Interoperable Payment Infrastructure and Retail Investment*

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ABSTRACT

Does eliminating payment frictions improve retail investor outcomes, or merely accelerate trading? We study India's 2016 launch of the Unified Payments Interface (UPI), a real-time, interoperable payment system that sharply reduced liquidity frictions between bank and brokerage accounts, using comprehensive data on 19.8 million retail investors. Exploiting cross-sectional variation in exposure to early-adopting banks, we show that a one-standard-deviation increase in UPI exposure raises monthly trading activity by 6.2% and active investors by 8.7%. These effects are driven by cross-platform interoperability rather than digitization alone: controlling for a closed, single-bank platform leaves UPI's impact largely intact, while the platform's independent effect is an order of magnitude smaller. Frictionless payments enable rapid responses to market stress, expand small-value transactions, and facilitate reallocation of idle cash balances into equity markets. However, long-horizon abnormal returns are negative across all experience groups, and the surge in retail participation does not generate persistent mispricing or broad volatility effects. Overall, inter-operable payment infrastructure expands retail participation and reshapes trading dynamics without improving long-run investor outcomes or materially impairing price efficiency.

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1 Introduction

The architecture of payment systems—particularly whether they enable instantaneous, low-friction movement of funds—has become increasingly consequential for financial markets. A growing number of countries have introduced real-time payment infrastructure, yet empirical evidence on how such systems affect trading behavior, market quality, and investor welfare remains limited. The central open question is whether reducing liquidity frictions improves allocative efficiency or instead encourages speculative, short-horizon trading that may degrade market outcomes. Unlike studies that emphasize financial inclusion or access to financial services, our focus is on the behavioral and market-level consequences of frictionless liquidity for retail trading and price formation.

This paper provides the first comprehensive evidence on how large-scale payment infrastructure affects trading behavior and market outcomes. India's Unified Payments Interface (UPI) offers a particularly powerful setting to study these effects. Launched in 2016 by the National Payments Corporation of India (NPCI), UPI enables real-time, 24/7 fund transfers across accounts held at different banks and brokerage platforms, regardless of the front-end application used, as transactions are routed through a common interoperable switch. By eliminating settlement delays that previously constrained retail investors' ability to respond to short-term price movements, UPI sharply compresses the time between cash availability and trade execution — representing a large and plausibly exogenous reduction in liquidity frictions. At the same time, its rapid and uneven adoption across regions creates substantial cross-sectional variation in exposure to this shock.

Using comprehensive retail trading data from the National Stock Exchange covering 19.8 million investors over 2015–2020, we document four main findings. First, exposure to UPI-enabled payment infrastructure generates a substantial increase in retail trading activity: a one-standard-deviation increase in UPI exposure raises monthly trading volume by 6.2% and the number of active investors by 8.7%. These effects are driven by cross-platform interoperability rather than digitization alone. Second, despite this expansion, we find no evidence that easier liquidity access improves investor performance — long-horizon abnormal returns are negative across experience groups. Third, we identify three complementary mechanisms: reduced funding frictions that enable rapid responses to market stress, disproportionate expansion of small-value transactions, and reallocation of idle cash balances into equity markets. Fourth, at the stock level, UPI exposure raises trading volume and price impact in retail-heavy stocks, but markets absorb the retail participation shock without persistent mispricing or broad volatility effects.

Our empirical strategy exploits cross-sectional variation in pre-existing exposure to banks that integrated early into UPI, interacted with the post-2016 period, within a difference-in-differences framework that leverages banks' heterogeneous geographic presence. We construct pincode-level measures of UPI exposure using regulatory bank-level deposit data from the Reserve Bank of India, defining exposure as the share of total deposits held by early UPI-adopting banks as of 2015. This dual variation, across time and geography, helps address potential confounding from

broader policy changes, as banks made UPI adoption decisions at the institution level rather than based on regional characteristics.[1]

Identifying the causal effects of UPI requires addressing the possibility that adoption is correlated with digital readiness or institutional characteristics. We address this concern using three complementary strategies. First, comprehensive balance tests reveal no systematic pre-treatment differences across demographic and economic characteristics. Second, we implement within-investor analyses that compare trading behavior across accounts linked to early- versus late-adopting banks, analogous to the approach in Khwaja and Mian (2008). Third, we exploit regional variation in bank holidays during which equity markets remain open while traditional banking channels are closed but UPI remains operational. On bank holidays, when conventional settlement systems are unavailable, the marginal effect of UPI exposure on trading activity becomes flatter, compressing the high–low exposure gap. This convergence pattern is consistent with substitution toward UPI when traditional funding channels are temporarily shut, supporting a liquidity and settlement friction mechanism rather than reflecting fixed cross-sectional differences in digital infrastructure.[2]

As a baseline result, pincodes with greater UPI exposure experience significantly higher retail trading activity following UPI's 2016 adoption. A one-standard-deviation increase in pincode-level UPI exposure increases the number of transactions by 69 per month and the number of active investors sevenfold. Event-study evidence reveals no pre-trends, supporting the parallel trends assumption.

Next, we provide evidence that interoperability, rather than digitization alone, drives our results. We compare UPI with the State Bank of India's proprietary digital platform, YONO, which offers real-time digital payments but operates within a closed, single-bank ecosystem.[3] After controlling for YONO exposure, UPI's estimated effect remains economically large and statistically significant, while YONO's independent effect is an order of magnitude smaller. These patterns point to a distinct role for open, interoperable payment architecture in facilitating retail trading activity.[4]

Having established that interoperability drives the participation effects, we next examine

[1] This approach works because UPI adoption requires having an account at a participating bank, and digital payment networks exhibit strong externality effects (e.g. Crouzet et al. (2023), Higgins (2022), Fafchamps et al. (2022)), creating persistent differences between areas served by early versus late adopters.

[2] We focus on 2015–2017 for this analysis, when traditional settlement systems such as NEFT and RTGS operated only during business hours and were closed on weekends and holidays.

[3] Both platforms provide real-time digital payment functionality. However, they differ in institutional scope and network reach. More recently, the updated YONO 2.0 app launched in 2025 allows both SBI and non-SBI customers to transfer funds; this expansion occurred after our sample period.

[4] Consistent with this interpretation, we observe an additional increase in UPI-induced trading activity following the Reserve Bank of India's September 2017 circular that strengthened interoperability by allowing large merchants and technology platforms to connect through multiple Payment Service Provider banks. This regulatory change expanded cross-platform access within the UPI network and sharply increased the scope for instantaneous, multi-bank fund transfers.

whether frictionless liquidity improves investor performance. To address this, we examine portfolio choices and abnormal returns across investors with varying exposure to UPI. We find no evidence that easier access to funds improves long-run investment outcomes. While trading intensity rises, particularly among more experienced investors, long-horizon abnormal returns are negative across the experience distribution, and changes in risk-taking and diversification are economically modest. These findings indicate that interoperability lowers funding frictions and expands activity, but does not translate into sustained performance gains.

We identify three complementary mechanisms through which open architecture transforms market participation: The first operates through reductions in transaction frictions that enable rapid responses to market events. Using high-frequency trading data from the Bombay Stock Exchange during 12-hour windows before and after two major flash crashes (September 11, 2019, and March 12, 2020), we show that trading activity increases disproportionately in high-UPI exposure pincodes during periods of extreme market stress. This finding demonstrates how instant settlement capabilities enable rapid market response that would be impossible under traditional banking constraints.

The second mechanism reflects the trading behavior of small-value investors. We observe a significant increase in the number of transactions and active traders below low monthly trading-value thresholds in high-UPI exposure areas. While this expansion raises overall trading volume, it is disproportionately concentrated in small-value trades, consistent with a reduction in the fixed costs of incremental capital deployment rather than a shift toward larger, long-horizon portfolio accumulation.[5]

Finally, we find evidence that UPI alters household liquidity management in ways that increase exposure to equity markets. Because UPI usage requires active bank account engagement, its diffusion is associated with a broader shift toward digital financial activity. We show that the effects of UPI on stock market trading are significantly stronger in regions with high ex ante cash usage, proxied by per-capita ATM withdrawals. These results suggest that frictionless payments facilitate the redeployment of liquid balances into market trading, particularly in areas where households previously relied heavily on cash.

Overall, we interpret our estimates as capturing the behavioral and allocative consequences of the interoperable UPI ecosystem rather than a narrow change in payment routing technology. The 2017 NPCI and RBI circulars jointly expanded payment interoperability, multi-bank access, and data-sharing APIs, enabling seamless cross-bank and cross-platform usage. Our empirical setting therefore reflects the bundled architectural reform that users and platforms experienced in practice — and that policymakers designing instant payment systems ultimately choose between.

Our paper makes three main contributions. First, we build on the foundational work of Dubey and Purnanandam (2023), who develop the bank-geography-based measure of UPI exposure that

[5]Following Lee and Radhakrishna (2000) and Malmendier and Shanthikumar (2007), we define small investors as those whose monthly transaction values fall in the bottom 30 percent of the trading value distribution in a given month.

we adopt. A key distinction from that line of work is that UPI's interoperability serves two functions: payments for goods and services, and fund transfers across financial accounts. The UPI-credit literature — including Alok et al. (2024) and Cramer et al. (2024) — primarily draws on the payments channel, showing how digitized transactions facilitate credit creation. Our paper is built entirely around the fund-transfer dimension, which reduces frictions in moving funds from bank accounts to brokerage accounts. This channel is critical for trading activity across both developed and developing economies, and although the exposure measure is shared, the economic mechanisms, empirical tests, and welfare implications are distinct.

Second, we contribute to research on payment systems and financial infrastructure, which emphasizes how architectural choices shape competition, efficiency, and stability (Duffie (2019); Parlour et al. (2022); Goldstein et al. (2022); He et al. (2023)). While this literature has focused primarily on institutional effects and system-level outcomes, we demonstrate how these architectural choices cascade down to affect individual household investment decisions. Copestake et al. (2025) show that UPI's open architecture reshapes competition in India's financial sector by enabling third-party apps and modular service provision. At a macro level, recent work has examined how instant payment systems transform financial intermediation: Ouyang (2021) analyzes cashless payments (Alipay) and inclusion, Ghosh et al. (2022) study fintech lending, and Sarkisyan (2023) shows how Brazil's Pix affects deposit competition. Liang et al. (2024) demonstrates that instant payments amplify monetary policy transmission through deposits, while Gonzalez et al. (2024) document that Pix increases banks' liquidity demands and risk-taking incentives. Hong et al. (2020) link fintech adoption to higher household participation in mutual funds, showing how digital payments spill over into investment behavior. Our results extend this nascent literature by providing the first systematic evidence on how payment architecture specifically affects participation and behavior in equity markets. Our results demonstrate that interoperability fundamentally changes how and when households engage with risky financial assets.

Finally, our results connect to the behavioral finance literature on retail trading. Classic studies show that individuals trade excessively, under-diversify, and under-perform (e.g., Odean (1999); Barber and Odean (2000)), while more recent work finds that zero-commission platforms trigger attention-driven trading and losses (Barber et al. (2022)) and that information frictions shape retail behavior (Huang et al. (2021)). We extend this literature by showing that open payment infrastructure produces similar effects through a different channel: by reducing frictions, UPI broadens participation but amplifies the very trading mistakes highlighted in prior research. This suggests that the behavioral costs of reduced frictions extend beyond commissions to payment architecture itself. In contrast, Huang et al. (2025) show that digital nudging and education can mitigate behavioral inertia; our findings underscore how access without knowledge support may instead exacerbate problematic trading.

2 Institutional Context and Data

Before the advent of UPI, retail trading in Indian equity markets was shaped by institutional frictions that increased settlement delays, transaction costs, and timing risk for retail investors. As seen in Figure A1 of the Appendix, transfers of funds into brokerage accounts relied primarily on the National Electronic Funds Transfer (NEFT) system, which processed transactions in hourly batches during limited banking hours and imposed settlement delays of one to two business days. Real-Time Gross Settlement (RTGS) allowed same-day transfers but carried high minimum thresholds (INR 2 lakh, about $2,400) and substantial fees, making it impractical for most retail trading activity.

These frictions were particularly relevant because brokerage and banking relationships were institutionally separate for the majority of retail investors. Using comprehensive account-level data, Chebotarev et al. (2025) document that only 28.8% of investors hold accounts with bank-affiliated brokerages. The remaining investors trade through standalone brokerage platforms that require interbank transfers to move funds between savings and trading accounts. Even among bank-affiliated brokerages, customers' primary savings accounts may be held at a different bank, again necessitating interbank transfers. While transfers between a bank and its own brokerage arm were typically quicker and free, such vertically integrated access was therefore limited in practice. As a result, most investors had to move funds across institutions prior to trading, facing delays, fees, and missed opportunities during volatile periods.

The organizational separation of banking and securities services compounded these frictions, requiring investors to maintain parallel relationships with banks and brokers through repeated documentation, in-person verification, and ongoing transaction costs. Outside major urban centers, investors often had to physically visit bank branches or broker offices to initiate trades, further constraining immediacy. The process for subscribing to initial public offerings was especially cumbersome, often requiring paper forms alongside demand drafts or checks. Together, these barriers reduced trading frequency and flexibility, particularly during periods of market volatility when immediacy was most valuable. In this environment, the introduction of an always-on, real-time, and interoperable payment rail, capable of seamlessly linking any bank account to any brokerage, had the potential to materially alter the timing and ease with which retail investors could deploy capital.

2.1 The Unified Payments Interface (UPI)

Against this backdrop of fragmented payment infrastructure, India's Unified Payments Interface represents a fundamental architectural shift toward open, interoperable systems. Launched in 2016 by the National Payments Corporation of India (NPCI), UPI has achieved unprecedented scale, processing over 16 billion transactions as of December 2024.[6] While instant payment sys-

[6]See Payment Systems Report 2024*, a bi-annual publication of the Reserve Bank of India.

tems have emerged globally, including Brazil's Pix and the United States' FedNow, UPI distinguishes itself through deep integration with securities trading infrastructure that directly addresses the pre-existing frictions in account funding and trade execution.

UPI's architecture comprises three distinct layers: (1) a settlement layer operated by NPCI, (2) a bank interface layer where Payment Service Providers (PSPs) connect to the system, and (3) a user interface layer featuring both bank-owned and third-party applications like Google Pay and PhonePe. Unlike closed-loop payment systems such as China's Alipay, Kenya's M-Pesa, or the United States' Zelle, which restrict transactions within proprietary networks, UPI operates as an open protocol enabling interoperability across banks, fintech applications, and trading platforms.

This open architecture directly transforms the trading experience through three key innovations. First, UPI enables instantaneous trading account funding, eliminating the 1-2 day settlement delays that previously created substantial opportunity costs during volatile market periods. The system also allows investors to block funds in their bank accounts for specific trades rather than transferring large sums to brokerage accounts upfront, enabling funds to earn interest until needed while reducing transaction costs. Figure A1 in the Appendix illustrates the contrast between traditional transfers and UPI-enabled flows.

Second, UPI provides a compliant bridge between the organizationally separated banking and securities operations through a streamlined three-step verification protocol, eliminating the need for parallel documentation and relationships.

Third, UPI has revolutionized the IPO subscription process. The One Time Mandate (OTM) system, introduced by SEBI in 2019 and integrated with UPI, allows retail investors to apply for IPOs using their UPI ID, with funds blocked, but not debited, in their bank account until share allotment. This eliminated the reliance on paper forms and demand drafts, significantly streamlining the process and reducing liquidity and execution frictions in primary market participation.[7]

Taken together, these innovations extend beyond incremental convenience. By dismantling long-standing frictions in payments, account linkage, and IPO participation, UPI has fundamentally altered the speed, cost, and flexibility with which retail investors can deploy capital in equity markets.

2.1.1 UPI vs. Other Instant Payment Systems

Instant payment systems represent a fundamental shift in payment infrastructure, enabling real-time, 24/7 settlement of transactions. These systems have emerged globally with varying implementation approaches, regulatory frameworks, and adoption rates. This section contextualizes UPI's adoption by comparing it with other major global payment systems.

In the United States, payment settlement has traditionally relied on batch-processing systems

[7] Figure A2 in the Appendix, drawn from the RBI's *Payment Systems Report*, documents the increasing adoption of UPI-enabled IPO applications.

with settlement times of 1-3 business days. The launch of FedNow in July 2023 introduced 24/7 instant payments, yet adoption has remained limited, particularly among major banks.[8] Its decentralized, voluntary implementation contrasts sharply with India's approach. Unlike the U.S., India mandated UPI integration for all banks with over 10 million customers, accelerating system-wide adoption. Additionally, whereas FedNow primarily facilitates bank-to-bank transfers, UPI operates as a holistic payment ecosystem, integrating banks, third-party apps (e.g., Google Pay, WhatsApp), and merchants through a unified framework.

In contrast, Brazil's Pix, started in 2020 shares several similarities with UPI. Both systems were developed by national payment authorities – NPCI in India and the Central Bank of Brazil for Pix – ensuring standardized implementation and broad adoption. They also employ simple, user-friendly identifiers (Taxpayer ID/phone numbers for Pix, Virtual Payment Address for UPI), reducing reliance on complex banking details. Consequently, both UPI and Pix have achieved significant penetration in their respective markets.

Despite this commonality, UPI differs from Pix in several ways. UPI separates the customer interface from the account-holding institution, allowing third-party applications such as Google Pay or PhonePe to initiate transactions directly from users' bank accounts. For example, a customer with an SBI account can use Google Pay to transfer funds to an HDFC account seamlessly. By contrast, Pix is integrated exclusively within banks' own applications, and transactions must be initiated through the user's banking interface. This architectural distinction gives UPI a broader and more competitive ecosystem, in which multiple banks and third-party providers interoperate on a common platform. Pix is also not natively integrated into brokerage account funding or securities trading workflows, which limits its relevance for studying retail trading behavior and market microstructure.

Other prominent instant payment systems include the European Union's SEPA Instant (2017), the UK's Faster Payments Service (2008), and Singapore's PayNow (2017). While these systems enable real-time payments, they primarily facilitate bank-to-bank transfers and do not support the kind of cross-platform, broker-integrated ecosystem that characterizes UPI.

UPI's distinctive contribution lies in its transformation of retail market participation mechanisms. By eliminating traditional payment frictions and establishing direct connectivity with trading infrastructure, UPI has fundamentally altered how retail investors access capital markets. This integration represents more than a technological advancement; it constitutes a structural change in trading infrastructure with direct implications for retail trading behavior, liquidity dynamics, and price formation.

[8] RTP (Real-Time Payments), launched in 2017 by The Clearing House, is a private, closed-network instant payment system operated by major U.S. banks. While it enables real-time settlement between participating banks, neither RTP nor FedNow provides native integration with brokerage accounts or trading platforms, in contrast to UPI's direct connectivity with securities market infrastructure.

2.2 UPI Exposure Measure

We measure local exposure to UPI using the pincode-level index developed in Alok et al. (2024), which captures pre-determined cross-sectional variation in exposure to early-adopting banks based on the geographic distribution of bank deposits prior to UPI's 2016 launch. This measure builds on the identification rationale in Dubey and Purnanandam (2023) and has been shown to predict subsequent digital transaction adoption at the ZIP-code level in India (Alok et al., 2024).

The construction of the index relies on two institutional features. First, access to UPI requires that an individual's bank participates in the UPI platform. Because banks joined UPI at different points in time, bank-level adoption generated plausibly exogenous variation in UPI availability across locations. Second, digital payment technologies exhibit strong network externalities (Higgins, 2022; Crouzet et al., 2023), implying that UPI usage is higher in areas served by banks that adopted UPI early and already maintained a large local deposit base.

Following Alok et al. (2024), UPI Exposure for pincode p is defined as the share of deposits held by early adopter banks relative to total bank deposits in that pincode, measured prior to the introduction of UPI:

$$\text{UPI Exposure}_p = \frac{\text{Total Deposits of Early Adopter Banks}_p}{\text{Total Deposits of All Banks}_p}. \tag{1}$$

Early adopter banks are those that were live on the UPI platform as of November 2016.[9] Bank-branch–level deposit data are obtained from the Reserve Bank of India's Basic Statistical Returns (BSR). We map bank branches to pincodes and aggregate deposits to the bank–pincode level using data as of March 31, 2016, the last fiscal year-end preceding UPI's widespread rollout.

This index exploits two sources of variation. First, UPI adoption occurred at the bank level rather than the branch or pincode level, generating cross-sectional variation in exposure that is orthogonal to local demand conditions. While early-adopting banks may differ systematically from late adopters, the geographic distribution of their deposits across pincodes is largely predetermined and not chosen in response to local trading conditions. This is particularly relevant in the Indian banking system, where lending and deposit-taking are dominated by a limited number of nationally or state-level operating scheduled commercial banks rather than narrowly localized institutions. Importantly, pincodes are defined by the Indian Postal Service rather than by administrative or political boundaries. As a result, policy interventions and regulatory programs – typically implemented at the district or state level – do not mechanically align with pincode boundaries, reducing concerns that differential policy exposure confounds our estimates.

Second, the measure leverages cross-pincode variation in the historical deposit shares of early and late adopter banks. The intuition is that pincodes in which early adopter banks were dominant

[9] The list of early adopter banks is published by the Government of India. The number of banks live on UPI increased from 21 in August 2016 to 362 by April 2023.

prior to UPI's launch were mechanically more exposed to UPI-enabled fund-transfer capabilities once the platform became operational. As documented in Alok et al. (2024), this exposure strongly predicts subsequent UPI transaction intensity, validating the index as a measure of differential access to UPI infrastructure.

We rely on this exposure measure as our primary specification because it is constructed entirely using pre-UPI balance-sheet information and is therefore orthogonal to contemporaneous shocks affecting cash usage, including the November 2016 demonetization episode.[10]

As a complementary approach, we also construct a time-varying Bartik-style instrument:

$$\text{UPI Bartik}_{p,t} = \text{National UPI}_t \times \frac{\text{UPI}_p}{\text{GDP}_p}, \tag{2}$$

where National UPI_t captures aggregate growth in UPI transactions over time, UPI_p denotes the total number of UPI transactions in pincode p as of September 2017, and GDP_p proxies for local economic activity using night-light intensity. This specification isolates plausibly exogenous time variation in UPI intensity while allowing for heterogeneous exposure across pincodes. We examine the identifying parallel-trends assumption underlying the Bartik design in Section 3. All Bartik measures are normalized to have mean zero and unit standard deviation.

FIGURE 1 ABOUT HERE

Figure 1 provides maps on the geographical distribution of the UPI exposure and UPI Bartik measure acorss different pincodes of India. From both maps, we see that pincodes in the central part of India have higher UPI exposure as indicated by the brighter colors for both measures. Table 1 shows that the average UPI exposure across pincodes is 0.65, indicating that, on average, early-adopter banks accounted for 65% of total bank deposits within a pincode in the pre-UPI period. This measure ranges from 0 to 1, reflecting substantial heterogeneity in early-adopter bank presence across Indian pincodes.

In Figure 3, we show that high-exposure pincodes exhibit higher UPI usage, validating our exposure measure.

2.3 Trading Data

We use data on the universe of individual investors' daily trading activity compiled by the National Stock Exchange of India over the period 2015 to the first quarter of 2020. We restrict our sample to individual investor accounts and trading of domestic stocks. For each trade, we have the key elements of a stock transaction, including the date of the transaction, account type, tickers traded, the number of shares purchased or sold, and the execution price. The NSE data also contains demographic details on the investors including each investor's gender, age, and residential pincodes. Following the procedure in Agarwal et al. (2026), we match the pincodes in the NSE

[10] Because the exposure index is based on deposit shares measured prior to UPI's introduction, it does not mechanically load on changes in payment behavior induced by demonetization.

dataset to the official list of post office pincodes published by the Indian government.[11]

2.3.1 Pincode-level Measures

As our main measures of stock market participation, we construct the following two variables: *Number of Transactions*, defined as the total number of trades within a pincode during a specific year-month, and *Number of Investors*, defined as the total number of active investors within a pincode during the same period. Active investors are those with recorded trading activity in the given month. The summary statistics in Table 1 show that each pincode averages approximately 1,108 transactions per month, involving 81 active investors.

Figure 2 illustrates the time trends of the average *Number of Transaction* and *Number of Investors* over our sample period. The figure shows that between 2015 and 2020, the monthly average number of transactions increased by 71.7% from 1254 to 2153 while the monthly average number of investors participating in the stock market also increased by 85.7%.

FIGURE 2 ABOUT HERE

To examine how these trading patterns correlate with UPI adoption, we classify pincodes into High UPI exposure (median and above) and Low UPI exposure (below median) pincodes. Figure 3 plots the difference in the number of transactions and investors between High and Low UPI exposure pincodes over time. The figure shows that the gap between high- and low-exposure areas widens over time, suggesting a positive correlation between UPI adoption and increased market participation.

FIGURE 3 ABOUT HERE

2.3.2 Investor-level Measures

As measures of market participation outcomes, we construct the following measures: First, we calculate the Buy-and-Hold Return (BHR) for different time horizons (1, 10, 25, and 140 trading days) as follows:

$$BHR_{S,i,d,D} = \frac{Price_{S,d+D} - Price_{S,d}}{Price_{S,d}} \tag{3}$$

Where $Price_{S,d(+D)}$ indicates the closing price for stock S on day d + D, where D equals 1, 10, 25, or 140. To isolate performance net of aggregate market movements, we further calculate abnormal returns (excess BHR) as:

$$ABRET_{i,s,d,D} = \mathbb{I}[Buy = 1|Sell = -1]_{S,i,d} \cdot (BHR_{i,s,d,D} - MR_{d,D}) \tag{4}$$

for investor i buying or selling stock s on day d over horizon D, where $MR_{d,D}$[12] denotes the market

[11] See https://data.gov.in/catalog/all-india-pincode-directory

[12] The market return data is directly calculated using the NIFTY 50, a stock market index of 50 of the largest Indian

buy-and-hold return over the same horizon. $\mathbb{I}[Buy = 1|Sell = -1]_{S,i,d}$ equals 1 if investor i executes a *buy* transaction for stock S on day d, and equals -1 if the transaction is a *sell*. This symmetric construction follows Barber et al. (2009) and ensures that buy and sell trades are benchmarked consistently. Finally, the monthly average abnormal return for each investor is calculated as the net-value–weighted average of transaction-level abnormal returns across all trades in that month, computed separately for each horizon D. We express abnormal returns in basis points (bps) to facilitate economic interpretation.

Next, to examine shifts in portfolio composition, we construct two measures: (1) *Risk Taking*, which is calculated as the ratio of the number of transactions in risky assets over the total number of transactions per investor-month. A security is classified as risky if the standard deviation of its price is above the median of all traded stocks in the market for that month. (2) *Portfolio Diversification*, which is constructed as a Herfindahl-Hirschman Index (HHI)-based measure, following Koch et al. (2021):

$$\text{Portfolio Diversification}_{i,t} = 1 - \sum_{T} \left(\frac{Turnover_{i,S,t}}{\sum_T Turnover_{i,S,t}}\right)^2 \tag{5}$$

where $Turnover_{i,S,t}$ is the summation of buy value and sell value that investor i traded on security S at month t.

Finally, we measure *Trading intensity* along both within-month and across-month margins. Within a month, we construct the number of active trading days (*No. of Active Days*) and the total number of trades executed by the investor (*No. of Trades*). We calculate *Cross-month Trading Gap* as the average length of time (days) between consecutive trades, including both within-month gaps and cross-month gaps between the last trade of the previous trading month to the first trade of the current month of trading.

Table 1 shows that at the individual investor level, the buy-and-hold returns (BHR), are on average negative for all windows and most negative for the 140-day window, suggesting that investors incur losses over the long term. The average risk taking is 0.791 suggesting that 79% of the transactions are in risky assets. The average representative investor maintains a diversified portfolio (Portfolio HHI of 0.564), with an average interval of 7.9 days between consecutive transactions.

TABLE 1 ABOUT HERE

2.4 Validating the UPI Exposure

A potential concern with the UPI Exposure measure is that pincodes with higher ex-ante UPI exposure differ systematically from lower-exposure regions along economic or financial dimensions prior to UPI adoption. To assess this, Table 2 documents pre-adoption differences across high- and low-exposure pincodes over the period January 2015 to October 2016. We report differences

companies listed on the National Stock Exchange.

in local economic activity (proxied by night-light intensity), stock-market participation (number of transactions and number of active investors), and pre-period growth in these outcomes. Growth rates are constructed using $\log(1+y)$ transformations, and missing growth observations are left as missing rather than imputed as zero. All reported t-statistics are based on standard errors clustered at the pincode level.

High- and low-exposure pincodes differ in pre-UPI levels of economic and trading activity, consistent with differential financial development. However, the identifying assumption for our difference-in-differences design is the absence of differential pre-trends. Table 2 shows that pre-period growth rates in both transactions and active investors are economically small and statistically indistinguishable across exposure groups. This is further supported by the event-study evidence presented below.

TABLE 2 ABOUT HERE

At the investor level, demographic differences are modest in magnitude: investors in high-exposure pincodes are slightly younger and marginally less likely to be female. The magnitudes are small relative to overall variation. Overall, the evidence indicates that while UPI exposure is correlated with baseline market development, it is not associated with differential pre-adoption trends in trading activity.

Appendix Table A3 reports analogous balance tests for the Bartik-based UPI exposure measure. Consistent with the results above, the Bartik exposure is not correlated with pre-period economic activity, trading outcomes, or investor demographics, supporting its use as a complementary source of identifying variation.

3 Main Results

3.1 Does UPI Exposure Increase Retail Trading Activity?

In this section, we examine whether areas with greater ex-ante exposure to UPI-enabled fund-transfer infrastructure experience changes in retail trading activity following the rollout of UPI in 2016. Using a classic Difference-in-Difference (DiD) framework, we estimate the following equation:

$$Y_{p,d,t} = \alpha_{d,t} + \gamma_p + \beta \cdot \text{Post} \times \text{UPI Exposure}_p + \varepsilon_{p,d,t} \tag{6}$$

where $Y_{p,d,t}$ represents either the *Number of transactions* or the *Number of active investors* in pincode p (in district d) in month t. UPI Exposure_p is a time-invariant measure of ex-ante exposure to UPI-enabled fund-transfer infrastructure in pincode p, as described in Section 2. Post_t is an indicator equal to one in November 2016 and thereafter, and zero otherwise. All specifications include pincode fixed effects, γ_p, which control for time-invariant local characteristics, and district–month fixed effects, $\alpha_{d,t}$, which absorb district-level policy changes and macroeconomic shocks that vary over time. Standard errors are clustered at the pincode level to account for serial correlation in the

dependent variable.

Because additional banks joined the UPI network over time, our design does not compare permanently treated and untreated groups. Instead, it estimates the differential impact of early exposure intensity following the national introduction. In this sense, the coefficient on $Post \times UPI\ Exposure$ captures the incremental effect of earlier and deeper integration into the UPI-enabled ecosystem.

As an alternative estimation, we replace the time-invariant UPI Exposure measure with the time-varying Bartik instrument for UPI adoption by using the following equation:

$$Y_{p,d,t} = \alpha_{d,t} + \gamma_p + \beta \cdot \text{UPI Bartik}_{p,t} + \varepsilon_{p,d,t} \tag{7}$$

where $\text{UPI Bartik}_{p,t}$ is the Bartik instrument, as defined in the previous section, with all other variables as described above.

Columns 1 and 2 of Table 3 report results using the continuous UPI Exposure measure, while Columns 3 and 4 use the UPI Bartik exposure. Column 1 shows that a one–standard deviation increase in UPI Exposure is associated with an average increase of 69 stock-market transactions per pincode–month. Relative to the sample mean of 1,108 transactions (Table 1), this corresponds to a 6.2% increase. Column 2 shows that the same increase in exposure is associated with an average increase of 7 active investors per month, representing an 8.7% increase relative to the mean of 81 investors.[13]

Columns 3 and 4 present estimates using the UPI Bartik measure, which captures differential exposure to the national UPI rollout. A one-unit increase in Bartik exposure is associated with an average monthly increase of 113 stock-market transactions and 13 active investors. To provide a more economically meaningful comparison, moving from the 25^{th} to the 75^{th} percentile of Bartik exposure is associated with an average increase of approximately 10 transactions and 1 investor per pincode–month, respectively.

TABLE 3 ABOUT HERE

The identifying assumption in the empirical specification above is that the treated and control pincodes would have been on similar trends absent the treatment. To justify this assumption, we estimate the dynamic effects of UPI exposure on retail trading activity to ensure there are no pre-trends in our outcome variable before the adoption of UPI. Specifically, we estimate a similar specification to equation 6 using the following equation:

$$Y_{p,d,t} = \alpha_{d,t} + \gamma_p + \sum_{k=q} \beta_k \cdot \mathbb{I}[Quarter = k] \times \text{UPI Exposure}_p + \varepsilon_{p,d,t} \tag{8}$$

where $\mathbb{I}[Quarter = k]$ is a series of indicator variables that take the value of 1 in each quarter, with

[13] Economic effects are reported for a one–standard deviation increase in UPI Exposure, obtained by scaling the estimated coefficients by the standard deviation of exposure (0.347; Table 1).

the quarter when UPI was adopted omitted as the reference period.

Figure 4 shows the dynamic effects by plotting the coefficient estimates of the indicator variables described in equation 8. For each of the dependent variables, the coefficients become positive and significant only in the quarter of the UPI adoption and thereafter, with the maximum effect in the 5th quarter after UPI adoption. While the magnitude of the coefficients slightly diminish after that, they are still positive and significant, suggesting persistent increases in trading activity following the introduction of UPI-enabled fund transfers.

FIGURE 4 ABOUT HERE

The role of UPI's interoperability. We next examine whether UPI's interoperability, that is, its open, multi-bank architecture, is the mechanism driving the increase in trading activity. Figure 4 traces the dynamic effects around September 2017, when the RBI strengthened interoperability by introducing a multi-bank Payment Service Provider (PSP) framework. Under this reform, third-party application providers (e.g., Google Pay, Paytm) were permitted to connect to UPI through multiple sponsor banks rather than a single bank.[14] As shown in Figure 4, the effects become more pronounced following the introduction of the multi-PSP framework, consistent with cross-platform interoperability further reducing frictions in funding and executing trades.

To distinguish interoperability from general digital banking capabilities, we compare UPI's impact against State Bank of India's proprietary digital banking platform, YONO. Unlike UPI's open, cross-bank architecture, YONO operates within a closed banking ecosystem and primarily serves SBI customers. This comparison allows us to test whether our baseline results reflect interoperability specifically, rather than generic digitization.

We restrict the sample to trading activity in SBI-linked accounts and construct pincode-level measures of YONO exposure analogous to our UPI exposure measure. We use both a value-based measure (transaction value in INR millions) and a volume-based measure. The specification mirrors Equation (6), replacing $Post \times UPIExposure$ with UPI exposure and YONO exposure jointly in the post-period sample beginning November 2017 when both YONO and UPI are present:

$$Y_{p,d,t} = \alpha_{d,t} + \beta_1 \cdot \text{UPI Exposure}_p + \beta_2 \cdot \text{Yono Exposure}_{p,t} + \varepsilon_{p,d,t} \tag{9}$$

Table 4 presents the results. Across all specifications, UPI exposure remains economically large and statistically significant after controlling for YONO exposure. In the value-based specification, moving from the 25$^{\text{th}}$ to the 75$^{\text{th}}$ percentile of UPI exposure is associated with a 26–28% increase in the number of transactions and investors. The corresponding interquartile effect for YONO exposure is approximately 5%. Even moving from the 10$^{\text{th}}$ to the 90$^{\text{th}}$ percentile, the implied effect of YONO exposure (13–21%) remains substantially smaller than that of UPI exposure (50–55%).[15]

[14] See NPCI Circular No. 32/2017-18.

[15] Economic magnitudes are computed using interquartile-range (IQR) shifts in exposure and scaled by the sample

In the volume-based specification, YONO exposure is statistically significant but economically modest. An interquartile increase in YONO volume is associated with a 3–4% increase in transactions and investors, compared to roughly 28–29% for UPI exposure. Importantly, the inclusion of YONO exposure does not materially attenuate the UPI coefficient. Thus, while proprietary digital intensity correlates with trading activity, its magnitude is an order of magnitude smaller than the effect associated with interoperable UPI exposure.

Since this analysis restricts the sample to SBI-linked trading, it also mitigates concerns that cross-bank differences in fundamentals are driving the results. Taken together, the YONO comparison suggests that the documented increase in retail trading activity is not driven by banks' general digital sophistication. Instead, the evidence points to a distinct and economically large role for interoperable payment architecture in facilitating retail participation in equity markets.

TABLE 4 ABOUT HERE

Robustness. Our preferred specification uses level outcomes because our estimand is the absolute expansion of trading activity induced by UPI. Level regressions quantify the increase in the number of transactions and active investors attributable to digital payment adoption, directly aligning with our objective of measuring how overall market activity changed following UPI adoption.

We conduct several robustness exercises in the Appendix to address concerns related to estimating effects in levels. Table A4 re-estimates the baseline on a balanced pincode panel to ensure results are not driven by entry or exit over time. Panel A retains the full distribution of outcomes, while Panel B caps extreme observations. The estimated effects remain economically and statistically similar across both panels. Table A5 scales outcomes by local population (per 1,000 residents), showing that results are not simply reflecting differences in market size. Table A6 estimates the specification separately by baseline outcome quintiles; effects are present across the pre-period distribution and are not driven solely by high-activity locations.

We also estimate proportional specifications using the inverse hyperbolic sine (asinh) transformation and Poisson pseudo–maximum likelihood with high-dimensional fixed effects (PPML-HDFE). These models retain zero outcomes and are well suited for skewed, non-negative data. However, zero outcomes constitute less than one percent of the sample, so the baseline level specifications are not materially driven by censoring at zero. Importantly, proportional models esti-

mean of the dependent variable. In the value-based specification for transactions (Column (1)), the 25^{th} and 75^{th} percentiles of UPI exposure are 0.427 and 0.959, implying an IQR of 0.532. Multiplying this by the estimated coefficient (619.036) yields an increase of 329.3 transactions per pincode-month. Relative to the sample mean of 1,232.8 transactions, this corresponds to a 26.7% increase. For investors (Column (2)), the IQR is 0.533. Multiplying this by the coefficient 49.239 yields an increase of 26.2 investors; scaled by the sample mean of 94.254 investors, this corresponds to a 27.8% increase. For YONO exposure (value-based), the 75^{th} percentile is 0.405 for transactions and 0.401 for investors (the 25^{th} percentile is zero in both cases). Multiplying 0.405 by the transaction coefficient in Column (1) (156.734) yields 63.5 additional transactions, corresponding to 5.15% of the mean. Using 0.401 and the investor coefficient in Column (2) (11.856) yields 4.8 additional investors, or 5.04% of the mean.

mate percentage rather than absolute treatment effects and therefore address a different question: how UPI affected proportional growth in trading activity conditional on baseline scale. Because treatment effects are heterogeneous across the pre-period distribution, pooled proportional estimates can differ from level specifications when high-activity pincodes exhibit smaller percentage responses relative to their baseline scale.

Appendix Table A7 shows that the asinh specification applied to unscaled outcomes produces negative average coefficients. However, once outcomes are normalized by their pre-period median, the asinh estimates become positive and statistically significant, indicating that baseline scale materially affects proportional estimands. Consistent with this interpretation, quintile-wise PPML regressions in Appendix Table A8 reveal positive and statistically significant proportional effects in lower-activity pincodes, with attenuated effects in the highest-activity quintile. Log specifications estimated by quintile (unreported) yield similar patterns. Taken together, these results indicate that UPI generated meaningful absolute increases in trading activity alongside heterogeneous proportional responses across the baseline distribution.

Placebo tests. As further robustness, we conduct two types of placebo tests. First, we randomly reassign UPI exposure across pincodes while maintaining the original exposure distribution. Specifically, we estimate equation (6) 500 times, each time with a randomly shuffled assignment of UPI exposure values across pincodes. We then compare the distribution of these placebo coefficients with our main results. As reported in Appendix Table A9, the average coefficient from these placebo regressions is close to zero and statistically insignificant, suggesting that our main findings are not driven by spurious correlations in the data.

Next, we conduct a placebo test examining whether UPI adoption affects institutional investor trading. While institutional investors may benefit from UPI's infrastructure in certain operational aspects, they should not experience the same changes in retail trading outcomes that we observe among retail investors. Institutional trading is typically characterized by large, planned transactions executed through dedicated channels rather than the small, frequent, and occasionally opportunistic trades that define retail activity. We repeat our baseline analysis from Table 3, replacing retail investor activity with institutional investor trading as the dependent variable. Appendix Table A10 shows that consistent with our hypothesis, there is no significant relationship between UPI exposure and institutional trading patterns in the post-adoption period. The coefficients on $UPI\ Exposure \times Post$ are statistically insignificant and economically small across all specifications.

The placebo tests provide additional evidence that our results are not driven by broader market trends, regional economic developments, or other confounding factors that would likely affect all investor types.

3.2 Identification using within-investor variation

To strengthen our causal interpretation, we address potential investor-level confounders by examining trading behavior across multiple brokerage accounts held by the same investor. For this test, we turn to proprietary investor-level data that allows us to identify whether a brokerage account is associated with an early UPI-adopting bank. If UPI-enabled fund transfers reduce frictions in trading, we expect that the same investor would execute more transactions through accounts linked to early UPI-adopting banks compared to their other accounts. We test this hypothesis using the following specification:

$$Y_{i,b,t} = \alpha_{i,t} + \gamma_b + \beta \cdot \text{Post}_t \times \text{Early Adopter}_b + \varepsilon_{i,b,t} \tag{10}$$

where Y represents the number of transactions by investor i through brokerage account b in month t. *Early Adopter* is an indicator that equals 1 if brokerage account b is associated with an early UPI-adopting bank. *Post* indicates the post-UPI adoption period starting from November 2016. The coefficient of interest, β captures the differential response in transactions for the same investor for the brokerage account linked to the early-adopter bank compared to the late-adopter bank.

TABLE 5 ABOUT HERE

Table 5 examines whether UPI exposure is associated with higher trading activity within investors who operate multiple brokerage accounts. Column 1 uses the broadest sample, consisting of all investors who held two or more brokerage accounts at any point during the sample period (2015–2020). Controlling for time-invariant investor characteristics (investor fixed effects), local economic conditions (district–month fixed effects), and brokerage-specific features (broker fixed effects), accounts linked to early UPI-adopting banks exhibit an average of 1.337 additional monthly transactions per account relative to non-early-adopter accounts held by the same investor.

Column 2 restricts the sample to investors who maintained two or more brokerage accounts in each month, providing a more stringent test focused on consistently active multi-account investors. The estimated effect remains similar in magnitude, though slightly smaller, at 1.294 additional transactions per account–month, indicating robustness to this stricter sample restriction.

Most stringently, Column 3 includes investor–month fixed effects, allowing us to compare trading activity across different accounts held by the same investor within the same month. Even under this demanding specification, accounts linked to early UPI-adopting banks exhibit significantly higher trading activity, with an estimated increase of 0.412 transactions per account–month. This specification, analogous to the approach in Khwaja and Mian (2008), controls for all investor-level demand shocks and time-varying investor characteristics, ensuring that the estimated effects are not driven by differences in investor demand or local economic conditions.

3.3 Identification using regional variation in bank holidays

A related identification concern is that early UPI-adopting banks may possess superior technological infrastructure, greater resources, or more innovative practices that independently drive higher trading activity among their customers. If such inherent bank characteristics explain our results, then the estimated effects would reflect persistent differences across banks rather than UPI-enabled interoperability.

To address this concern, we exploit regional variation in bank holidays when stock markets remain open.[16] During these holidays, traditional banking channels are closed while UPI remains operational across all banks.

We focus this analysis on the 2015–2017 period, when conventional settlement systems such as NEFT and RTGS operated only during business hours and were closed on weekends and bank holidays. In this institutional setting, UPI was the only real-time, always-on payment rail available for interbank transfers. In later years, NEFT and RTGS were gradually expanded toward 24×7 availability (NEFT in late 2019 and RTGS in 2020), reducing the sharpness of holiday-related funding constraints. Restricting attention to the earlier period therefore provides the cleanest setting to isolate the value of interoperable, always-on settlement infrastructure.

If bank quality were the primary driver, the relative advantage of exposure to technologically superior (often private) banks should persist or even widen on holidays, when offline rails are unavailable. We formally evaluate this prediction using the following triple-difference specification:

$$\begin{aligned} Y_{p,d,D} = \alpha_{d,t} + \gamma_p + \delta_D + \theta_{s,D} + \beta_1 \cdot \text{Bank Holiday}_{p,D} + \beta_2 \cdot Post \times \text{Bank Holiday}_p + \\ \beta_3 \cdot Post \times \text{UPI Exposure}_p + \beta_4 \cdot \text{UPI Exposure}_p \times \text{Bank Holiday}_{p,D} + \\ \beta_5 \cdot \text{UPI Exposure}_p \times Post \times \text{Bank Holiday}_{p,D} + \varepsilon_{p,d,D} \end{aligned} \quad (11)$$

Where: $Y_{p,d,D}$ represents either the number of transactions or investors in pincode p, district d, at day D; *Bank Holiday* is an indicator for bank holidays and other variables are as before. $\alpha_{d,t}$ represents district-month fixed effects; γ_p represents pincode fixed effects; δ_D represents day fixed effects; $\theta_{s,D}$ represents state-day fixed effects. The inclusion of these comprehensive fixed effects allows us to control for time-varying district-level economic conditions, pincode-level time-invariant characteristics, overall daily market movements, and state-specific daily factors.

TABLE 6 ABOUT HERE

Table 6 reports the results. Consistent with the baseline specification, the coefficient on $Post \times UPI\ Exposure$ remains positive and statistically significant, indicating that after UPI's introduction, trading activity increases more in pincodes with higher pre-existing exposure to early-adopting

[16] We hand collected all bank holidays in each state and the NSE holiday from 2015 and 2017, detail are shown in table A1 and A2 in the appendix.

banks. However, the triple interaction term $Post \times UPI\ Exposure \times Bank\ Holiday$ is negative and statistically significant, implying that the exposure gradient flattens on bank holidays. While higher-exposure areas continue to exhibit higher trading levels overall, the differential increase associated with exposure is smaller when conventional settlement rails are unavailable.

This convergence pattern is consistent with a liquidity-friction mechanism. On regular trading days, higher-exposure regions benefit more from UPI's interoperable settlement infrastructure. On bank holidays, however, traditional funding channels are shut for all regions. Because lower-exposure areas relied more heavily on these conventional rails prior to UPI, they exhibit relatively larger adjustments when those rails are unavailable, compressing cross-sectional differences.

This pattern is difficult to reconcile with explanations based on fixed bank quality or technological sophistication. If early-adopting banks were inherently superior, their relative advantage should persist or even strengthen when offline channels are unavailable. Instead, the high–low exposure gap narrows precisely when conventional settlement infrastructure is shut down. Because the specification includes rich fixed effects, common seasonal, festival, and state-level shocks are differenced out. Taken together, these findings support an interpretation in which UPI's payment architecture relaxes settlement frictions rather than reflecting persistent differences in bank characteristics.

Finally, one concern could be that the varying local effects of demonetization are driving our results rather than UPI adoption. Chodorow-Reich et al. (2020) show that districts experiencing more severe demonetization had faster adoption of alternative payment technologies, which then could lead to greater stock market investment. As shown in Alok et al. (2024), the UPI Exposure measure is not correlated with the distance to the currency chests, a key determinant of the local severity of the demonetization effects in Chodorow-Reich et al. (2020), and thus our empirical strategy relies on the variation in UPI orthogonal to the demonetization event.

Overall, using multiple identification strategies, we provide robust evidence that the expansion of open payment infrastructure through UPI causally increased the intensity, frequency, and execution flexibility of retail trading activity.

3.4 Heterogeneity

We next examine whether the impact of UPI exposure varies systematically across investor characteristics and trading channels. Using investor-level demographic information from the NSE data, we estimate the baseline difference-in-differences specification separately by age group (Young (18–30 years old), Middle-aged (30–55 years old), and Mature (above 55 years old)), gender, and brokerage type (FinTech vs. Non-FinTech). The dependent variables are the Number of Transactions and the Number of Investors aggregated at the pincode–month level.

Figure 5 presents heterogeneous treatment effects from the baseline difference-in-differences specification across investor subgroups while the corresponding regression estimates are reported in Table A11 in the Appendix. Each point represents the estimated coefficient on $Post \times UPI\ Exposure$,

with horizontal bars denoting ±1 standard error. Panel A reports effects on the number of transactions, and Panel B reports effects on the number of active investors. In levels, middle-aged and male investors exhibit the largest increases in transactions, while mature investors show little response. However, when scaled by subgroup-specific baseline means (reported in Table A11), younger and female investors exhibit larger proportional increases in trading activity. Panel B shows similar patterns for investor participation: UPI exposure significantly increases the number of active investors across all groups, with particularly strong proportional effects among younger investors and those trading through FinTech brokerages.[17]

Table 7 examines whether the effect of UPI exposure differs across trading channels. Columns (1) and (2) estimate the baseline difference-in-differences specification separately for transactions executed through physical locations and internet-based channels (specifically securities trading through wireless technology (STWT))) respectively. The coefficient on $Post \times UPI\ Exposure$ is negative and statistically significant for physical-location transactions, indicating an average post-adoption decline of 99.21 transactions in more exposed areas. In contrast, the coefficient is positive and significant for internet-based transactions, implying an average increase of 134.15 transactions.

To assess economic magnitudes, we scale the estimated coefficients by the corresponding channel-specific sample means reported in the table. These estimates imply a 31.02% decline in physical-location transactions and a 57.81% increase in internet-based transactions. Taken together, the results indicate a reallocation of trading activity away from physical execution toward internet-based channels. This pattern is consistent with UPI lowering transaction and coordination costs associated with funding and settlement, thereby facilitating more seamless integration between digital payment infrastructure and online trading execution. Notably, the increase in internet-based execution more than offsets the decline in physical transactions, suggesting that UPI primarily affects how trades are executed rather than suppressing trading activity.

TABLE 7 ABOUT HERE

Together, these results indicate that the effects of UPI exposure are concentrated in segments of the retail market where trading is more sensitive to reductions in funding and execution frictions. Stronger responses among younger investors, women, and users of FinTech brokerages are consistent with UPI disproportionately affecting trading frequency and channel choice rather than baseline ownership or access. We next turn to geographic heterogeneity, which captures differences in the local environment in which payment infrastructure is used

3.4.1 Heterogeneity by Urban and Rural Regions

In this section, we examine whether the effects of UPI exposure vary systematically across urban and rural regions. Urban areas differ from rural areas along several economically relevant

[17] Proportional effects are computed by scaling subgroup-specific coefficients by the corresponding sample means reported in Table A11.

dimensions, including baseline digital infrastructure, merchant density, smartphone penetration, and the intensity of multi-bank and multi-platform financial relationships. These differences may influence how reductions in funding frictions translate into changes in trading behavior.

To explore this heterogeneity, we estimate the baseline specification separately for urban and rural pincodes. Urban–rural classification is determined at the pincode level using post office type.[18]

Table 8 reports the results. In absolute terms, the estimated effects of UPI exposure on trading activity are larger in urban areas than in rural areas. The increase in the number of transactions and active investors associated with UPI exposure is statistically greater in urban pincodes, with the difference in coefficients significant at the 1% level. Because levels mechanically reflect baseline market size, we report proportional effects relative to pre-UPI means to isolate economic significance net of baseline scale. For transactions, the estimated increase corresponds to a 32.8% rise relative to the pre-period mean in rural areas, compared to an 11.9% increase in urban areas. For the number of investors, the proportional increases are 45.2% in rural regions and 16.8% in urban regions. Thus, while the absolute expansion in trading activity is larger in urban areas, the proportional response is economically substantial - and in some cases larger - in rural regions.

These results indicate that the impact of UPI exposure varies across local environments. In settings with deeper digital infrastructure and more intensive financial connectivity, characteristics more common in urban regions, reductions in settlement frictions translate into larger absolute changes in trading activity. At the same time, the proportional response in rural areas highlights that interoperability meaningfully affects trading behavior even in less digitally dense markets.

We interpret these findings as heterogeneity in treatment intensity rather than as standalone evidence of a distinct mechanism. The results are consistent with the idea that the effects of interoperable payment infrastructure depend on the surrounding financial and digital ecosystem, but they do not rely on differences in market access or financial literacy alone.

4 Consequences of UPI Exposure for Retail Trading Outcomes

A growing literature examines how retail trading affects market quality, including liquidity, volatility, and price impact (e.g., Boehmer et al. (2021); Eaton et al. (2022)). These studies show that retail participation can influence volatility, liquidity, and price impact, though the direction and magnitude depend on investor sophistication and market structure. We contribute to this literature by examining how a digital payment innovation that reduces funding frictions affects secondary market activity and market quality. We examine both investor-level and stock-level consequences of UPI adoption.

[18]Post offices in India are classified into five categories by the Department of Posts: DO (Divisional Office), GPO (General Post Office), HO (Head Office), SO (Sub Office), and BO (Branch Office). We classify a pincode as rural only if its post office type is BO.

4.1 Investor-Level Consequences

We first examine whether increased exposure to UPI through reduced funding frictions affects investor performance and trading behavior. Specifically, we estimate the following investor-month specification:

$$Y_{i,t} = \alpha_i + \gamma_{d,t} + \beta \left(Post_t \times UPI\ Exposure_p \right) + \varepsilon_{i,t}, \tag{12}$$

where $Y_{i,t}$ denotes investor-level outcomes (both abnormal returns and trading behaviors) for investor i in month t. $UPI\ Exposure_p$ measures pre-existing UPI exposure in investor i's pincode, and $Post_t$ equals one after UPI adoption. All specifications include investor fixed effects (α_i) and district-by-month fixed effects ($\gamma_{d,t}$). Standard errors are clustered at the investor level. Identification comes from differential post-adoption changes among investors residing in areas with higher pre-existing UPI exposure.

Panel A of Table 9 reports abnormal buy-and-hold returns (in bps) measured over three horizons: 1 trading day, 25 trading days, and 140 trading days. Across all horizons, the coefficient on $Post \times UPI\ Exposure$ is negative and statistically significant. At the 1-day horizon, the estimate is 2.19 basis points; at the 25-day horizon, 2.44 basis points; and at the 140-day horizon, 24.21 basis points. The magnitude of underperformance increases sharply with the holding period. While short-horizon effects are modest, the long-horizon effect is economically meaningful, indicating that investors residing in high-exposure areas experience substantially lower realized returns after UPI adoption. This pattern suggests that reductions in funding frictions increase trading activity without improving investment quality, and may in fact exacerbate long-run performance deterioration.

Panel B examines changes in trading behavior. UPI exposure is associated with statistically significant but economically small adjustments in portfolio composition: risk taking declines slightly and diversification increases modestly. In contrast, the most pronounced effects appear in trading intensity. The cross-month trading gap declines, indicating that investors return to the market more quickly across months. Both the number of active trading days and the number of trades per month increase significantly. These results indicate that UPI primarily increases trading intensity rather than materially altering portfolio composition or risk exposure.

The return patterns in Panel A align closely with the behavioral adjustments documented in Panel B. Investors in higher-exposure regions trade more frequently and re-enter the market more quickly, yet exhibit progressively worse performance over longer horizons. This pattern is consistent with intensified participation rather than improved information acquisition driving post-adoption outcomes.

We next examine whether the effects of UPI exposure vary with investor experience. We measure experience dynamically using lagged cumulative participation: for each investor i in month t, experience is the cumulative number of months in which the investor traded up to *t-1*

(i.e., excluding month t). Each month, we sort investors into experience quartiles based on this lagged cumulative measure, and omit the least experienced quartile (Q1) as the baseline group. We then estimate a fully saturated specification that interacts $Post \times UPI\ Exposure$ with indicators for experience quartiles (with the least experienced quartile omitted):

$$Y_{i,t} = \alpha_i + \gamma_{d,t} + \beta \left(Post_t \times UPIExposure_p \right) + \sum_{q=2}^{4} \theta_q \left(Post_t \times UPIExposure_p \times Q_{i,q,t} \right) + \varepsilon_{i,t}, \tag{13}$$

where $Q_{i,q,t}$ is an indicator that investor i is in experience quartile q in month t, constructed from the lagged cumulative active-month measure. Following Seru et al. (2010), this lagged construction prevents mechanical sorting driven by post-UPI trading activity within month t. Accordingly, the heterogeneity estimates reflect differences across investors at distinct stages of market participation rather than endogenous reclassification induced by the reform. All main effects and lower-order interactions are included but suppressed for brevity.

Panel A of Table 10 reports heterogeneity in abnormal buy-and-hold returns (in basis points) across experience quartiles. The economically meaningful magnitudes are the implied effects of a one-standard-deviation increase in exposure for each quartile (reported at the bottom of the panel), rather than the raw interaction coefficients themselves. For the least experienced investors (Q1), the implied one-SD exposure effects are negative at all horizons: -0.864 bps (1 day), -1.419 bps (25 days), and -5.500 bps (140 days). For more experienced investors (particularly Q3 and Q4), short- and medium-horizon under-performance attenuates substantially. At the 1-day horizon, the implied effect becomes progressively less negative and turns slightly positive for the most experienced investors (Q4: +0.216 bps). At the 25-day horizon, the implied effects are positive for Q3 and Q4, indicating that more experienced investors briefly offset the short-run decline observed for Q1. However, at the 140-day horizon, abnormal returns remain negative across all quartiles, although the magnitude of losses is smaller for experienced investors than for Q1. Overall, experience mitigates short- and medium-run return declines and can temporarily reverse them for the most seasoned traders, but it does not eliminate longer-horizon under-performance associated with increased trading access.

Figure 6 visualizes the long-horizon performance consequences of UPI exposure. The figure plots the estimated abnormal buy-and-hold returns across experience quartiles. Consistent with the regression results in Table 10, abnormal returns at short horizons are small and in some cases slightly positive, particularly among more experienced investors. However, at longer horizons (140 trading days), abnormal returns are negative across the distribution. The decline is economically meaningful and not confined to inexperienced investors.

Panel B shows that the return heterogeneity coincides with sharp differences in trading responses across the experience distribution.For the least experienced investors (Q1), the implied

one-standard-deviation exposure effects are economically negligible for active trading days and number of trades, and small for risk taking and diversification. In contrast, for Q2–Q4 investors, UPI exposure generates large increases in trading intensity: the implied effects on both active trading days and the number of trades rise strongly with experience, with the largest increases for Q4. The cross-month trading gap also declines for Q2–Q4 (becoming negative), indicating quicker return to trading following inactivity, whereas the effect for Q1 is slightly positive. Changes in risk taking and diversification are statistically significant for some experience groups, but the magnitudes are small compared to the much larger increases in trading activity.

To address potential endogeneity concerns, Appendix Table A12 redefines experience using cumulative trading months measured strictly in the pre-UPI period (Jan 2015 - Oct 2016) and fixes quartile classification throughout the sample. This approach ensures that experience is entirely predetermined and not influenced by post-adoption trading behavior, but it necessarily reduces the sample to investors active prior to UPI adoption. Consistent with the baseline results, short-horizon return declines attenuate with experience, while long-horizon abnormal returns remain negative across all quartiles, with the largest losses borne by the least experienced investors. Trading responses also remain concentrated among more experienced investors. Thus, the heterogeneity patterns are not mechanically driven by post-UPI participation and are robust to using strictly pre-period measures of investor experience.

Taken together, the heterogeneity results show that UPI exposure does not primarily activate inexperienced investors. Instead, it disproportionately increases trading among investors who are already engaged in the market. Experience substantially attenuates short-horizon return declines but long-horizon abnormal returns remain negative across all quartiles, with the largest losses borne by the least experienced investors. The dominant margin of adjustment is trading intensity rather than meaningful changes in portfolio risk or diversification. This pattern is consistent with reduced transaction frictions amplifying trading activity, particularly among seasoned participants, but not preventing longer-run underperformance.

4.2 Market-Level Effects

A central question is whether UPI-induced retail trading alters broader market functioning. While the earlier section documents increased trading activity at the investor level, it remains unclear whether these changes affect liquidity, volatility, or price efficiency at the stock level. While retail participation can increase trading activity and depth, it may also raise volatility or price impact if retail trading is less informed or more episodic.

Our setting complements Eaton et al. (2022), who study exogenous reductions in retail trading due to brokerage outages. In contrast, we examine exogenous increases in retail participation driven by payment interoperability. This allows us to assess how increased retail access affects market activity and quality.

We estimate the following stock-month specification:

$$Y_{s,t} = \alpha_s + \gamma_t + \beta_1 \left(UPI\ Intensity_{s,t} \times Post_t \right) + \varepsilon_{s,t}, \tag{14}$$

where $Y_{s,t}$ denotes stock-level outcomes for stock s in month t, $UPI\ Intensity_{s,t}$ measures the share of trading originating from high-UPI-exposure pincodes, and $Post_t$ equals one after UPI adoption. All specifications include stock fixed effects (α_s) and month fixed effects (γ_t). Standard errors are clustered at the stock level. To assess whether effects are concentrated in retail-heavy stocks, we estimate fully interacted specifications:

$$\begin{aligned} Y_{s,t} = \alpha_s + \gamma_t &+ \beta_1 \left(UPI\ Intensity_{s,t} \times Post_t \right) \\ &+ \beta_2 \left(UPI\ Intensity_{s,t} \times Post_t \times TopRetail_{s,t} \right) + \varepsilon_{s,t}, \end{aligned} \tag{15}$$

where $TopRetail_{s,t}$ is an indicator equal to one for stocks with above-median retail participation. Table 11 reports the results.

Panel A of Table 11 examines trading volume and annualized volatility. Trading volume is measured as monthly NSE traded value in INR million. Volatility is computed as the standard deviation of daily excess returns within the month and annualized.

We find that $Post \times UPI\ Intensity$ is positive and statistically significant for trading volume, indicating that, following UPI adoption, stocks whose order flow is more heavily sourced from high-UPI-exposure pincodes experience larger increases in trading activity. The triple interaction term $Post \times UPI\ Intensity \times Top\ Retail$ is also positive and significant, implying that the post-adoption increase in trading volume is significantly amplified in retail-heavy stocks. Economically, among stocks with above-median retail participation, a one-standard-deviation increase in UPI intensity is associated with an additional 8.4%[19] increase in trading volume relative to stocks with below-median retail participation.

In contrast to trading volume, we do not find robust evidence that UPI adoption increases return volatility. The coefficient on $Post \times UPI\ Intensity$ is statistically insignificant, and the triple interaction with Top Retail is also insignificant. Thus, although stocks more exposed to UPI-linked retail order flow experience higher trading activity, this increase does not translate into statistically or economically meaningful changes in return volatility, even among retail-heavy stocks. Overall, Panel A indicates that UPI adoption raises market activity without generating a corresponding increase in return variability.

Panel B investigates whether increased retail participation affects market quality. We focus on Amihud (2002) illiquidity as a measure of price impact, which is defined as:

[19]The standard deviation of *UPI intensity* is 0.045. Multiplying this by the estimated triple-interaction coefficient (3910.778) implies an incremental increase of approximately INR 176 million in monthly trading volume per stock (3910.778 × 0.045). Relative to the sample mean monthly traded value of INR 2,104.868 million, this corresponds to an 8.4% increase.

$$\text{Illiquidity}_{s,t} = \frac{1}{D_{s,t}} \sum_{d=1}^{D_{s,t}} \frac{|R_{s,d}|}{\text{Volume}_{s,d}}, \tag{16}$$

where $R_{s,d}$ denotes daily returns and $\text{Volume}_{s,d}$ denotes daily traded value in INR. Higher values indicate greater price impact per unit of volume and thus lower liquidity.

In baseline specifications, the average effect of UPI intensity on illiquidity is small. However, once we allow for heterogeneity by retail participation, the interaction term $Post \times UPI\ Intensity \times Top\ Retail$ is positive and statistically significant. This implies that in retail-heavy stocks, those with above-median retail participation, greater exposure to UPI-linked order flow is associated with higher price impact after adoption. Thus, while trading volume increases, price impact rises modestly in retail-heavy stocks, indicating that liquidity does not improve proportionately with trading activity.

We next examine short-term return reversal, measured as the within-month autocorrelation of daily excess returns. Because stronger reversal corresponds to more negative serial correlation, a negative regression coefficient implies increased short-horizon reversal. In the fully interacted specification (col. 4), $Post \times UPI\ Intensity$ is negative and statistically significant for below-median retail stocks, indicating that greater UPI-linked trading is associated with stronger short-term reversal in these stocks. However, the triple interaction $Post \times Top\ Retail \times UPI\ Intensity$ is positive and significant. The implied net effect for retail-heavy stocks is positive, indicating that UPI-linked trading makes returns more persistent rather than more reversible in those stocks. Thus, while UPI exposure is associated with stronger short-term reversal in low-retail stocks, it generates more persistent price impact in retail-heavy stocks.

An important nuance is that although Amihud illiquidity increases in retail-heavy stocks, we do not observe a corresponding rise in return predictability or systematic reversals. Instead, short-horizon return dynamics become modestly more persistent in these stocks, while remaining unpredictable at longer horizons. This pattern suggests that UPI-induced retail order flow affects market microstructure, raising price impact and altering short-run dynamics, without generating sustained mispricing. While we observe stronger short-term reversal in low-retail stocks, we do not find pervasive increases in return predictability in retail-heavy stocks. We next examine more directly whether UPI exposure leads to systematic return predictability.

Panel C of Table 11 evaluates whether UPI-induced trading is associated with stock-level excess returns. We follow the empirical framework of Eaton et al. (2022) and estimate stock-month regressions of monthly excess returns on $UPI\ Intensity_{s,t} \times Post_t$, controlling for lagged returns, aggregate retail order flow imbalance, market capitalization, book-to-market ratio, and return skewness. Monthly excess returns are constructed as the stock's first-to-last trading-day return within the month, net of the corresponding monthly market return, using Prowess daily stock prices.

While the level term for $UPI\ Intensity_{s,t}$ is negative and statistically significant - indicating that stocks with higher UPI-linked trading intensity exhibit lower subsequent excess returns on average - the interaction term $Post_t \times UPI\ Intensity_{s,t}$ is statistically insignificant across specifications. This implies that the introduction of UPI does not alter the pre-existing relationship between trading intensity and future returns.

These findings suggest that although digital payment interoperability increases trading activity and modestly affects liquidity conditions, the additional UPI-induced retail flow does not lead to detectable changes in average excess returns or systematic return predictability.[20]

Taken together, the investor- and market-level evidence points to a consistent conclusion: UPI expands access and speeds capital movement, increasing participation and trading frequency. Ex ante, such reductions in funding frictions could plausibly have improved investor performance by enabling faster responses to information, or alternatively generated price instability through uninformed retail pressure. We find no evidence of improved performance and no systematic evidence of return predictability at the stock level. While trading activity rises and liquidity conditions adjust in certain stocks, these changes do not translate into persistent mispricing. Because UPI primarily reduces payment frictions rather than altering the information environment, the marginal trades it enables do not appear to be more informed. Investors trade more, but not better; and although their activity affects short-run microstructure dynamics, it does not generate sustained distortions in prices. The evidence therefore supports a friction-reduction channel that amplifies activity without an accompanying information channel that improves performance or creates systematic inefficiencies.

5 Economic Mechanisms

In this section, we examine three economic mechanisms through which exposure to UPI-enabled fund-transfer infrastructure affects retail trading behavior and market outcomes. These mechanisms operate through reductions in funding frictions, changes in the composition of retail trading, and shifts in household liquidity management. Importantly, these channels do not operate through expanded access or financial inclusion per se, but through changes in the speed, cost, and flexibility with which existing bank deposits can be deployed into market transactions.

First, UPI reduces funding frictions by enabling instantaneous, 24×7 transfers between bank accounts and brokerage or demat accounts. Unlike legacy systems such as NEFT and RTGS, which operate in batches, impose settlement delays, or require high minimum transfer amounts, UPI allows investors to fund trades in real time, increasing the value of immediacy during volatile market conditions.[21]

[20] Note, UPI could have also increased retail investment in mutual funds. This can potentially explain why our retail level outcomes do not translate to market-level effects.

[21] NEFT is a system for transferring funds electronically between bank accounts in batches and thus takes some time to settle. RTGS allows for immediate, individual fund transfers but is usually used for large, high-value transactions and thus has a minimum transaction amount.

Second, by lowering the fixed and variable costs of transferring funds into trading accounts, UPI alters the composition of retail trading activity. These lower frictions disproportionately facilitate small-value and high-frequency transactions, leading to greater trading intensity without a commensurate increase in average position size. Finally, UPI affects household liquidity management by reducing the wedge between transaction balances and marketable balances. By allowing funds to be rapidly redeployed from bank accounts into brokerage accounts, UPI increases the effective liquidity available for trading, particularly in cash-intensive regions where idle balances were previously costly to mobilize.

We investigate each of these mechanisms in detail below.

5.1 Funding Immediacy and Timing Frictions

A central mechanism through which UPI affects retail trading behavior is by reducing funding immediacy constraints. Prior to UPI, transferring funds into trading accounts relied on legacy systems such as NEFT or checks, which operated in batches, were restricted to banking hours, and often involved settlement delays. During periods of sharp price movements, these frictions limited investors' ability to respond in real time, increasing the opportunity cost of delayed execution. By enabling instantaneous, 24×7, low-cost transfers between bank accounts and brokerage accounts, UPI substantially lowers the timing frictions associated with deploying capital into market transactions.

To test this mechanism, we exploit two large, exogenous market stress events using high-frequency, time-stamped transaction data from the Bombay Stock Exchange. Specifically, we examine retail trading behavior in a 12-hour window before and after the flash crash episodes of September 11, 2019, and March 12, 2020. On September 11, 2019, the Indian stock market faced its worst day of the year, with the BSE Sensex[22] falling 793 points and erasing 3.3 trillion INR in investor wealth. The decline was driven by policy concerns, including a proposed increase in the public shareholding threshold, a tax surcharge on high-income earners affecting foreign portfolio investors, and a lack of economic stimulus. On March 12, 2020, markets experienced another sharp fall as the Sensex plunged 2,919 points (8.18%) and the Nifty 50 dropped 868 points (8.30%) due to global fears of a recession sparked by the COVID-19 pandemic and its designation as a global health crisis by the World Health Organization.[23] Thus, both events were characterized by abrupt, market-wide price declines driven by policy uncertainty and global macroeconomic shocks, respectively, and thus provide a setting in which funding immediacy is particularly valuable.

To analyze how time-sensitive investor reactions to significant market movements vary with differing UPI exposures, we calculate the trading activities of each active investor during the 12 trading hours before and after each market crash. To address concerns that observed patterns

[22]The BSE Sensex is a free-float market-weighted stock market index of 30 companies listed on the Bombay Stock Exchange.

[23]See links 1 and 2 for both details on each crash.

may be specific to a single event, we estimate a model that incorporates data from both crashes. Specifically, we estimate the following equation at the investor–hour level:

$$Y_{i,h} = \delta_i + \gamma_h + \beta \cdot \text{UPI Exposure}_i \times \text{Post Crash}_h + \varepsilon_{i,h} \tag{17}$$

where $Y_{i,h}$ denotes the number of transactions executed by investor i in hour h, UPI Exposure_i is the exposure of the investor's residential pincode to UPI-enabled fund transfers, and PostCrash_h is an indicator equal to one for hours following the market crash. Investor fixed effects, δ_i, control for time-invariant heterogeneity in trading propensity, while hour fixed effects, γ_h, absorb common intraday patterns in market activity.

The coefficient β captures whether investors with greater exposure to UPI exhibit stronger trading responses in the immediate aftermath of large market shocks. A positive estimate is consistent with UPI reducing funding immediacy constraints that would otherwise limit investors' ability to trade during periods when timing is critical. For identification, we exploit sharp, plausibly exogenous market-wide shocks and compare post-crash trading responses across investors with differing exposure to UPI-enabled fund transfers, while controlling for investor fixed effects and and hour-of-day fixed effects that absorb common market-wide intraday trading patterns.

TABLE 12 ABOUT HERE

As reported in Table 12, we find a consistently positive and statistically significant interaction between UPI Exposure and PostCrash. Investors residing in higher-exposure areas execute more trades and transact across a broader set of securities in the hours following market crashes, both when the two events are pooled and when they are examined separately. These patterns indicate that reductions in funding timing frictions materially affect retail investors' responsiveness to short-lived market opportunities.[24]

Taken together, these results provide direct evidence that UPI operates through a funding immediacy channel, enabling faster deployment of existing bank balances into market transactions during periods of heightened volatility. This mechanism is distinct from changes in market access or participation and instead reflects how payment infrastructure shapes the speed and flexibility of retail trading behavior.

5.2 Reduction in Fixed Costs of Small-Value Trading

A second mechanism through which UPI may affect retail trading behavior is by reducing the fixed costs associated with funding and executing small-value trades. Prior to UPI, transferring funds into brokerage accounts often involved delays, minimum transfer thresholds, and nontrivial coordination costs, making incremental capital deployment costly. These frictions discouraged small, repeated trades and favored less frequent, larger transfers. By enabling instantaneous,

[24] While UPI increases investors' ability to transact during crash windows, improved immediacy does not mechanically imply improved performance. Liquidity access enables faster responses to price dislocations, but the profitability of those trades depends on whether investors provide liquidity to temporary price pressure or instead engage in momentum-driven selling.

low-cost transfers of small amounts, UPI lowers the fixed cost of incremental trading and allows investors to reduce the cost of deploying capital in smaller increments. Fixed costs disproportionately deter small trades, so reductions in transfer frictions should generate larger responses precisely in this segment.

To test this mechanism, we examine whether UPI exposure disproportionately increases trading activity in small-value transactions relative to larger-value trades. We estimate the following specification at the pincode–month level:

$$Small_Value_Trading_{p,d,t} = \gamma_p + \alpha_{d,t} + \beta \cdot \text{Post}_t \times \text{UPI Exposure}_p + \varepsilon_{p,d,t}, \tag{18}$$

where $Small_Value_Trading_{p,d,t}$ captures either (i) the number of transactions below a given trading-value cutoff or (ii) the number of active investors who execute at least one transaction below that cutoff in pincode p (located in district d) and month t. All specifications include pincode and district–month fixed effects, thereby controlling for time-invariant local characteristics and district-level shocks that vary over time.

Table 13 presents the results. Panel A shows that UPI exposure is associated with a substantial increase in the number of small-value transactions in higher-exposure areas following UPI's rollout. Although trading activity increases across both small- and large-value categories, the estimated effect is significantly larger for transactions below the cutoff. The difference in coefficients between small- and large-value trades is statistically significant across both trading-value thresholds, indicating a disproportionate expansion in small-value trading.

Panel B shows a parallel pattern for investor counts. The number of active small-value traders increases significantly following UPI adoption, and the increase is statistically larger than that observed for larger-value traders.

Taken together, these findings are consistent with UPI lowering the fixed costs associated with small-value trading and generating relatively stronger growth in trading activity in this segment.

5.3 Liquidity Reallocation

A third mechanism operates through changes in household liquidity management. Prior to UPI, households in cash-intensive regions faced substantial frictions in mobilizing transaction balances for financial market activity. Even when savings were held in bank accounts, delays and coordination costs associated with transferring funds into brokerage accounts effectively segmented cash-like balances from trading-ready brokerage balances. By enabling instantaneous and low-cost fund transfers, UPI reduces this segmentation and allows idle or precautionary balances to be redeployed into market transactions more easily.

To test this mechanism, we examine whether the effect of UPI exposure on retail trading activity is stronger in areas with higher ex-ante reliance on cash. We proxy for cash intensity using per-capita ATM withdrawals in the pre-UPI period and classify pincodes in the top tercile of ATM

withdrawals as cash-intensive. Regions with high pre-UPI ATM withdrawals are interpreted as areas where households frequently convert deposits into cash for transactional or precautionary purposes. UPI reduces the need for such withdrawals by providing an always-on digital payment rail. In these regions, deposits that would otherwise have been withdrawn as cash remain within the banking system and become more readily deployable for equity transactions.

We estimate the following triple-difference specification:

$$\begin{aligned} Y_{p,d,t} = \alpha_{d,t} + \gamma_p + \beta_1 \cdot \text{Post} \times \text{UPI Exposure}_p \\ + \beta_2 \cdot \text{Post} \times \mathbf{1}_{Top\ Tercile,p} \\ + \beta_3 \cdot \text{Post} \times \text{UPI Exposure}_p \times \mathbf{1}_{Top\ Tercile,p} + \varepsilon_{p,d,t}. \end{aligned} \tag{19}$$

where $\mathbf{1}_{Top\ Tercile,p}$ indicates pincodes in the top tercile of pre-UPI ATM withdrawals per capita. The coefficient β_3 captures whether the marginal effect of UPI exposure following roll-out is larger in cash-intensive areas.

Table 14 reports the results. Pincodes with high ex-ante cash usage exhibit substantially larger increases in retail trading activity following UPI adoption. In Column 1, the number of transactions increases by an additional 310 per month in top-tercile cash areas, relative to a baseline increase of 63 transactions in lower-cash areas. Column 2 shows a similarly amplified response in high-cash areas relative to other regions.

These findings indicate that UPI operates by reducing frictions associated with mobilizing transaction balances for market activity, particularly in environments where households previously relied heavily on cash. Rather than generating new savings or bank relationships, UPI facilitates the reallocation and timely deployment of existing liquid balances into market transactions.

Taken together, our empirical analysis identifies four distinct but complementary mechanisms through which UPI affects retail trading behavior. UPI reduces funding immediacy constraints during time-sensitive market events, lowers the fixed costs of small-value trading, amplifies behavioral responses through interoperable digital networks, and facilitates the reallocation of transaction balances into marketable liquidity. These channels operate by altering the speed, cost, and flexibility with which existing bank deposits can be deployed into financial markets.

Our findings speak directly to the broader trade-off highlighted in recent discussions of payment interoperability (Kundu, 2025). While interoperable digital public infrastructures can democratize access and scale participation, they may simultaneously reshape trading behavior by reducing transactional frictions. In our setting, UPI increases retail trading activity, particularly in small-value transactions, and is associated with negative long-horizon abnormal returns across experience groups. At the same time, we find no evidence of persistent return predictability at the stock level. These results suggest that the consequences of interoperability depend not only on expanded access, but also on how reduced frictions affect trading intensity without altering the underlying information environment.

6 Conclusion

This paper provides the first systematic evidence on how payment system architecture shapes retail trading behavior and market outcomes. We study the 2016 national introduction of India's Unified Payments Interface (UPI), a real-time, interoperable digital payment platform, and exploit cross-sectional variation in exposure to banks that integrated early into the network. A one-standard-deviation increase in UPI exposure raises monthly transactions by 6.2% and active investors by 8.7%, with effects operating through reduced funding frictions, greater responsiveness to market stress, shifts toward smaller-value trades, and reallocation of idle cash into equity markets. These effects are specific to interoperable payment design — closed, single-bank platforms exhibit an order of magnitude smaller impact.

Expanded access, however, does not translate into improved investment performance. Long-horizon abnormal returns are negative across experience groups, and portfolio adjustments are modest. At the stock level, UPI exposure raises trading volume and price impact in retail-heavy stocks, but markets absorb the surge in retail activity without persistent mispricing or broad volatility effects.

Overall, our findings highlight a central equilibrium feature of frictionless finance: payment architecture changes who trades and how they trade, but does not improve long-run investor outcomes or fundamentally alter price efficiency. India's setting is particularly instructive because UPI operates at a scale and level of capital market integration that other instant payment systems have not yet achieved — processing over 16 billion monthly transactions with direct connectivity to securities trading infrastructure. As instant payment systems worldwide move toward deeper integration with capital markets, our results suggest that the consequences of such integration depend not only on the access it creates, but on how reduced frictions interact with the behavioral tendencies of newly enabled participants.

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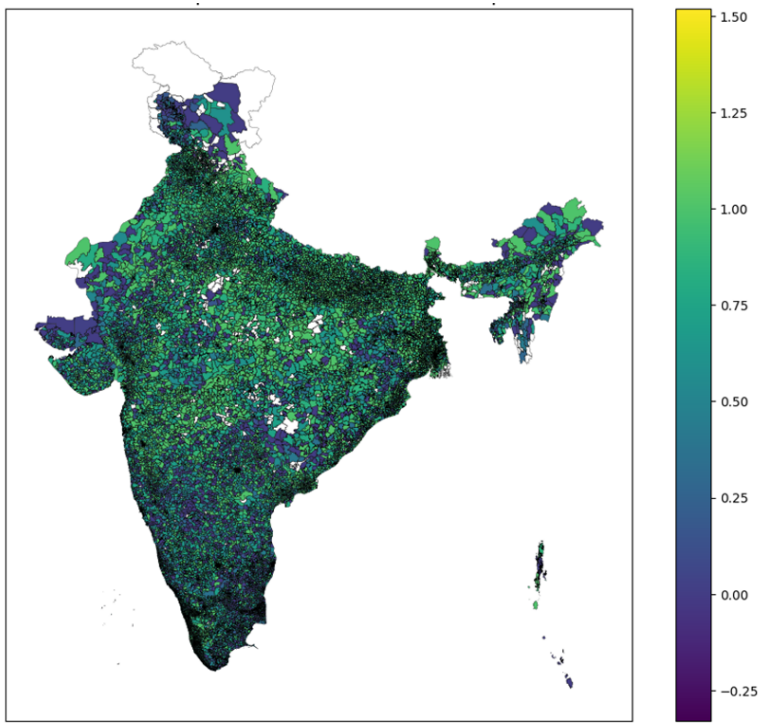
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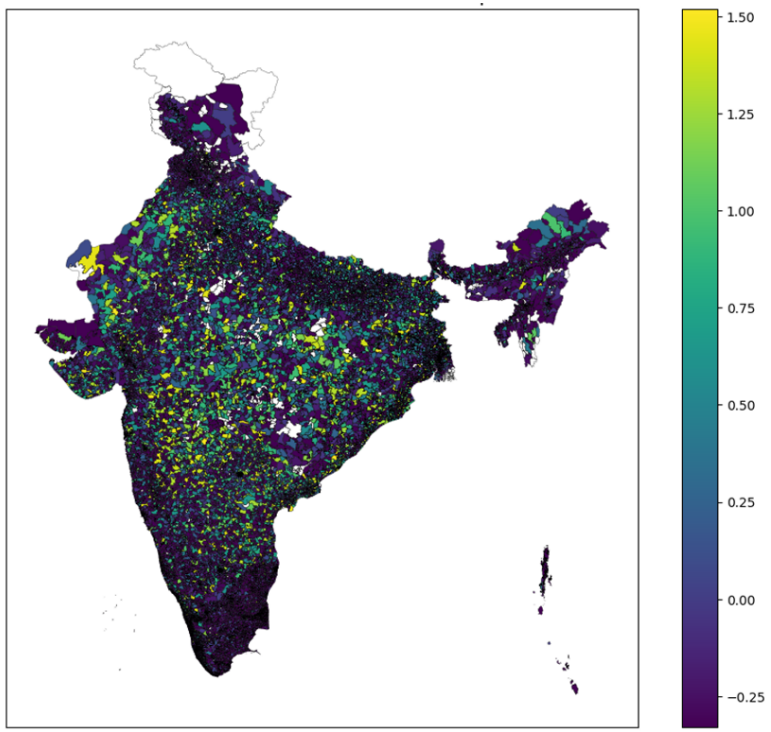
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Figure 1
Geographic Variation in UPI Exposure

The two maps plot the geographic distribution of UPI exposure (left) and UPI Bartik (right) across all pincodes in India. UPI Exposure is defined as the ratio of deposits for early adopter banks to total deposits as defined in Equation (1). The classification of early adopter banks is as of November 2016 and is provided by the Government of India. Bank-branch level deposit data is from Basic Statistical Returns (BSR) provided by the Reserve Bank of India. UPI Bartik uses the national-level variation in UPI adoption over time and interacts it with the relative importance of early UPI adoption in a given pincode. The measures are averages across all months over our sample periods. Brighter colors indicate pincodes with higher UPI exposure.



(A) UPI Exposure India Pincode Heatmap



(B) UPI Bartik India Pincode Heatmap

Figure 2
Stock Market Participation over Sample Period

This figure shows the number of transactions by retail investors (blue bold line) and number of retail investors (red dashed line), over time, averaging across all the pincodes. The data is at monthly frequency for the period Q1 2015 to Q1 2020.

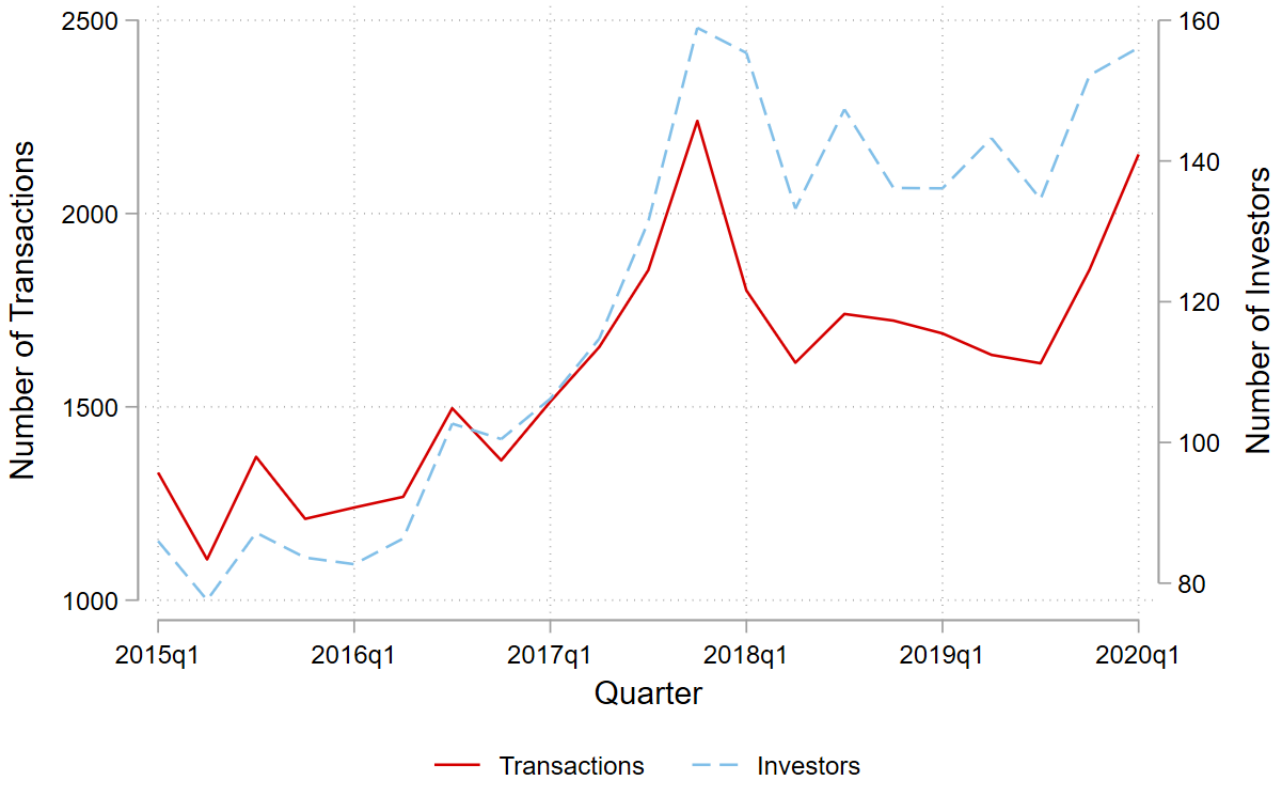
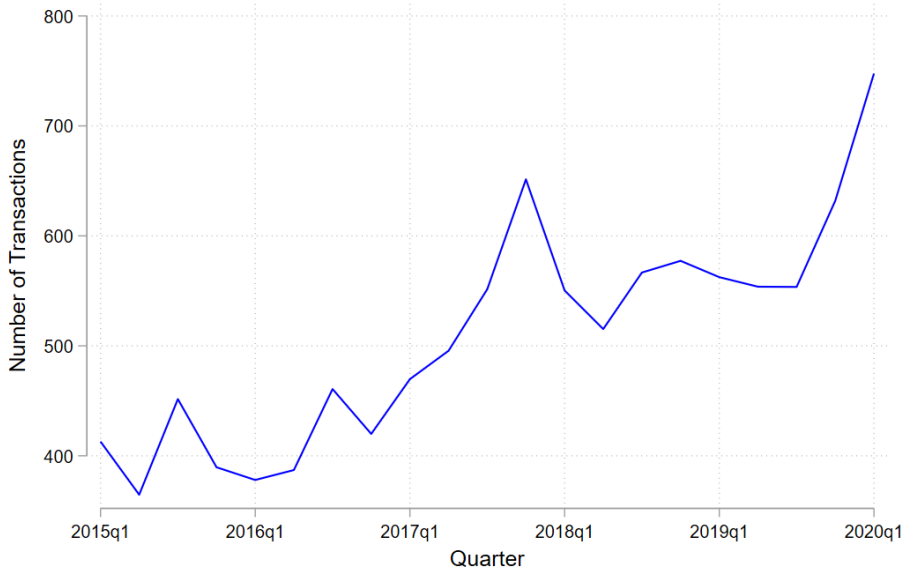
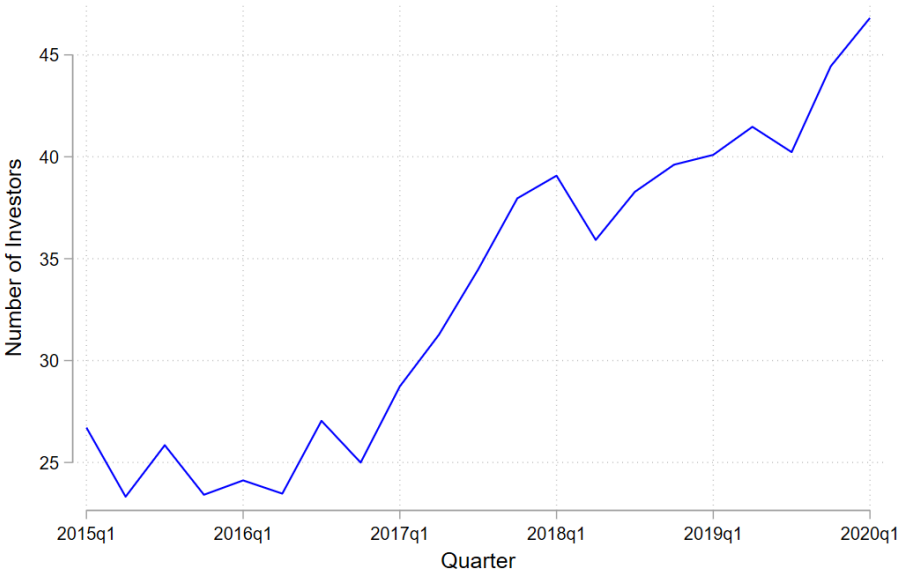


Figure 3
UPI and Stock Market Participation

This figure illustrates the difference in the average number of retail transactions (left) and retail investors (right) between high- and low-UPI exposure pincodes. Trading data is at the monthly frequency from first quarter of 2015 to first quarter of 2020 and aggregated to the pincode level separately. The difference between the two groups (high-low) is plotted below. High (low) refers to pincodes with above (below) the median values of UPI exposure.



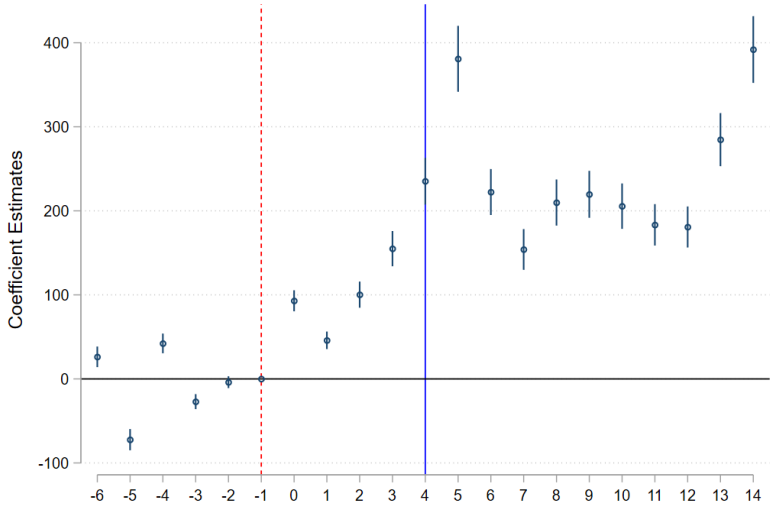
(A) Difference Between Number of Transactions (High Low)



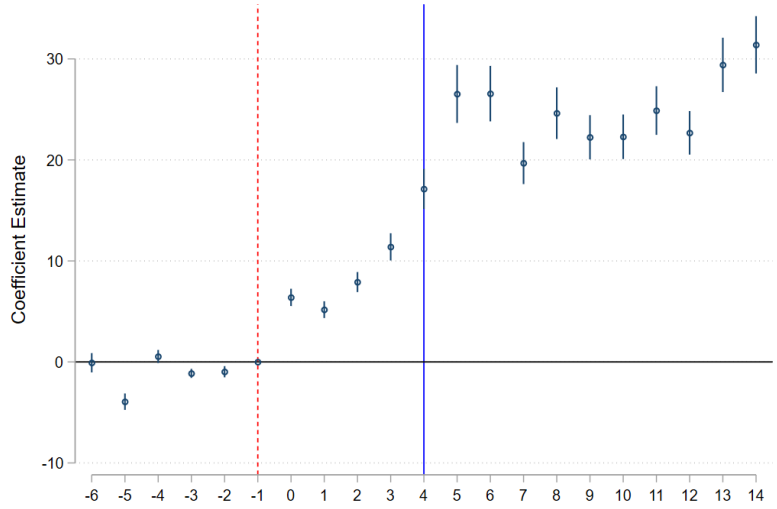
(B) Difference Between Number of Investors (High Low)

Figure 4
Dynamic Effects of UPI on Stock Market Participation

This figure presents treatment dynamics using the specification in equation 6 for the number of retail transactions (left) and retail investors (right). The underlying data, aggregated at the pincode level, is at a quarterly frequency from Q1 2015 to Q4 2019. Each dot represents the point estimate, with vertical lines indicating 95% confidence intervals. The regression includes pincode and district-month fixed effects, with standard errors that are heteroskedasticity-robust and clustered at the pincode level. The dashed red vertical line marks the pre-treatment quarter (Q2 2016) and the solid blue line marks September 2017, a circular released by the Reserve Bank of India that strengthened the open banking system.



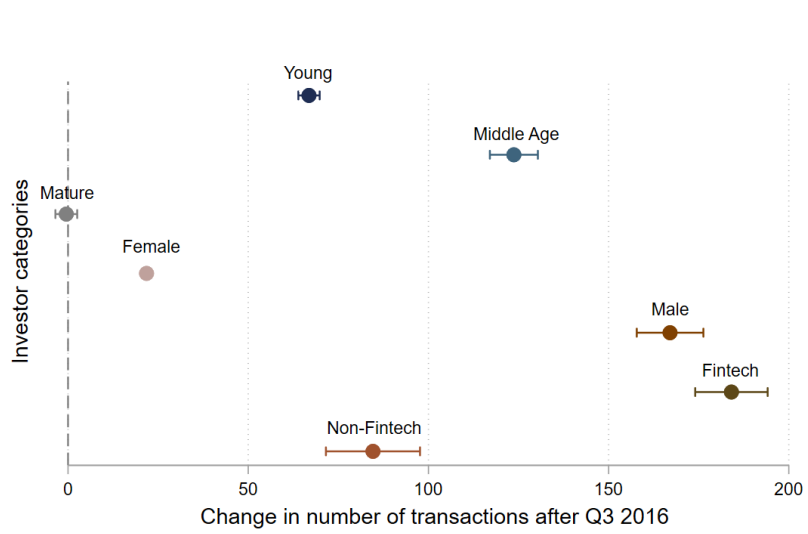
(A) Number of Transactions



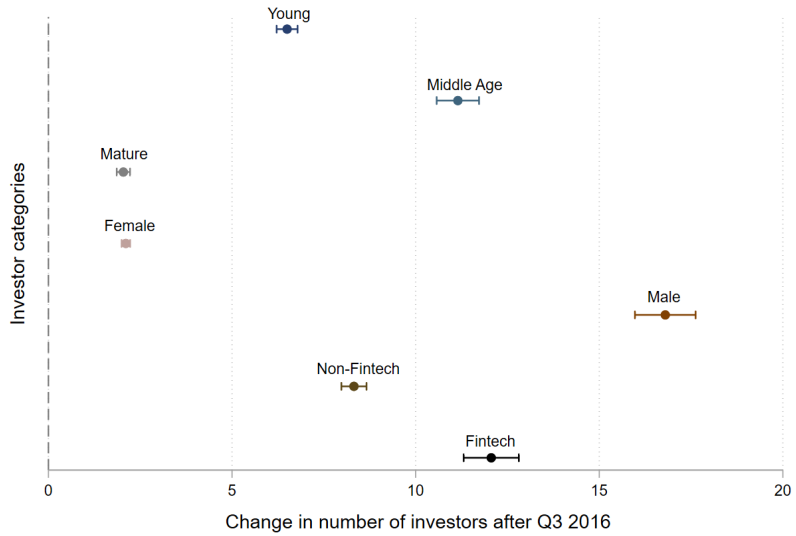
(B) Number of Investors

Figure 5
UPI and Stock Market Participation: Heterogeneity

This figure presents heterogeneous treatment effects using the baseline difference-in-differences specification for the number of retail transactions (left) and retail investors (right). The underlying data is aggregated at the pincode-month level from January 2015 to January 2020. Each dot represents the coefficient on the interaction term Post × UPI Exposure, and the horizontal bars denote ±1 standard error. The regression includes pincode and district-month fixed effects, with standard errors that are heteroskedasticity-robust and clustered at the pincode level.



(A) Number of Transactions



(B) Number of Investors

Figure 6
Consequences: Heterogeneous Effects on Abnormal Returns by Trading Experience

The figure plots the implied effect of a one-standard-deviation increase in UPI exposure on abnormal returns over three horizons (1-day, 25-day, and 140-day), across experience quartiles constructed using cumulative active trading months. The underlying data is aggregated at the investor-month level from January 2015 to January 2020. The regression includes investor and district-month fixed effects, with standard errors that are heteroskedasticity-robust and clustered at the investor level.

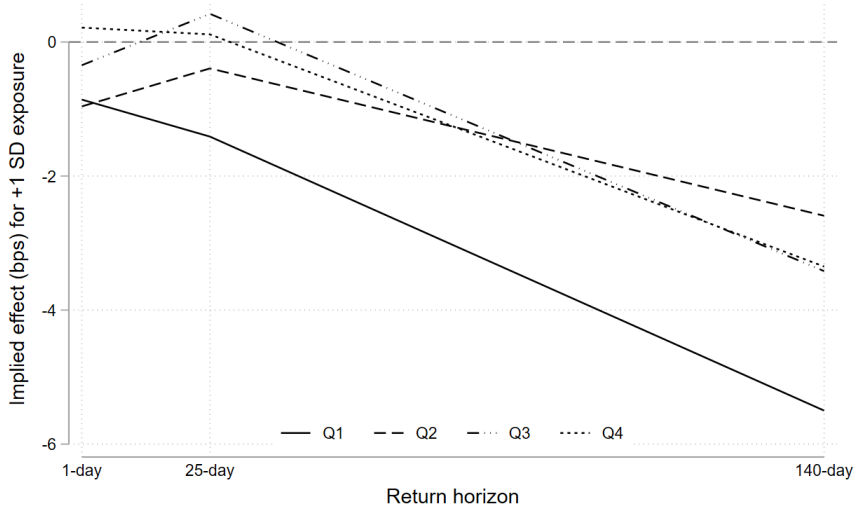


Table 1
Summary Statistics

Variable	N	Mean	S.D.	Min	P25	Median	P75	Max
Pincode-month level								
UPI Exposure	1,135,393	0.650	0.347	0	0.433	0.764	0.957	1
UPI Bartik	1,124,040	0	1	-0.288	-0.288	-0.288	-0.275	9.102
Post	1,135,393	0.639	0.480	0	0	1	1	1
Number of Transactions	1,124,040	1108.415	2935.442	0	55	205	654	27006
Number of Investors	1,124,040	81.075	217.121	0	5	16	47	2059
Investor-month level								
Abnormal Returns (1 day)	134,124,543	-25.945	246.322	-1216.257	-117.190	-20.010	69.437	1335.946
Abnormal Returns (10 days)	134,088,542	-41.645	564.816	-2515.510	-275.407	-24.274	201.377	2527.479
Abnormal Returns (25 days)	134,033,007	-56.752	838.777	-3535.240	-402.693	-27.765	311.266	3494.306
Abnormal Returns (140 days)	126,141,147	-236.127	2002.884	-7531.617	-1111.610	-81.168	678.538	6617.815
Number of Trades	134,242,917	10.395	18.293	1.000	1.000	3.000	10.000	143.000
Number of Active Days	134,242,917	4.637	4.999	1.000	1.000	2.000	6.000	21.000
Risk Taking	106,713,579	0.791	0.234	0.200	0.600	0.875	1.000	1.000
Portfolio Diversification	132,900,513	0.564	0.421	0.000	0.000	0.750	0.942	0.998
Cross-month Trading Gap	134,242,917	7.937	9.741	0.000	1.550	4.000	10.667	47.000

This table reports the summary statistics for key variables in our analysis. The table summarizes mean values of the key variables for high and low UPI Exposure and the difference between them. The variables included are the pincode-month level stock market participation, namely, *Number of Transactions* and *Number of Investors*) and investor trading behavior measures (*buy and hold returns (BHR)*, *Risk Taking*, *Portfolio Diversification*, and *Trading Speed*) at the investor level). The data is from January 2015 to January 2020.

Table 2
Balance Test by UPI Exposure

Variable	(1) High UPI Exposure N	Mean/(SE)	(2) Low UPI Exposure N	Mean/(SE)	(3) Pairwise t-test Mean Difference
Pincode: NSE Sample					
Economic Activity	9,306	10.684	9,307	8.059	2.625***
		(13.894)		(13.323)	(0.200)
Number of Transactions	203,007	1104.105	203,444	805.929	298.176***
		(3051.367)		(2464.830)	(38.925)
Number of Investors	203,470	78.094	203,631	55.665	22.428***
		(222.382)		(175.687)	(2.824)
Growth in Number of Transactions	200,403	0.007	199,554	0.006	0.001
		(0.516)		(0.544)	(0.001)
Growth in Number of Investors	200,436	0.008	199,673	0.008	0.000
		(0.332)		(0.350)	(0.000)
Investor: NSE Sample					
Age	656,357	37.768	655,889	37.998	-0.230***
		(12.849)		(13.253)	(0.023)
Female	629,007	0.119	626,393	0.134	-0.015***
		(0.324)		(0.341)	(0.001)

This table compares ex-ante differences across high-exposure and low-exposure pincodes in the following variables for the period of January 2015 to October 2016: Levels of economic activity as proxied by night-light intensity; Stock market activity as proxied by the Number of Transactions, Number of Investors, Growth in Number of Transactions, and Growth in Number of Investors; and Investor demographics including age and gender.

Table 3
UPI and Stock Market Participation

DV	Number of Transactions (1)	Number of Investors (2)	Number of Transactions (3)	Number of Investors (4)
UPI Exposure X Post	197.961***	20.310***		
	(11.717)	(1.059)		
UPI Bartik			113.260***	13.399***
			(4.263)	(0.365)
Pincode FE	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y
N	1,121,378	1,121,385	1,110,925	1,110,994
Adj. R^2	0.964	0.964	0.965	0.967

This table presents the difference-in-difference (DiD) and Bartik instrument estimates for the impact of UPI exposure on stock market participation. Observations are at the pincode-month level for the period January 2015 to January 2020. The dependent variable in columns 1 and 3 is the *Number of Transactions* and the dependent variable in columns 2 and 4 is the *Number of Investors*. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. UPI Bartik is calculated as the product of the national UPI over time and the pincode-level UPI-GDP measured as September 2017. The variable *Post* is a dummy variable equal to 1 from November 2016 onward. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table 4
Comparing Open vs. Closed Payment Systems: UPI Expsoure and SBI's YONO

Yono Measure	Value		Volume	
	Number of Transactions	Number of Investors	Number of Transactions	Number of Investors
	(1)	(2)	(3)	(4)
UPI Exposure	619.036***	49.239***	655.435***	51.939***
	(74.880)	(5.669)	(41.494)	(3.177)
Yono Exposure	156.734	11.856	1.485***	0.113***
	(105.238)	(7.971)	(0.547)	(0.041)
District-Month FE	Y	Y	Y	Y
N	495,562	494,719	495,562	494,719
Adj. R^2	0.341	0.348	0.323	0.329

This table shows the comparison of UPI exposure (an open payment system) with YONO, a digital banking platform (a closed payment system) launched by the State Bank of India (SBI) in 2017. The sample is restricted to accounts linked to SBI. YONO exposure is value (columns 1-2) or volume (columns 3-4) of YONO transactions as of Q4 2018. Observations are at the pincode-month level. The dependent variables are *Number of Transactions* (columns 1 amd 3) and *Number of Investors* (columns 2 and 4). Columns 1–2 use YONO calculated in value (millions of INR) and columns 3–4 use YONO calculated in volume terms. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. All regressions include district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table 5
UPI and Stock Market Participation: Within-Investor Variation

Sample	Investor with two or more brokers during entire sample period	Investor with two or more brokers in each month	
DV	Number of Transactions		
	(1)	(2)	(3)
Post X Early UPI Enabled Brokerage	1.337***	1.294***	0.412**
	(0.159)	(0.223)	(0.207)
Investor FE	Y	Y	
District-Month FE	Y	Y	Y
Broker FE	Y	Y	Y
Investor-Month FE			Y
N	55,767,926	15,616,204	15,616,200
Adj. R^2	0.387	0.314	0.155

This table presents the difference-in-difference (DiD) estimates for the impact of UPI adoption on stock market participation. Observations are at the investor level for the period January 2015 to January 2020. The dependent variable is *Number of Transactions* for each investor level. Column 1 is restricted to the sample of investors with two or more brokerage accounts during the sample period. Columns 2 and 3 is further restricted to investors with two or more brokerage accounts in each month. Early UPI Enabled Brokerage is a dummy variable that equals 1 if the bank associated with the brokerage account is an early UPI adopter. The variable *Post* is a dummy variable equal to 1 from November 2016 and later. Investor, district-month, and broker fixed effects are included in columns 1 and 2; and district-month, broker, and investor-month fixed effects are included in column 3. Standard errors are clustered at the investor level and reported in parentheses.

Table 6
UPI and Stock Market Participation: Bank Holiday

	Number of Transactions (1)	Number of Investors (2)	Number of Transactions (3)	Number of Investors (4)
Bank Holiday	-0.679***	-0.485***		
	(0.151)	(0.044)		
Post X UPI Exposure	10.109***	3.324***	10.097***	3.331***
	(0.673)	(0.205)	(0.677)	(0.208)
Bank Holiday X UPI Exposure	-2.667***	-0.917***	-2.953***	-0.936***
	(0.240)	(0.066)	(0.257)	(0.072)
Post X Bank Holiday	1.471***	0.535***		
	(0.350)	(0.103)		
Post X Bank Holiday X UPI Exposure	-0.903*	-0.600***	-0.890	-0.644***
	(0.500)	(0.143)	(0.555)	(0.159)
Pincode FE	Y	Y	Y	Y
Day FE	Y	Y		
State-Day FE			Y	Y
District-Month FE	Y	Y	Y	Y
N	2,058,065	2,058,068	2,058,019	2,058,022
Adj. R^2	0.952	0.962	0.953	0.963

This table presents the triple difference-in-difference (DDD) estimates for the impact of UPI adoption on stock market participation at the pincocde level. The unit of observation is the pincode-day, covering the bank holiday January 2015 to December 2017. The dependent variable in columns 1 and 3 is the *Number of Transactions* for each pincode-day, and the dependent variable in columns 2 and 4 is the *Number of Investors*. *Bank Holiday* is an indicator for bank holidays, but when the National Stock Exchange (NSE) is open. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 onward. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table 7
UPI and Stock Market Participation: Physical Trading vs Internet Trading

DV	Number of Transactions	
	Physical	Internet-based
	(1)	(2)
UPI Exposure X Post	-99.211***	134.152***
	(6.283)	(5.789)
Pincode FE	Y	Y
District-Month FE	Y	Y
N	1,121,313	1,121,363
Adj. R^2	0.833	0.761
Sample Mean	319.782	232.049

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on stock market participation. Observations are at the pincode-month level for the period January 2015 to January 2020. The dependent variable in all columns is the *Number of Transactions*. Column 1 is the corresponding dependent variable calculated for physical trading, while column 2 is the corresponding dependent variable calculated for internet-based trading. UPI exposure is a continuous variable that measures the pincode-level exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 and later. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table 8
Heterogeneity by Urban and Rural Regions

DV	Number of Transactions		Number of Investors	
	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)
UPI Exposure X Post	141.005***	457.899***	13.323***	45.687***
	(9.596)	(87.925)	(0.859)	(7.767)
Pincode FE	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y
N	950,783	161,162	950,714	161,249
Adj. R^2	0.934	0.969	0.929	0.975
T-test	(2)-(1)		(4)-(3)	
	316.893***		32.365***	
	(88.447)		(7.815)	
Pre-UPI Mean	430.222	3855.075	29.502	271.655

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on stock market participation. Observation are at the pincode-month for the period January 2015 to January 2020. The dependent variable in columns 1 and 3 is the *Number of Transactions* in each pincode-month, and the dependent variable in columns 2 and 4 is the *Number of Investors*. Columns 1 and 3 (columns 2 and 4) are the corresponding dependent variables for rural (urban) investors. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. *Post* is a dummy variable equal to 1 from November 2016 and later. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table 9
Consequences: Return & Trading Behavior

Panel A: Abnormal Returns

DV	Abnormal Return (bps)		
Holding Period	1 Trading Day	25 Trading Days	140 Trading Days
	(1)	(2)	(3)
UPI Exposure × Post	-2.193***	-2.442*	-24.212***
	(0.406)	(1.306)	(3.359)
Investor FE	Y	Y	Y
District-Month FE	Y	Y	Y
N	128,491,962	128,404,742	120,991,407
Adj. R^2	0.067	0.042	0.069

Panel B: Trading Behavior

DV	Risk Taking	Diversification	Cross-month Trading Gap	No. of Active Days	No. of Trades
	(1)	(2)	(3)	(4)	(5)
UPI Exposure × Post	-0.002***	0.003***	-0.039**	0.040***	0.125**
	(0.001)	(0.001)	(0.018)	(0.014)	(0.055)
Investor FE	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y
N	104,313,004	129,936,407	130,101,352	130,544,232	129,937,319
Adj. R^2	0.295	0.331	0.155	0.497	0.460

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on stock market participation at the investor level. The unit of observation is the investor-month, covering the period from January 2015 to January 2020. Panel A examines abnormal returns measured over 1, 25, and 140 trading days. Panel B examines trading behavior, including risk taking, portfolio diversification, cross-month trading gap, the number of active trading days, and the number of trades. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 onward. All regressions include investor and district-month fixed effects, as indicated. Standard errors are clustered at the investor level and reported in parentheses.

Table 10
Consequences: Heterogeneity by Cumulative Trading Months

Panel A: Excess Returns

DV	Abnormal Returns (bps)		
Holding Period	1 Trading Day	25 Trading Days	140 Trading Days
	(1)	(2)	(3)
Post × UPI Exposure	-4.596***	-7.549***	-29.412***
	(0.632)	(2.105)	(5.139)
Post × UPI Exposure × Q2 Trading Experience	-0.544	5.441**	15.549**
	(0.787)	(2.649)	(6.228)
Post × UPI Exposure × Q3 Trading Experience	2.742***	9.798***	11.132*
	(0.764)	(2.607)	(6.265)
Post × UPI Exposure × Q4 Trading Experience	5.747***	8.156***	11.494*
	(0.834)	(2.772)	(6.956)
Investor FE	Y	Y	Y
District-Month FE	Y	Y	Y
N	128,491,962	128,404,742	120,991,407
Adj. R^2	0.067	0.042	0.069
1 SD exposure effect (bps):			
Q1: Post × Exposure	-0.864	-1.419	-5.500
Q2: Post × Exposure + Post × Exposure × Q2	-0.966	-0.452	-2.592
Q3: Post × Exposure + Post × Exposure × Q3	-0.349	0.423	-3.418
Q4: Post × Exposure + Post × Exposure × Q4	0.216	0.114	-3.351

Table 10
Consequences: Heterogeneity by Cumulative Trading Months

Panel B: Trading Behavior

DV	Risk Taking	Diversification	Cross-month Trading Gap	No. of Active Days	No. of Trades
	(1)	(2)	(3)	(4)	(5)
Post × UPI Exposure	-0.001	0.002	0.067***	0.001	-0.037
	(0.001)	(0.001)	(0.018)	(0.010)	(0.040)
Post × UPI Exposure × Q2 Trading Experience	-0.001*	0.003***	-0.195***	0.042***	0.105**
	(0.001)	(0.001)	(0.034)	(0.013)	(0.052)
Post × UPI Exposure × Q3 Trading Experience	-0.002***	0.003***	-0.221***	0.057***	0.210***
	(0.001)	(0.001)	(0.033)	(0.015)	(0.062)
Post × UPI Exposure × Q4 Trading Experience	-0.000	0.002	-0.153***	0.098***	0.516***
	(0.001)	(0.002)	(0.036)	(0.025)	(0.120)
Investor FE	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y
N	104,313,004	129,936,407	131,281,827	130,544,232	129,937,319
Adj. R^2	0.297	0.346	0.339	0.516	0.468
1 SD exposure effect (bps):					
Q1: Post × Exposure	-0.0002	0.0004	0.0126	0.0002	-0.0070
Q2: Post × Exposure + Post × Exposure × Q2	-0.0004	0.0009	-0.0241	0.0081	0.0128
Q3: Post × Exposure + Post × Exposure × Q3	-0.0006	0.0009	-0.0290	0.0109	0.0325
Q4: Post × Exposure + Post × Exposure × Q4	-0.0002	0.0008	-0.0162	0.0186	0.0901

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on stock market participation at the investor level across experience quartiles, defined based on cumulative months of active trading. The unit of observation is the investor-month, covering the period from January 2015 to January 2020. Panel A examines abnormal returns measured over 1, 25, and 140 trading days. Panel B examines trading behavior, including risk taking, portfolio diversification, cross-month trading gap, the number of active trading days, and the number of trades. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 onward. The bottom panel reports the implied effect of a one-standard-deviation increase in UPI exposure for each experience quartile. All regressions include investor and district-month fixed effects, as indicated. Standard errors are clustered at the investor level and reported in parentheses.

Table 11
Stock Level Consequences

Panel A: Trading Volume & Annualized Volatility

DV	Trading Volume		Annualized Volatility	
	(1)	(2)	(3)	(4)
UPI Intensity	-700.557	180.037	-5.675***	-4.134***
	(601.505)	(476.892)	(0.986)	(1.019)
Post × UPI Intensity	4743.742***	1925.514***	0.542	-1.621
	(863.720)	(463.856)	(1.105)	(1.203)
Top Retail		670.630		2.871**
		(734.033)		(1.228)
Top Retail × UPI Intensity		-881.203		-3.158
		(1322.126)		(2.132)
Post × Top Retail		-1591.264*		-0.439
		(924.235)		(1.346)
Post × Top Retail × UPI Intensity		3910.778**		2.086
		(1674.837)		(2.340)
Security FE	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y
N	77,948	77,948	77,163	77,163
Adj. R^2	0.785	0.787	0.313	0.327

Panel B: Amihud Illiquidity & Return Reversal

DV	Amihud Illiquidity		Return Reversal	
	(1)	(2)	(3)	(4)
UPI Intensity	0.042	0.060**	0.048	0.137
	(0.026)	(0.027)	(0.113)	(0.124)
Post × UPI Intensity	0.019	-0.000	-0.077	-0.356***
	(0.026)	(0.029)	(0.114)	(0.136)
Top Retail		0.049*		0.230
		(0.028)		(0.143)
Top Retail × UPI Intensity		-0.083*		-0.303
		(0.049)		(0.248)
Post × Top Retail		-0.055**		-0.365**
		(0.027)		(0.152)
Post × Top Retail × UPI Intensity		0.089*		0.655**
		(0.048)		(0.263)
Security FE	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y
N	77,682	77,682	30,518	30,518
Adj. R^2	0.602	0.602	0.056	0.060

Table 11
Stock Level Consequences

Panel C: Monthly Excess Returns

DV	Monthly excess returns (1)	Monthly excess returns (2)
UPI Intensity	-0.128***	-0.118***
	(0.022)	(0.025)
Post × UPI Intensity	-0.003	-0.025
	(0.023)	(0.028)
Top Retail		0.017
		(0.025)
Top Retail × UPI Intensity		-0.022
		(0.043)
Post × Top Retail		-0.001
		(0.029)
Post × Top Retail × UPI Intensity		0.015
		(0.051)
Returns [-1]	-0.036***	-0.036***
	(0.006)	(0.006)
Trade Imbalance [-1]	0.016***	0.016***
	(0.005)	(0.005)
Market Cap [-1]	-0.043***	-0.043***
	(0.002)	(0.002)
Book-to-Market	0.001***	0.001***
	(0.000)	(0.000)
Return Skewness	-0.001**	-0.001**
	(0.001)	(0.001)
Security FE	Y	Y
Year-Month FE	Y	Y
N	61,089	61,089
Adj. R^2	0.162	0.163

This table presents difference-in-differences (DiD) estimates for the impact of UPI exposure on stock-level trading measures. The unit of observation is the stock–month, covering the period from January 2015 to January 2020. Panel A examines effects on trading volume and annualized volatility. Panel B reports results for Amihud illiquidity and return reversal. Panel C reports results for stock returns. UPI exposure is a continuous variable that measures the pincode-level exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 and later. Specifications in even-numbered columns include triple interactions with a Top Retail indicator. All regressions include security and year–month fixed effects. Standard errors are clustered at the security level and are reported in parentheses.

Table 11
Stock Level Consequences

Panel C: Monthly Excess Returns

DV	Forward Monthly Excess Returns (1)	Forward Monthly Excess Returns (2)
UPI Intensity	0.003	0.010
	(0.024)	(0.025)
Post × UPI Intensity	0.017	0.018
	(0.024)	(0.028)
Top Retail		0.007
		(0.026)
Top Retail × UPI Intensity		-0.028
		(0.045)
Post × Top Retail		-0.015
		(0.029)
Post × Top Retail × UPI Intensity		0.026
		(0.050)
Returns [-1]	-0.007	-0.007
	(0.005)	(0.005)
Trade Imbalance [-1]	0.013***	0.013***
	(0.005)	(0.005)
Market Cap [-1]	-0.041***	-0.041***
	(0.002)	(0.002)
Book-to-Market	0.002***	0.002***
	(0.000)	(0.000)
Return Skewness [-1]	0.000	0.000
	(0.001)	(0.001)
Security FE	Y	Y
Year-Month FE	Y	Y
N	61,055	61,055
Adj. R^2	0.162	0.162

This table presents difference-in-differences (DiD) estimates for the impact of UPI exposure on stock-level trading measures. The unit of observation is the stock–month, covering the period from January 2015 to January 2020. Panel A examines effects on trading volume and annualized volatility. Panel B reports results for Amihud illiquidity and return reversal. Panel C reports results for stock returns. UPI exposure is a continuous variable that measures the pincode-level exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 and later. Specifications in even-numbered columns include triple interactions with a Top Retail indicator. All regressions include security and year–month fixed effects. Standard errors are clustered at the security level and are reported in parentheses.

Table 12
Mechanism: Funding Immediacy and Trading Responses to Market Crashes

BSE Test

Event	2019 & 2020		2019		2020	
DV	Number of Trans-actions	Number of Tickers Traded	Number of Trans-actions	Number of Tickers Traded	Number of Trans-actions	Number of Tickers Traded
	(1)	(2)	(3)	(4)	(5)	(6)
Post Crash X UPI Exposure	0.009***	0.007***	0.009*	0.013***	0.009***	0.004***
	(0.003)	(0.001)	(0.005)	(0.002)	(0.003)	(0.002)
Investor FE	Y	Y	Y	Y	Y	Y
Hour FE	Y	Y	Y	Y	Y	Y
N	14,389,045	14,449,978	5,436,143	5,460,537	9,034,245	9,071,293
Adj. R^2	0.117	0.132	0.163	0.182	0.116	0.132

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on stock market participation during a stock market crash. Observations are at the investor-hour level and span a 12-trading-hour-window before and after the two stock market crashes in 2019 and 2020. The dependent variable in columns 1, 3, and 5 is the *Number of Transactions* by each investor-hour, while the dependent variable in columns 2, 4, and 6 is the *Number of Tickers*. Columns 1 and 2 represents the average effects across the two market crashes, while columns 3 (5), and 4 (6) are the corresponding dependent variables for the event in 2019 (and 2020) respectively. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. *Post Crash* is a dummy variable, which takes value 1 for the 12-hours post market crash. All regressions include investor and hour effects as indicated. Standard errors are clustered at the investor level and reported in parentheses.

Table 13
Mechanism: Reduction in Fixed Costs of Small-Value Trading

Panel A: Number of Transactions

DV	Number of Transactions			
Cut-Off	Trading Value – 30,000		Trading Value –50,000	
Small Value Trades	Y	N	Y	N
	(1)	(2)	(3)	(4)
UPI Exposure X Post	115.799***	81.948***	131.587***	66.377***
	(7.090)	(4.795)	(8.079)	(3.794)
Pincode FE	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y
N	1,121,397	1,121,374	1,121,387	1,121,358
Adj. R^2	0.962	0.954	0.964	0.947
T-test	(2)-(1)		(4)-(3)	
	-33.851***		-65.210***	
	(8.559)		(8.926)	

Panel B: Number of Investors

DV	Number of Investors			
Cut-Off	Trading Value – 30,000		Trading Value –50,000	
Small Value Traders	Y	N	Y	N
	(1)	(2)	(3)	(4)
UPI Exposure X Post	17.032***	9.450***	18.103***	7.742***
	(0.862)	(0.518)	(0.928)	(0.421)
Pincode FE	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y
N	1,121,381	1,121,370	1,121,388	1,121,391
Adj. R^2	0.963	0.960	0.964	0.956
T-test	(2)-(1)		(4)-(3)	
	-7.582***		-10.361***	
	(1.006)		(1.019)	

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on stock market participation. Observations are at the pincode-month level for the period January 2015 to January 2020.The dependent variable in panel A is the *Number of Transactions*, computed separately for small-value trades (columns 1 and 3) and large-value trades (columns 2 and 4). The dependent variable in panel B is the *Number of Investors*, computed separately for small-value traders (columns 1 and 3) and large-value traders (columns 2 and 4). UPI exposure is a continuous variable that measures the pincode-level exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 and later. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table 14
Mechanism: Liquidity Reallocation

DV	Number of Transactions (1)	Number of Investors (2)
$\mathbb{1}_{Top\ Tercile} \times$ UPI Exposure $\times\ \mathbb{1}_{Post}$	309.773***	36.636***
	(36.852)	(3.309)
$\mathbb{1}_{Top\ Tercile} \times \mathbb{1}_{Post}$	220.990***	23.613***
	(25.100)	(2.217)
UPI Exposure $\times\ \mathbb{1}_{Post}$	63.057***	5.317***
	(13.594)	(1.224)
R^2	0.967	0.968
District-Month FE	Y	Y
Pincode FE	Y	Y
N	981336	981377

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on stock market participation. Observations are at the pincode-month for the period January 2015 to January 2020. The dependent variable in column 1 is the *Number of Transactions* and the dependent variable in column 2 is the *Number of Investors*. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. *Post* is a dummy variable equal to 1 from November 2016 and later. $\mathbb{1}_{Top\ Tercile}$ is an indicator equal to one for pincodes with in the top tercile of the amount of ATM withdrawals per capita as of March 2016. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Interoperable Payment Infrastructure and Retail Investment

Online Appendix

Figure A1
Stock Market Fund Transfer: Traditional Processes vs. UPI.

The infographic below presents a comparison of the transfer of funds for stock market investment using traditional processes like NEFT versus UPI. The comparison bar at the bottom highlights that UPI trades are cheaper, available 24 x 7, and take lesser time compared to traditional methods.

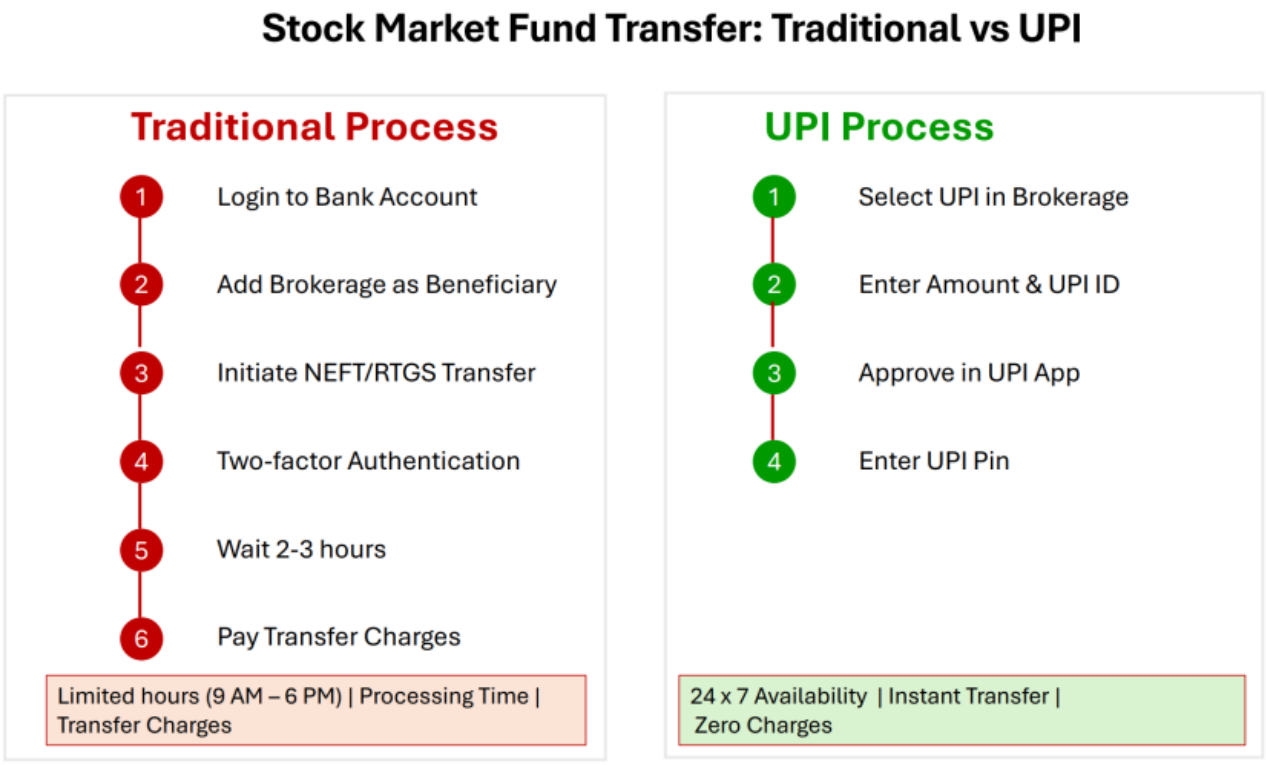
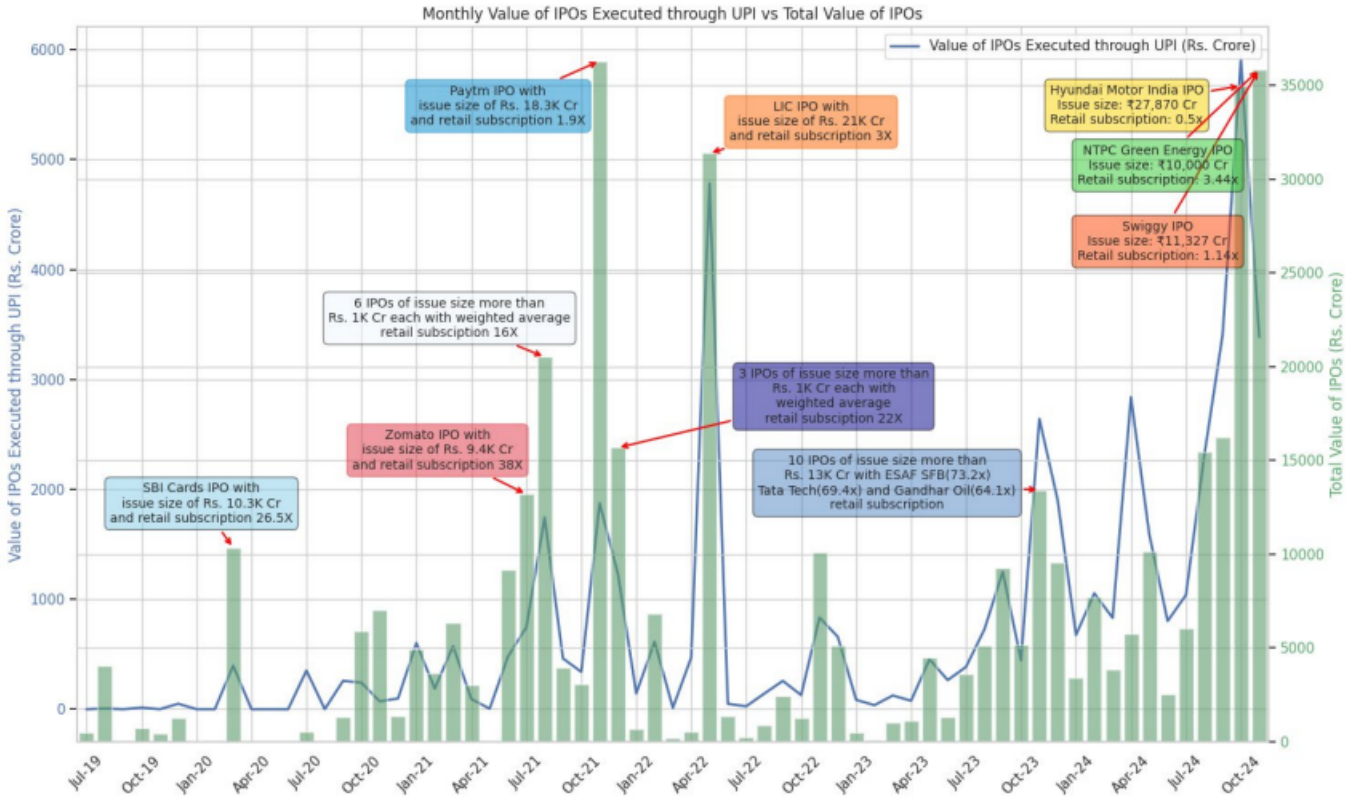


Figure A2
Trends in Increasing Usage of UPI IPO. Source: RBI Payment Systems Report

In 2019, SEBI (Securities and Exchange Board of India) introduced the One Time Mandate (OTM) functionality for IPO payments, integrated with UPI, which allowed investors to authorize payments towards an IPO using a UPI ID and PIN. This substantially streamlined the process, making it convenient for investors, and shortened the time required for public issues to be listed. The picture below, from the 2024 Reserve Bank of India (RBI) Payment Systems Report, shows that the peak transaction value for IPOs executed through UPI, each month from July 2019 to October 2024, coincides with the peak of total issue size of IPOs for that month.



Source: RBI Payment Systems Report

Table A1
Bank Holidays in Each State

State	Date
Andaman & Nicobar Islands	2015-01-15, 2015-01-26, 2015-03-06, 2015-04-03, 2015-07-18, 2015-08-15, 2015-08-28, 2015-09-05, 2015-09-25, 2015-10-02, 2015-10-22, 2015-10-24, 2015-11-11, 2015-11-25, 2015-12-24, 2015-12-25
Andhra Pradesh	2015-01-04, 2015-01-15, 2015-01-26, 2015-02-17, 2015-03-05, 2015-03-21, 2015-04-03, 2015-04-14, 2015-07-18, 2015-08-15, 2015-09-17, 2015-09-24, 2015-10-02, 2015-10-20, 2015-10-22, 2015-12-24, 2015-12-25, 2016-01-15, 2016-01-26, 2016-03-07, 2016-03-23, 2016-04-08, 2016-04-14, 2016-04-15, 2016-07-06, 2016-08-15, 2016-09-05, 2016-10-02, 2016-10-09, 2016-10-11, 2016-10-12, 2016-10-30, 2016-12-12, 2016-12-25, 2017-01-26, 2017-03-29, 2017-04-01, 2017-04-05, 2017-04-14, 2017-05-01, 2017-06-26, 2017-08-15, 2017-08-25, 2017-09-02, 2017-09-29, 2017-09-30, 2017-10-02, 2017-10-19, 2017-11-04, 2017-12-01, 2017-12-25
Arunachal Pradesh	2015-01-14, 2015-01-20, 2015-01-26, 2015-03-06, 2015-04-03, 2015-04-15, 2015-05-04, 2015-07-18, 2015-08-15, 2015-10-02, 2015-10-22, 2015-11-11, 2015-11-25, 2015-12-01, 2015-12-25
Assam	2015-01-14, 2015-01-26, 2015-03-05, 2015-04-03, 2015-04-14, 2015-04-15, 2015-05-01, 2015-07-18, 2015-08-15, 2015-09-25, 2015-10-02, 2015-10-22, 2015-10-23, 2015-11-10, 2015-11-25, 2015-12-25, 2017-01-14, 2017-01-26, 2017-03-12, 2017-04-14, 2017-04-15, 2017-05-01, 2017-06-26, 2017-08-15, 2017-09-02, 2017-09-29, 2017-09-30, 2017-10-02, 2017-10-18, 2017-10-19, 2017-11-04, 2017-12-25
Bihar	2015-01-26, 2015-03-05, 2015-03-06, 2015-03-22, 2015-03-28, 2015-04-03, 2015-04-14, 2015-05-01, 2015-07-18, 2015-08-15, 2015-09-05, 2015-09-25, 2015-10-02, 2015-10-22, 2015-10-23, 2015-10-24, 2015-11-11, 2015-11-17, 2015-11-18, 2015-12-25, 2016-01-26, 2016-03-22, 2016-03-23, 2016-03-24, 2016-03-25, 2016-04-14, 2016-04-15, 2016-05-01, 2016-07-06, 2016-08-15, 2016-08-25, 2016-09-13, 2016-10-02, 2016-10-10, 2016-10-11, 2016-10-12, 2016-10-30, 2016-11-06, 2016-11-07, 2016-12-25, 2017-01-26, 2017-03-13, 2017-03-14, 2017-03-22, 2017-04-05, 2017-04-14, 2017-05-01, 2017-06-26, 2017-08-14, 2017-08-15, 2017-09-02, 2017-09-29, 2017-09-30, 2017-10-01, 2017-10-02, 2017-10-19, 2017-10-26, 2017-10-27, 2017-12-25

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Table A1 – continued from previous page

State	Date
Chandigarh	2015-01-05, 2015-01-26, 2015-02-03, 2015-02-17, 2015-03-06, 2015-04-02, 2015-04-03, 2015-08-15, 2015-09-25, 2015-10-02, 2015-10-22, 2015-10-27, 2015-11-11, 2015-11-25, 2015-12-25, 2017-01-05, 2017-01-26, 2017-02-10, 2017-02-24, 2017-03-13, 2017-04-01, 2017-04-04, 2017-04-13, 2017-04-14, 2017-06-26, 2017-08-15, 2017-10-02, 2017-10-05, 2017-10-19, 2017-11-04, 2017-12-25
Chhattisgarh	2015-01-04, 2015-01-26, 2015-02-17, 2015-03-06, 2015-04-02, 2015-05-04, 2015-07-18, 2015-08-15, 2015-09-05, 2015-09-25, 2015-10-02, 2015-10-22, 2015-10-24, 2015-11-11, 2015-11-25, 2015-12-24, 2015-12-25, 2016-01-26, 2016-03-07, 2016-03-23, 2016-03-24, 2016-03-25, 2016-04-19, 2016-04-20, 2016-05-21, 2016-07-06, 2016-08-15, 2016-08-25, 2016-09-05, 2016-09-13, 2016-10-02, 2016-10-11, 2016-10-12, 2016-10-30, 2016-11-14, 2016-12-12, 2016-12-19, 2016-12-25
Dadra & Nagar Haveli	2016-01-26, 2016-03-24, 2016-03-25, 2016-04-20, 2016-07-06, 2016-08-02, 2016-08-15, 2016-09-05, 2016-09-13, 2016-10-02, 2016-10-11, 2016-10-12, 2016-10-30, 2016-11-14, 2016-12-12, 2016-12-25
Delhi	2015-01-26, 2015-03-06, 2015-04-02, 2015-05-04, 2015-07-18, 2015-08-15, 2015-09-05, 2015-09-25, 2015-10-02, 2015-10-22, 2015-10-24, 2015-11-11, 2015-11-25, 2015-12-24, 2015-12-25, 2016-01-26, 2016-03-24, 2016-03-25, 2016-04-20, 2016-05-21, 2016-07-06, 2016-08-15, 2016-09-13, 2016-10-02, 2016-10-11, 2016-10-12, 2016-10-30, 2016-11-14, 2016-12-12, 2016-12-25, 2017-01-26, 2017-03-13, 2017-04-01, 2017-04-09, 2017-04-14, 2017-05-10, 2017-06-26, 2017-08-15, 2017-09-02, 2017-09-30, 2017-10-01, 2017-10-02, 2017-10-19, 2017-11-04, 2017-12-02, 2017-12-25
Goa	2015-01-26, 2015-03-06, 2015-03-21, 2015-04-03, 2015-04-14, 2015-05-01, 2015-07-18, 2015-08-15, 2015-09-17, 2015-09-18, 2015-09-25, 2015-10-02, 2015-10-22, 2015-11-10, 2015-12-03, 2015-12-19, 2015-12-25, 2016-01-26, 2016-03-24, 2016-03-25, 2016-04-08, 2016-04-14, 2016-05-01, 2016-07-07, 2016-08-15, 2016-09-05, 2016-09-06, 2016-09-13, 2016-10-02, 2016-10-11, 2016-10-29, 2016-12-03, 2016-12-19, 2016-12-25, 2017-01-26, 2017-03-13, 2017-03-28, 2017-04-01, 2017-04-14, 2017-05-01, 2017-06-26, 2017-08-15, 2017-08-25, 2017-08-26, 2017-09-02, 2017-09-30, 2017-10-02, 2017-10-18, 2017-12-04, 2017-12-19, 2017-12-25

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State	Date
Gujarat	2015-01-04, 2015-01-14, 2015-01-26, 2015-03-06, 2015-04-14, 2015-07-18, 2015-08-15, 2015-08-29, 2015-09-17, 2015-09-25, 2015-10-02, 2015-10-22, 2015-11-11, 2015-11-12, 2015-12-24, 2015-12-25, 2016-01-14, 2016-01-26, 2016-03-07, 2016-03-24, 2016-04-14, 2016-04-15, 2016-07-06, 2016-08-15, 2016-08-18, 2016-08-25, 2016-09-05, 2016-10-11, 2016-10-31, 2016-11-01, 2016-12-12, 2017-01-14, 2017-01-26, 2017-02-24, 2017-03-13, 2017-04-01, 2017-04-04, 2017-04-14, 2017-06-26, 2017-08-07, 2017-08-15, 2017-08-25, 2017-09-02, 2017-09-30, 2017-10-02, 2017-10-19, 2017-10-20, 2017-12-02, 2017-12-25
Haryana	2015-01-26, 2015-02-03, 2015-02-17, 2015-04-02, 2015-04-14, 2015-07-18, 2015-08-15, 2015-09-05, 2015-10-02, 2015-10-22, 2015-10-27, 2015-11-11, 2015-11-25, 2015-12-25, 2016-01-26, 2016-02-22, 2016-03-06, 2016-03-07, 2016-03-24, 2016-04-14, 2016-04-20, 2016-05-21, 2016-07-06, 2016-08-15, 2016-08-25, 2016-10-02, 2016-10-11, 2016-10-30, 2016-11-14, 2016-12-25, 2017-01-26, 2017-02-10, 2017-02-24, 2017-03-13, 2017-04-01, 2017-04-09, 2017-04-14, 2017-05-10, 2017-06-26, 2017-08-15, 2017-09-30, 2017-10-02, 2017-10-19, 2017-11-04, 2017-12-25
Himachal Pradesh	2015-01-26, 2015-02-03, 2015-03-06, 2015-03-28, 2015-04-15, 2015-05-04, 2015-07-18, 2015-08-15, 2015-08-29, 2015-09-05, 2015-09-25, 2015-10-02, 2015-10-24, 2015-10-27, 2015-10-30, 2015-11-11, 2015-11-13, 2015-12-25, 2016-01-16, 2016-01-26, 2016-02-22, 2016-03-07, 2016-03-24, 2016-04-15, 2016-05-21, 2016-06-07, 2016-07-06, 2016-08-15, 2016-08-18, 2016-08-25, 2016-09-13, 2016-10-02, 2016-10-11, 2016-10-19, 2016-10-30, 2016-11-01, 2017-01-05, 2017-01-26, 2017-02-10, 2017-02-24, 2017-03-13, 2017-04-01, 2017-04-04, 2017-04-15, 2017-05-10, 2017-06-26, 2017-08-07, 2017-08-15, 2017-09-02, 2017-09-30, 2017-10-02, 2017-10-08, 2017-10-19, 2017-10-21, 2017-11-04

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State	Date
Jammu & Kashmir	2015-01-04, 2015-01-09, 2015-01-26, 2015-02-17, 2015-03-05, 2015-03-21, 2015-04-14, 2015-05-04, 2015-07-05, 2015-07-13, 2015-07-15, 2015-07-17, 2015-07-18, 2015-08-15, 2015-09-05, 2015-09-25, 2015-09-26, 2015-10-02, 2015-10-22, 2015-10-24, 2015-11-11, 2015-11-25, 2015-12-05, 2015-12-25, 2016-01-26, 2016-03-07, 2016-03-23, 2016-04-08, 2016-04-13, 2016-04-14, 2016-05-21, 2016-07-01, 2016-07-03, 2016-07-05, 2016-07-07, 2016-07-13, 2016-08-15, 2016-08-25, 2016-09-13, 2016-09-14, 2016-10-02, 2016-10-11, 2016-10-12, 2016-10-30, 2016-11-25, 2016-12-05, 2016-12-12, 2016-12-16, 2016-12-25, 2017-01-26, 2017-02-24, 2017-03-12, 2017-03-28, 2017-04-01, 2017-04-13, 2017-04-14, 2017-05-10, 2017-06-22, 2017-06-23, 2017-06-26, 2017-07-05, 2017-07-13, 2017-08-15, 2017-09-02, 2017-09-03, 2017-09-30, 2017-10-01, 2017-10-02, 2017-10-19, 2017-11-04, 2017-12-01, 2017-12-05, 2017-12-08, 2017-12-25
Jharkhand	2015-01-04, 2015-01-26, 2015-03-06, 2015-03-23, 2015-03-28, 2015-04-02, 2015-04-03, 2015-05-04, 2015-07-18, 2015-08-15, 2015-09-24, 2015-09-25, 2015-10-02, 2015-10-21, 2015-10-22, 2015-10-24, 2015-11-11, 2015-11-17, 2015-11-25, 2015-12-24, 2015-12-25, 2016-01-26, 2016-03-07, 2016-03-22, 2016-03-24, 2016-03-25, 2016-04-14, 2016-04-15, 2016-04-19, 2016-05-21, 2016-07-06, 2016-08-15, 2016-08-25, 2016-09-13, 2016-10-02, 2016-10-10, 2016-10-11, 2016-10-12, 2016-10-30, 2016-11-14, 2016-12-12, 2016-12-25, 2017-01-26, 2017-02-24, 2017-03-13, 2017-03-30, 2017-04-01, 2017-04-04, 2017-04-14, 2017-05-10, 2017-06-26, 2017-08-15, 2017-09-02, 2017-09-28, 2017-09-29, 2017-09-30, 2017-10-02, 2017-10-19, 2017-10-26, 2017-11-04, 2017-12-02, 2017-12-25
Karnataka	2015-01-04, 2015-01-15, 2015-01-26, 2015-02-17, 2015-03-21, 2015-04-02, 2015-04-03, 2015-04-14, 2015-04-21, 2015-05-01, 2015-07-18, 2015-08-15, 2015-09-17, 2015-09-24, 2015-10-02, 2015-10-12, 2015-10-22, 2015-10-23, 2015-10-24, 2015-10-27, 2015-11-01, 2015-11-10, 2015-11-12, 2015-11-18, 2015-12-24, 2015-12-25, 2016-01-15, 2016-01-26, 2016-03-07, 2016-03-25, 2016-04-08, 2016-04-14, 2016-04-19, 2016-05-01, 2016-05-09, 2016-07-06, 2016-08-15, 2016-09-05, 2016-09-13, 2016-09-30, 2016-10-02, 2016-10-10, 2016-10-11, 2016-10-12, 2016-10-15, 2016-10-29, 2016-10-31, 2016-11-01, 2016-11-17, 2016-12-12, 2016-12-25, 2017-01-14, 2017-01-26, 2017-02-24, 2017-03-29, 2017-04-14, 2017-04-29, 2017-05-01, 2017-06-26, 2017-08-15, 2017-08-25, 2017-09-02, 2017-09-19, 2017-09-30, 2017-10-02, 2017-10-05, 2017-10-18, 2017-10-20, 2017-11-01, 2017-11-06, 2017-12-01, 2017-12-25

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State	Date
Kerala	2015-01-03, 2015-01-26, 2015-02-17, 2015-04-03, 2015-04-05, 2015-04-14, 2015-04-15, 2015-05-01, 2015-07-18, 2015-08-15, 2015-08-27, 2015-08-28, 2015-08-30, 2015-09-21, 2015-09-24, 2015-10-02, 2015-10-22, 2015-10-23, 2015-11-10, 2015-12-25, 2016-01-26, 2016-03-07, 2016-03-25, 2016-03-27, 2016-04-14, 2016-05-01, 2016-07-06, 2016-08-15, 2016-09-13, 2016-09-14, 2016-09-16, 2016-09-21, 2016-10-02, 2016-10-10, 2016-10-11, 2016-10-29, 2016-12-12, 2016-12-25, 2017-01-26, 2017-02-24, 2017-04-01, 2017-04-14, 2017-04-16, 2017-05-01, 2017-06-25, 2017-08-15, 2017-09-01, 2017-09-03, 2017-09-04, 2017-09-06, 2017-09-21, 2017-09-29, 2017-09-30, 2017-10-02, 2017-10-18, 2017-12-02, 2017-12-25
Madhya Pradesh	2015-01-04, 2015-01-26, 2015-02-17, 2015-03-06, 2015-03-28, 2015-04-02, 2015-05-04, 2015-07-18, 2015-08-15, 2015-09-25, 2015-10-02, 2015-10-22, 2015-10-24, 2015-11-11, 2015-11-25, 2015-12-25, 2016-01-26, 2016-03-07, 2016-03-23, 2016-04-15, 2016-04-19, 2016-05-21, 2016-07-06, 2016-08-15, 2016-08-18, 2016-08-25, 2016-09-13, 2016-10-02, 2016-10-11, 2016-10-12, 2016-10-30, 2016-11-14, 2016-12-12, 2016-12-25, 2017-01-26, 2017-03-13, 2017-04-01, 2017-04-05, 2017-04-14, 2017-05-10, 2017-06-26, 2017-08-07, 2017-08-15, 2017-09-02, 2017-09-30, 2017-10-02, 2017-10-19, 2017-11-04, 2017-12-02, 2017-12-25
Maharashtra	2015-01-04, 2015-01-26, 2015-02-17, 2015-02-19, 2015-03-06, 2015-03-21, 2015-03-28, 2015-04-02, 2015-04-03, 2015-04-14, 2015-05-01, 2015-05-04, 2015-07-18, 2015-08-15, 2015-08-18, 2015-09-17, 2015-09-25, 2015-10-02, 2015-10-22, 2015-10-24, 2015-11-11, 2015-11-12, 2015-11-25, 2015-12-24, 2015-12-25, 2016-01-26, 2016-02-19, 2016-03-07, 2016-03-24, 2016-03-25, 2016-04-08, 2016-04-14, 2016-04-15, 2016-04-19, 2016-05-01, 2016-05-21, 2016-07-06, 2016-08-15, 2016-08-17, 2016-09-05, 2016-09-13, 2016-10-02, 2016-10-11, 2016-10-12, 2016-10-30, 2016-10-31, 2016-11-14, 2016-12-12, 2016-12-25, 2017-01-26, 2017-02-19, 2017-02-24, 2017-03-13, 2017-03-28, 2017-04-01, 2017-04-04, 2017-04-09, 2017-04-14, 2017-05-01, 2017-05-10, 2017-06-26, 2017-08-15, 2017-08-17, 2017-08-25, 2017-09-02, 2017-09-30, 2017-10-01, 2017-10-02, 2017-10-19, 2017-10-20, 2017-11-04, 2017-12-01, 2017-12-25

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Table A1 – continued from previous page

State	Date
Manipur	2015-01-01, 2015-01-03, 2015-01-26, 2015-02-15, 2015-03-06, 2015-03-21, 2015-04-03, 2015-04-14, 2015-05-01, 2015-07-18, 2015-08-13, 2015-08-15, 2015-10-02, 2015-10-21, 2015-11-01, 2015-11-11, 2015-11-13, 2015-12-25, 2016-01-01, 2016-01-22, 2016-01-26, 2016-02-15, 2016-03-23, 2016-03-25, 2016-04-08, 2016-04-13, 2016-05-01, 2016-07-06, 2016-08-13, 2016-08-15, 2016-08-25, 2016-10-02, 2016-10-09, 2016-10-30, 2016-11-01, 2016-12-12, 2016-12-25, 2016-12-26, 2016-12-31
Meghalaya	2015-01-01, 2015-01-26, 2015-03-06, 2015-04-03, 2015-07-11, 2015-07-17, 2015-07-18, 2015-08-15, 2015-09-25, 2015-10-02, 2015-10-22, 2015-10-23, 2015-11-06, 2015-11-11, 2015-11-23, 2015-12-12, 2015-12-18, 2015-12-25, 2015-12-26, 2015-12-30
Mizoram	2015-01-01, 2015-01-02, 2015-01-26, 2015-02-20, 2015-03-06, 2015-04-03, 2015-05-04, 2015-06-15, 2015-06-30, 2015-07-18, 2015-08-15, 2015-10-02, 2015-10-22, 2015-11-11, 2015-12-24, 2015-12-25, 2015-12-26, 2015-12-31
Nagaland	2015-01-01, 2015-01-26, 2015-04-03, 2015-07-18, 2015-08-15, 2015-09-25, 2015-10-02, 2015-10-21, 2015-10-22, 2015-11-11, 2015-11-25, 2015-12-01, 2015-12-24, 2015-12-25, 2015-12-26, 2015-12-27
Orissa	2015-01-24, 2015-01-26, 2015-02-17, 2015-03-06, 2015-03-28, 2015-04-03, 2015-04-14, 2015-07-18, 2015-08-15, 2015-09-05, 2015-09-17, 2015-09-18, 2015-10-02, 2015-10-21, 2015-10-23, 2015-10-24, 2015-11-11, 2015-12-25, 2016-01-26, 2016-02-12, 2016-03-07, 2016-03-24, 2016-03-25, 2016-04-13, 2016-04-14, 2016-04-15, 2016-06-15, 2016-07-06, 2016-07-07, 2016-08-15, 2016-08-25, 2016-09-05, 2016-09-06, 2016-10-02, 2016-10-09, 2016-10-11, 2016-10-12, 2016-10-20, 2016-12-25, 2017-01-26, 2017-02-01, 2017-02-24, 2017-03-13, 2017-04-01, 2017-04-04, 2017-04-14, 2017-06-15, 2017-06-26, 2017-08-14, 2017-08-15, 2017-08-25, 2017-09-02, 2017-09-19, 2017-09-29, 2017-09-30, 2017-10-02, 2017-10-05, 2017-10-19, 2017-12-25
Pondicherry	2015-01-01, 2015-01-03, 2015-01-15, 2015-01-16, 2015-01-26, 2015-04-03, 2015-04-14, 2015-05-01, 2015-07-18, 2015-08-15, 2015-08-16, 2015-09-17, 2015-09-24, 2015-10-02, 2015-10-10, 2015-10-21, 2015-11-01, 2015-11-10, 2015-12-24, 2015-12-25, 2016-01-01, 2016-01-15, 2016-01-16, 2016-01-26, 2016-03-25, 2016-04-14, 2016-07-07, 2016-08-15, 2016-08-16, 2016-09-05, 2016-09-13, 2016-10-02, 2016-10-29, 2016-11-01, 2016-12-12, 2016-12-25

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State	Date
Punjab	2015-01-26, 2015-02-03, 2015-03-06, 2015-03-28, 2015-05-22, 2015-07-18, 2015-08-15, 2015-09-05, 2015-10-02, 2015-10-22, 2015-10-27, 2015-11-11, 2015-11-25, 2015-12-25, 2016-01-16, 2016-01-26, 2016-02-22, 2016-03-24, 2016-04-15, 2016-06-08, 2016-07-06, 2016-08-15, 2016-08-25, 2016-10-02, 2016-10-11, 2016-10-16, 2016-10-30, 2016-11-14, 2016-12-25, 2017-01-05, 2017-01-26, 2017-02-10, 2017-03-13, 2017-04-01, 2017-04-04, 2017-06-16, 2017-06-26, 2017-08-15, 2017-09-30, 2017-10-02, 2017-10-05, 2017-10-19, 2017-11-04, 2017-12-25
Rajasthan	2015-01-26, 2015-03-06, 2015-04-02, 2015-04-14, 2015-07-18, 2015-08-15, 2015-08-29, 2015-09-25, 2015-10-02, 2015-10-22, 2015-10-24, 2015-11-11, 2015-11-25, 2015-12-25, 2016-01-26, 2016-03-24, 2016-04-14, 2016-04-15, 2016-04-19, 2016-07-06, 2016-08-15, 2016-08-18, 2016-08-25, 2016-09-13, 2016-10-02, 2016-10-11, 2016-10-12, 2016-10-30, 2016-10-31, 2016-11-14, 2016-12-25, 2017-01-26, 2017-03-13, 2017-04-01, 2017-04-04, 2017-04-14, 2017-06-26, 2017-08-07, 2017-08-15, 2017-09-02, 2017-09-30, 2017-10-02, 2017-10-19, 2017-10-20, 2017-11-04, 2017-12-25
Sikkim	2015-01-01, 2015-01-14, 2015-01-24, 2015-01-26, 2015-02-19, 2015-03-06, 2015-03-28, 2015-04-03, 2015-04-14, 2015-05-01, 2015-05-16, 2015-06-02, 2015-07-13, 2015-07-18, 2015-08-15, 2015-09-05, 2015-10-02, 2015-11-11, 2015-11-13, 2015-12-12, 2015-12-22, 2015-12-23, 2015-12-24, 2015-12-25
Tamil Nadu	2015-01-01, 2015-01-04, 2015-01-15, 2015-01-16, 2015-01-17, 2015-01-26, 2015-03-21, 2015-04-02, 2015-04-03, 2015-04-14, 2015-05-01, 2015-07-18, 2015-08-15, 2015-09-05, 2015-09-17, 2015-09-24, 2015-10-02, 2015-10-21, 2015-10-22, 2015-10-23, 2015-11-10, 2015-12-23, 2015-12-25, 2016-01-01, 2016-01-15, 2016-01-16, 2016-01-17, 2016-01-26, 2016-03-25, 2016-04-08, 2016-04-14, 2016-04-19, 2016-05-01, 2016-07-07, 2016-08-15, 2016-08-25, 2016-09-05, 2016-09-13, 2016-10-02, 2016-10-10, 2016-10-11, 2016-10-12, 2016-10-29, 2016-12-12, 2016-12-25, 2017-01-01, 2017-01-14, 2017-01-15, 2017-01-16, 2017-01-26, 2017-03-29, 2017-04-01, 2017-04-09, 2017-04-14, 2017-05-01, 2017-06-26, 2017-08-14, 2017-08-15, 2017-08-25, 2017-09-02, 2017-09-29, 2017-09-30, 2017-10-01, 2017-10-02, 2017-10-18, 2017-12-01, 2017-12-25

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State	Date
Telangana	2015-01-04, 2015-01-15, 2015-01-26, 2015-02-17, 2015-03-21, 2015-04-03, 2015-04-05, 2015-04-14, 2015-07-18, 2015-08-15, 2015-09-17, 2015-09-24, 2015-10-02, 2015-10-22, 2015-10-24, 2015-11-11, 2015-12-25, 2016-01-15, 2016-01-26, 2016-03-07, 2016-03-23, 2016-04-05, 2016-04-08, 2016-04-14, 2016-04-15, 2016-05-01, 2016-07-06, 2016-08-15, 2016-08-25, 2016-09-05, 2016-10-02, 2016-10-09, 2016-10-11, 2016-10-12, 2016-10-30, 2016-11-14, 2016-12-12, 2016-12-25, 2017-01-14, 2017-01-26, 2017-02-24, 2017-03-12, 2017-03-29, 2017-04-01, 2017-04-05, 2017-04-14, 2017-05-01, 2017-06-26, 2017-08-15, 2017-08-25, 2017-09-02, 2017-09-29, 2017-09-30, 2017-10-01, 2017-10-02, 2017-10-19, 2017-11-04, 2017-12-01, 2017-12-25
Tripura	2015-01-23, 2015-01-26, 2015-04-14, 2015-04-15, 2015-04-21, 2015-05-01, 2015-07-18, 2015-07-24, 2015-08-08, 2015-08-15, 2015-09-25, 2015-10-02, 2015-10-12, 2015-10-20, 2015-10-21, 2015-10-22, 2015-10-24, 2015-10-26, 2015-11-11, 2015-12-25, 2016-01-26, 2016-02-12, 2016-04-13, 2016-04-14, 2016-04-20, 2016-07-06, 2016-07-12, 2016-07-26, 2016-08-15, 2016-09-13, 2016-09-30, 2016-10-02, 2016-10-10, 2016-10-11, 2016-10-12, 2016-10-15, 2016-10-29
Uttar Pradesh	2015-01-04, 2015-01-26, 2015-02-17, 2015-03-05, 2015-03-06, 2015-03-28, 2015-04-02, 2015-04-03, 2015-04-14, 2015-05-03, 2015-05-04, 2015-07-18, 2015-08-15, 2015-08-29, 2015-09-05, 2015-09-25, 2015-10-02, 2015-10-21, 2015-10-22, 2015-10-24, 2015-11-11, 2015-11-12, 2015-11-13, 2015-11-25, 2015-12-25, 2016-01-26, 2016-03-07, 2016-03-23, 2016-03-24, 2016-03-25, 2016-04-14, 2016-04-15, 2016-04-20, 2016-04-21, 2016-05-21, 2016-07-06, 2016-08-15, 2016-08-18, 2016-08-25, 2016-09-13, 2016-10-02, 2016-10-10, 2016-10-11, 2016-10-12, 2016-10-30, 2016-10-31, 2016-11-01, 2016-11-14, 2016-12-12, 2016-12-25, 2017-01-26, 2017-02-24, 2017-03-12, 2017-03-13, 2017-04-04, 2017-04-09, 2017-04-11, 2017-04-14, 2017-05-10, 2017-06-26, 2017-08-07, 2017-08-15, 2017-09-02, 2017-09-29, 2017-09-30, 2017-10-01, 2017-10-02, 2017-10-19, 2017-10-20, 2017-10-21, 2017-11-04, 2017-12-02, 2017-12-25

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State	Date
Uttarakhand	2015-01-04, 2015-01-26, 2015-02-17, 2015-03-05, 2015-03-06, 2015-03-28, 2015-04-03, 2015-04-14, 2015-05-04, 2015-07-18, 2015-08-15, 2015-08-29, 2015-09-05, 2015-09-25, 2015-10-02, 2015-10-22, 2015-11-11, 2015-11-12, 2015-11-13, 2015-11-25, 2015-12-25, 2016-01-26, 2016-03-07, 2016-03-23, 2016-03-24, 2016-03-25, 2016-04-15, 2016-05-21, 2016-07-06, 2016-08-15, 2016-08-18, 2016-08-25, 2016-09-13, 2016-10-02, 2016-10-11, 2016-10-30, 2016-10-31, 2016-11-01, 2016-11-14, 2016-12-12, 2016-12-25, 2017-01-26, 2017-02-24, 2017-03-12, 2017-03-13, 2017-04-01, 2017-04-04, 2017-04-14, 2017-05-10, 2017-06-26, 2017-08-07, 2017-08-15, 2017-09-02, 2017-09-30, 2017-10-02, 2017-10-19, 2017-10-20, 2017-10-21, 2017-11-04, 2017-12-02, 2017-12-25
West Bengal	2015-01-12, 2015-01-23, 2015-01-25, 2015-01-26, 2015-03-05, 2015-04-03, 2015-04-14, 2015-04-15, 2015-05-01, 2015-05-09, 2015-07-18, 2015-08-15, 2015-09-25, 2015-10-02, 2015-10-12, 2015-10-20, 2015-10-21, 2015-10-22, 2015-10-24, 2015-10-26, 2015-11-10, 2015-11-25, 2015-12-25, 2016-01-12, 2016-01-23, 2016-01-26, 2016-02-13, 2016-03-23, 2016-03-25, 2016-04-14, 2016-05-01, 2016-05-08, 2016-07-06, 2016-08-15, 2016-09-13, 2016-09-30, 2016-10-02, 2016-10-08, 2016-10-09, 2016-10-10, 2016-10-11, 2016-10-12, 2016-10-15, 2016-10-29, 2016-11-14, 2016-12-25, 2017-01-12, 2017-01-23, 2017-01-26, 2017-02-01, 2017-03-12, 2017-04-14, 2017-05-01, 2017-05-09, 2017-06-26, 2017-08-15, 2017-09-02, 2017-09-19, 2017-09-27, 2017-09-28, 2017-09-29, 2017-09-30, 2017-10-01, 2017-10-02, 2017-10-05, 2017-10-19, 2017-11-04, 2017-12-25

Note: This table lists all bank holidays at each state from 2015 to 2017.

Table A2
NSE Holidays

Year	Holiday Dates
2015	01/26, 02/17, 03/06, 04/02, 04/03, 04/14, 05/01, 09/17, 09/25, 10/02, 10/22, 11/11, 11/12, 11/25, 12/25
2016	01/26, 03/07, 03/24, 03/25, 04/14, 04/15, 04/19, 07/06, 08/15, 09/05, 09/13, 10/11, 10/12, 10/31, 11/14
2017	01/26, 02/24, 03/13, 03/28, 04/04, 04/14, 05/01, 05/10, 06/26, 08/15, 08/17, 08/25, 10/02, 10/19, 10/20, 12/01, 12/25

Note: This table presents the National Stock Exchange (NSE) holidays for the years 2015 through 2017.

Table A3
Balance Test for UPI Bartik

Variable	(1) High UPI Bartik N	(1) High UPI Bartik Mean/(SE)	(2) Low UPI Bartik N	(2) Low UPI Bartik Mean/(SE)	(1-2) Pairwise t-test Mean Difference
Pincode: NSE Sample					
Economic Activity	9,306	14.875	9,307	21.007	-6.132
		(13.768)		(17.679)	
Number of Transactions	204,638	1628.943	202,416	288.837	1340.106
		(3668.848)		(1059.22)	
Number of Investors	204,638	114.776	202,416	19.525	95.251
		(266.315)		(74.212)	
Growth in Number of Transactions	204,638	0.177	202,416	0.314	-0.137
		(1.616)		(2.705)	
Growth in Number of Investors	204,638	0.045	202,416	0.049	-0.004
		(0.371)		(0.48)	
Investor: NSE Sample					
Age	659,574	37.649	659,676	38.117	-0.468
		(12.849)		(13.253)	
Female	659,574	0.117	659,676	0.135	-0.018
		(0.321)		(0.343)	

This table compares ex-ante differences in key variables for high low values of Bartik UPI for the period of January 2015 to June 2016. Key variables include levels of economic activity as proxied by night-light intensity, stock market activity as proxied by the number of transactions, number of investors; growth in the number of transactions, and growth in number of investors. UPI Bartik is calculated as the product of the national UPI over time and the pincode-level UPI-GDP measured as September 2017. The average for the high-UPI Bartik (column 2), low-UPI Bartik (column 2), and the difference between the two (column 3) is shown.

Table A4
Effects of UPI on Stock Market Participation: Balanced Panel

Panel A: Retaining the Outliers

DV	Number of Transactions	Number of Investors
	(1)	(2)
UPI Exposure × Post	268.689***	30.322***
	(19.567)	(2.330)
Pincode FE	Y	Y
District-Month FE	Y	Y
N	1,132,770	1,132,770
Adj. R^2	0.952	0.944

Panel B: Capping the Outliers

DV	Number of Transactions	Number of Investors
	(1)	(2)
UPI Exposure × Post	209.914**	21.631***
	(12.697)	(1.195)
Pincode FE	Y	Y
District-Month FE	Y	Y
N	1,132,770	1,132,770
Adj. R^2	0.975	0.975

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on stock market participation on a balanced panel. Observations are at the pincode-month level for the period January 2015 to January 2020. In panel A, baseline regression has been run on the full sample, including outliers. In panel B, outliers have been replaced with the capped values (1/99 percentile value). The dependent variable in column 1 in both the panels is the *Number of Transactions* and the dependent variable in column 2 in both the panels is the *Number of Investors*. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 onward. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table A5
Effects of UPI on Stock Market Participation: Per 1000 residents

DV	Number of transactions per 1000 residents (1)	Number of investors per 1000 residents (2)
UPI Exposure × Post	5.976***	0.600***
	(1.346)	(0.138)
Pincode FE	Y	Y
District-Month FE	Y	Y
N	1,120,463	1,120,470
Adj. R^2	0.963	0.959

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on population-normalized stock market participation. Observations are at the pincode-month level for the period January 2015 to January 2020. The dependent variable in column 1 is the *Number of Transactions per 1000 residents* and the dependent variable in column 2 is the *Number of Investors per 1000 residents*. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 onward. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table A6
Effects of UPI on Stock Market Participation: Quintile-wise regressions

Panel A: Number of Transactions

DV	Number of Transactions				
	Q1	Q2	Q3	Q4	Q5
	(1)	(2)	(3)	(4)	(5)
UPI Exposure × Post	11.859***	12.251***	32.728***	51.932***	276.002***
	(2.522)	(4.309)	(6.952)	(13.251)	(104.805)
Pincode FE	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y
N	219,417	221,064	219,844	218,868	210,274
Adj. R^2	0.526	0.561	0.590	0.684	0.960
Pre-UPI mean	9.595	48.916	129.329	371.354	4345.152

Panel B: Number of Investors

DV	Number of Investors				
	Q1	Q2	Q3	Q4	Q5
	(1)	(2)	(3)	(4)	(5)
UPI Exposure × Post	0.520***	0.805***	0.601*	2.569***	28.797***
	(0.125)	(0.209)	(0.337)	(0.732)	(9.361)
Pincode FE	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y
N	219,661	221,369	218,624	218,441	209,498
Adj. R^2	0.702	0.743	0.803	0.859	0.967
Pre-UPI mean	1.238	4.222	9.520	25.452	302.288

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on stock market participation across quintiles defined by pre-trading activity. Observations are at the pincode-month level for the period January 2015 to January 2020. The dependent variables are the *Number of Transactions* and the *Number of Investors* in panel A and panel B respectively. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 onward. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table A7
Effects of UPI on Stock Market Participation: Asinh Transformation

Panel A: Unscaled

DV	Number of Transactions	Number of Investors
	(1)	(2)
UPI Exposure × Post	-0.195***	-0.067***
	(0.021)	(0.009)
Pincode FE	Y	Y
District-Month FE	Y	Y
N	1,121,378	1,121,385
Adj. R^2	0.873	0.958

Panel B: Pre-scaled Median

DV	Number of Transactions	Number of Investors
	(1)	(2)
UPI Exposure × Post	0.047***	0.050***
	(0.008)	(0.006)
Pincode FE	Y	Y
District-Month FE	Y	Y
N	1,121,378	1,121,385
Adj. R^2	0.943	0.979

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on stock market participation after transforming the outcomes using the inverse hyperbolic function. Observations are at the pincode-month level for the period January 2015 to January 2020. The dependent variable in both the panels is *asinh(y)*. In panel A, *y* denotes the *Number of Transactions* and the *Number of Investors* in columns 1 and 2 respectively. In panel B, *y* denotes the *Number of Transactions scaled by Pre-Median* and the *Number of Investors scaled by Pre-Median* in columns 1 and 2 respectively. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 onward. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table A8
Effects of UPI on Stock Market Participation
PPMLHDFE Estimates for Quintile-wise regressions

Panel A: Number of Transactions

DV	Number of Transactions				
	Q1	Q2	Q3	Q4	Q5
	(1)	(2)	(3)	(4)	(5)
UPI Exposure × Post	0.151***	0.042	0.095***	0.062***	-0.032
	(0.040)	(0.027)	(0.024)	(0.022)	(0.024)
Pincode FE	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y
N	217,814	221,062	219,844	218,868	210,274

Panel B: Number of Investors

DV	Number of Investors				
	Q1	Q2	Q3	Q4	Q5
	(1)	(2)	(3)	(4)	(5)
UPI Exposure × Post	0.063**	0.032**	0.010	0.018	-0.025
	(0.026)	(0.016)	(0.013)	(0.012)	(0.018)
Pincode FE	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y
N	218,130	221,367	218,624	218,441	209,498

This table presents difference-in-differences (DiD) PPMLHDFE estimates of the impact of UPI exposure on log-level measures of stock market participation across quintiles defined by pre-trading activity. Observations are at the pincode-month level for January 2015 to January 2020. The dependent variables are *Number of Transactions* in panel A and the *Number of Investors* in panel B. UPI exposure is a continuous variable measuring regional exposure, as defined in equation 1. *Post* equals one from November 2016 onward. All regressions include pincode and district-month fixed effects. Standard errors are clustered at the pincode level and reported in parentheses.

Table A9
Effects of UPI on Stock Market Participation: Placebo Test

Randomization Runs	100		500	
DV	Number of Transactions	Number of Investors	Number of Transactions	Number of Investors
	(1)	(2)	(3)	(4)
UPI Exposure X Post	0.589	0.129	0.414	0.123
	(15.196)	(1.288)	(15.145)	(1.237)
Pincode FE	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y

This table presents the results of a placebo test using difference-in-difference (DiD) estimates to assess the impact of UPI exposure on stock market participation at the regional (pincode) level. Columns 1 and 2 report estimates based on 100 randomizations, while Columns 3 and 4 present results from 500 randomizations. The unit of observation is the pincode-month, covering the period from January 2015 to January 2020. The dependent variable in columns 1 and 3 is the *Number of Transactions* and the dependent variable in columns 2 and 4 is the *Number of Investors*. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 onward. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table A10
Effects of UPI on Stock Market Participation: Placebo Test – Institutional Investors

DV	Number of Transactions	Number of Investors
	(1)	(2)
UPI Exposure X Post	11.859	0.466
	(7.625)	(0.319)
Pincode FE	Y	Y
District-Month FE	Y	Y
N	193,597	193,602
Adj.-R2	0.913	0.943

This table presents the results of a placebo test using difference-in-difference (DiD) estimates to assess the impact of UPI exposure on stock market participation of *institutional investors* at the regional (pincode) level. The unit of observation is the pincode-month, covering the period from January 2015 to January 2020. Column 1 presents the results for *Number of Transactions*, while columns 2 presents the results for *Number of Investors*. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 onward. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table A11
UPI and Stock Market Participation: Heterogeneity

Panel A: Number of Transactions

DV	Number of Transactions						
	Young	Middle Age	Mature	Female	Male	FinTech	Non-FinTech
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
UPI Exposure X Post	66.864***	123.710***	-0.478	21.786***	167.045***	184.076***	84.613***
	(2.960)	(6.661)	(3.026)	(1.324)	(9.244)	(10.049)	(13.050)
Pincode FE	Y	Y	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y	Y	Y
N	1,121,353	1,121,376	1,121,368	1,121,405	1,121,369	1,132,770	1,132,770
Adj. R^2	0.813	0.955	0.963	0.846	0.962	0.778	0.953
Sample Mean	127.048	604.343	352.849	60.914	878.793	265.338	1309.999

Panel B: Number of Investors

DV	Number of Investors						
	Young	Middle Age	Mature	Female	Male	FinTech	Non-FinTech
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
UPI Exposure X Post	6.500***	11.150***	2.041***	2.109***	16.799***	8.320***	12.059***
	(0.286)	(0.577)	(0.181)	(0.116)	(0.826)	(0.342)	(0.751)
Pincode FE	Y	Y	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y	Y	Y
N	1,121,361	1,121,403	1,121,379	1,121,400	1,121,377	1,121,388	1,121,389
Adj. R^2	0.835	0.965	0.981	0.904	0.961	0.763	0.975
Sample Mean	11.710	46.692	21.602	4.994	64.620	11.597	69.933

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on stock market participation. Observations are at the pincode-month level for the period January 2015 to January 2020. The dependent variable in panel A is the *Number of Transactions* in each pincode-month, and the dependent variable in panel B is the *Number of Investors*. Columns 1, 2 and 3 are the corresponding dependent variables calculated for each age group, while columns 4 and 5 are the corresponding dependent variables calculated for gender group, while columns 6 and 7 are the corresponding variables calculated for FinTech and Non-FinTech brokers. UPI exposure is a continuous variable that measures the pincode-level exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 and later. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses.

Table A12
Consequences: Heterogeneity by Pre-Period Cumulative Trading Months

Panel A: Abnormal Returns

DV	Abnormal Returns (bps)		
Holding Period	1 Trading Day	25 Trading Days	140 Trading Days
	(1)	(2)	(3)
Post × UPI Exposure	-15.407***	-21.455***	-103.519***
	(2.104)	(6.588)	(15.795)
Post × UPI Exposure × Q2 Trading Experience	6.983***	14.377*	60.578***
	(2.331)	(7.343)	(17.737)
Post × UPI Exposure × Q3 Trading Experience	11.973***	20.800***	91.608***
	(2.221)	(6.990)	(16.873)
Post × UPI Exposure × Q4 Trading Experience	15.530***	20.604***	85.944***
	(2.129)	(6.677)	(16.049)
Investor FE	Y	Y	Y
District-Month FE	Y	Y	Y
N	90,658,051	90,626,473	87,345,101
Adj. R^2	0.068	0.033	0.053
1 SD exposure effect (bps):			
Q1: Post × Exposure	-2.773	-3.862	-18.633
Q2: Post × Exposure + Post × Exposure × Q2	-1.516	-1.274	-7.729
Q3: Post × Exposure + Post × Exposure × Q3	-0.618	-0.118	-2.144
Q4: Post × Exposure + Post × Exposure × Q4	0.022	-0.153	-3.164

Table A12
Consequences: Heterogeneity by Pre-Period Cumulative Trading Months

Panel B: Trading Behavior

DV	Risk Taking	Diversification	Cross-month Trading Gap	No. of Active Days	No. of Trades
	(1)	(2)	(3)	(4)	(5)
Post × UPI Exposure	-0.001	0.006**	-0.052	-0.021	-0.109*
	(0.002)	(0.003)	(0.041)	(0.018)	(0.063)
Post × UPI Exposure × Q2 Trading Experience	-0.003	-0.003	0.212***	0.026	0.058
	(0.002)	(0.003)	(0.054)	(0.023)	(0.078)
Post × UPI Exposure × Q3 Trading Experience	-0.004*	0.000	0.085*	0.036	0.077
	(0.002)	(0.003)	(0.051)	(0.024)	(0.083)
Post × UPI Exposure × Q4 Trading Experience	-0.000	-0.003	-0.016	0.103***	0.408***
	(0.002)	(0.003)	(0.045)	(0.023)	(0.090)
Investor FE	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y
N	74,728,834	91,394,140	91,598,047	91,908,495	91,308,910
Adj. R^2	0.291	0.317	0.149	0.509	0.472
1 SD exposure effect (bps):					
Q1: Post × Exposure	-0.0002	0.0011	-0.0094	-0.0038	-0.0196
Q2: Post × Exposure + Post × Exposure × Q2	-0.0007	0.0005	0.0288	0.0009	-0.0092
Q3: Post × Exposure + Post × Exposure × Q3	-0.0009	0.0011	0.0059	0.0027	-0.0058
Q4: Post × Exposure + Post × Exposure × Q4	-0.0002	0.0005	-0.0122	0.0148	0.0538

This table presents the difference-in-difference (DiD) estimates for the impact of UPI exposure on stock market participation at the investor level across experience quartiles, defined based on pre-period cumulative trading months. The quartile classification is fixed throughout the sample. The unit of observation is the pincode-month, covering the period from January 2015 to January 2020. Panel A examines abnormal returns measured over 1, 25, and 140 trading days. Panel B examines trading behavior, including risk taking, portfolio diversification, cross-month trading gap, the number of active trading days, and the number of trades. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. The variable *Post* is a dummy variable equal to 1 from November 2016 onward. The bottom panel reports the implied effect of a one-standard-deviation increase in UPI exposure for each experience quartile. All regressions include investor and district-month fixed effects, as indicated. Standard errors are clustered at the investor level and reported in parentheses.