as Google Pay and Paytm. Borrowers with credit histories potentially benefit from the speed and convenience of credit access through UPI apps. In addition, lenders can also tap into new customers through these apps, allowing them to expand markets. The reduction in loan processing times, expanded customer base, and better speed and convenience can all improve credit access.

We begin with simple univariate analysis and examine how credit access changes post-UPI adoption. Table 2 reports the results for fintechs in Panel A.i and banks in A.ii. The first (last) 3 columns show the number of loan originations for pincodes with below (above) median exposure. Columns 1 and 4 show the pre-period average, and columns 2 and 5 show the post-period average (where post is November 2016 and later). Panel B reports the results using the number of loans per 1000 capita. Columns 3 and 4 present the difference pre- and post-adoption for low- and high-exposure pincodes and show that for both sets of pincodes the loans and loans per capita increase. The difference between the low- and high-exposure effects corresponding to the DiD estimate in column 7 shows a differentially higher increase in high-exposure regions across borrower categories for fintechs. In sharp contrast, bank lending increases to subprime and prime borrowers with muted effects for the new-to-credit category. Panel B repeats the analysis with loans per capita as the dependent variable and shows similar effects. These univariate analyses suggest that the effects on the extensive margin (captured by the new-to-credit category) are primarily driven by fintechs. Bank lending is highest for prime borrowers, followed by subprime borrowers, albeit to a smaller extent.

Temporal dynamics To support our identification strategy, we first assess the parallel trends assumption more formally. The identification assumption in our difference-in-differences setup requires that, conditional on district-month fixed effects, treated and control pincodes exhibit parallel trends in the counterfactual absent treatment. Since this is a fundamentally untestable assumption, we provide support by examining the pre-trends in an event study analysis. Figure 4 plots the coefficient estimates (β_τ) over time using Equation 4. The dependent variable is the number of loans by fintechs and banks in panels A and B, respectively. Each point on the navy-blue line shows the difference-in-differences estimate for each month in the period October 2015-January 2019 relative to the baseline at October 2015. The vertical dotted lines denote the 95% confidence intervals around the point estimates. Consistent with parallel pre-treatment trends, we do not observe a statistically significant difference across high- and low-exposure regions in the pre-treatment period for either fintechs (panel A) or banks (panel B). Post-UPI launch, we observe a differential increase in credit access in high UPI exposure pincodes. Interestingly, the take-up in loans is gradual, reflecting the time needed for lenders to incorporate digital payments data into their credit scoring models.

Difference-in-differences estimates Estimates from the difference-in-differences specification from Equation 3 are shown in Table 3. The dependent variable is the number of loans and represents the combined intensive and extensive margin effect. Given our particular interest in how UPI affects financial deepening, we focus on the new-to-credit borrowers in column 1. This represents the extensive margin. The coefficient on the interaction term, UPI Exposure$\times$Post, in column 1 shows a 0.992 higher loans in high exposure pincodes, representing a 5% increase relative to the pre-treatment mean.[15] Subprime lending increased by 19% (column 2) and prime lending increased by 36% (column 3), with total credit access increasing by 25% (see Internet Appendix Table IA3). Columns 5–8 show the controls for population and report the estimates using loans per 1000 capita as the dependent variable. Estimates suggest a 0.012 higher loan per 1000 capita in high exposure pincodes, representing a 4% increase relative to the pre-treatment mean. For subprime borrowers, loans per capita increased by 17% (column 2), for prime borrowers by 29% (column 3), and the total by 20% (Internet Appendix Table IA3).

We turn to the heterogeneity by lenders in Table 4. Fintech loans increase by 0.818 loans, corresponding to a meaningful 45x increase from a minuscule average of 0.018 loans in the pre-period (column 1, Panel A). This stands in sharp contrast to the near-zero effect on new-to-credit borrowers for banks (column 1, Panel B). This finding underscores the novelty of our results.

[15] In the interest of brevity, we use high exposure to imply a change in UPI exposure from 0 to 1.

Digital payment infrastructure through UPI led to financial deepening, as borrowers without previous access to formal credit were able to borrow from fintechs post-UPI. The effects are similar using loans per 1000 capita in columns 5–8. Fintech loans increase by 0.013 loans per 1000 capita, corresponding to a meaningful 33x increase (column 5, Panel A) with a near-zero effect on new-to-credit borrowers for banks.

Subprime and prime lending, on the other hand, increases for both fintechs and banks (columns 2 and 3). Lending to the marginal subprime borrowers, albeit those who already have access to credit markets, highlights that UPI leads to financial inclusion on the intensive margin also. This effect is driven by both fintechs (20x increase) and banks (12% increase). Similarly, prime lending increases by 44x for fintechs and 32% for banks. Loans per capita (columns 5–8) show a similar increase for fintechs and banks for subprime, prime, and total lending. However, only fintechs increase loans per capita by 33x for new-to-credit borrowers with no effect for banks.

The striking credit increase to prime (and to some extent the financially-included subprime) borrowers is not surprising given the speed and convenience that UPI provides. In addition, third-party payment service providers such as Google Pay (GPay) have partnered with banks and enabled digital-only, small-ticket, paperless loans to individuals and merchants on the GPay application with approval and disbursal in real-time. Due to the digital nature of loan applications, borrowers' transaction costs in applying for loans and banks' cost of offering and servicing smaller ticket loans have gone down.[16] Consistent with this thesis, RBI data indicates that nearly 35% of traditional banks' unsecured digital lending originated on third-party digital platforms such as GPay in 2021. Thus, UPI enabled an expansion of small-ticket credit loans, even for prime borrowers. Another factor is that lenders were able to access a larger pool of customers through UPI and reach prime borrowers in smaller towns and villages. Both fintechs and banks seem to have taken advantage of the speed and convenience provided by UPI to cater to prime and subprime borrowers.

While overall credit expanded, the most transformative impact of UPI on financial deepening was on the extensive margin. We find that new-entrant fintechs drove a rapid and large-scale

[16]The average loan size in GPay is under $360 in size, and 80% of these loans have been credited to Indians living in smaller cities and towns. (source: TechCrunch report, October 19, 2023)

expansion of credit to marginal borrowers. Strikingly, the increase in lending to entirely new-to-credit individuals was driven exclusively by fintechs, a more telling sign of financial deepening than the larger, but less surprising, increase in credit to already-served prime borrowers. The digitally verifiable cash flow trail enables better credit risk assessment of borrowers with no previous history of borrowing from the formal credit markets. UPI decreased the information asymmetry between lenders and borrowers, reducing the cost of customer acquisition. Importantly, UPI shifted the onus of creating digital transaction history from lender-driven infrastructure to public infrastructure, and explains why we find an increase in financial deepening driven by fintech lending.

As an illustration of how fintechs incorporate digital transaction history into their credit score models, we obtain loan-level data from one of the largest fintechs in India. Using this loan-level data, we show that UPI transactions are positively correlated with the lender's internal credit score. This is also reflected in cheaper access to credit (lower interest rate) and larger loan amounts. See Appendix A for a discussion of the underlying data and the empirical results in Appendix Table A4 and Internet Appendix Table IA4.

Why don't banks expand credit access to new-to-credit borrowers? Likely, fintechs can quickly adapt to technological innovations (Fuster et al., 2019) in contrast to banks (Mishra et al., 2022). Since Fintechs operate digitally, they need lower capital expenditure to scale up as opposed to traditional banks that have high fixed costs and are slow to adopt new technology. Further, new-to-credit borrowers typically take smaller loans than prime borrowers, a segment with low-profit margins. To be profitable through small-ticket loans, lenders need to scale up quickly, and hence, it may be more profitable for banks to serve the already included borrowers who demand larger loans. Overall, the results suggest a segmentation of customers served by fintechs and banks. The growth in fintech credit and the lack thereof in bank credit to the underserved categories of borrowers also helps allay concerns that the estimated effects are driven by economy-wide changes.

Controlling for granular local economic factors A concern with our findings is that some unobservable time-varying attributes or policy changes at the pincode level correlated with the launch of UPI may have led to the documented effects. As described in Appendix Table A2, our measure of UPI exposure is uncorrelated with ex-ante credit, growth in economic activity, measures of banking penetration, and employment rate, conditional on district fixed effects. Additionally, we do not see any clear pre-treatment differential trend in our outcome variables. Moreover, since districts represent economically integrated geographic areas, we control for district-time fixed effects in all our analyses. Thus, it is unlikely that some unobservable time-varying confounders influence our findings.

Nonetheless, for additional robustness, we modify our empirical design to control for time-varying factors within a geographic unit level that is smaller than a district. First, we construct grids by dividing the Indian map into rectangular units of size 0.4×0.4 degrees. A grid is bigger than a pincode but smaller than a district. A median grid has 12 pincodes, compared to a median district with 27 pincodes. As some pincodes can belong to multiple grids, we assign them to the grid where they contribute the maximum area. Finally, we restrict our sample to grids where there is variation in UPI exposure, ensuring that our estimates are identified through within-grid variation in UPI exposure across pincodes. Essentially, the grid$\times$month-year fixed effects strategy compares pincode clusters within a narrower geography compared to a district while controlling for time-varying local economic factors. We rerun our baseline specifications, Equation 4 and Equation 3, and control for local time-varying economic factors in a non-parametric manner using grid $\times$ time fixed effects. Internet Appendix Figure IA5 shows the temporal dynamics. Consistent with our baseline results reported in Figure 4, we do not observe a discernible pre-trend. Internet Appendix Tables IA5–IA6 reports the results. The results remain robust.

Comparing within neighborhood pairs Our granular pincode-level data allows us to use a more rigorous identification strategy by comparing neighboring pincode pairs. We define a "pincode pair" as any two pincodes that share a boundary and assign each pair a unique ID (pair-id). To exploit variation, we restrict our sample to pairs where the two pincodes have different levels of UPI exposure. This design allows us to include pair-id $\times$ quarter fixed effects, isolating the treatment effect by comparing outcomes between abutting neighbors. These results are reported in Appendix Tables IA7–IA8 and remain robust. Figure IA6 shows the temporal dynamics. Consistent with our baseline results reported in Figure 4, we do not observe a discernible pre-trend.

5 Complementarities to Financial Inclusion

We shed light on the driving forces behind UPI's financial deepening effect by exploring two additional complementarities to financial inclusion: (i) access to low-cost, high-speed internet, and (ii) access to bank accounts.

Access to low-cost high-speed internet Given the role of new technology and alternative data in credit risk evaluation, digital inclusion complements banking technology in expanding financial inclusion (Berg et al., 2020). UPI use requires access to fast, reliable, and low-cost internet. To examine this idea, we use the rapid expansion of Reliance Jio (Figure 5, Panel A), which launched 4G services in September 2016, as an experimental setting. Our empirical design exploits the proximity of pincodes to a Jio Tower as a source of exogenous variation in cheap and reliable internet access. The average distance to a 4G tower decreased from 15.1 km in 2016 to 2.1 km in 2020. Costs of internet usage went down dramatically, and the price of 1 GB of data fell from ₹228 in 2015 to ₹9 in 2020 (Panel B). The digital gap across regions also narrowed as Jio's 4G network coverage expanded (Panel C). Formally, to estimate the effect of complementarity between UPI exposure and proximity to a Jio tower, we use the specification:

$$\begin{aligned} Y_{pt} =& \delta_{gt} + \alpha_{d(p)t} + \theta_p + \gamma \times \text{Early}_{\text{Jio}} \times \text{Post}_t + \chi \times \text{UPI Exposure}_p \times \text{Post}_t + \\ & \eta \times \text{Early}_{\text{Jio}} \times \text{UPI Exposure}_p \times \text{Post}_t + \epsilon_{pt} \end{aligned} \tag{5}$$

for pincode p belonging to district $d(p)$ in month-year t. Observations are at the pincode-month level and span October 2015 to January 2019. Analogous to the way we define UPI exposure,

$Early_{Jio}$ identifies pincodes that received a Jio tower within 6 km by 2017 Q1.[17] Other variables are as defined in Equation 3.

Since we exploit variation in the timing of Jio entry across pincodes, one concern could be that the entry decision is correlated with time-varying factors related to our variables of interest and credit outcomes. To mitigate these concerns, we first examine ex-ante differences in economic activity and credit across the late and early adopter pincodes in Appendix Table A5. Jio entered areas with lower nightlight growth first. Though Jio's entry decision is not random, early entry pincodes have lower economic growth (as proxied by night lights), biasing the estimates towards zero. There is no difference in the number of new-to-credit, prime, or subprime accounts across early and late Jio pincodes. Moreover, given the low R-squares, these predictors are not quantitatively important in determining the Jio entry decision. Most of the variation in Jio's entry into a pincode remains unexplained by credit growth or economic activity at the pincode level.[18]

Second, we control for district-specific time-varying aggregate shocks using district$\times$month-year fixed effects in our regressions. Thus, any potential district-level time-varying factor correlated with Jio's entry is controlled. Further, the event study plots for the early adopter versus late adopter pincodes confirm that early and late Jio pincodes have similar trends in the pre-period (Figure 6). Together, these tests help allay concerns regarding the endogeneity of Jio's entry decision confounding estimates.

Table 5 examines how Jio moderates UPI's impact on credit. Financial deepening, as proxied by new-to-credit borrowers (column 1), is led by fintechs in high UPI exposure pincodes in early Jio adopter regions (Panel A). Compared to the baseline effect (coefficient of the interaction term of UPI Exposure $\times$ Post), the coefficient of the triple interaction term shows that early Jio pincodes have nearly twice the baseline effect. Banks, on the other hand, continue not to play a role in financial deepening (Panel B). This important heterogeneity highlights the strong complementarity in payment technology and reliable and low-cost internet access.[19] Banks and fintechs cater to

[17] A tower provides reliable internet access within a 6 km radius. Table IA9 confirms that early Jio adopter regions are associated with greater UPI usage.

[18] See Acemoglu et al. (2004) and Hoynes et al. (2016) who make a similar argument based on low R-squares as supporting evidence for exogeneity of the decision to place in an area.

[19] While early Jio rollout positively impacted credit, consistent with prior work (D'Andrea and Limodio, 2024),

subprime and prime borrowers with access to cheap and fast internet, moderating UPI's impact. In columns 5–8, we repeat the analysis with loans per capita as the dependent variable. For banks, there appears to be a weak negative effect on new-to-credit in low-exposure regions with early JIO access. However, the total differential effect (the sum of the coefficient on "$\text{Early}_{\text{Jio}} \times$ UPI Exposure $\times$ Post" + the coefficient on "$\text{Early}_{\text{Jio}} \times$ Post") on lending to new-to-credit borrowers is statistically indistinguishable from zero, a result that holds across subsample tests (Internet Appendix Table IA10).

Our thesis is that Jio brought down the cost of the Internet, expanding credit access among marginal borrowers. However, one could argue that coefficient estimates capture the direct effect of internet access rather than the cost of access. Two observations counter this claim. First, the coefficient on the interaction term for exposure and Post shows that UPI did not affect credit access in areas where Jio entered late. In addition, we restrict to the subsample of late-Jio pincodes and confirm that the effects are primarily driven by the early adopter pincodes (Internet Appendix Table IA10). Since most of the areas were already covered by non-Jio towers, these results show that the cost of access primarily drives the credit effects.

Access to bank accounts Customers need a bank account to use UPI. A previous large-scale universal banking program, JDY, dramatically increased households' access to bank accounts in previously financially excluded regions. We examine whether access to JDY accounts and UPI together enabled credit access to underserved borrowers using the specification:

$$\begin{aligned} Y_{pt} = &\alpha_{d(p)t} + \delta_{gt} + \theta_p + \beta \times \text{Post}_t \times \text{UPI Exposure}_p + \gamma \times \text{High JDY}_p \times \text{UPI Exposure}_p \\ &+ \eta \times \text{Post}_t \times \text{UPI Exposure}_p \times \text{High JDY}_p + \epsilon_{pt} \end{aligned} \tag{6}$$

for pincode p belonging to district $d(p)$ in month-year t. High JDY_p is one for pincodes in the top tercile based on the total number of JDY account openings as of November 2016. Other variables are defined in Equation 3. η measures the differential impact on credit in high-exposure pincodes

our focus is on its complementarity with UPI. This analysis reinforces our central finding: even with the catalyst of cheaper and better internet, banks did not increase lending to new-to-credit borrowers, highlighting their limited role in financial deepening via UPI (Table 5).

with a greater number of JDY account holders relative to low-exposure pincodes with a smaller number of JDY account holders.

Table 6 presents the results for the new-to-credit loans. Credit increase in high-exposure pincodes is differentially higher in pincodes with high penetration of JDY accounts for fintech loans to new-to-credit borrowers (columns 1 and 3). However, as before, bank lending to this group of borrowers does not increase (columns 2 and 4).[20] For ease of interpretation, we also re-estimate our baseline difference-in-differences regression from Equation 3 separately for the high- and low-JDY subsamples in Internet Appendix Table IA11 and find similar results. These tests further strengthen the thesis that UPI enabled underserved and unserved borrowers to access credit markets, contrary to developed countries where fintech increased lending to borrowers previously served by traditional banks (Fuster et al., 2019; Seru, 2020). These results highlight the complementarity between bank accounts for the unbanked and digital payment infrastructure with UPI in expanding credit access.

6 Aggregate Credit and Credit Risk

Our findings thus far suggest that UPI deepened financial markets by expanding access for marginal and underserved borrowers. This section examines the aggregate implications for credit markets by addressing three key questions: First, did the increase in the number of borrowers translate to a higher total volume of credit, or merely a reallocation of it? Second, are the new loan amounts economically meaningful for these borrowers? Finally, did this credit expansion increase systemic risk, or did it reach previously overlooked, creditworthy individuals?

Aggregate Credit. To estimate the aggregate effect on credit supply, we run our baseline DiD specification with the total flow of credit volume at the pincode-level as the dependent variable. The results, shown in Table 7, indicate a ₹1.99 million differential increase in loan value in

[20]For banks, there appears to be a weak negative effect on new-to-credit credit in low-exposure regions with more JDY accounts for loans per capita. However, the total differential effect (the sum of the coefficient on "High JDY $\times$ UPI Exposure $\times$ Post" + the coefficient on "High JDY $\times$ Post") on lending to new-to-credit borrowers is statistically indistinguishable from zero, a result that holds across subsample tests (Internet Appendix Table IA11).

high-exposure pincodes (High-Exposure$\times$Post). This represents an economically significant 22% increase relative to the pre-treatment mean.

Interestingly, there are important differences in the size of loans offered by banks and fintechs. Combining with the estimates from Table 4, the average loan size of UPI-led increase in credit for banks is ₹167,329 (= ₹1.938 million/11.582), 9.4x the size of average loans offered by fintechs (₹17,851 =₹0.053 million/2.969), indicating that fintechs specialize in small-ticket loans to marginal borrowers, while banks offer larger loans to ex-ante included borrowers.

These results suggest that UPI's introduction led to an overall increase in the volume of credit supplied by both banks and fintechs, with fintechs doing more small-ticket loans.

Economic Significance of Fintech Credit. Next, we provide back-of-the-envelope calculations to assess whether these new fintech loans are economically significant for the underserved borrowers receiving them. We benchmark the loan amounts against Monthly Per Capita Expenditure (MPCE).[21] The average increase for fintech borrowers translates to an average loan size of ₹17,851, which is nearly 2.8 times the urban MPCE and 4.7 times the rural MPCE.

For the new-to-credit and subprime segments, a more appropriate benchmark may be the MPCE of lower-income groups. Using the bottom 50$^{\text{th}}$ percentile of MPCE (₹2,001 for urban and ₹1,373 for rural), the average loan amount represents 8.9 times the urban MPCE and 13 times the rural MPCE. We also benchmark against other financial metrics; the average fintech loan size is equivalent to 7.6% of the average annual income of ₹234,551 and 25% of the average income of ₹71,163 for the bottom 50$^{\text{th}}$ percentile, and 1.14 times the average monthly savings.[22] These comparisons confirm that the credit extended is economically meaningful for the recipients.

Credit Risk and Loan Defaults. Finally, a crucial question is whether this financial deepening and credit expansion translated into higher loan defaults. By design, expanding credit to marginal borrowers, who are inherently riskier, would be expected to increase aggregate default rates. However, our primary interest is whether the UPI-enabled lending technology led to higher

[21]Data for MPCE is from Household Consumption Expenditure Survey Data from the Ministry of Statistics and Program Implementation, Government of India website. Urban (rural) MPCE is ₹6,459 (₹3,773).

[22]The income estimates are from Bharti et al. (2024).

default rates compared to traditional methods, holding borrower risk constant. To test this, we compare default rates within specific credit risk categories (new-to-credit, subprime, and prime) across regions with varying UPI exposure.

Univariate statistics (Panel A, Table 8) show that while overall default rates rose post-UPI, there was no statistically significant, meaningful differential increase in defaults in high-exposure pincodes. Though subprime category exhibits an increase in default rates in the univariate statistics, our preferred regression specification, which controls for local time-varying factors in (Panel B, Table 8) shows no effect on default rates. If anything, for new-to-credit borrowers, default rates are 5.3% lower, suggesting that fintechs effectively used the alternative data generated via UPI to better assess risk among these borrowers. Overall, these results indicate that digital payment data enabled lenders to identify and serve creditworthy, yet underserved, borrowers without taking on excessive default risk.

7 Additional Results

Robustness of the exposure measure As a robustness exercise, we redefine the UPI exposure variable. Instead of using the continuous exposure measure we have used till now we define a binary measure where High Exposure that corresponds to pincodes with above median values of UPI exposure. As before, High UPI exposure is a good predictor of actual UPI transactions (Table IA12). Figure IA7 shows that the pre-trends are parallel, consistent with the identifying assumption for DiD. Overall, results are consistent with our baseline (Tables IA13). As before, financial deepening is led by fintechs with no effect for bank lending to the new-to-credit borrowers. Both banks and fintechs increase lending to prime and subprime borrowers.

Controlling for demonetization In November 2016, the Indian government announced demonetization that made 86% of the cash in circulation illegal tender. This coincides with the launch of UPI (Chodorow-Reich et al., 2020), raising the concern that our results are driven by demonetization and not due to UPI. Demonetization can affect credit in two ways. Cash shortage induced by demonetization could have led to greater UPI adoption (Chodorow-Reich et al., 2020), increasing credit access. This thesis is consistent with our findings, with the intensity of cash shortage being another source of variation in UPI adoption. However, other effects of demonization could also explain the credit uptake: demonetization increased deposits in the banking sector, relaxing banks' liquidity constraints, resulting in an increase in bank lending (Chanda and Cook, 2022). While plausible, these effects were not sustained over the longer term as depositors pulled out deposits in search of yields post-demonetization due to a drop in banks' deposit rates (Subramanian and Felman, 2019). Further, it is not obvious why the flow of deposits to banks should increase fintech credit to new-to-credit segments. We find a continued increase in credit well after demonetization ended.

Importantly, all our baseline specifications (Equation 3) control for granular district-by-month fixed effects. This ensures that we are comparing pincodes within very narrow geographies, thus, controlling for demonetization-induced cash shortages that depended on factors such as the distance to the nearest currency printing mints (Chodorow-Reich et al., 2020). Mints first distribute their printed currency to currency chests nationwide (designated bank branches), which then send out the cash to nearby branches across banks. Hence, proximity to currency chests was a strong determinant of cash availability during demonetization.

For robustness, we provide additional evidence to rule out these concerns of possible confounders. We obtain data on the distance to the nearest currency chest (Chodorow-Reich et al., 2020). Reassuringly, the distance to currency chests is uncorrelated with our exposure measure, implying that our baseline UPI exposure measure captures UPI variation orthogonal to the demonetization-induced UPI uptake. In addition, in Appendix Table A6, we repeat our baseline analysis after controlling for the interaction between a pincode's distance from the currency chest and with year-month dummies. These dummies control for any time-varying changes in economic outcomes correlated with the intensity of the demonetization shock/cash shortage, which also impacts credit. Results remain qualitatively unchanged, helping allay concerns that the demonetization episode drives estimates and strengthens the causal interpretation of our findings.

8 Conclusion

Nearly 850 million individuals in India are credit unserved or underserved. A first-order question in financial inclusion is: how do we expand credit access to the marginal population? This study provides new evidence on how public digital infrastructure can reshape credit markets. Analyzing the rollout of a nationwide interoperable payment system, our analysis highlights a crucial distinction between intermediaries: while banks leveraged digital infrastructure primarily to increase lending to prime and subprime borrowers, fintechs extended credit across all segments and uniquely served new-to-credit customers who previously lacked any pathway into formal markets. This distinction underscores the complementary roles of incumbents and new entrants in promoting financial inclusion.

We further show how digital infrastructure interacts with other forms of inclusion. Regions with affordable internet access and widespread bank account penetration saw the greatest expansion in credit, pointing to the layered nature of inclusion. Digital rails seem more effective in enhancing financial inclusion when built on basic access to accounts and connectivity. Importantly, we find that the credit expansion associated with digital payments did not translate into higher default rates. This suggests that verifiable transaction histories help lenders identify underserved but creditworthy borrowers, expanding access without increasing risk.

Taken together, these findings extend the literature beyond single-firm studies of fintech lending. While previous work has shown that alternative data can help specific intermediaries expand credit, our results demonstrate that system-wide, publicly provided infrastructure can foster financial deepening at scale. This broader perspective is essential for assessing how technological change affects credit markets, particularly in economies where large shares of the population remain outside formal finance.

Bibliography

Acemoglu, D., D. Autor, and D. Lyle (2004). Women, war, and wages: The effect of female labor supply on the wage structure at midcentury. Journal of Political Economy 112(3), 497–551.

Agarwal, S., S. Alok, P. Ghosh, S. Ghosh, T. Piskorski, and A. Seru (2017). Banking the unbanked: What do 255 million new bank accounts reveal about financial access? Columbia Business School Research Paper (17-12).

Agarwal, S., S. Alok, P. Ghosh, and S. Gupta (2020). Financial inclusion and alternate credit scoring for the millennials: role of big data and machine learning in fintech. Business School, National University of Singapore Working Paper, SSRN 3507827.

Bachas, P., P. Gertler, S. Higgins, and E. Seira (2018). Digital financial services go a long way: Transaction costs and financial inclusion. In AEA Papers and Proceedings, Volume 108, pp. 444–448. American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203.

Bachas, P., P. Gertler, S. Higgins, and E. Seira (2021). How debit cards enable the poor to save more. The Journal of Finance 76(4), 1913–1957.

Balyuk, T., A. N. Berger, and J. Hackney (2022). What is fueling fintech lending? the role of banking market structure. University of Southern California Working paper.

Bartlett, R., A. Morse, R. Stanton, and N. Wallace (2022). Consumer-lending discrimination in the fintech era. Journal of Financial Economics 143(1), 30–56.

Beerli, A., J. Ruffner, M. Siegenthaler, and G. Peri (2021). The abolition of immigration restrictions and the performance of firms and workers: evidence from switzerland. American Economic Review 111(3), 976–1012.

Berg, T., V. Burg, A. Gombović, and M. Puri (2020). On the rise of fintechs: Credit scoring using digital footprints. The Review of Financial Studies 33(7), 2845–2897.

Berg, T., A. Fuster, and M. Puri (2022). Fintech lending. Annual Review of Financial Economics 14, 187–207.

Bharti, N. K., L. Chancel, T. Piketty, and A. Somanchi (2024). Income and wealth inequality in india, 1922-2023: The rise of the billionaire raj. World Inequality Lab Working Paper.

BIS (2019). Annual report. Technical report.

Breza, E., M. Kanz, and L. F. Klapper (2024). Learning to navigate a new financial technology: Evidence from payroll accounts. Journal of Finance, Forthcoming.

Chanda, A. and C. J. Cook (2022). Was india's demonetization redistributive? insights from satellites and surveys. Journal of Macroeconomics 73, 103438.

Chioda, L., P. Gertler, S. Higgins, and P. C. Medina (2024). Fintech lending to borrowers with no credit history.

Chodorow-Reich, G., G. Gopinath, P. Mishra, and A. Narayanan (2020). Cash and the economy: Evidence from india's demonetization. The Quarterly Journal of Economics 135(1), 57–103.

Cramer, K. F. (2025, 06). Bank presence and health. Review of Finance, rfaf032.

Dubey, T. S. and A. Purnanandam (2024, May). Can Cashless Payments Spur Economic Growth? University of Michigan Working Paper.

Dupas, P., D. Karlan, J. Robinson, and D. Ubfal (2018). Banking the unbanked? evidence from three countries. American Economic Journal: Applied Economics 10(2), 257–97.

D'Andrea, A. and N. Limodio (2024). High-speed internet, financial technology, and banking. Management Science 70(2), 773–798.

Federal Deposit Insurance Corporation (2021). 2021 FDIC national survey of unbanked and underbanked households. Survey report, Federal Deposit Insurance Corporation, Washington, DC.

Fuster, A., M. Plosser, P. Schnabl, and J. Vickery (2019, May). The Role of Technology in Mortgage Lending. The Review of Financial Studies 32(5), 1854–1899.

Ghosh, P., B. Vallee, and Y. Zeng (2024). Fintech lending and cashless payments. Journal of Finance Forthcoming.

Gopal, M. and P. Schnabl (2022). The rise of finance companies and fintech lenders in small business lending. The Review of Financial Studies 35(11), 4859–4901.

Higgins, S. (2024). Financial technology adoption: Network externalities of cashless payments in mexico. American Economic Review 114(11), 3469–3512.

Hoynes, H., D. W. Schanzenbach, and D. Almond (2016, April). Long-run impacts of childhood access to the safety net. American Economic Review 106(4), 903–34.

Kalda, A. and N. Neshat (2024). Forming banking relationships: The role of hiring bankers. Kelley School of Business Research Paper (2024-4713812).

Kanz, M. (2016, October). What does debt relief do for development? evidence from india's bailout for rural households. American Economic Journal: Applied Economics 8(4), 66–99.

Mishra, P., N. Prabhala, and R. G. Rajan (2022, July). The Relationship Dilemma: Why Do Banks Differ in the Pace at Which They Adopt New Technology? The Review of Financial Studies 35(7), 3418–3466.

Moscona, J., N. Nunn, and J. A. Robinson (2020). Segmentary Lineage Organization and Conflict in Sub-Saharan Africa. Econometrica 88(5), 1999–2036.

Rao, A. (2023, September). Upi goes global: The past, present & future of india's payments platform. WION. September 1.

Seru, A. (2020). Regulation, technology and the banking sector. Technical report.

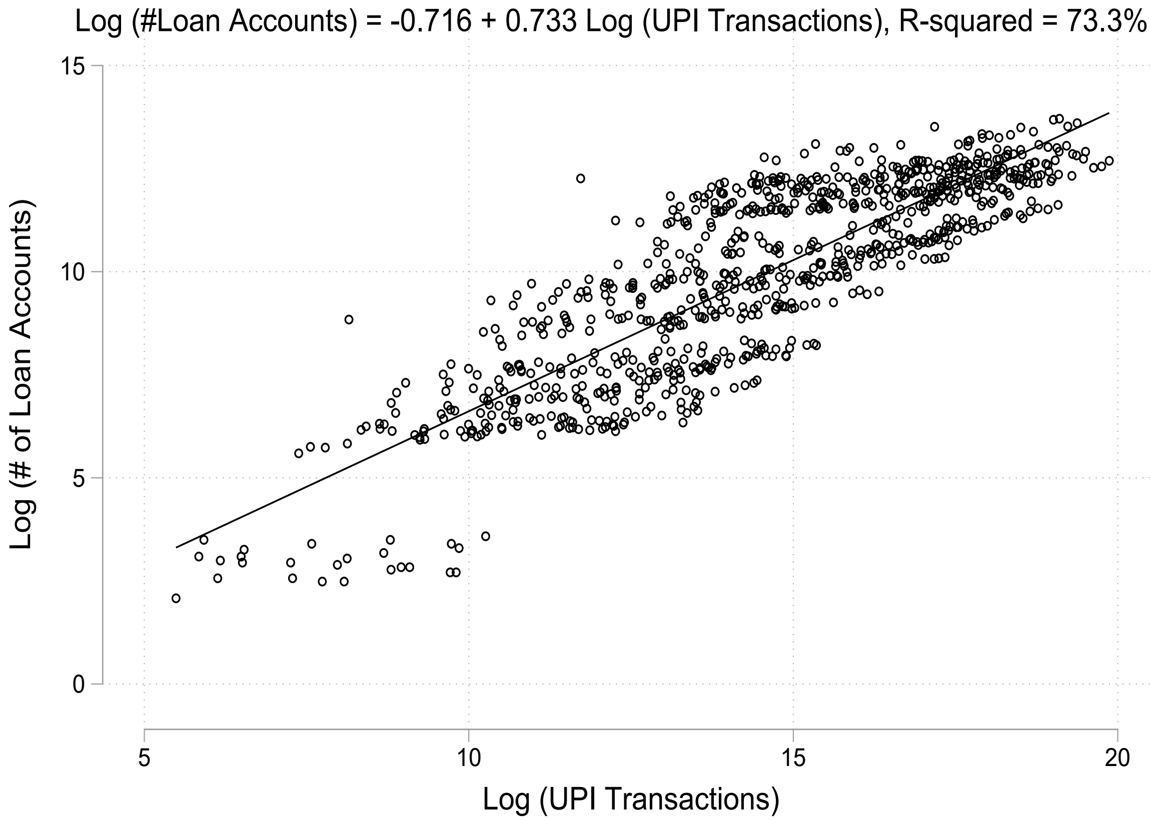
Subramanian, A. and J. Felman (2019). India's Great Slowdown: What Happened? What's the Way Out? CID Faculty Working Paper No. 370.

The Economic Times (2023, September). World Bank lauds India's Digital Public Infrastructure in G20 document. The Economic Times. September 8.

The Indian Express (2020). Bill gates calls india's digital finance approach a global model. The Indian Express. December 8.

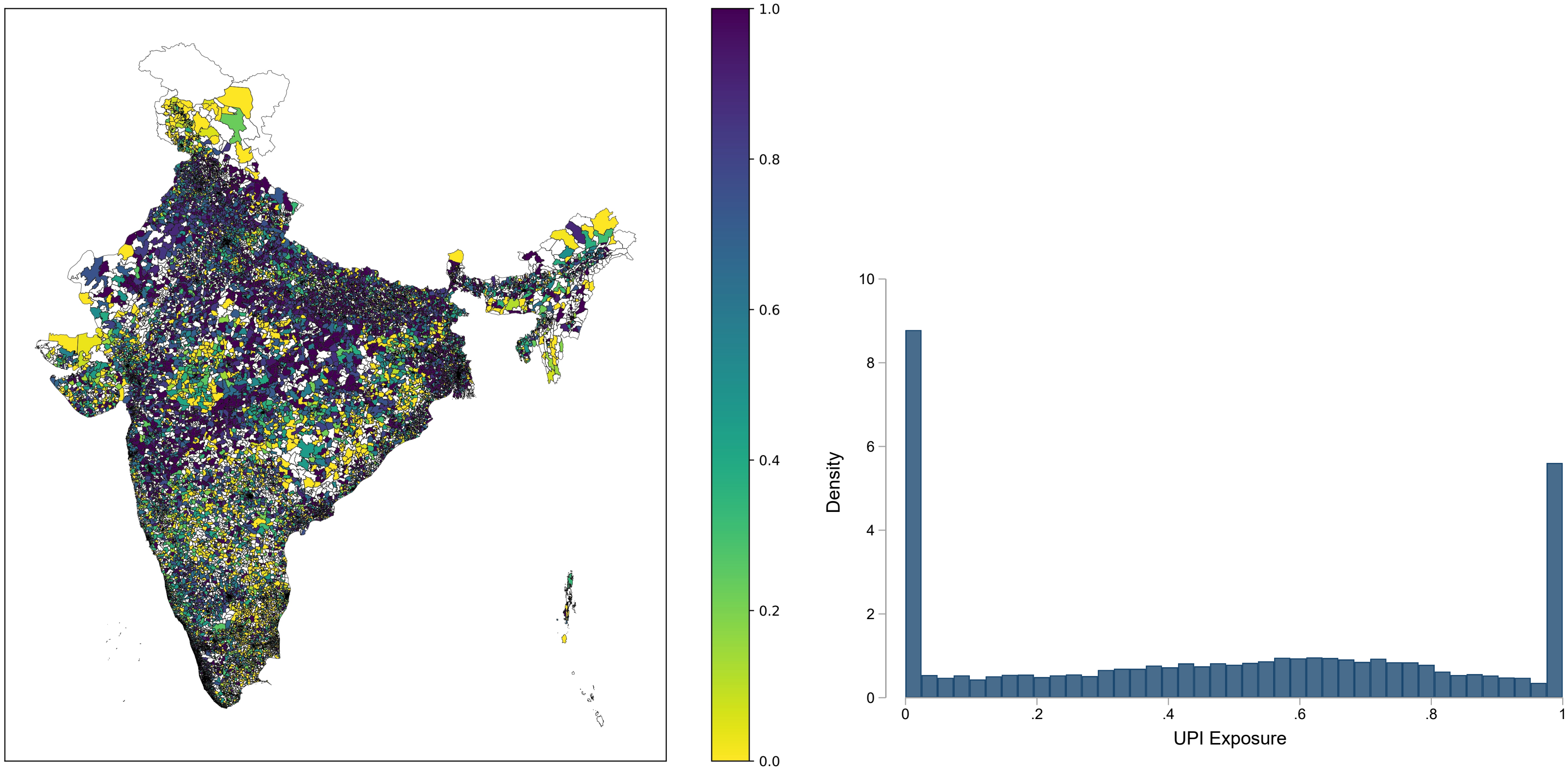
Yadav, P. (2024, August). US Fed's Christopher Waller hails India's UPI as blueprint for global payment systems. CNBCTV18. August 28.

Figure 1
Aggregate Relationship Between UPI and Credit Accounts



Notes: This figure shows the cross-sectional relationship between the log of UPI transactions (x-axis) and the log of the number of loan accounts (y-axis). The data covers the period January 2017 - January 2019, with each dot representing a state-month observation. The black line is the line of best fit. The text above the graph shows the estimated regression specification for the line of best fit.

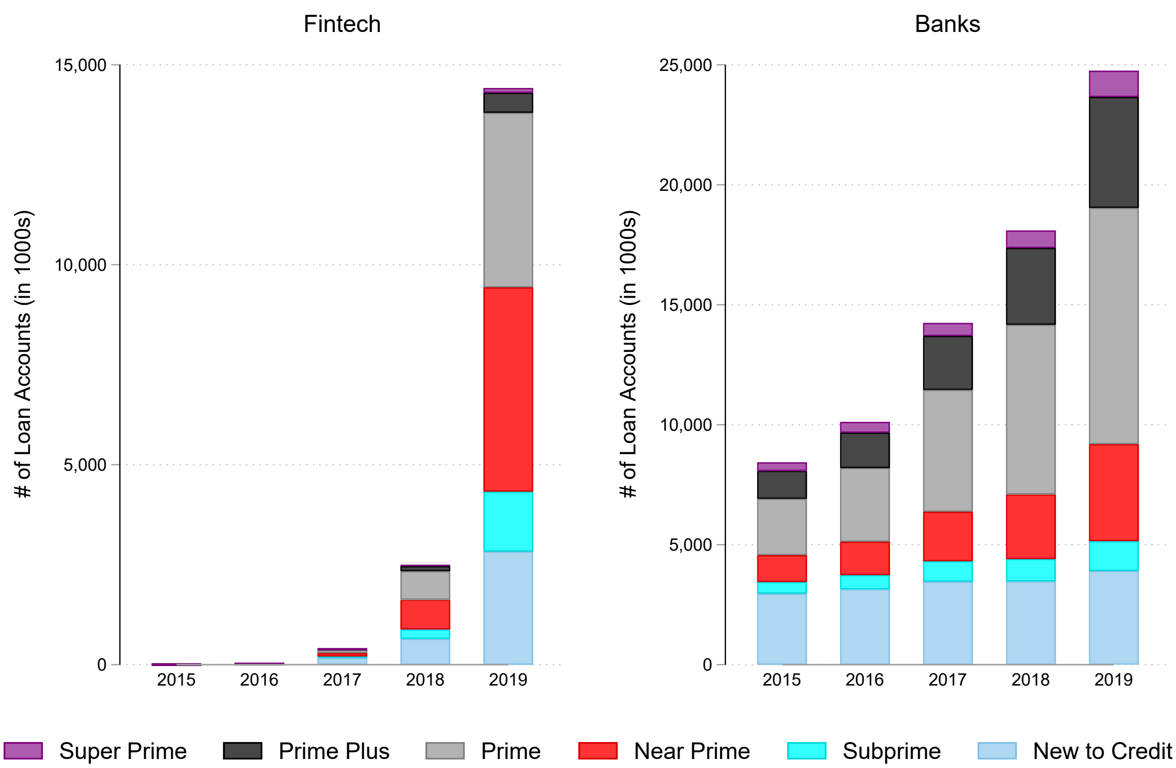
Figure 2
Variation in UPI Exposure



Panel A: Variation across pincode **Panel B: Distribution of exposure measure**

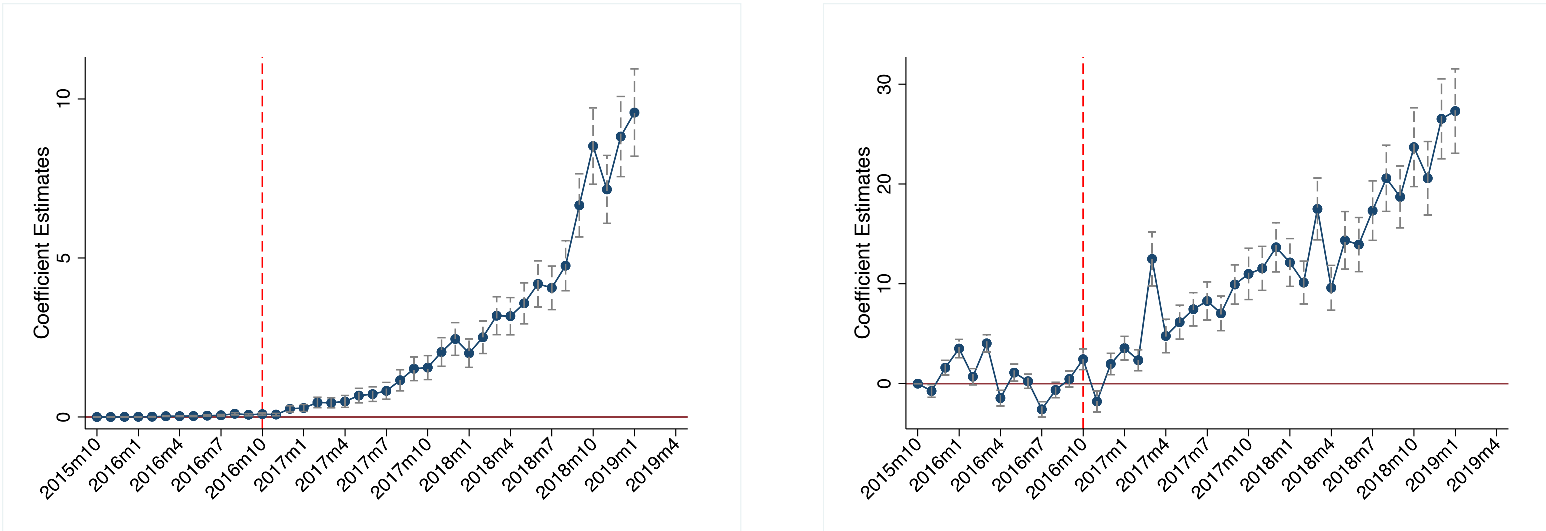
Notes: This figure shows the variation in the value of UPI exposure across pincodes. Exposure measure is defined as the ratio of deposit accounts for early adopter banks to total deposit accounts as defined in Equation (1). UPI Exposure is bounded between 0 and 1. Panel A shows the variation on a map, with darker shades corresponding to higher levels of UPI exposure. Panel B shows the same information as a histogram. The classification of early adopter banks is based on information provided by the Government of India and as of Q3 2016. Deposit data is from Basic Statistical Returns (BSR) provided by the Reserve Bank of India.

Figure 3
Credit Composition by Lender



Notes: This figure shows the trends and composition of the number of loan accounts (in 1000s) by fintechs and banks, respectively. For each of these lenders, each stacked colored bar represents the borrower category, ranging from super prime at the top to new-to-credit at the bottom. The trends cover the period 2015-2019.

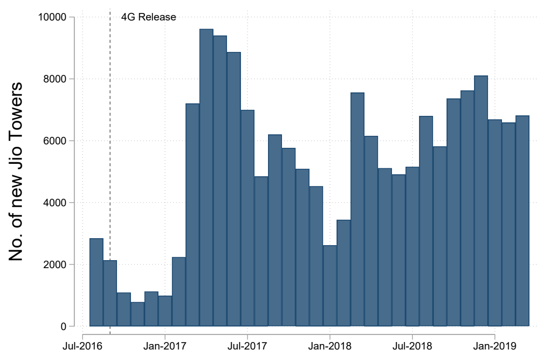
Figure 4
Treatment Dynamics: Impact on Credit Access



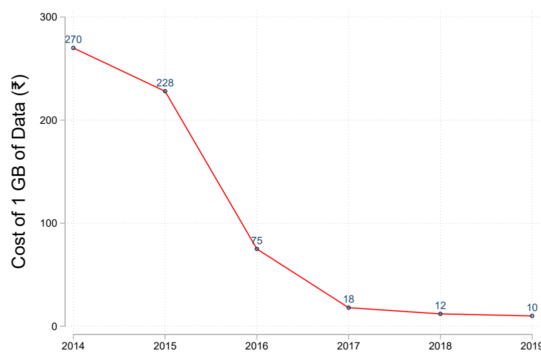
A: Fintechs **B: Banks**

Notes: This figure shows the continuous treatment dynamics using the specification in Equation (4) for fintechs (Panel A) and banks (Panel B). The dependent variable is the total number of loans in both panels. The continuous treatment used is the UPI Exposure as defined in (1). Observations are at the pincode-month level for the period October 2015 to January 2019. Each point on the navy line shows the point estimate. The grey dotted lines indicate the 95% confidence intervals. Pincode and district-month-year fixed effects are included. Standard errors are heteroskedasticity robust and clustered at the pincode level. The dashed red line marks the pre-treatment month (October 2016).

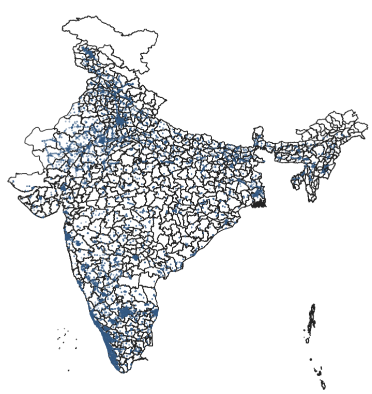
Figure 5
The Jio Revolution



Panel A: New towers



Panel B: Falling data costs

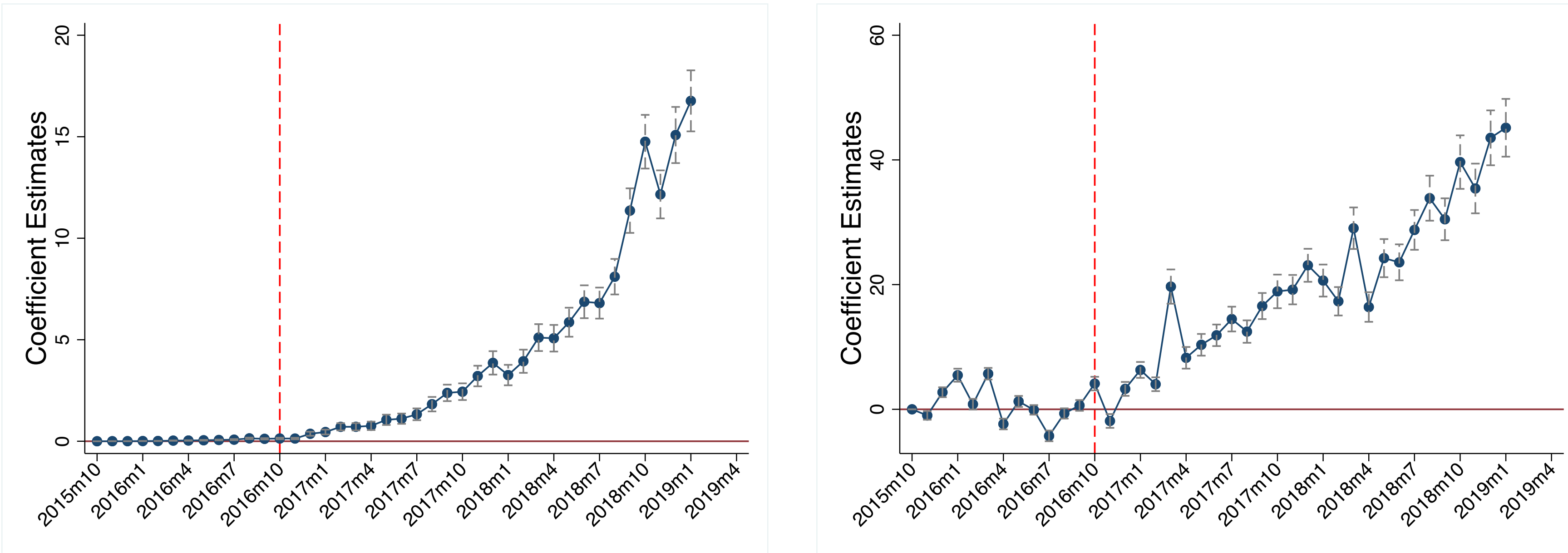


Panel C: Distance to Jio Tower

Notes: This figure shows the rapid growth and accessibility of Reliance Jio as an internet provider. Panel A shows the number of new Jio towers activated every month between August 2016 and March 2019. The dotted line marks September 2016, when 4G internet was activated. Panel B shows the cost of 1 GB of data (in ₹), over the period 2014–2019. Panel C shows the cumulative number of Jio towers in 2016 (left map) versus 2020 (right map). Each blue point represents an active Jio tower.

Figure 6

Complementarities to Financial Inclusion (UPI and Internet Access): Treatment Dynamics for the Impact on Credit Access



A: Fintech **B: Banks**

Notes: This figure shows the triple difference treatment dynamics estimates for the impact of UPI exposure on credit in pincodes with early access to a Jio tower, relative to those with late access, for fintechs (Panel A) and banks (Panel B). The dependent variable is the total number of loans in both panels. $\text{Early}_{\text{Jio}}$ takes a value 1 when a pincode's distance to an active 4G Jio tower is less than 6 km, as of Q1 2017. UPI exposure is defined as in Equation (1). Observations are at the pincode-month level for the period October 2015 to January 2019. Each point on the navy line shows the point estimate of the interaction between UPI Exposure, $\text{Early}_{\text{Jio}}$, and an indicator for each time period. The grey dotted lines indicate the 95% confidence intervals. Pincode and district-month-year fixed effects are included. Standard errors are heteroskedasticity robust and clustered at the pincode level. The dashed red line marks the pre-treatment month (October 2016).

Table 1
Summary Statistics

	Mean	Median	St. Dev
	UPI Measures		
UPI Exposure (N=12,576)	0.47	0.49	0.36
₹UPI in Millions	8.35	2.13	16.24
#UPI Transactions in 1000s	3.67	1.07	6.67
₹UPI per capita	155.54	31.79	362.36
#UPI Transactions per 1000 capita	69.18	15.70	155.92
	Total Credit		
Total Loans	82.61	21.00	188.87
Loans per 1000 capita	1.51	0.39	3.58
Total New-to-credit loans	20.31	6.00	41.22
Total New-to-credit loans per 1000 capita	0.35	0.11	0.76
Total Subprime loans	4.80	1.00	11.35
Total Subprime loans per 1000 capita	0.09	0.02	0.22
Total Prime loans	44.71	10.00	108.53
Total Prime loans per 1000 capita	0.83	0.18	2.06
	Fintech Credit		
Fintech New-to-credit loans	1.31	0.00	4.43
Fintech New-to-credit loans per 1000 capita	0.02	0.00	0.08
Fintech Subprime loans	0.44	0.00	1.73
Fintech Subprime loans per 1000 capita	0.01	0.00	0.03
Fintech Prime loans	1.53	0.00	5.84
Fintech Prime loans per 1000 capita	0.03	0.00	0.10
Total Fintech loans	4.64	0.00	16.47
Total Fintech loans per 1000 capita	0.08	0.00	0.28
	Bank Credit		
Bank New-to-credit loans	18.76	6.00	37.84
Bank New-to-credit loans per 1000 capita	0.33	0.10	0.70
Bank Subprime loans	4.29	1.00	9.89
Bank Subprime loans per 1000 capita	0.08	0.02	0.20
Bank Prime loans	42.94	9.00	103.18
Bank Prime loans per 1000 capita	0.80	0.18	1.96
Total Bank loans	77.38	20.00	174.69
Total Bank loans per 1000 capita	1.42	0.37	3.32
Default rate	0.03	0.00	0.04

Notes: This table presents the summary statistics for observations at the pincode-month level. The table summarizes data for UPI Exposure, UPI Transactions (in Rupee and number of transactions), the total number of loans, and the number of loans per 1000 capita by borrow category (new-to-credit, subprime, and prime) and lender type (fintechs and banks), and default rates. The period is from October 2015 to January 2019. Variables are normalized by the 2016 pincode-level population as indicated. UPI Exposure is calculated as in Equation (1).

Table 2
Univariate Differences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Number of loans						
Dependent variable:	# of loans						
	Low Exposure			High Exposure			DiD
	Pre	Post	Post-Pre	Pre	Post	Post-Pre	High-Low
	(i) Fintechs						
New-to-credit	0.006	1.263	1.257***	0.029	2.613	2.584***	1.327***
Subprime	0.004	0.405	0.401***	0.021	0.884	0.863***	0.462***
Prime	0.008	1.421	1.413***	0.038	3.089	3.051***	1.638***
	(ii) Banks						
New-to-credit	11.617	12.634	1.016***	24.455	25.588	1.134***	0.117
Subprime	2.111	3.212	1.101***	4.759	6.180	1.421***	0..320***
Prime	16.696	31.476	14.780***	40.113	68.411	28.298***	13.519***
	Panel B: Loans per capita						
Dependent variable:	# of loans per 1000 capita						
	(i): Fintechs						
New-to-credit	0.000	0.023	0.022***	0.001	0.042	0.041***	0.018***
Subprime	0.000	0.007	0.007***	0.000	0.014	0.013***	0.006***
Prime	0.000	0.027	0.027***	0.001	0.052	0.051***	0.024***
	(ii): Banks						
New-to-credit	0.235	0.244	0.009***	0.408	0.418	0.009***	0.000
Subprime	0.046	0.065	0.019***	0.084	0.107	0.023***	0.004***
Prime	0.374	0.653	0.278***	0.717	1.187	0.470***	0.191***

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the univariate differences by UPI exposure at the pincode-month level. The dependent variable in Panel A is the total number of loans, and in Panel B, the total loans per 1000 capita. High and low exposure identify pincodes with above and below median UPI Exposure as calculated from Equation (1). Observations are at the pincode-month level for the period October 2015–January 2019. Pre refers to the period before November 2016, and Post refers to the period thereafter. Means for the pre- versus post and high versus low-exposure are as indicated. The difference between the post versus pre for low-exposure pincodes is shown in column 3. The difference between the post versus pre for high exposure pincodes is shown in column 6. The difference-in-differences (column 6-column 3) is shown in column 7.

Table 3
Impact on Credit Access

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	# of loans				# of loans per 1000 capita			
Borrower Category:	New-to-Credit	Subprime	Prime	Total	New-to-Credit	Subprime	Prime	Total
UPI Exposure × Post	0.922***	0.676***	10.270***	14.674***	0.012***	0.011***	0.161***	0.228***
	(0.146)	(0.067)	(0.879)	(1.284)	(0.003)	(0.001)	(0.019)	(0.026)
R^2	0.960	0.926	0.944	0.955	0.938	0.878	0.937	0.950
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	18.227	3.502	28.590	58.621	0.325	0.066	0.549	1.104
Post-UPI Mean	21.339	5.427	52.579	94.325	0.368	0.098	0.964	1.710
Dep. var mean	20.328	4.801	44.783	82.721	0.354	0.088	0.829	1.513
N	508840	508840	508840	508840	508760	508760	508760	508760

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the difference-in-difference estimates for the impact of UPI exposure on new-to-credit (columns 1 and 5), subprime (columns 2 and 6), prime (columns 3 and 7), and total (columns 4 and 8) loans. The dependent variable is the number of loans in columns 1–4 and loans per 1000 capita in columns 5–8. For the per capita normalization, the population is as of 2016. Observations are at the pincode-month level for the period October 2015 to January 2019. UPI exposure is defined in Equation (1). Post is a dummy, which takes the value 1 from November 2016 onward. Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Table 4
Impact on Credit Access for Fintechs and Banks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	# of loans				# of loans per 1000 capita			
Borrower Category:	New-to-Credit	Subprime	Prime	Total	New-to-Credit	Subprime	Prime	Total
	Panel A: Fintech							
UPI Exposure × Post	0.818***	0.256***	1.023***	2.969***	0.012***	0.004***	0.018***	0.048***
	(0.063)	(0.022)	(0.079)	(0.230)	(0.001)	(0.000)	(0.002)	(0.005)
R^2	0.694	0.658	0.701	0.727	0.626	0.570	0.642	0.685
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	0.018	0.013	0.023	0.070	0.000	0.000	0.001	0.002
Post-UPI Mean	1.960	0.652	2.281	6.911	0.032	0.011	0.040	0.118
Dep. var mean	1.329	0.444	1.547	4.688	0.022	0.007	0.027	0.080
N	504280	504280	504280	504280	504200	504200	504200	504200
	Panel B: Banks							
UPI Exposure × Post	0.080	0.414***	9.160***	11.582***	0.000	0.007***	0.143***	0.181***
	(0.112)	(0.047)	(0.797)	(1.059)	(0.003)	(0.001)	(0.017)	(0.021)
R^2	0.966	0.937	0.951	0.966	0.942	0.884	0.943	0.962
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	18.045	3.440	28.447	58.159	0.321	0.065	0.544	1.092
Post-UPI Mean	19.124	4.700	50.021	86.777	0.330	0.086	0.918	1.576
Dep. var mean	18.773	4.291	43.010	77.476	0.327	0.079	0.797	1.419
N	508840	508840	508840	508840	508760	508760	508760	508760

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the difference-in-difference estimates for the impact of UPI exposure for fintechs (Panel A) and banks (Panel B) for new-to-credit, subprime, prime, and total loans. Observations are at the pincode-month level for the period October 2015 to January 2019. The dependent variable is the total number of loans in columns 1–4 and loans per 1000 capita in columns 5–8. For the per capita normalization, the population is as of 2016. UPI exposure is defined as in Equation (1). Post is a dummy, which takes the value 1 from November 2016 onward. Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Table 5
Complementarities to Financial Inclusion: Internet Access

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:		# of loans				# of loans per 1000 capita		
Borrower Category:	New-to-Credit	Subprime	Prime	Total	New-to-Credit	Subprime	Prime	Total
				Panel A: Fintech				
$\text{Early}_{\text{Jio}} \times$ UPI Exposure $\times$ Post	0.779***	0.320***	1.159***	3.221***	0.014***	0.006***	0.021***	0.057***
	(0.105)	(0.037)	(0.130)	(0.381)	(0.002)	(0.001)	(0.003)	(0.007)
UPI Exposure $\times$ Post	0.337***	0.062***	0.317***	0.999***	0.004***	0.001**	0.005***	0.014***
	(0.065)	(0.022)	(0.075)	(0.225)	(0.001)	(0.000)	(0.001)	(0.004)
$\text{Early}_{\text{Jio}} \times$ Post	0.701***	0.228***	0.886***	2.581***	0.010***	0.003***	0.013***	0.038***
	(0.054)	(0.019)	(0.067)	(0.196)	(0.001)	(0.000)	(0.001)	(0.004)
R^2	0.697	0.660	0.704	0.730	0.628	0.572	0.644	0.688
Pre-UPI Mean	0.017	0.013	0.023	0.069	0.000	0.000	0.001	0.002
Post-UPI Mean	1.960	0.652	2.281	6.911	0.032	0.011	0.040	0.118
Dep. var mean	1.314	0.439	1.529	4.635	0.022	0.007	0.027	0.079
N	504280	504280	504280	504280	504200	504200	504200	504200
				Panel B: Banks				
$\text{Early}_{\text{Jio}} \times$ UPI Exposure $\times$ Post	-0.048	0.347***	9.860***	12.021***	0.003	0.006***	0.171***	0.219***
	(0.181)	(0.083)	(1.300)	(1.720)	(0.005)	(0.002)	(0.027)	(0.035)
UPI Exposure $\times$ Post	0.096	0.192***	3.124***	4.180***	-0.001	0.003***	0.040***	0.050***
	(0.116)	(0.047)	(0.719)	(0.955)	(0.003)	(0.001)	(0.014)	(0.018)
$\text{Early}_{\text{Jio}} \times$ Post	0.166	0.465***	8.744***	11.349***	-0.005*	0.006***	0.126***	0.150***
	(0.110)	(0.050)	(0.701)	(0.934)	(0.003)	(0.001)	(0.014)	(0.019)
R^2	0.966	0.937	0.951	0.967	0.942	0.884	0.944	0.962
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	18.036	3.435	28.404	58.088	0.321	0.065	0.544	1.091
Post-UPI Mean	19.124	4.700	50.021	86.777	0.330	0.086	0.918	1.576
Dep. var mean	18.762	4.286	42.943	77.376	0.327	0.079	0.796	1.417
N	508840	508840	508840	508840	508760	508760	508760	508760

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the triple difference estimates for the impact of UPI exposure on credit in pincodes with early access to a Jio tower, relative to those with late access, for fintechs (Panel A) and banks (Panel B) on new-to-credit (columns 1 and 5), subprime (columns 2 and 6), prime (columns 3 and 7), and total (columns 4 and 8) loans. The dependent variable is the number of loans in columns 1–4 and the loans per 1000 capita in columns 5–8. For the per capita normalization, the population is as of 2016. Observations are at the pincode-month level for the period October 2015 to January 2019. UPI exposure is defined in Equation (1). $\text{Early}_{\text{Jio}}$ takes a value 1 when a pincode's distance to an active 4G Jio tower is less than 6 km, as of Q1 2017. Post is a dummy, which takes the value 1 from November 2016 onward. Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Table 6
Complementarities to Financial Inclusion: Access to bank accounts

	(1)	(2)	(3)	(4)
Dependent variable:	# of New-to-Credit loans		# of New-to-Credit loans per 1000 capita	
Lender Type:	Fintech	Banks	Fintech	Banks
UPI Exposure × JDY × Post	1.029***	-0.173	0.014***	0.003
	(0.104)	(0.193)	(0.002)	(0.005)
UPI Exposure × Post	0.126*	0.168	0.003**	-0.001
	(0.075)	(0.137)	(0.002)	(0.004)
High JDY × Post	0.984***	0.190	0.008***	-0.008***
	(0.055)	(0.117)	(0.001)	(0.003)
R^2	0.699	0.966	0.628	0.942
Pincode FE	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y
Pre-UPI Mean	0.018	18.045	0.000	0.321
Post-UPI Mean	1.960	19.124	0.032	0.330
Dep. var mean	1.329	18.773	0.022	0.327
N	504280	508840	504200	508760

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the triple difference estimates for the differential impact of UPI exposure on credit in pincodes with a high number of Jan Dhan Yojana (JDY) accounts for fintechs and banks for the new-to-credit loans. Observations are at the pincode-month level for the period October 2015 to January 2019. The dependent variable is the number of new-to-credit loans in columns 1 and 2 and loans per 1000 capita in columns 3 and 4. For per capita normalization, the population as of 2016 is used. High JDY identifies pincodes with the number of cumulative JDY bank accounts, as of November 2016, lying above the first tercile. UPI exposure is defined as in Equation (1). Post is a dummy, which takes the value 1 from November 2016 onwards. Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Table 7
Benchmarking Aggregate Credit

	(1)	(2)	(3)
Lender type:	Fintechs & Banks	Fintech	Banks
Dependent Variable:	Amt (₹Million)		
UPI Exposure × Post	1.990***	0.053***	1.938***
	(0.147)	(0.005)	(0.143)
R^2	0.958	0.744	0.959
Pincode FE	Y	Y	Y
District-time FE	Y	Y	Y
Pre-UPI Mean	9.110	0.002	9.094
Post-UPI Mean	13.698	0.128	13.548
Dep. var mean	12.207	0.087	12.101
N	508840	504280	508840

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the difference-in-difference estimates for the impact of UPI exposure on total credit amount for both fintechs and banks (column 1) and separately for fintechs (column 2) and banks (column 3). Observations are at the pincode-month level for the period October 2015 to January 2019. The dependent variable is the total loan amount in ₹million. UPI exposure is defined in Equation (1). Post is a dummy, which takes the value 1 from November 2016 onward. Pincode-clustered standard errors are reported in parentheses.

Table 8
Impact on Default

Panel A: Univariate Analysis

Dependent variable:	Default Rate						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Low Exposure			High Exposure			DiD
	Pre	Post	Post-Pre	Pre	Post	Post-Pre	High-Low
				(i) Fintechs			
New-to-credit	0.064	0.086	0.022*	0.066	0.086	0.019***	-0.003
Subprime	0.135	0.105	-0.031**	0.130	0.108	-0.022***	0.009
Prime	0.043	0.048	0.005	0.026	0.049	0.023***	0.018***
				(ii) Banks			
New-to-credit	0.016	0.032	0.016***	0.017	0.033	0.016***	0.000
Subprime	0.042	0.064	0.022***	0.043	0.067	0.024***	0.002
Prime	0.011	0.026	0.014***	0.011	0.026	0.015***	0.001

Panel B: Impact on default rates

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Default Rate					
Lender Type:	FinTech			Banks		
Borrower category:	New-to-credit	Subprime	Prime	New-to-credit	Subprime	Prime
UPI Exposure × Post	-0.053*	0.006	0.010	0.000	0.001	-0.001
	(0.030)	(0.056)	(0.020)	(0.001)	(0.003)	(0.000)
R^2	0.269	0.326	0.258	0.148	0.172	0.157
Pincode FE	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	0.058	0.128	0.028	0.015	0.043	0.010
Post-UPI Mean	0.086	0.105	0.048	0.029	0.066	0.024
Dep. var mean	0.086	0.106	0.048	0.025	0.059	0.019
N	110912	58192	105843	476009	318377	489934

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the mean default rate at the pincode-month level for fintechs and banks (Panel A), and the difference-in-differences estimates for the differential impact of UPI exposure on default rates (Panel B). Default rate is defined as the number of defaults divided by total loans in a pincode-month. High and low exposure correspond to a dummy variable that identifies pincodes with above/below median exposure, defined as in Equation (1). Data spans the period October 2015 to January 2019. In Panel A, means for the pre versus post and high versus low exposure are as indicated. The difference between post and pre for low-exposure pincodes is shown in column 3. The difference between the post and pre for high exposure pincodes is shown in column 6. The difference-in-differences estimate for the univariate analysis in Panel A (column 6-column 3) is shown in column 7. In Panel B, Columns 1-3 show results for the subsample of fintech loans, and columns 4-6 show results for the subsample of bank loans. Each column corresponds to new-to-credit (columns 1 and 4), subprime (columns 2 and 5), and prime (columns 3 and 6) loans. The dependent variable is the default rate. Post is a dummy, which takes the value 1 from November 2016 onward. Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Breaking Barriers to Financial Access: Cross-Platform Digital Payments and Credit Markets

Appendix

A Digital verifiability of cashflows: Evidence from a single fintech lender

This section shows how fintechs incorporate UPI's digital transaction trails into their lending decisions based on loan-level data from a single fintech lender.

Loan-level data is from one of the largest Fintech firms in India, catering to very small merchants, such as roadside kiosks. This dataset provides rich data on borrower characteristics, the loan contract, and the lender's internal assessment of the borrower's credit risk profile, allowing us to pin down the mechanism facilitating credit access. The fintech firm focuses on streamlining transaction methods for small enterprises and provides an array of services through its smartphone application and QR code payment platform. These services enable partner merchants to use QR code stickers that customers can easily scan to complete transactions using a variety of digital payment methods, such as UPI, credit/debit cards, and digital wallets, essentially eliminating the need for physical point-of-sale (POS) terminals. This seamless mode of digital payments is valuable to both customers and merchants. The lending arm of the business targets small and medium-sized businesses to offer merchant cash advance (MCA) loans to its partner merchants. We obtain detailed loan-level information on loans granted to small informal roadside kiosks for the period January 2020–October 2023. We observe information on the date of the loan application, the pincode of the applicant, loan size, interest rate, lender-assigned internal credit scores, and the volume and value of transactions made through UPI from their QR platform.

We establish the direct link between UPI transactions and loan disbursement to supplement our main tests with this loan-level data. These tests also serve as an independent test of the external validity of our findings. This lender specializes in lending to small and micro enterprises, tracking all QR-code-based UPI transactions done by the kiosk using the lender's payment app. For each borrower, we obtain data on the value and frequency of UPI transactions, the sanctioned loan amount, the loan interest rate, and the internal credit score estimated by the lender's proprietary algorithm. Only a subset of the borrowers is assigned an internal credit score by the lender. Data spans the period 2020–2023. We examine the link between an individual's UPI transactions and credit outcomes using the specification:

$$Y_{it} = \alpha_{s(i)t} + \beta \times \mathrm{X} + \epsilon_{it}$$

for a merchant i belonging to a pincode $p(i)$ and state $s(i)$ in month t. Y_{it} takes the following values: loan amount sanctioned, the interest rate, a dummy for whether the lender assigned a borrower an internal credit score, and the lender's internal credit score. X takes the following values: Log of QR-UPI Transaction count$_{it}$ and Log of QR-UPI Transaction Values$_{it}$. $\alpha_{s(i)t}$ are state-time fixed effects. Standard errors are clustered by pincode.

Table A4 presents the results. The descriptive statistics in Panel A suggest a similar, though slightly lower, mean exposure of 0.47 relative to the baseline exposure mean of 0.49. Panel B, columns 1–4, show that the value and frequency of a kiosk's UPI transactions positively correlate with the loan size and negatively correlate with the interest rate. A smaller sample of these borrowers is also assigned an internal credit score by the lender. In columns 5–6, we examine whether the value and frequency of a kiosk's UPI transactions are associated with the likelihood of having an internal credit score. A one percent increase in the value or frequency of transactions is also positively associated with a one percent higher likelihood of being assigned an internal credit score. Finally, in columns 6–8, we restrict attention to the sample of borrowers with an internal credit score and again find a positive correlation between UPI transactions and credit score. For robustness, in Internet Appendix Table IA4 we repeat the tests in columns 1–4 only for the subsample of borrowers with an internal credit score. The results remain robust. Overall, these results are consistent with the idea that lenders are incorporating a digital cashflow trail created by UPI in their credit decisions.

Table A1
Variable Definitions and Common Terms

Variable	Definition
Unified Payments Interface (UPI)	An instant payment system set up by the National Payments Corporation of India (NPCI). It facilitates instant fund transfer between two bank accounts using mobile devices via payment applications.
Banks	Scheduled Commercial Banks comprising public sector banks and private sector banks.
Fintechs	CIBIL classification based on their operational structure.
Prime	Credit score bucket assigned by CIBIL, for borrowers with a score in the range 731 and above
Subprime	Credit score bucket assigned by CIBIL, for borrowers with scores in the range 300-680
New-to-Credit	Credit score bucket assigned by CIBIL, for borrowers who are taking a loan for the first time, and have no credit score.
UPI Exposure	Total deposit accounts by early UPI adopter banks as a share of total deposit accounts in a pincode for the year 2015.
Jan Dhan Yojana (JDY)	A financial inclusion scheme launched by the Government of India (GoI) in 2014. It aims to provide basic financial services like savings accounts, need-based credit, and insurance to the financially excluded and weaker sections of society. Services include zero-balance bank accounts, debit cards, and accidental insurance coverage
Reliance Jio	An Indian telecommunications company, launched in 2016, and is a provider of 5G, 4G+, and 4G mobile and internet services. It is the largest mobile network operator in the world. It provides multiple internet-related products like Jio 5G sim cards, Jio Fiber broadband internet, Jio cinema OTT platform, and so on.
$\text{Early}_{\text{Jio}}$	A dummy variable taking value 1 if the distance of the nearest Reliance Jio 4G tower from a pincode is less than 6 km, as of 2017 Q1.
$\text{Early}_{\text{Non-Jio}}$	A dummy variable taking value 1 if the distance of the nearest non-Reliance Jio 4G tower from a pincode is less than 6 km, as of 2017 Q1. A non-Reliance Jio tower is defined as any tower owned by Airtel, Vi, and BSNL 4G towers, which is the closest to the pincode.

Notes: This table defines variables and common terms used in the paper.

Table A2
Balance Tests for UPI Exposure

	(1)	(2)	(3)
Dependent Variable		UPI Exposure	
# New-to-credit accounts	0.0011	0.0011	0.0003
	(0.0008)	(0.0008)	(0.0009)
# Subprime accounts	-0.0014	-0.0017	-0.0018
	(0.0016)	(0.0016)	(0.0017)
# Prime accounts	0.0009	0.0010	0.0011
	(0.0008)	(0.0008)	(0.0008)
# Total accounts	-0.0004	-0.0004	-0.0003
	(0.0006)	(0.0006)	(0.0007)
Total population	-0.0284	-0.0298	-0.0295
	(0.0387)	(0.0386)	(0.0470)
Night light intensity growth		-0.0414	-0.0663
		(0.0370)	(0.0622)
Fraction of households with bank accounts			-0.0090
			(0.0373)
Fraction of households with credit cards			0.0769
			(0.0943)
Fraction of households with health insurance			-0.0281
			(0.0367)
Fraction of households with mobile phones			0.0398
			(0.0361)
Unemployment rate			-0.0436
			(0.0375)
R^2	0.0091	0.0094	0.0119
District FE	Y	Y	Y
N	12,721	12,444	5,641

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the correlation between UPI exposure as defined in Equation (1) and the pre-period (October 2015 to October 2016) means of several pincode-level characteristics. The population is as of 2016. Night light intensity growth as of 2015. The fraction of households with bank accounts, the fraction of households with credit cards, the fraction of households with health insurance, the fraction of households with mobile phones, and the unemployment rate come from the CMIE Consumer Pyramids Household Survey, and are as of 2016. All variables are winsorized at the 1st and 99th percentiles. Fixed effects are included as indicated. District-clustered standard errors are reported in parentheses.

Table A3
Impact on UPI

	(1)	(2)	(3)	(4)
Dependent variable:	# UPI in 1000s	₹UPI in Mn.	# UPI per 1000 capita	₹UPI per capita
UPI Exposure	1.544***	3.770***	25.427***	64.240***
	(0.118)	(0.276)	(3.815)	(8.498)
R^2	0.510	0.489	0.399	0.393
District-time FE	Y	Y	Y	Y
Dep. var mean	3.677	8.373	69.335	155.833
N	238350	238350	238300	238300

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the OLS estimates for the impact of UPI exposure on UPI transactions. Observations are at the pincode-month level and span the period January 2017-January 2019. The dependent variables in columns (1) and (2) are the total number of UPI transactions (in 1000s) and total amount of UPI transactions (in ₹million) respectively and columns (3) and (4) are the number of UPI transactions per 1000 capita and the amount of UPI transactions per capita (in ₹), respectively. For the per capita normalization, the population is as of 2016. UPI exposure is defined as in Equation (1). Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Table A4
Digital Verifiability of Cash flows: Evidence from a Large Fintech Lender

Panel A: Summary statistics

Variable	Mean	St. Dev.	p25	p50	p75
UPI					
UPI Exposure	0.47	0.34	0.12	0.5	0.85
Credit Variables					
Loan Size (in ₹000's)	109.94	124.07	30.00	70.00	140.00
Interest Rate (in %)	1.97	0.28	1.75	2.00	2.10
QR Transactions					
Log(Amount of QR Txns in a month)(in ₹)	9.78	1.45	9.09	9.92	10.69
Log(Count of QR Txns in a month)(in units)	5.29	1.56	4.45	5.49	6.36
Borrower Variables					
Data Reporting System Score (in units)	15.08	4.56	12.00	15.25	18.75
No Prior Formal Loans Dummy (0 to 1)	0.89	0.31	1.00	1.00	1.00
Repeat Borrower Dummy (0 to 1)	0.38	0.49	0.00	0.00	1.00
N		50,643			

Panel B: Loan-level analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Loan Size (in 000's)		Interest Rate (in %)		Internal Credit Score		Internal Credit Score Dummy	
Log(QR T.Value)	34.695***		-0.023***		1.533***		0.010***	
	(0.871)		(0.001)		(0.033)		(0.001)	
Log(QR T.Count)		27.904***		-0.019***		1.314***		0.011***
		(0.689)		(0.001)		(0.031)		(0.001)
R^2	0.166	0.140	0.106	0.104	0.239	0.224	0.933	0.933
State Time FE	Y	Y	Y	Y	Y	Y	Y	Y
Dep Var Mean	109.516	109.516	1.936	1.936	15.055	15.055	0.479	0.479
N	39602	39602	39602	39602	18973	18973	39602	39602

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the summary statistics for the loan level observations (Panel A) and presents evidence regarding the digital verifiability of cash flow through QR-based UPI transactions and credit outcomes using data from a large Fintech lender (Panel B). Panel A summarizes data for UPI Exposure, credit variables, QR Transaction variables, and borrower-level variables. The data covers the time period 2020 to 2023. Observations are at the loan level. In Panel B, the dependent variable in columns 1–2 is the loan size in thousands. The dependent variable in columns 3–4 is the interest rate in percent. The dependent variable in columns 5–6 is the internal credit score dummy that identifies customers who have been assigned an internal credit rating by the fintech lender. QR-UPI T.Value and QR-UPI T.Count are monthly QR-code-based UPI transaction values, and transaction frequency is at the borrower-month of the loan level. Data is for 2020-2023. Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Table A5
Determinants to Early Jio entry

	(1)	(2)
Dependent variable:	$\text{Early}_{\text{Jio}}$	
# New-to-credit accounts	0.001	0.002
	(0.001)	(0.002)
# Subprime accounts	-0.001	-0.002
	(0.003)	(0.003)
# Prime accounts	-0.001	-0.001
	(0.002)	(0.002)
# Total accounts	0.001	0.001
	(0.001)	(0.001)
Total Population		-0.350***
		(0.090)
Night Light Intensity growth		-0.441***
		(0.075)
R^2	0.048	0.060
District-time FE	N	N
N	12477	12755

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the correlation between the $\text{Early}_{\text{Jio}}$ dummy and the Pre period (October 2015 to October 2016) means of several other characteristics. $\text{Early}_{\text{Jio}}$ takes a value 1 when a pincode's distance to an active 4G Jio tower is less than 6 km, as of Q1 2017. Night light intensity growth as of 2015 and Population as of 2016. All variables are winsorized at the 1st and 99th percentiles. District-clustered standard errors are reported in parentheses.

Table A6
Robustness to Demonetization: Impact on Credit Access

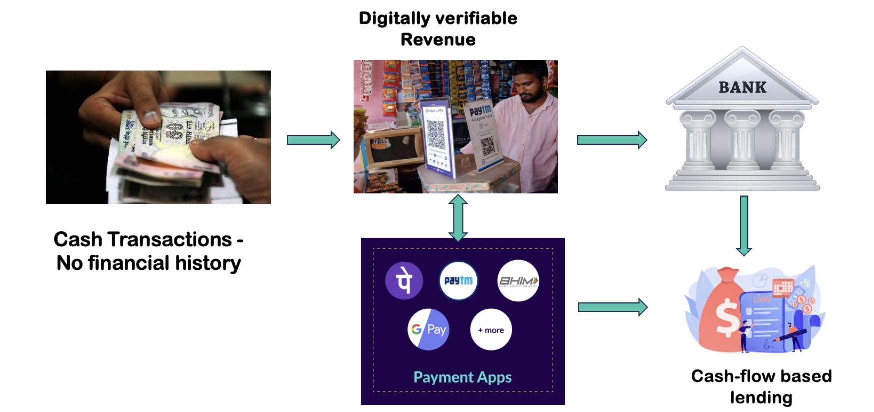
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	# of loans				# of loans per 1000 capita			
Borrower Category:	New-to-Credit	Subprime	Prime	Total	New-to-Credit	Subprime	Prime	Total
	Panel A: Fintech							
UPI Exposure × Post	0.762***	0.236***	0.948***	2.752***	0.012***	0.004***	0.017***	0.045***
	(0.063)	(0.022)	(0.078)	(0.228)	(0.001)	(0.000)	(0.002)	(0.005)
R^2	0.698	0.660	0.705	0.731	0.628	0.571	0.644	0.688
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
$Dist_{CC}$ × Month Control	Y	Y	Y	Y	Y	Y	Y	Y
Dep. var mean	1.329	0.444	1.547	4.688	0.022	0.007	0.027	0.080
N	504280	504280	504280	504280	504200	504200	504200	504200
	Panel B: Banks							
UPI Exposure × Post	0.076	0.388***	8.668***	10.947***	0.000	0.006***	0.137***	0.173***
	(0.111)	(0.046)	(0.789)	(1.049)	(0.003)	(0.001)	(0.017)	(0.021)
R^2	0.966	0.937	0.951	0.966	0.942	0.884	0.944	0.962
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
$Dist_{CC}$ × Month Control	Y	Y	Y	Y	Y	Y	Y	Y
Dep. var mean	18.773	4.291	43.010	77.476	0.327	0.079	0.797	1.419
N	508840	508840	508840	508840	508760	508760	508760	508760

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the difference-in-difference estimates for the impact of UPI exposure for fintechs (Panel A) and banks (Panel B) for the new-to-credit, prime, and subprime borrowers. Observations are at the pincode-month level for the period October 2015 to January 2019. The dependent variable is the number of loans in columns 1–4 and the loans per 1000 capita in columns 5–8. For the per capita normalization, the population is as of 2016. UPI exposure is defined as in Equation (1). Post is a dummy, which takes the value 1 from November 2016 onward. Fixed effects are included as indicated. $Dist_{CC}$ × Month Control is the interaction of the distance of a pincode to the nearest currency chest and month-year t. Pincode clustered standard errors are reported in parentheses.

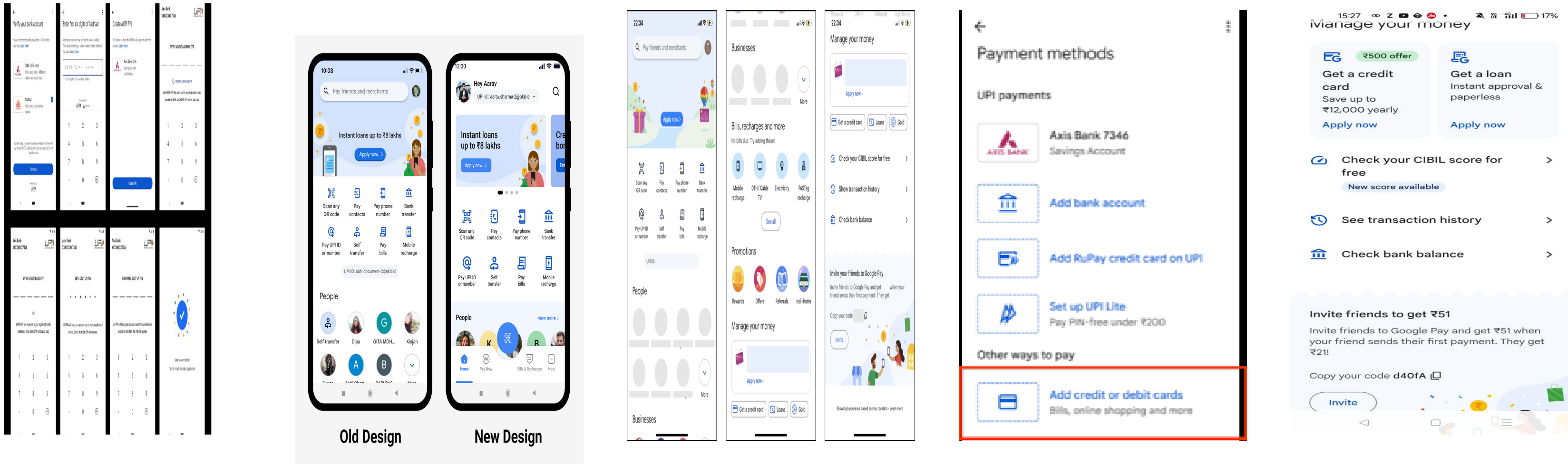
Internet Appendix

Figure IA1
UPI and Credit: Schematic Diagram



Notes: This figure is a schematic representation of how the introduction of UPI leads to an increased disbursal of credit. The leftmost box refers to a pre-UPI banking stage, where most transactions occur in cash, leading to a lack of documented history. Introducing payment apps based on UPI (bottom middle box) leads to digital verifiability of cash flows. UPI payments are often made through QR codes (top middle box). This information is consequently also available to lenders like banks (top rightmost box), who then use this information to determine creditworthiness and lend based on cash flows (bottom rightmost box).

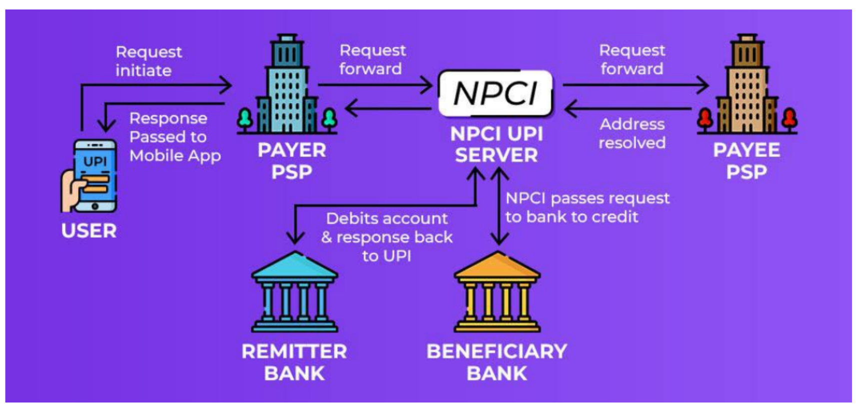
Figure IA2
UPI Loan Application Navigation Page



Panel A: Account Opening **Panel B: Landing Page** **Panel C: Google Pay Interface** **Panel D: Payment Method** **Panel E: Loan Application**

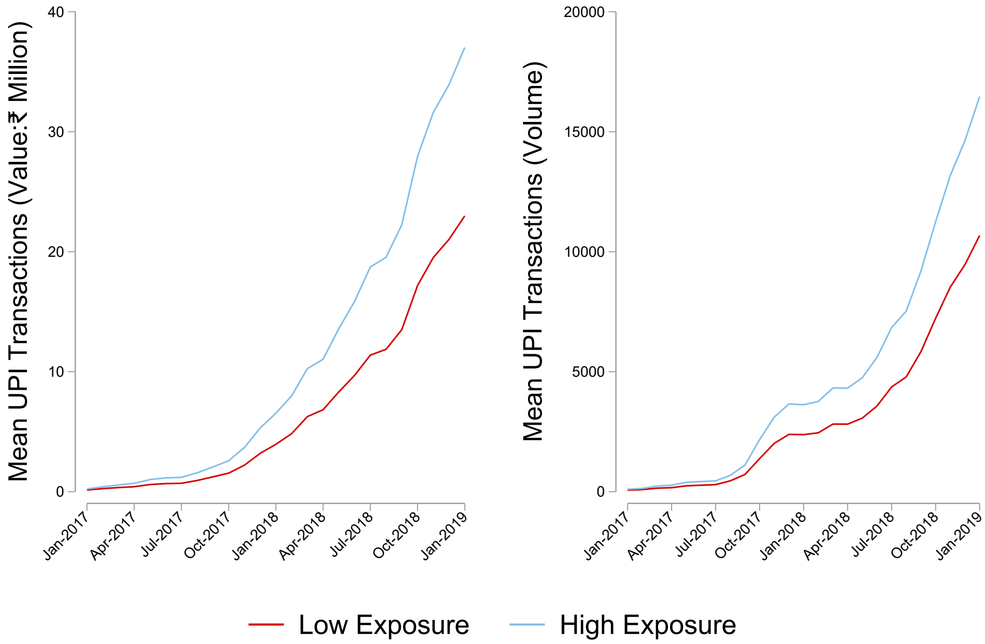
Notes: This figure shows the various stages of navigating UPI as a payment system. It begins with the process of account opening (Panel A), an illustrative landing page (Panel B), the interface of a payment system called Google Pay (Panel C), the various payment methods available (Panel D), and the option to apply for a loan (Panel E).

Figure IA3
UPI Payments: Flow Chart



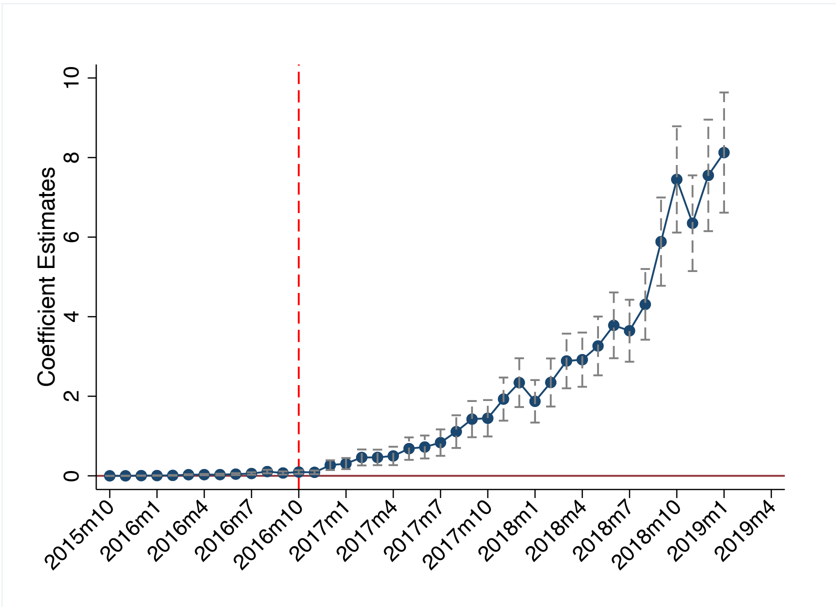
Notes: This figure shows the underlying technical infrastructure of UPI. The remitter and Beneficiary Bank refer to the sender and receiver bank, respectively. Payer and Payee PSP refer to the sender's and receiver's payment service provider. Source: IMF

Figure IA4
Trends in UPI Transactions by UPI Exposure

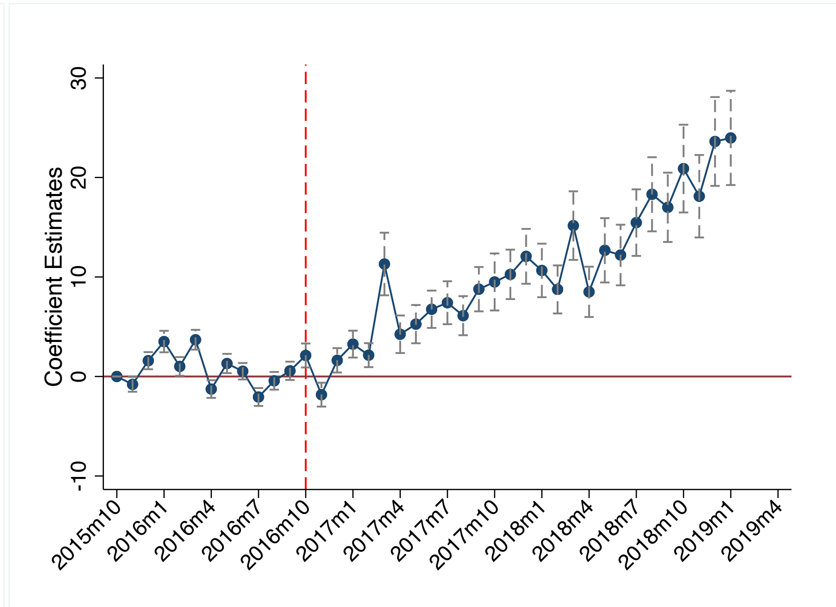


Notes: This figure shows the mean value of UPI Transactions (in ₹million) and the mean number of UPI Transactions for low UPI exposure pincodes (red line) and high UPI exposure pincodes (blue line). UPI exposure is defined as in Equation (1). High (low) UPI exposure corresponds to above (below) median UPI exposure. The data is at a monthly frequency and covers the period January 2017 to January 2019.

Figure IA5
Robustness With Grid Fixed Effect: Treatment Dynamics for the Impact on Credit Access



A: Fintech

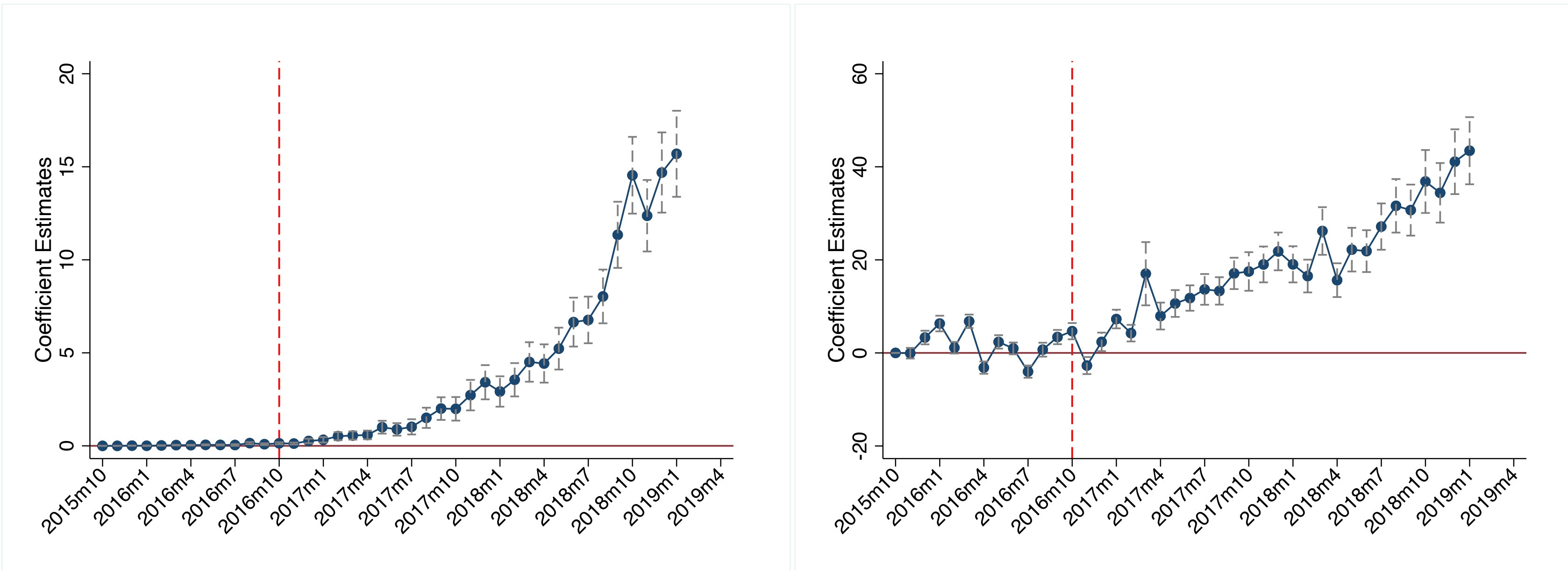


B: Banks

Notes: This figure shows the continuous treatment dynamics using the specification in Equation (4) for fintechs (Panel A) and banks (Panel B). The dependent variable is the number of loans. The continuous treatment used is the UPI Exposure as defined in Equation (1). Observations are at the pincode-month level for the period October 2015 to January 2019. Each point on the navy line shows the point estimate. The grey dotted lines indicate the 95% confidence intervals. Pincode, grid-month-year, and district-month-year fixed effects are included. Grids are constructed by dividing the Indian map into rectangular units of size 0.4×0.4 degrees and are more granular than districts. Standard errors are heteroskedasticity robust and clustered at the pincode level. The dashed red line marks the pre-treatment month (October 2016).

Figure IA6

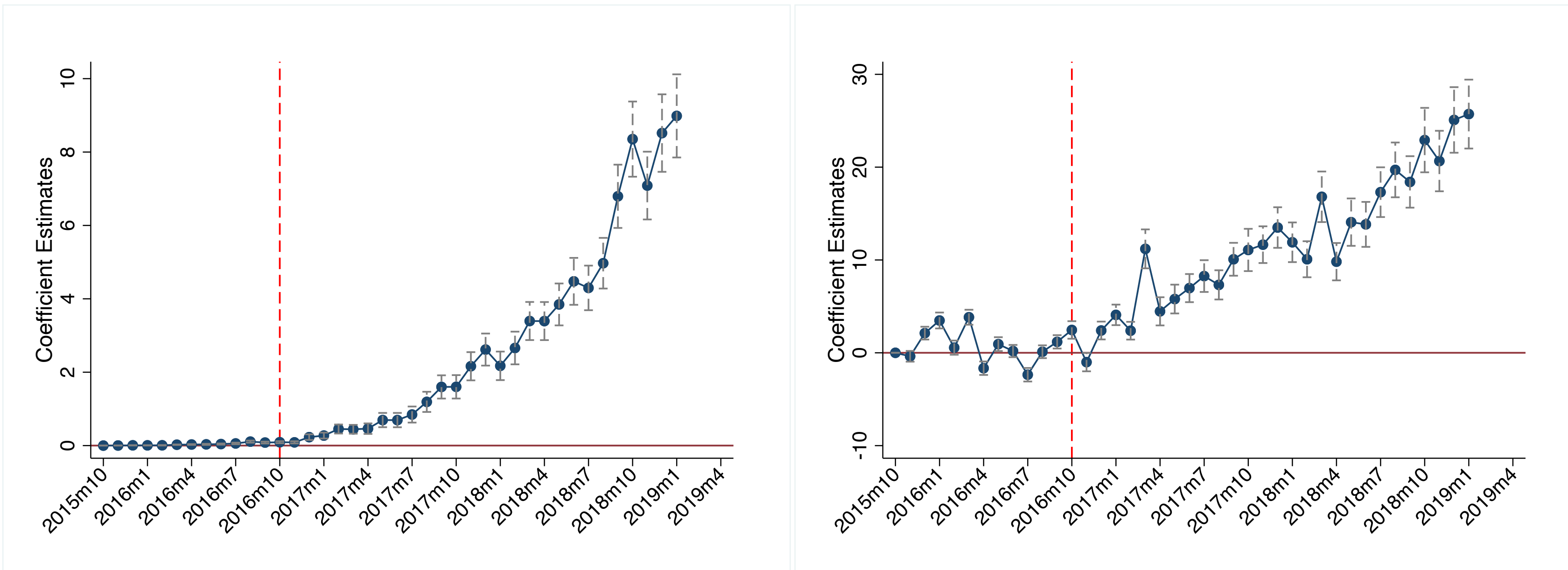
Robustness With Neighborhood pairs: Treatment Dynamics for the Impact on Credit Access



A: Fintech **B: Banks**

Notes: This figure shows the continuous treatment dynamics using the specification in Equation (4) for fintechs (Panel A) and banks (Panel B). We define a "pincode pair" as any two neighboring pincodes that share a boundary. Each pair is assigned a unique ID. For this analysis, we restrict the sample to pairs where the two pincodes have different levels of UPI exposure. The dependent variable is the number of loans. The continuous treatment used is the UPI Exposure as defined in Equation (1). Observations are at the pincode-month level for the period October 2015 to January 2019. Each point on the navy line shows the point estimate. The grey dotted lines indicate the 95% confidence intervals. Pincode and neighborhood-month-year fixed effects are included. Standard errors are heteroskedasticity robust and clustered at the pincode level. The dashed red line marks the pre-treatment month (October 2016).

Figure IA7
Robustness With Binary High UPI Exposure: Treatment Dynamics for the Impact on Credit Access



A: Fintech **B: Banks**

Notes: This figure shows the continuous treatment dynamics using the specification in Equation (4) for fintechs (Panel A) and banks (Panel B). The dependent variable is the number of loans. UPI Exposure as defined in (1). The treatment used is high UPI exposure, which takes a value of 1 for above median UPI exposure and 0 otherwise. Observations are at the pincode-month level for the period October 2015 to January 2019. Each point on the navy line shows the point estimate. The grey dotted lines indicate the 95% confidence intervals. Pincode and district-month-year fixed effects are included. Standard errors are heteroskedasticity robust and clustered at the pincode level. The dashed red line marks the pre-treatment month (October 2016).

Table IA1
Validation of Datasets

	RBI	NPCI	
	Bank Credit	UPI Transactions	
		Value	Volume
Bank Credit (CIBIL)	0.82	-	-
UPI Transactions (Value: Dataset)	-	0.97	-
UPI Transactions (Volume: Dataset)	-	-	0.97

Notes: This table reports the correlations between credit and UPI data and aggregate statistics at the same available from public sources. RBI reports aggregate data on outstanding consumer loans, while NPCI provides aggregate statistics on UPI transactions. The proprietary credit and UPI transaction data are aggregated to the country level, and the correlations with the numbers reported by RBI and NPCI are presented.

Table IA2
Summary Statistics

	Mean	Median	St. Dev
Loan amount (in ₹Million)	12.21	3.65	25.49
Fintech loan amount (in ₹Million)	0.09	0.00	0.34
Bank loan amount (in ₹Million)	12.11	3.63	25.15

Notes: This table presents the summary statistics for observations at the pincode-month level. The table summarizes data for the total loan amount in ₹Million separately for fintech and banks for the period October 2015–January 2019.

Table IA3
Impact on Credit: Economic Magnitudes

Dependent variable:	# of loans				# of loans per 1000 capita			
	New- -to-credit	Subprime	Prime	Total	New- to-credit	Subprime	Prime	Total
Fintech	45	20	44	42	33	20	32	26
Banks	0	0.12	0.32	20	0	0.11	0.26	0.16
Total	0.05	0.19	0.36	0.25	0.04	0.17	0.29	0.20

Notes: This table presents estimates of economic significance for regressions estimated in Equation 3 and presented in Table 3 and Table 4. Each number refers to the coefficient scaled by the pre-period mean. Rows correspond to lender type, and columns correspond to borrower category (new-to-credit, subprime, prime, and total).

Table IA4
Robustness for Digital Verifiability of Cash flows: Subsample of Loans with Internal Credit Scores

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Loan Size (in 000's)		Interest Rate (in %)		Internal Credit Score	
Log(QR T.Value)	39.731***		-0.030***		1.533***	
	(1.226)		(0.002)		(0.033)	
Log(QR T.Count)		33.430***		-0.028***		1.314***
		(0.945)		(0.001)		(0.031)
R^2	0.173	0.155	0.080	0.081	0.239	0.224
State Time FE	Y	Y	Y	Y	Y	Y
Dep Var Mean	106.355	106.355	1.892	1.892	15.055	15.055
N	18973	18973	18973	18973	18973	18973

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents evidence regarding the digital verifiability of cash flows through QR-based UPI transactions and credit outcomes using data from a large Fintech lender. Observations are at the loan level. Data is for 2020-2023. The dependent variable in columns 1–2 is the lender's loan size in thousands. The dependent variable in columns 3–4 is the interest rate in (%). The dependent variable in columns 5–6 is the internal credit score dummy that identifies customers who have been assigned an internal credit rating by the fintech lender. The dependent variable in columns 5–6 is the internal credit score. QR-UPI T.Value and QR-UPI T.Count are monthly QR-code-based UPI transaction values, and transaction frequency is at the loan-borrower-month level. Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Table IA5
Robustness With Grid Fixed Effects: Impact on UPI

	(1)	(2)	(3)	(4)
Dependent variable:	# UPI in 1000s	₹UPI in Mn.	# UPI per 1000 capita	₹UPI per capita
UPI Exposure	1.430***	3.501***	24.349***	61.966***
	(0.139)	(0.327)	(4.379)	(9.762)
R^2	0.586	0.566	0.482	0.479
District-time FE	Y	Y	Y	Y
Grid-time FE	Y	Y	Y	Y
Dep. var mean	3.683	8.387	70.228	157.763
N	231975	231975	231900	231900

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the OLS estimates for the impact of exposure on UPI transactions. Observations are at the pincode-month level for the period January 2017 to January 2019. The dependent variables in columns (1) and (2) are the total number of UPI transactions (in 1000s) and total amount of UPI transactions (in ₹million) respectively and columns (3) and (4) are the number of UPI transactions per 1000 capita and the amount of UPI transactions per capita (in ₹), respectively. For the per capita normalization, the population is as of 2016. UPI exposure is defined as in Equation (1). Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Table IA6
Robustness With Grid Fixed Effects: Impact on Credit Access

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	# of loans				# of loans per 1000 capita			
Borrower Category:	New-to-Credit	Subprime	Prime	Total	New-to-Credit	Subprime	Prime	Total
Panel A: Fintech								
UPI Exposure × Post	0.750***	0.230***	0.905***	2.665***	0.011***	0.004***	0.015***	0.042***
	(0.073)	(0.025)	(0.088)	(0.261)	(0.001)	(0.000)	(0.002)	(0.005)
R^2	0.740	0.705	0.755	0.775	0.676	0.620	0.698	0.737
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Grid-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	0.018	0.013	0.023	0.071	0.000	0.000	0.001	0.002
Post-UPI Mean	1.974	0.659	2.305	6.975	0.033	0.011	0.040	0.120
Dep. var mean	1.338	0.449	1.563	4.731	0.022	0.007	0.027	0.081
N	496640	496640	496640	496640	496600	496600	496600	496600
Panel B: Banks								
UPI Exposure × Post	0.074	0.358***	7.939***	10.072***	-0.002	0.006***	0.116***	0.145***
	(0.128)	(0.053)	(0.888)	(1.183)	(0.003)	(0.001)	(0.019)	(0.024)
R^2	0.970	0.944	0.958	0.971	0.947	0.892	0.951	0.966
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Grid-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	18.140	3.462	28.711	58.611	0.323	0.065	0.550	1.101
Post-UPI Mean	19.232	4.724	50.482	87.471	0.333	0.086	0.928	1.591
Dep. var mean	18.877	4.314	43.406	78.092	0.330	0.079	0.805	1.432
N	501040	501040	501040	501040	500920	500920	500920	500920

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the difference-in-differences estimates for the impact of UPI exposure for fintechs (Panel A) and banks (Panel B) for new-to-credit, subprime, prime, and total loans. Observations are at the pincode-month level for the period October 2015 to January 2019. The dependent variable is the number of loans in columns 1–4 and the loans per 1000 capita in columns 5–8. For the per capita normalization, the population is as of 2016. UPI exposure is defined as in Equation (1). Post is a dummy, which takes the value 1 from November 2016 onward. Fixed effects are included as indicated. Pincode clustered standard errors are shown in parentheses.

Table IA7
Robustness with Pincode Pairs: Impact on UPI

	(1)	(2)	(3)	(4)
Dependent variable:	# UPI in 1000s	₹UPI in Mn.	# UPI per 1000 capita	₹UPI per capita
UPI Exposure	2.548***	6.156***	23.585***	60.119***
	(0.201)	(0.484)	(4.234)	(9.552)
R^2	0.736	0.737	0.749	0.759
Neighbourhood-time FE	Y	Y	Y	Y
Dep. var mean	4.153	9.605	63.878	145.467
N	170100	170100	170000	170000

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the OLS estimates for the impact of exposure on UPI transactions. Observations are at the pincode-month level for the period January 2017 to January 2019. We define a "pincode pair" as any two neighboring pincodes that share a boundary. Each pair is assigned a unique ID. For this analysis, we restrict the sample to pairs where the two pincodes have different levels of UPI exposure. The dependent variables in columns (1) and (2) are the total number of UPI transactions (in 1000s) and total amount of UPI transactions (in ₹million) respectively and columns (3) and (4) are the number of UPI transactions per 1000 capita and the amount of UPI transactions per capita (in ₹), respectively. For the per capita normalization, the population is as of 2016. UPI exposure is defined as in Equation (1). Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Table IA8
Robustness With Pincode Pairs: Impact on Credit Access

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	# of loans				# of loans per 1000 capita			
Borrower Category:	New-to-Credit	Subprime	Prime	Total	New-to-Credit	Subprime	Prime	Total
	Panel A: Fintech							
UPI Exposure × Post	1.330***	0.413***	1.578***	4.679***	0.011***	0.004***	0.015***	0.042***
	(0.114)	(0.038)	(0.135)	(0.402)	(0.001)	(0.000)	(0.002)	(0.005)
R^2	0.868	0.859	0.882	0.889	0.864	0.835	0.883	0.899
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
Neighbourhood-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	0.023	0.018	0.032	0.095	0.000	0.000	0.001	0.002
Post-UPI Mean	2.604	0.876	3.117	9.330	0.034	0.011	0.042	0.125
Dep. var mean	1.765	0.597	2.114	6.328	0.023	0.008	0.029	0.085
N	428000	428000	428000	428000	427840	427840	427840	427840
	Panel B: Banks							
UPI Exposure × Post	-0.134	0.647***	14.594***	17.822***	-0.005	0.007***	0.138***	0.163***
	(0.219)	(0.090)	(1.310)	(1.779)	(0.003)	(0.001)	(0.018)	(0.023)
R^2	0.985	0.973	0.979	0.986	0.976	0.950	0.980	0.986
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
Neighbourhood-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	23.386	4.431	37.230	75.862	0.319	0.065	0.551	1.099
Post-UPI Mean	24.962	5.937	66.504	114.373	0.331	0.085	0.941	1.600
Dep. var mean	24.450	5.448	56.990	101.857	0.327	0.078	0.814	1.437
N	428000	428000	428000	428000	427840	427840	427840	427840

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the difference-in-differences estimates for the impact of UPI exposure for fintechs (Panel A) and banks (Panel B) on their new-to-credit, subprime, and prime loans. Observations are at the pincode-month level for the period October 2015 to January 2019. We define a "pincode pair" as any two neighboring pincodes that share a boundary. Each pair is assigned a unique ID. For this analysis, we restrict the sample to pairs where the two pincodes have different levels of UPI exposure. The dependent variable is the number of loans in columns 1–4 and the loans per 1000 capita in columns 5–8. For the per capita normalization, the population is as of 2016. UPI exposure is defined as in Equation (1). Post is a dummy, which takes the value 1 from November 2016 onward. Fixed effects are included as indicated. Pincode clustered standard errors are shown in parentheses.

Table IA9
Connectivity with Jio: Impact on UPI

	(1)	(2)	(3)	(4)
Dependent variable:	# UPI in 1000s	₹UPI in Mn.	# UPI per 1000 capita	₹UPI per capita
$\text{Early}_{\text{Jio}}$	1.542***	3.832***	27.602***	68.002***
	(0.090)	(0.212)	(2.187)	(4.827)
R^2	0.515	0.494	0.402	0.396
District-time FE	Y	Y	Y	Y
Dep. var mean	3.677	8.373	69.335	155.833
N	238350	238350	238300	238300

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the OLS estimates for the impact of early exposure to Jio towers on UPI transactions. Observations are at the pincode-month level for the period January 2017 to January 2019. The dependent variables in columns (1) and (2) are the total number of UPI transactions (in 1000s) and total amount of UPI transactions (in ₹million) respectively and columns (3) and (4) are the number of UPI transactions per 1000 capita and the amount of UPI transactions per capita (in ₹), respectively. For the per capita normalization, the population is as of 2016. $\text{Early}_{\text{Jio}}$ is 1 for pincodes with distance to a Jio tower less than 6 km, as of 2017 Q1. Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Table IA10
Robustness for Connectivity to Jio: Impact on Credit Access by Jio Subsamples

Panel A: Early Access	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	# of loans							
	FinTech				Banks			
Borrower Category:	New-to-Credit	Subprime	Prime	All	New-to-Credit	Subprime	Prime	All
UPI Exposure × Post	1.011***	0.353***	1.390***	3.904***	0.113	0.497***	12.188***	15.310***
	(0.108)	(0.039)	(0.137)	(0.399)	(0.192)	(0.081)	(1.415)	(1.877)
R^2	0.735	0.701	0.744	0.766	0.970	0.942	0.955	0.969
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	0.017	0.013	0.023	0.069	18.036	3.435	28.404	58.088
Post-UPI Mean	2.673	0.955	3.387	9.929	27.189	6.886	75.160	128.430
Dep. var mean	1.314	0.439	1.529	4.635	18.762	4.286	42.943	77.376
N	292800	292800	292800	294800	294800	294800	294800	

Panel B: Late Access								
Borrower Category:	New-to-Credit	Subprime	Prime	All	New-to-Credit	Subprime	Prime	All
UPI Exposure × Post	0.303***	0.055***	0.241***	0.837***	-0.083	0.172***	2.218***	2.802***
	(0.054)	(0.015)	(0.052)	(0.165)	(0.099)	(0.035)	(0.453)	(0.612)
R^2	0.565	0.422	0.471	0.563	0.900	0.832	0.881	0.918
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	0.017	0.013	0.023	0.069	18.036	3.435	28.404	58.088
Post-UPI Mean	0.962	0.230	0.741	2.704	7.892	1.655	15.166	28.946
Dep. var mean	1.314	0.439	1.529	4.635	18.762	4.286	42.943	77.376
N	207840	207840	207840	207840	210520	210520	210520	210520

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(Continued) Robustness for Connectivity to Jio: Impact on Credit Access by Jio Subsamples

Panel C: Early Access	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	# of loans per 1000 capita							
	FinTech				Banks			
Borrower Category:	New-to-Credit	Subprime	Prime	All	New-to-Credit	Subprime	Prime	All
UPI Exposure × Post	0.017***	0.006***	0.026***	0.069***	0.000	0.009***	0.200***	0.251***
	(0.002)	(0.001)	(0.003)	(0.008)	(0.005)	(0.002)	(0.030)	(0.038)
R^2	0.663	0.610	0.680	0.718	0.947	0.893	0.948	0.964
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	0.000	0.000	0.001	0.002	0.322	0.065	0.546	1.094
Post-UPI Mean	0.045	0.016	0.059	0.170	0.470	0.125	1.375	2.326
Dep. var mean	0.022	0.007	0.027	0.080	0.328	0.079	0.798	1.421
N	292800	292800	292800	294800	294800	294800	294800	

Panel D: Late Access

Borrower Category:	New-to-Credit	Subprime	Prime	All	New-to-Credit	Subprime	Prime	All
UPI Exposure × Post	0.004***	0.001*	0.003***	0.010***	-0.002	0.003**	0.031***	0.039**
	(0.001)	(0.000)	(0.001)	(0.004)	(0.003)	(0.001)	(0.012)	(0.015)
R^2	0.516	0.365	0.449	0.566	0.896	0.773	0.888	0.934
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	0.000	0.000	0.001	0.002	0.322	0.065	0.546	1.094
Post-UPI Mean	0.016	0.004	0.013	0.046	0.138	0.031	0.293	0.548
Dep. var mean	0.022	0.007	0.027	0.080	0.328	0.079	0.798	1.421
N	207840	207840	207840	207840	210520	210520	210520	210520

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the difference-in-differences estimates for the differential impact of UPI exposure on fintechs and banks in pincodes with subsamples of early (Panels A and C) and late access (Panels B and D) to a Reliance Jio tower. Columns 1–4 present the estimates for fintechs, and columns 5–8 present estimates for banks for new-to-credit (columns 1 and 5), subprime (columns 2 and 6), prime (columns 3 and 7), and total loans (columns 4 and 8). Observations are at the pincode-month level for the period October 2015 to January 2019. The dependent variable is the number of loans in columns 1–4 and the loans per 1000 capita in columns 5–8. For the per capita normalization, the population is as of 2016. UPI exposure is defined as in Equation (1). Early (Late) Jio refers to subsamples when a pincode's distance to an active 4G Jio tower is less (more) than 6 km, as of 2017 Q1. Fixed effects are included as indicated. Pincode clustered standard errors are shown in parentheses.

Table IA11
Robustness for Bank Account Access: Impact on Credit Access by JDY subsamples

Panel A: High JDY	(1)	(2)	(3)	(4)
Dependent variable:	# of New-to-Credit loans		# of New-to-Credit loans per 1000 capita	
Lender Type:	Fintech	Banks	Fintech	Banks
UPI Exposure × Post	1.021***	0.163	0.016***	0.005
	(0.096)	(0.178)	(0.002)	(0.004)
R^2	0.738	0.969	0.684	0.956
Pincode FE	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y
Pre-UPI Mean	0.026	25.159	0.001	0.391
Post-UPI Mean	2.617	26.448	0.039	0.401
Dep. var mean	1.775	26.029	0.026	0.398
N	301480	303080	301400	303000

Panel B: Low JDY				
Lender Type:	Fintech	Banks	Fintech	Banks
UPI Exposure × Post	0.185***	-0.022	0.004**	-0.005
	(0.061)	(0.122)	(0.002)	(0.004)
R^2	0.672	0.941	0.542	0.903
Pincode FE	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y
Pre-UPI Mean	0.005	7.499	0.000	0.217
Post-UPI Mean	0.985	8.279	0.023	0.226
Dep. var mean	0.667	8.025	0.016	0.223
N	199280	202000	199280	202000

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the difference-in-differences estimates for the differential impact of UPI exposure for fintechs and banks in pincodes with high JDY bank accounts (Panel A) and low JDY bank accounts (Panel B). Observations are at the pincode-month level for the period October 2015 to January 2019. The dependent variable is the number of new-to-credit loans in columns 1 and 2 and loans per 1000 capita in columns 3 and 4. For per capita normalization, the population as of 2016 is used. UPI exposure is defined as in Equation (1). High (low) JDY refers to pincodes where the cumulative number of JDY bank accounts is above (below) the first tercile as of November 2016. Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Table IA12
Robustness With Binary High UPI Exposure: Impact on UPI

	(1)	(2)	(3)	(4)
Dependent variable:	# UPI in 1000s	₹UPI in Mn.	# UPI per 1000 capita	₹UPI per capita
High UPI Exposure	1.481***	3.659***	21.170***	53.115***
	(0.092)	(0.221)	(2.554)	(5.769)
R^2	0.515	0.494	0.400	0.394
District-time FE	Y	Y	Y	Y
Dep. var mean	3.677	8.373	69.335	155.833
N	238350	238350	238300	238300

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the OLS estimates for the impact of exposure on UPI transactions. Observations are at the pincode-month level for the period January 2017 to January 2019. The dependent variables in columns (1) and (2) are the total number of UPI transactions (in 1000s) and total amount of UPI transactions (in ₹million) respectively and columns (3) and (4) are the number of UPI transactions per 1000 capita and the amount of UPI transactions per capita (in ₹), respectively. For the per capita normalization, the population is as of 2016. High exposure is a dummy variable that identifies pincodes with above-median exposure, defined using the continuous measure in Equation (1). Fixed effects are included as indicated. Pincode-clustered standard errors are reported in parentheses.

Table IA13
Robustness With Binary High UPI Exposure: Impact on Credit Access

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	# of loans				# of loans per 1000 capita			
Borrower Category:	New-to-Credit	Subprime	Prime	Total	New-to-Credit	Subprime	Prime	Total
	Panel A: Fintech							
High UPI Exposure × Post	0.823***	0.264***	1.027***	3.010***	0.011***	0.004***	0.015***	0.041***
	(0.054)	(0.019)	(0.067)	(0.196)	(0.001)	(0.000)	(0.001)	(0.004)
R^2	0.695	0.658	0.702	0.728	0.627	0.570	0.642	0.686
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	0.018	0.013	0.023	0.070	0.000	0.000	0.001	0.002
Post-UPI Mean	1.960	0.652	2.281	6.911	0.032	0.011	0.040	0.118
Dep. var mean	1.329	0.444	1.547	4.688	0.022	0.007	0.027	0.080
N	504280	504280	504280	504280	504200	504200	504200	504200
	Panel B: Banks							
High UPI Exposure × Post	-0.071	0.390***	9.091***	11.214***	-0.001	0.006***	0.123***	0.156***
	(0.102)	(0.040)	(0.704)	(0.940)	(0.002)	(0.001)	(0.013)	(0.017)
R^2	0.966	0.937	0.951	0.966	0.942	0.884	0.943	0.962
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y
District-time FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-UPI Mean	18.045	3.440	28.447	58.159	0.321	0.065	0.544	1.092
Post-UPI Mean	19.124	4.700	50.021	86.777	0.330	0.086	0.918	1.576
Dep. var mean	18.773	4.291	43.010	77.476	0.327	0.079	0.797	1.419
N	508840	508840	508840	508840	508760	508760	508760	508760

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the difference-in-differences estimates for the impact of UPI exposure for fintechs (Panel A) and banks (Panel B) for new-to-credit, subprime, and prime credit. Observations are at the pincode-month level for the period October 2015 to January 2019. The dependent variable is the number of loans in columns 1–4 and the loans per 1000 capita in columns 5–8. For the per capita normalization, the population is as of 2016. High exposure is a dummy variable that identifies pincodes with above-median exposure, defined using the continuous measure in Equation (1). Post is a dummy, which takes the value 1 from November 2016 onward. Fixed effects are included as indicated. Pincode clustered standard errors are shown in parentheses.