Funding the Trade:

Payment Interoperability and Retail Investors*

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ABSTRACT

Does payment infrastructure shape who trades and how well they invest? We study India's Unified Payments Interface, an interoperable payment rail, using transactions of 19.8 million retail investors. A one-standard-deviation increase in exposure to early-adopting banks raises monthly transactions 6.2% and active investors 8.7%. A proprietary single-bank payment platform produces much smaller effects. Responses concentrate in small trades and cash-intensive regions and intensify around market crashes. High- and low-exposure regions converge on bank holidays, when conventional rails close. Additional trading comes from experienced investors, while long-horizon return declines fall on inexperienced investors. Thus, open rails expand participation without improving performance.

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1 Introduction

On January 28, 2021, Robinhood restricted purchases of GameStop after an extraordinary clearinghouse collateral call, thrusting the plumbing of financial markets into the public spotlight.[1] The subsequent policy debate focused largely on clearing and settlement. But the episode also highlights that trading rests on an infrastructure for moving money that investors rarely notice until it becomes constrained.[2] Before an investor can buy a share, money must first reach the brokerage account. How easily it gets there depends on the payment system. Finance has studied extensively the frictions investors face in acquiring information and executing trades. It has paid much less attention to the friction that comes one step earlier: funding the trade. What happens when this friction is removed—not for a subset of investors or within a particular brokerage, but across an entire economy?

Digital payment systems can serve two rather different purposes. They allow households to pay merchants and other households, facilitating faster and verifiable transactions and, potentially, formalization and access to credit. But they also allow households to move their own liquidity across financial accounts, from a bank deposit, for example, to a brokerage account, instantly and at any time. The latter function has received far less attention. Moreover, speed alone may not be enough. A fast payment system confined to a particular bank or platform still leaves investors dependent on where they hold their money. Interoperability, that is, the ability to move funds across banks and platforms on common rails, removes this constraint. It puts liquidity within immediate reach of the investor, wherever that liquidity is held.

The consequences of removing this funding friction for financial markets are ambiguous. Easier funding may allow investors to act on information when it arrives, rebalance portfolios when they choose, and participate in opportunities that would otherwise pass them by. But delay can also be a useful friction. By separating an impulse to trade from the ability to fund it, payment frictions may curb short-horizon or speculative trading.

[1] See: https://www.wsj.com/finance/stocks/online-brokerages-restrict-trading-on-gamestop-amc-amid-frenetic-trading-11611849934

[2] Before the U.S. House Committee on Financial Services, Robinhood CEO Vladimir Tenev urged real-time settlement, stressing the importance of market infrastructure for retail trading. See: https://www.govinfo.gov/content/pkg/CHRG-117hhrg43966/html/CHRG-117hhrg43966.htm

Removing them could therefore bring more households into equity markets while inducing investors to trade more aggressively and, perhaps, less profitably. And if the resulting retail flow is sufficiently large, it may affect price formation itself. Whether easier movement of money improves participation and market efficiency, or simply facilitates more trading, is ultimately an empirical question.

We study this question using India's Unified Payments Interface (UPI). Launched in 2016 by the National Payments Corporation of India (NPCI), UPI created common, interoperable rails over which funds could move in real time, 24 hours a day, across banks and financial platforms. Before UPI, moving funds from a bank account to a brokerage account could require bank-specific transfer mechanisms and was subject to the operating constraints of conventional interbank rails. UPI sharply compressed the distance, in both time and institutional steps, between money in a bank account and money available to trade. Importantly, it did so without changing brokerage commissions or the cost of executing a trade. UPI therefore offers a particularly useful setting in which to ask whether the ability to move liquidity itself shapes participation in financial markets. At the same time, its rapid and uneven adoption across regions creates substantial cross-sectional variation in exposure to this shock.

Using retail trading data from the National Stock Exchange covering 19.8 million investors from 2015 to 2020, we establish four main results. First, exposure to UPI substantially raises retail trading activity: a one-standard-deviation increase raises monthly transactions by 6.2% and the number of active investors by 8.7%. The increase runs through the number of investors active in a given month, reflecting both investors appearing in the transaction data for the first time in at least five years and, to a greater extent, previously observed investors returning after a spell of inactivity. Holding the investor fixed, those same investors trade on more days and execute more trades.

Second, the architecture of the payment system matters. We compare UPI with YONO, the State Bank of India's proprietary digital platform, which offered real-time digital payments but within a closed, single-bank ecosystem during our sample period.[3] UPI's effect remains economically large after controlling for YONO exposure, while the corresponding YONO effect is substantially smaller. We then exploit a September 2017 NPCI reform

[3]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.

that strengthened UPI's multi-bank Payment Service Provider architecture. Following the reform, the effect of UPI exposure nearly doubles for transactions and more than doubles for active investors, with both increases statistically significant. Together, these results indicate that speed alone is not sufficient: the ability to move funds across institutions on common rails is central to UPI's effect on market participation.

Third, easier funding does not improve investment performance. Long-horizon abnormal returns decline with UPI exposure, and that decline falls on the least experienced investors, precisely those who account for little of the additional trading, while returns for the most experienced are unchanged. Removing the funding friction therefore allows investors to act more readily without making their investment decisions more profitable, and shifts the cost of that activity toward those least equipped to absorb it. Access to liquidity is not access to better information.

Finally, the substantial increase in retail flow has muted consequences for aggregate price quality. UPI-linked retail activity is associated with higher trading volume, but volatility, price impact, return reversal, and forward returns are unchanged on average. The additional flow tilts toward stocks with greater institutional ownership. Thus, easier funding changes who trades and how much they trade, but neither improves investment performance nor broadly impairs price formation.

Our empirical strategy exploits differences in the geographic presence of banks that integrated early with UPI. Following the foundational approach of Dubey and Purnanandam (2023), we use Reserve Bank of India deposit data to measure, for each pincode, the pre-UPI share of deposits held at early-adopting banks. We interact this predetermined exposure with the introduction of UPI in a difference-in-differences framework. Banks joined UPI at different times at the institution level, while their geographic footprints (and hence local exposure to early adoption) were inherited from the pre-UPI period. Areas served more heavily by early adopters were therefore more exposed to the reduction in funding frictions after 2016.[4]

A natural concern is that these areas were simply more digitally ready, or were served by better banks. Three pieces of evidence help distinguish this explanation from the

[4]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 (2024), Fafchamps et al. (2022)), creating persistent differences between areas served by early versus late adopters.

funding-friction interpretation. First, high- and low-exposure areas exhibit no differential pre-trends in trading, and controlling directly for baseline digital readiness and financial access leaves the estimates essentially unchanged. Second, within-investor tests compare trading across accounts linked to early- and late-adopting banks and yield similar results. Third, we exploit regional bank holidays, when equity markets remain open and UPI operates but conventional interbank rails were closed.[5] If our estimates merely reflected fixed differences in bank or customer quality, there is little reason for the UPI exposure gradient to change on these days. Instead, it flattens: lower-exposure areas, ordinarily more dependent on conventional payment rails, disproportionately increase trading when those rails close. The result is difficult to reconcile with a generic digital-adoption or financial-inclusion story, but follows naturally if UPI reduces the friction involved in funding a trade.

Additional mechanism tests reinforce this interpretation. Using high-frequency trading data from the Bombay Stock Exchange during 12-hour windows before and after two major flash crashes (July 8, 2019, and March 12, 2020), we show that trading rises on both sides of the market but roughly twice as much for purchases as for sales. The asymmetry matters: selling releases funds already inside the brokerage account, whereas buying requires liquidity to be moved in. Second, we find that the increase in activity is concentrated in small-value trades and is strongest in areas with high pre-UPI cash use, proxied by per-capita ATM withdrawals. Taken together, these patterns point to the same mechanism. Open payment rails make small, incremental deployments of household liquidity into equity markets easier when investors wish to act.

Our paper makes three main contributions. First, we contribute to the emerging literature on instant payment systems and payment architecture. Recent work shows that instant payment systems reshape financial intermediation through their effects on competition, deposits, liquidity, and technology adoption. For example, Sarkisyan (2023) shows that Brazil's Pix intensifies deposit competition, Gonzalez et al. (2024) document its effects on banks' liquidity management and risk-taking, Liang et al. (2024) show that instant payments strengthen monetary policy transmission, Copestake et al. (2025) demonstrate how UPI's open architecture reshapes financial-sector competition and Higgins (2024) shows

[5]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.

how network externalities shape the adoption of cashless payment technologies. We extend this literature beyond banks and payment networks to capital markets, showing that payment architecture also shapes household participation in equity markets. Specifically, we provide the first evidence that an interoperable payment system changes not only how households move money, but who participates in equity markets, how actively they trade, and how that flow propagates to prices. More broadly, our findings suggest that payment systems should be viewed not only as infrastructure for banking and payments, but also as an important component of capital market infrastructure.

Second, we identify a distinct friction through which financial infrastructure affects household investment decisions. The household finance literature has shown that lower trading costs, zero-commission platforms, and digital brokerages increase retail trading activity (Barber and Odean, 2000; Barber et al., 2022). In contrast, UPI leaves brokerage commissions and execution costs unchanged. Because the shock operates on funding rather than execution, our estimates speak to a friction distinct from the trading costs studied in prior work, and one that has largely been treated as fixed. Three pieces of evidence support the funding-friction interpretation: the exposure gradient flattens on bank holidays, the buy-side response around market crashes is roughly twice the sell-side response, and responses are concentrated among small trades and in cash-intensive regions. Our empirical design builds on the foundational work of Dubey and Purnanandam (2023), who develop the bank-geography measure of UPI exposure that has been widely adopted to study the effects of UPI adoption. While that literature (Alok et al., 2024; Cramer et al., 2024) uses UPI to study credit, formalization, and financial inclusion, we examine its implications for capital markets.

Finally, we contribute to the behavioral finance literature on retail trading. Prior work documents that individual investors trade excessively, under-diversify, and underperform (Odean, 1999; Barber and Odean, 2000), while frictionless trading platforms encourage attention-driven trading and losses (Barber et al., 2022). Our contribution is not that lower frictions increase trading—that is well established—but rather where the friction resides and what its removal does not accomplish. The friction we study lies upstream of the brokerage, in the payment rails themselves. Removing this funding friction substantially increases participation and trading intensity, yet the resulting return decline concentrates among the least experienced investors, while aggregate price quality, including volatility, price impact, and return reversal, is left intact. These findings suggest that improving access through payment infrastructure alone does not improve investment outcomes. Rather, reducing funding frictions changes who participates and how intensely they trade, while leaving aggregate price formation largely unchanged and redistributing the cost of trading toward less experienced participants.

Section 2 below details the institutional context and data. In section 3, we first establish that interoperable payment infrastructure expands retail participation. We then examine whether this increased access improves investor performance and affects market quality in section 4. Having documented these consequences, we return to the underlying channels in section 5 and identify three mechanisms: reductions in funding frictions, expansion of small-ticket trading, and reallocation of idle liquidity into equity markets. Section 6 concludes.

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. Funds became tradable only after the broker credited them to the client's account and released margin, so the end-to-end interval between initiating a transfer and being able to trade typically extended to one to two business days, reflecting both batch settlement and broker-side posting. 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. 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 monthly transactions as of December 2024.[6] While instant payment systems have emerged globally, including Brazil's Pix and the United States' FedNow, UPI is particularly relevant for securities trading because its interoperable architecture became integrated into brokerage-funding workflows, directly addressing 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.

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

This open architecture transformed the trading experience as brokerage platforms adopted UPI-enabled funding. UPI allowed investors to fund trading accounts in real time, substantially reducing the delays and initiation frictions that previously created opportunity costs during volatile market periods. Figure A1 in the Appendix illustrates the contrast between traditional transfers and UPI-enabled flows. By dismantling this longstanding friction in the payment leg, UPI 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 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.[7] Its decentralized, voluntary implementation contrasts sharply with India's approach. Unlike the U.S., India mandated UPI integration for all banks, 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, launched 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 mar-

[7]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.

kets.

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 more closely within participants' own applications, and third-party initiation has played a smaller role. UPI's deeper separation of the customer interface from the account-holding institution supports a broader set of third-party providers interoperating 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 interoperable platform, broker-integrated ecosystem that characterizes UPI.

UPI's distinctive contribution lies in its transformation of retail market participation mechanisms. By sharply reducing traditional payment frictions and becoming integrated with brokerage-funding 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.

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, 2024; 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.[8] 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

[8]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.

banks were dominant prior to UPI's launch were mechanically more exposed to UPI-enabled fund-transfer capabilities once the platform became operational. Consistent with Alok et al. (2024), we find that this exposure predicts subsequent UPI transaction intensity in our own data (Appendix Table A1).

We rely on this exposure measure as our primary specification because it is constructed entirely using pre-UPI balance-sheet information and therefore cannot be mechanically affected by contemporaneous shocks to cash usage, including the November 2016 demonetization episode.

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 allows aggregate UPI growth to load differentially across pincodes according to local adoption intensity. Because the exposure component is measured after UPI's launch, it may partly reflect endogenous local adoption. We therefore report these estimates as a check on functional form rather than as independent identification. The Bartik measure is trimmed at the 1st and 99th percentiles and normalized to have mean zero and unit standard deviation.

Figure A2 provides a map of the geographical distribution of the UPI exposure across different pincodes of India. From the map, we see that pincodes in the central part of India have higher UPI exposure as indicated by the brighter colors. 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.

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 contain 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 dataset to the official list of post office pincodes published by the Indian government.[9].

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 1 illustrates the time trends of the average *Number of Transactions* 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%.

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 2 plots the difference between High and Low UPI exposure pincodes in the number of transactions and investors over time. The gap widens over the sample period, suggesting a positive correlation between UPI exposure and market participation. Because this difference is measured in levels between groups with different baselines, we treat it as descriptive motivation and test formally for differential pre-trends in the event-study analysis below (Figure 3).

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

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, 25, and 140 trading days) as follows:

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

where

$$ExecutionPrice_{S,i,j,d} = \begin{cases} \dfrac{BuyValue_{S,i,j,d}}{BuyQuantity_{S,i,j,d}}, & \text{if } BuyQuantity_{S,i,j,d} \geq SellQuantity_{S,i,j,d}, \\ \dfrac{SellValue_{S,i,j,d}}{SellQuantity_{S,i,j,d}}, & \text{if } SellQuantity_{S,i,j,d} > BuyQuantity_{S,i,j,d}. \end{cases} \tag{4}$$

where $Price_{S,d+D}$ denotes the closing price of stock S on day d+D, and $ExecutionPrice_{S,i,j,d}$ denotes the average execution price associated with transaction record j of investor i in stock S on day d. The buy-side execution price is used when the buy quantity is at least as large as the sell quantity, and the sell-side execution price is used otherwise. The horizon D equals 1, 25, or 140 trading days. To isolate performance net of aggregate market movements, we further calculate abnormal returns (excess BHR) as:

$$ABRET_{S,i,j,d,D} = Direction_{S,i,j,d} \cdot \left(BHR_{S,i,j,d,D} - MR_{d,D}\right), \tag{5}$$

where

$$Direction_{S,i,j,d} = \begin{cases} 1, & \text{if } BuyQuantity_{S,i,j,d} \geq SellQuantity_{S,i,j,d}, \\ -1, & \text{if } SellQuantity_{S,i,j,d} > BuyQuantity_{S,i,j,d}. \end{cases} \tag{6}$$

Here, j denotes a transaction record of investor i trading stock S on day d over horizon D, and $MR_{d,D}$ denotes the market buy-and-hold return over the same horizon. The variable $Direction_{S,i,j,d}$ equals 1 for records with non-negative net quantity, defined as buy quantity minus sell quantity, and equals -1 for records with negative net quantity. 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-month is calculated as the absolute-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 share of an investor's transactions in a given month that involve risky securities. 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*, a Herfindahl-Hirschman Index (HHI)-based measure of the concentration of an investor's monthly trading across securities:

$$\text{Portfolio Diversification}_{i,t} = 1 - \sum_{s \in \mathcal{S}_{i,t}} \left(\frac{\text{Turnover}_{i,s,t}}{\sum_{k \in \mathcal{S}_{i,t}} \text{Turnover}_{i,k,t}} \right)^2 \tag{7}$$

where $\mathcal{S}_{i,t}$ is the set of securities investor i trades in month t, and $\text{Turnover}_{i,s,t}$ is the sum of the rupee buy value and sell value that investor i transacts in security s during month t. The measure is bounded in $[0, 1)$ and increases in the number of securities traded and the evenness of turnover across them; it equals zero when all turnover is concentrated in a single security. Investor-months with no trades are excluded. Unlike a holdings-based HHI, which reflects the cumulative outcome of past portfolio decisions and typically adjusts only gradually, the turnover-based measure captures the composition of an investor's active trading decisions in real time. This distinction is particularly relevant in our setting, where UPI adoption plausibly affects the ease and immediacy of funding trades rather than the investor's pre-existing stock of holdings. A flow-based measure is therefore better suited to capturing contemporaneous changes in trading behavior.

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 number of calendar days between consecutive active trading dates, including both within-month gaps and the gap between the investor's last active trading date before the current month and the first active trading date in the current month.

Table 1 shows that at the individual investor level, abnormal returns are on average negative at all horizons and most negative at 140 days (-295 bps), indicating that investor trades underperform the market benchmark over longer horizons. The average risk taking is 0.634 indicating that in a typical investor-month, 63% of transactions are in securities classified as risky. *Portfolio Diversification* averages 0.419, corresponding to a turnover HHI of 0.581. The distribution is wide: at least a quarter of investor-months take a value of zero, indicating turnover concentrated in a single security. Trading intensity is concentrated in a small number of days within the month. *Number of Trades* averages 10.4 per investor-month against a median of 3, and *Number of Active Days* averages 4.6 against a median of 2, so both distributions are right-skewed and the means reflect a minority of highly active investor-months. *Cross-month Trading Gap*, the average number of calendar days between consecutive active trading dates, averages 7.9 days with a median of 4.0. Because this measure spans the interval between the last active day of one month and the first of the next in addition to within-month intervals, it exceeds the spacing implied by active days within a single month.

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 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. December 2014 data are appended to provide the lag required to compute growth in January 2015. For the pincodes not observed in December 2014, the January 2015 growth rate is left missing rather than imputed as zero. Columns 1 and 2 report means with standard deviations in parentheses. Column 3 reports the difference in means with standard errors clustered at the pincode level in parentheses.

The unit of observation varies across rows: economic activity and investor demographics are measured at the pincode level; transaction and investor counts, and their pre-period growth rates, at the pincode–month level. Investor demographics are averaged across investors within each pincode after restricting the data to one observation per investor. The resulting pincode-level measures are equally weighted across pincodes, so that each pincode contributes equally rather than in proportion to its investor count.

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.

Once investor demographics are aggregated to the pincode level with each pincode weighted equally, differences across high- and low-exposure pincodes are negligible: average investor age is 0.17 years higher in high-exposure pincodes (42.32 vs. 42.15), and the female investor share is identical at 0.045.

3 Main Results

3.1 Does UPI Exposure Increase Retail Trading Activity?

We begin by testing whether greater exposure to UPI increased retail participation in equity markets. We estimate the following continuous-treatment difference-in-differences specification:

$$Y_{p,d,t} = \alpha_{d,t} + \gamma_p + \beta \cdot \text{Post} \times \text{UPI Exposure}_p + \varepsilon_{p,d,t} \tag{8}$$

where $Y_{p,d,t}$ is the number of transactions, the number of active investors, or the number of transactions per active investor in pincode p, located in district d, during month t. UPI Exposure$_p$ is the pre-UPI share of local deposits held at early-adopting banks, as defined in Section 2. Post$_t$ equals one from November 2016 onward. Pincode fixed effects, γ_p, absorb time-invariant local characteristics, while district–month fixed effects, $\alpha_{d,t}$, absorb time-varying shocks common to pincodes within a district. Standard errors are clustered at the pincode level.

Because banks continued to join UPI after its launch, the design does not compare permanently treated and untreated locations. Instead, β captures whether pincodes with earlier and deeper exposure to the UPI network experienced larger changes in trading activity after the national rollout.

Table 3 reports the baseline results. A one-standard-deviation increase in UPI Exposure raises the number of transactions by approximately 69 per pincode–month, or 6.2% of the sample mean of 1,108 transactions. The same increase in exposure raises the number of active investors by approximately 7 per month, or 8.7% of the sample mean of 81 investors.[10] By contrast, Column 3 shows no detectable change in transactions per active investor. Thus, the aggregate increase in trading operates primarily through the monthly participation margin: more investors become active in a given month, rather than the average active investor executing more transactions.

Figure 3 examines the timing of these effects using the event-study specification:

$$Y_{p,d,t} = \alpha_{d,t} + \gamma_p + \sum_{q \in \mathcal{Q} \setminus \{2016Q2\}} \beta_q \, \mathbb{I}(\text{Quarter}_t = q) \, \text{UPI Exposure}_p + \varepsilon_{p,d,t} \tag{9}$$

where $\mathcal{Q} = \{\text{Q1}\,2015, \ldots, \text{Q1}\,2020\}$, and Q2 2016, the final quarter preceding the UPI's public launch, is omitted as the reference period. The pre-adoption coefficients fluctuate around zero and exhibit no systematic trend. After UPI becomes operational, the coefficients turn positive and remain elevated. The transaction response rises during the initial expansion of the network and subsequently plateaus, while the effect on the number of active investors continues to increase through the end of the sample. This pattern is consistent with participation accumulating as UPI adoption spreads.

The number of active investors can increase because of investors trading for the first time or because dormant investors return to the market. Brokerage-account opening dates are available for only about 66% of investors, and 90.6% of the observed opening dates precede November 2016. We therefore distinguish these channels using transaction histories extending back to 2010, which provide a five-year burn-in period before the January 2015–January 2020 regression sample. We define *first appearances* as investors trading for the first time anywhere in this extended history. We define *reactivations* as previously observed investors returning after at least 1, 3, 6, or 12 complete months of inactivity. An investor who traded before 2015 and returns during the sample therefore counts as a reactivation rather than a first appearance. Because the inactivity thresholds are nested, the number of qualifying reactivations mechanically declines as the threshold becomes more restrictive.

[10]Economic magnitudes are calculated by multiplying the estimated coefficients by the standard deviation of UPI Exposure, 0.347, reported in Table 1.

Appendix Table A2 shows that UPI exposure increases both first appearances and reactivations. A one-standard-deviation increase in exposure raises first appearances by approximately 0.56 investors per pincode–month and raises reactivations by between 1.75 and 0.21 investors as the inactivity threshold increases from one to twelve months. The participation response therefore includes investors appearing for the first time in at least five years of transaction history, but is quantitatively larger for previously active investors returning to the market. UPI expands the active investor pool not simply through new participation, but also by making it easier for existing account holders to re-enter.

The findings are robust to alternative exposure measures and controls for baseline financial access. Appendix Table A3 replaces the time-invariant exposure measure with the standardized, time-varying UPI Bartik measure. A one-standard-deviation increase in this measure is associated with 113 additional monthly transactions and 13 additional active investors, broadly consistent with the baseline estimates. Appendix Table A4 further interacts Post with pre-UPI measures of bank-account, credit-card, and demat-account ownership from the CMIE People of India survey. Although this restriction reduces the sample to approximately 445,000 pincode–months, the transaction coefficient changes only from 279.3 to 271.9 after including these controls, while the investor coefficient changes from 31 to 30. The baseline results therefore do not appear to reflect pre-existing differences in financial access or digital readiness.

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. To distinguish interoperability from general digital banking capabilities, we provide two complementary tests. First, we compare UPI with the State Bank of India's proprietary digital platform, YONO. During our sample period, YONO operated as SBI's proprietary, bank-specific digital interface. Although SBI customers could transfer funds to other banks through YONO, such transfers continued to rely on conventional payment rails such as NEFT or RTGS. UPI instead provided a common interoperable rail through which funds could move directly across participating banks and platforms. Thus, while both provided digital payment functionality, only UPI provided a common payment rail designed for interoperability across participating banks and platforms. Second, we examine whether UPI's effect strengthened after a September 2017 reform that expanded its multi-bank Payment Service Provider (PSP) architecture.

For the first test, we restrict the sample to trading through SBI-linked accounts and jointly include UPI exposure and pincode–month YONO activity, measured using either transaction value (INR millions) or volume. Because both systems are available from November 2017 onward, we estimate the following specification in the post-period:

$$Y_{p,d,t} = \alpha_{d,t} + \beta_1 \cdot \text{UPI Exposure}_p + \beta_2 \cdot \text{YONO Exposure}_{p,t} + \varepsilon_{p,d,t} \quad (10)$$

Unlike our baseline pre/post design, this specification exploits cross-pincode variation within districts after both systems are operating; pincode fixed effects are therefore omitted, as they would absorb the time-invariant UPI exposure measure. We interpret the coefficients as conditional associations and focus on their relative economic magnitudes within the same sample. Because the sample is restricted to SBI-linked accounts, own-bank payment capability is held fixed, and pincode UPI exposure captures the breadth of the surrounding interoperable network—the banks, brokers, and applications an SBI customer can reach on common rails. Coefficients in this table are not comparable in level to the baseline estimates, since restricting to SBI-linked accounts lowers the mean of the dependent variable; all magnitudes below are scaled by the corresponding mean of this sample.

Panel A of Table 4 shows a clear difference. Moving from the 10^{th} (no exposure) to the 90^{th} percentile (full exposure) of UPI exposure is associated with 56–62% more transactions and 56–62% more active investors relative to the sample mean, depending on whether YONO exposure is measured in value or volume terms. The corresponding shift in local YONO activity is associated with an increase of only 3–8%. The YONO coefficients are also weak statistically: in value terms the coefficient is significant only at the 10% level for transactions and insignificant for investors, while the volume-based coefficients are precisely estimated but economically small. Including YONO exposure leaves the UPI coefficient essentially unchanged.

Two features make this comparison conservative. The sample holds the bank relationship fixed, so cross-bank differences in customer composition cannot drive the contrast. And YONO exposure is measured as realized local activity rather than as predetermined exposure, so it absorbs local income, digital sophistication, and unobserved demand for financial services—forces that would bias its coefficient upward. Even so, proprietary digital activity is associated with substantially smaller increases in market participation than exposure to the interoperable UPI network.[11]

Our second test exploits the September 2017 expansion of UPI's multi-bank PSP framework. The reform allowed third-party application providers to connect to UPI through multiple sponsor banks rather than relying on a single PSP bank, expanding cross-platform connectivity within the network.[12] Panel B of Table 4 compares the effect of UPI exposure in the early post-UPI period (November 2016–August 2017) with the period beginning in September 2017. To make the estimates comparable, each post-period is separately stacked with the same pre-UPI sample. The effect on transactions rises from 118.8 in the early period to 223.9 after September 2017, while the effect on active investors rises from 9.5 to 23.8. Both differences are statistically significant at the 1% level. Thus, the UPI effect nearly doubles for transactions and more than doubles for investor participation after the network becomes more interoperable.

The timing evidence does not by itself rule out broader growth and maturation of the UPI network after 2017. Taken together with the YONO comparison, however, it points consistently in the same direction: real-time digital payments alone are not sufficient to explain the participation response. The ability to move funds across institutions and platforms on common rails appears to be an important component of UPI's effect on retail investment.

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.

[11] Economic magnitudes multiply the estimated coefficient by the 10^{th}–90^{th} percentile change in each exposure measure and scale by the sample mean of the dependent variable (21.427 transactions and 2.009 active investors per pincode–month). The interdecile range is 1.000 for UPI exposure, 0.532 for YONO transaction value (INR millions), and 39 for YONO transaction volume. We compare the two measures across their realized distributions rather than across standard-deviation shifts because local YONO activity is zero in more than three-quarters of pincode–months and highly right-skewed, so a standard-deviation change lies outside the support of the large majority of observations.

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

We conduct several robustness exercises in the Appendix to address concerns related to estimating effects in levels. Table A5 re-estimates the baseline on a balanced pincode panel to ensure results are not driven by changes in sample composition arising from the trimming of extreme observations. 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 A6 scales outcomes by local population (per 1,000 residents), showing that results are not simply reflecting differences in market size. Table A7 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 alternative specifications using the inverse hyperbolic sine (asinh) transformation and Poisson pseudo–maximum likelihood with high-dimensional fixed effects (PPMLHDFE). Both approaches accommodate zero outcomes and are commonly used for skewed, non-negative data. However, zero outcomes constitute less than one percent of our sample, so the baseline level specifications are not materially driven by censoring at zero. As Chen and Roth (2024) show, coefficients from log-like transformations such as asinh are not generally interpretable as percentage effects when outcomes can equal zero and can be sensitive to the units in which the outcome is measured. We therefore treat the asinh estimates as functional-form robustness checks rather than as directly comparable percentage effects.

Appendix Table A8 illustrates this sensitivity. The asinh specification applied to unscaled outcomes produces negative average coefficients, whereas normalizing outcomes by their pre-period median yields positive and statistically significant estimates. Because pre-median scaling alters the estimand and the implicit weighting across pincodes, the two specifications are not directly comparable; the contrast indicates that baseline scale affects transformed-outcome estimands rather than validating either estimate. Consistent with this interpretation, quintile-wise PPML regressions in Appendix Table A9 show positive proportional effects concentrated in lower-activity pincodes, with attenuated effects in the highest-activity quintile. Log specifications estimated by quintile (unreported) yield similar patterns. Taken together, UPI generated robust absolute increases in trading activity; the proportional evidence points to larger relative responses in low-activity pincodes but is sensitive to functional form and scaling.

3.2 Identification

Our identifying assumption is that, absent UPI, high- and low-exposure pincodes would have followed parallel trends in retail trading activity. The event-study evidence in Section 3.1 supports this directly, and the balance tests in Section 2.4 show that pre-period growth in trading activity is statistically indistinguishable across exposure groups despite differences in baseline levels. The remaining concern is that early-adopter-bank exposure proxies for some other determinant of trading. We consider three such confounds.

First, investors residing in high-exposure areas may differ systematically (e.g., wealthier, younger, or more active) so that the effect reflects investor selection rather than payment frictions; we address this by holding the investor fixed, comparing the same investor's trading across accounts linked to early- and late-adopting banks (Section 3.2.1). Second, early-adopting banks may themselves be better-resourced or more technologically sophisticated ("digital readiness" story) so that their customers trade more for reasons unrelated to interoperability; we exploit bank holidays, when conventional settlement rails close for all banks (Section 3.2.2): were bank quality the driver, the exposure gradient should persist or widen on holidays, yet it flattens. Third, exposure may proxy for unobserved local characteristics (e.g., wealth, financial development, or digital infrastructure) that raise trading regardless of UPI. Randomly reassigning exposure across pincodes yields null effects, and institutional investors in the same locations show no post-adoption response, indicating the effect is specific to friction-sensitive retail trading rather than to high-exposure areas in general (Section 3.2.3).

3.2.1 Holding the investor fixed

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:

$$Y_{i,b,t} = \alpha_{i,t} + \gamma_b + \beta \cdot \text{Post}_t \times \text{Early UPI Enabled Brokerage}_b + \varepsilon_{i,b,t} \tag{11}$$

where Y represents the number of transactions by investor i through brokerage account b in month t. *Early UPI Enabled Brokerage* 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. Because investor-month fixed effects absorb the investor's total monthly activity, this specification identifies the allocation of trading across an investor's accounts rather than the level of trading, which the pincode- and investor-level specifications address.

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.

3.2.2 Shutting down conventional rails

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.[13]

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 provided an always-on, interoperable payment rail that substantially reduced the frictions involved in initiating 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. The interoperability channel predicts the opposite: because UPI remains operational across all banks on holidays, closing conventional rails pushes lower-exposure pincodes, which relied on them more heavily, toward UPI as well, compressing the cross-sectional gradient. The gradient on ordinary days reflects depth of adoption rather than a binary access constraint, and holidays remove the conventional outside option that sustains it. The two hypotheses are therefore distinguished by the sign of the triple interaction, which we evaluate using the following specification:

[13] We hand collected all state-wise bank holidays and NSE holidays for 2015–2017 from the Reserve Bank of India holiday database (https://rbi.org.in/Scripts/HolidayMatrixDisplay.aspx).

$$
\begin{aligned}
Y_{p,d,D} = \alpha_{d,t} + \gamma_p + \delta_D + \theta_{s,D} + \beta_1 \cdot \text{Bank Holiday}_{p,D} + \beta_2 \cdot \textit{Post} \times \text{Bank Holiday}_p + \\
\beta_3 \cdot \textit{Post} \times \text{UPI Exposure}_p + \beta_4 \cdot \text{UPI Exposure}_p \times \text{Bank Holiday}_{p,D} + \\
\beta_5 \cdot \text{UPI Exposure}_p \times \textit{Post} \times \text{Bank Holiday}_{p,D} + \varepsilon_{p,d,D} \quad (12)
\end{aligned}
$$

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 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 banks. However, the triple interaction term *Post* $\times$ *UPI Exposure* $\times$ *Bank Holiday* is negative and statistically significant at the 1% level across all specifications. The exposure gradient therefore 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.

Because the specification includes rich fixed effects, common seasonal, festival, and state-level shocks are differenced out. The same pattern separates the fund-transfer channel from a general payments or inclusion effect: broader digital engagement would not respond specifically to the closure of conventional funding rails. Taken together, these findings support an interpretation in which UPI's payment architecture relaxes settlement frictions rather than reflecting persistent differences in bank characteristics.

3.2.3 Additional Threats and Placebo

A remaining concern is that UPI Exposure proxies for unobserved local characteristics — wealth, financial development, or digital infrastructure — that could raise trading activity regardless of UPI. Two placebo tests address this. First, we randomly reassign UPI exposure across pincodes, preserving the original exposure distribution, and re-estimate equation (8) on each shuffled assignment. Appendix Table A10 reports average placebo coefficients that are close to zero and statistically insignificant across both 100 and 500 randomizations, indicating that our results are not an artifact of the cross-sectional distribution of exposure.

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 A11 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 or by shocks affecting retail and institutional traders alike.

Separately, 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 is unlikely to be driven by the demonetization channel operating through currency-chest proximity.

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.

3.3 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 (31–55 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 4 presents heterogeneous treatment effects from the baseline difference-in-differences specification across investor subgroups while the corresponding regression estimates are reported in Table A12 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 sample means (reported in Table A12), 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.[14]

Appendix Table A13 examines whether the effect of UPI exposure differs across trading channels. A one-standard-deviation increase in UPI exposure shifts execution toward digital channels, reducing transactions at physical locations by 10.8% of the channel mean and raising internet-based transactions by 20.1%, so that trading is reallocated rather than suppressed.

[14]Proportional effects are computed by scaling subgroup-specific coefficients by the corresponding sample means reported in Table A12.

3.3.1 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 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.[15]

Table 7 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, a one-unit increase in UPI Exposure corresponds to a 32.8% rise relative to the pre-period mean in rural areas, compared to 11.9% in urban areas. For the number of investors, the corresponding 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 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

[15] 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.

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

Having established that UPI exposure raises retail participation, and that this increase reflects relaxation of settlement frictions rather than fixed differences in bank quality (Sections 3.2.1 and 3.2.2), we now trace the consequences of this expansion for investor performance and trading behavior. We read all results through the funding-friction channel motivated above. All outcomes are measured on the direct-equity portfolio, not total household wealth; we do not model fund, derivative, or other positions here.

To answer this, we estimate the following investor-month specification:

$$Y_{i,t} = \alpha_i + \gamma_{d,t} + \beta \left(Post_t \times UPI\ Exposure_p \right) + \varepsilon_{i,t}, \tag{13}$$

where $Y_{i,t}$ denotes investor-level outcomes (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 8 reports abnormal buy-and-hold returns (in bps) measured over three horizons: 1 trading day, 25 trading days, and 140 trading days. The coefficient on $Post \times UPI\ Exposure$ is negative at all three horizons, significantly so at the 1% level at 1 and 140 days. Scaling by a one-standard-deviation increase in exposure gives implied effects of -0.45 bps (1 day), -0.19 bps (25 days), and -3.08 bps (140 days).[16] The magnitudes are modest at short horizons but scale sharply with the holding period: easier funding improves access without improving performance, and the deterioration concentrates at long horizons. Returns are measured across the full population of investors active in a

[16]Implied effects are obtained by multiplying the estimated coefficients by the standard deviation of UPI Exposure in the investor-month sample (0.187).

given month, which remains dominated by experienced incumbents both before and after UPI; the estimates therefore do not reflect a compositional shift toward inexperienced traders, a point we confirm directly in the experience heterogeneity below. We document the underlying funding-friction channel in Section 5.1, including its asymmetric effect on purchases versus sales.[17]

Panel B traces this pattern to trading behavior. UPI exposure is associated with statistically significant but economically small adjustments in portfolio composition (in direct-equity holding): risk taking declines slightly and diversification increases modestly. The clearer effects appear in how often investors trade. Within the month, both the number of active trading days and the number of trades increase significantly; across months, the cross-month trading gap declines, indicating that investors return to the market more quickly. Because these specifications include investor fixed effects, they identify changes for a given investor rather than the entry of new ones,[18] though returns are observed only in months when an investor trades, so the estimates reflect the realized performance of UPI-induced trading rather than a fixed set of investment decisions. UPI thus changes how often investors trade much more than the composition of what they trade, and the more intensive, faster-turnover activity it induces coincides with the long-horizon underperformance in Panel A.

Heterogeneity by experience. We next ask whether these effects vary with investor experience, measured dynamically as lagged cumulative participation: for investor i in month t, the cumulative number of months traded through $t-1$. Each month we sort investors into experience quartiles on this lagged measure and omit the least-experienced quartile (Q1) as the baseline. Following Seru et al. (2010), the lagged construction prevents mechanical sorting on post-UPI activity within month t, so the estimates reflect differences across stages of participation rather than classification based on contemporaneous trading. We estimate:

[17] We interpret these estimates as effects on realized abnormal performance, not welfare. Faster, cheaper market access also has value that does not show up in equity returns — being able to move cash in and out when needed — so a negative abnormal-return effect need not mean investors are worse off overall.

[18] The absence of an aggregate effect on transactions per active investor in Table 3 is not inconsistent with these within-investor results: Table 3 averages over the changing set of investors active in each pincode-month, whereas Table 8 holds the investor fixed and asks whether a given investor trades more intensively when active.

$$
\begin{aligned}
Y_{i,t} = \alpha_i + \gamma_{d,t} + \beta\left(Post_t \times UPIExposure_p\right) \\
+ \sum_{q=2}^{4} \theta_q \left(Post_t \times UPIExposure_p \times Q_{i,q,t}\right) + \varepsilon_{i,t},
\end{aligned}
\tag{14}
$$

where $Q_{i,q,t}$ indicates investor i's experience quartile q in month t. All main effects and lower-order interactions are included but suppressed for brevity.

Panel A of Table 9 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: -0.875 bps (1 day), -1.422 bps (25 days), and -5.225 bps (140 days). The return decline generally attenuates with experience. At 1 day the effect is smaller in magnitude for Q3 (-0.348 bps) and indistinguishable from zero for Q4; at 25 days it is indistinguishable from zero for Q2 through Q4. At 140 days the point estimates remain negative throughout, but their magnitude falls monotonically with experience, from -5.225 bps for Q1 to -1.208 bps for Q4, which is not statistically distinguishable from zero. Long-horizon underperformance is therefore concentrated among the least experienced investors rather than uniform across the experience distribution. Raw returns by quartile (Appendix Table A14) show the same attenuation at short and medium horizons, though the 140-day gradient is imprecisely estimated once market movements are not netted out. A specification fixing experience strictly over the pre-UPI period (Appendix Table A15) reproduces the pattern at all horizons.

Panel B shows a sharply different pattern for trading intensity. The implied effects on active days and number of trades are negligible for Q1 but increase across Q2–Q4, peaking among the most experienced investors. The cross-month trading gap likewise narrows for Q2–Q4, indicating faster re-entry, while it changes little for Q1. Changes in risk taking and diversification are comparatively small. Thus, the additional within-investor trading induced by UPI is concentrated among more experienced investors, both in how frequently they trade and how quickly they return to the market.

The return results run in the opposite direction: the deterioration in abnormal performance is concentrated among the least experienced investors, who exhibit little of the increase in trading intensity. This distinction helps separate the funding-friction channel from a broad financial-inclusion mechanism. UPI expands monthly participation, as shown earlier, but conditional on the investor, the strongest trading response comes from those already experienced in the market. The evidence therefore points primarily to easier deployment of capital by existing participants rather than to expanded access alone.

Market-Level Effects Having documented effects at the investor level, we ask whether UPI-linked retail trading is associated with changes in market functioning at the stock level. Retail participation can deepen markets and raise activity, but if retail order flow is less informed or more episodic it can also raise volatility and price impact. Our setting complements Eaton et al. (2022), who study *reductions* in retail trading from brokerage outages; we examine increases associated with greater exposure to payment interoperability.

Because the UPI Exposure measure is defined over pincodes while market-level outcomes are defined over stocks, the exposure measure cannot enter the stock–month regressions directly. We therefore map each stock's order flow back to the residential pincodes of the trading investors and define

$$\text{UPI Intensity}_{s,t} = \frac{\sum_p \text{High}_p \cdot n_{s,p,t}}{\sum_p n_{s,p,t}} \in [0,1], \tag{15}$$

the share of stock s's month-t trades sourced from above-median-exposure pincodes, where $n_{s,p,t}$ is the number of such trades originating in pincode p and High_p equals one if $\text{Exposure}_p \geq \text{median}_p(\text{Exposure})$.[19]

We regress stock-level outcomes on UPI $\text{Intensity}_{s,t}$ and its interaction with $Post_t$, including stock and month fixed effects with standard errors clustered at the stock level; the $Post_t$ main effect is absorbed by the month fixed effects. Outcomes are monthly traded value, annualized volatility, Amihud (2002) illiquidity, within-month return reversal, and forward monthly excess returns, all defined in the note to Table 10. UPI Intensity enters

[19]This measure is constructed from contemporaneous order flow, so both its cross-sectional and time variation may respond to the same stock-month shocks that drive volume. Stock and month fixed effects absorb time-invariant stock characteristics and common market movements, but we read these results as descriptive associations rather than causal effects. Notably, endogeneity of this form would tend to generate spurious associations with volatility and price impact, which we do not find.

on its natural $[0, 1]$ scale, so the interaction captures the association with a one-unit shift in order-flow composition; stock-level summary statistics appear in Appendix Table A16.

Table 10 reports the results. $Post \times UPI\ Intensity$ is positive and significant only for volume: stocks drawing more of their flow from high-exposure pincodes exhibit larger post-adoption increases in activity. A one-standard-deviation increase in UPI Intensity ($\sigma = 0.045$) is associated with roughly INR 213 million, or 10.14% of the sample mean. We find no significant changes in volatility, price impact, return reversal, or forward returns: greater UPI-linked activity is not associated with detectable deterioration in average market quality.[20]

The pattern is also stronger among stocks with greater institutional ownership: interacting $Post \times UPI\ Intensity$ with pre-period institutional ownership shows the volume response concentrating in institutionally held securities, with null volatility and price-impact interactions (Appendix Table A17). The additional UPI-linked activity is therefore concentrated in stocks with deeper institutional participation, consistent with greater capacity to absorb retail flow without moving prices.

5 Economic Mechanisms

The participation effects documented above could reflect two conceptually distinct roles of UPI. As a payments and financial-inclusion technology, UPI may broaden households' engagement with formal finance and draw new investors into equity markets. As a fund-transfer rail, however, it may primarily reduce the time and cost of moving existing balances from bank accounts into brokerage accounts. These explanations generate different predictions about who responds and when the response is strongest. A broad inclusion channel should operate predominantly through first-time participation and should not depend sharply on the availability of conventional transfer rails. A funding-friction channel instead predicts stronger responses among existing investors and greater sensitivity to the availability of alternative payment rails.

The evidence thus far is more consistent with the funding-friction channel. The participation response is larger for previously observed investors returning to the market than

[20]Reversal is estimated on stock-months with enough consecutive daily returns to compute the autocorrelation ($N \approx 30{,}500$, about 40% of the Amihud sample), so those estimates are substantially less precise and we do not draw a bound from them.

for first appearances (Section 3.1), while within-investor responses concentrate among more experienced traders (Section 4). The exposure gradient also narrows on bank holidays, when conventional interbank rails are unavailable (Section 3.2.2). Below, we provide a further test by comparing purchases and sales around sharp market declines.

Appendix Table A18 shows that the baseline effect is robust to conditioning on phone and internet access. The positive interaction with internet access suggests that connectivity amplifies, rather than explains, the effect. We next examine three implications of the funding-friction channel: funding immediacy, small-value trading, and liquidity mobilization.

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 July 8, 2019, and March 12, 2020. On July 8, 2019, the Indian stock market faced its worst day of the year, with the BSE Sensex[21] 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

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

World Health Organization.[22] 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 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:

$$Y_{i,h} = \delta_i + \gamma_h + \beta \cdot \text{UPI Exposure}_i \times \text{Post Crash}_h + \varepsilon_{i,h} \tag{16}$$

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 hour-of-day fixed effects that absorb common market-wide intraday trading patterns.

As reported in Table 11, we find a consistently positive and statistically significant interaction between UPI Exposure and Post Crash. 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 show that UPI exposure predicts stronger immediate trading

[22]See Livemint (2019) and The Hindu BusinessLine (2020) for details on the July 2019 and March 2020 market crashes, respectively.

responses to sharp market declines.[23]

A key implication of the funding-immediacy mechanism is that the effects should be asymmetric across purchases and sales. Purchasing securities requires investors to transfer funds into brokerage accounts before trades can be executed, whereas selling securities does not require an immediate transfer of funds out of the brokerage ecosystem. As a result, reductions in payment frictions should disproportionately affect buy-side trading activity during periods in which rapid market responses are valuable. Panel B tests this prediction by separately analyzing buy and sell transactions following the crash events.

The asymmetry results in Panel B are consistent with the funding-immediacy mechanism. The post-crash response is substantially stronger for buy transactions than for sell transactions, consistent with a friction that binds on the side of the market requiring investors to deploy funds.

5.2 Reduction in Fixed Costs of Small-Value Trading

A second mechanism operates through the fixed costs of small-value trades. Because the minimum thresholds and coordination costs of pre-UPI transfers fell disproportionately on small, repeated transactions, by making incremental transfers cheap, UPI should raise small-value activity most. Fixed costs deter small trades in proportion to their size, so the response should be largest at the bottom of the size distribution and attenuate as the size threshold rises.

To test this mechanism, we classify each transaction record's buy and sell sides separately and estimate, at the pincode–month level:

$$Small\_Value\_Trading_{p,d,t} = \gamma_p + \alpha_{d,t} + \beta \cdot \text{Post}_t \times \text{UPI Exposure}_p + \varepsilon_{p,d,t}, \qquad (17)$$

where $Small\_Value\_Trading_{p,d,t}$ captures either (i) the number of transaction sides below a given trading-value cutoff or (ii) the number of investors executing at least one transaction side below that cutoff in pincode p (located in district d) and month t. We estimate

[23] 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.

the specification separately for sides below and above cutoffs of ₹1,000, ₹2,000, ₹5,000 and ₹10,000. All specifications include pincode and district–month fixed effects. Because level coefficients partly reflect differences in baseline activity across categories, we report each estimate as a percentage of its own pre-UPI mean and test the difference between the two categories on that scale.

Table 12 presents the results. Panel A shows that transaction counts rise in both size categories, with larger proportional increases for small-value trades relative to their respective pre-UPI baselines. The gap narrows monotonically as the cutoff rises: it is significant at the 1% level at cutoffs through ₹5,000 but becomes statistically indistinguishable from zero at ₹10,000. The response for larger trades remains close to the pooled response across cutoffs, consistent with a particularly strong increase at the small end of the trade-size distribution. Panel B shows a similar pattern for investor counts. The proportional increase is larger among investors executing small-value trades, and the gap narrows as the cutoff rises, although it remains significant at the 1% level at all four cutoffs. Because investors executing both small- and large-value trades appear in both categories, the categories overlap and their counts do not sum to the pooled total.

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 Cash Intensity and Liquidity Mobilization

A third mechanism operates through changes in household liquidity management. Pre-UPI transfer frictions segmented cash-like balances from trading-ready brokerage balances, especially in cash-intensive regions; by collapsing that segmentation, UPI may let idle or precautionary balances be redeployed into markets.

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. High pre-UPI ATM withdrawals identify areas in which households relied more heavily on cash transactions and where the gains from easier digital mobilization of bank balances should therefore be larger.

We estimate the following triple-difference specification:

$$
\begin{aligned}
Y_{p,d,t} = \alpha_{d,t} + \gamma_p + \beta_1 \cdot \text{Post} \times \text{UPI Exposure}_p \\
+ \beta_2 \cdot \text{Post} \times \mathbf{1}_{Top\ Tercile,p} \\
+ \beta_3 \cdot \text{Post} \times \text{UPI Exposure}_p \times \mathbf{1}_{Top\ Tercile,p} + \varepsilon_{p,d,t}.
\end{aligned}
\tag{18}
$$

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 13 reports the results. Pincodes with high ex-ante cash usage exhibit substantially larger increases in retail trading activity following UPI adoption. In Column 1, a one-unit increase in UPI exposure raises monthly transactions by 63 in lower-cash areas and by an additional 314 in top-tercile cash areas, implying a total effect of 377 there. Column 2 shows a similarly amplified response in high-cash areas relative to other regions.

These findings are consistent with UPI reducing the friction involved in mobilizing existing liquid balances for market activity. The effect is strongest precisely where households relied most heavily on cash before UPI, as the funding-friction channel predicts. Because we do not observe household withdrawals, bank balances, or brokerage funding flows directly, we interpret this evidence as greater ease of liquidity mobilization rather than direct evidence of a reallocation of deposits into equities.

Taken together, the mechanism tests point to a common funding channel. UPI increases investors' ability to deploy funds when timing is valuable, lowers the friction associated with small-value transactions, and has larger effects where existing liquidity was previously harder to mobilize. These patterns suggest that interoperable payment infrastructure affects retail trading by changing the speed, cost, and flexibility with which investors can deploy existing financial resources.

6 Conclusion

This paper provides the first systematic evidence that the architecture of a payment system shapes not only how people pay but how they invest. Studying India's 2016 launch of UPI and exploiting cross-sectional variation in exposure to early-adopting banks, we find that a one-standard-deviation increase in UPI exposure raises monthly trading activity by 6.2% and active investors by 8.7%, drawing in first-time investors, and to a greater extent, bringing previously active investors back to the market. This is consistent with a fund-transfer channel that enables rapid responses to market stress, cheaper small-value trades, and the redeployment of idle cash into equities. Interoperability, rather than digitization alone, appears to be the key feature relaxing this friction: the corresponding association for a proprietary, bank-specific digital platform with comparable digital functionality (YONO) is less than one-sixth as large.

Easier access, however, does not improve realized investment performance. The same channel that pulls already-active investors into trading more (more often and faster) is associated with lower long-horizon abnormal returns, while portfolio composition changes only marginally. The decline is not evenly borne: it falls on the least experienced investors, who account for little of the additional trading. Easier access therefore redistributes the cost of trading toward those least equipped to absorb it. Yet, these investor-level shortfalls do not aggregate into market-wide distortions: trading volume rises sharply, but volatility, price impact, return reversal, and forward returns are unchanged on average. The incremental flow concentrates in institutionally held stocks, leaving aggregate price quality intact.

These results recast payment infrastructure as financial-market infrastructure. The rails that move money between accounts are not neutral plumbing for commerce; their design determines who can participate in securities markets, how quickly, and at what cost, placing payment architecture alongside trading venues and clearing systems as a determinant of investor behavior. India's setting is instructive because UPI's scale and capital-market integration exceed those of any other instant-payment system. As systems such as Pix, SEPA Instant, and FedNow deepen their links to capital markets, our evidence suggests that their consequences for financial markets depend not only on whether they expand participation, but on who bears the cost of the trading they enable.

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Figure 1
Stock Market Participation over Sample Period

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

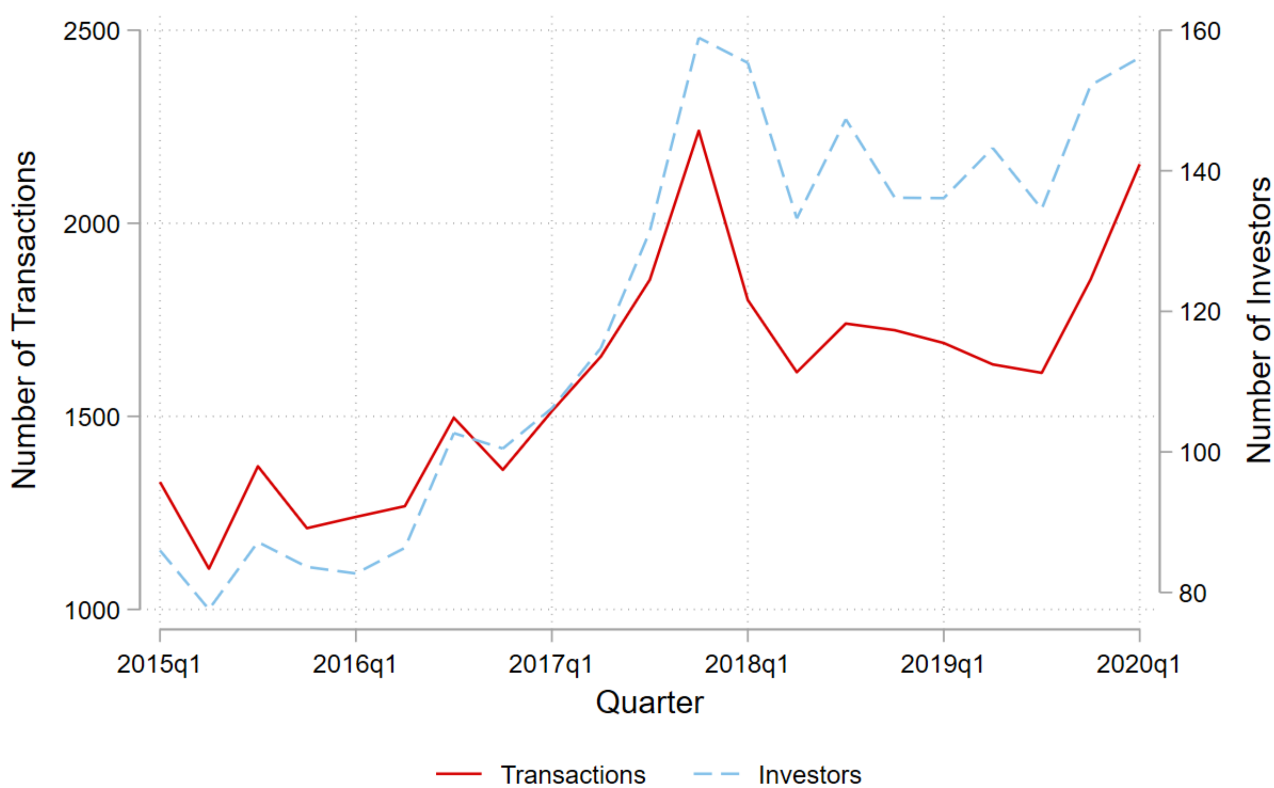
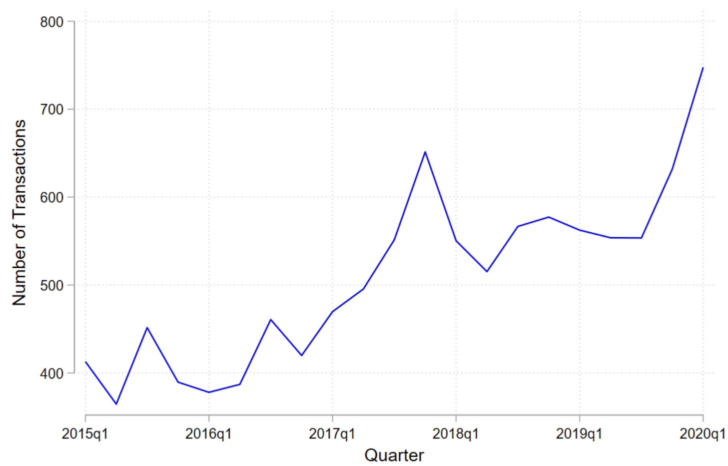
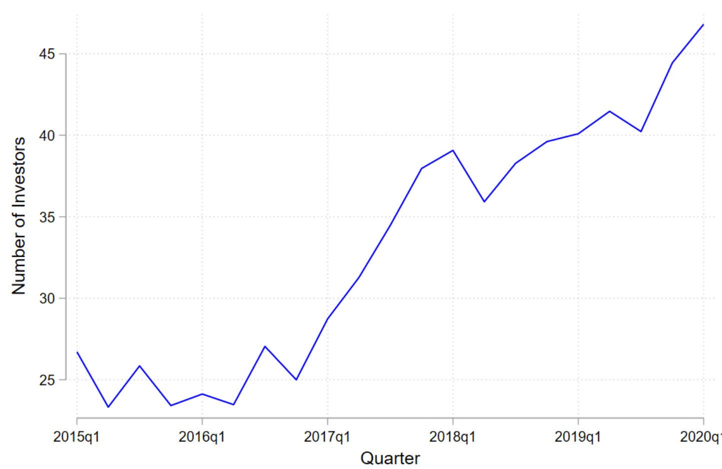


Figure 2
UPI and Stock Market Participation

This figure illustrates the difference in the average number of retail transactions (panel A) and retail investors (panel B) 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 averaged across pincode-month observations within each quarter and exposure group. 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.



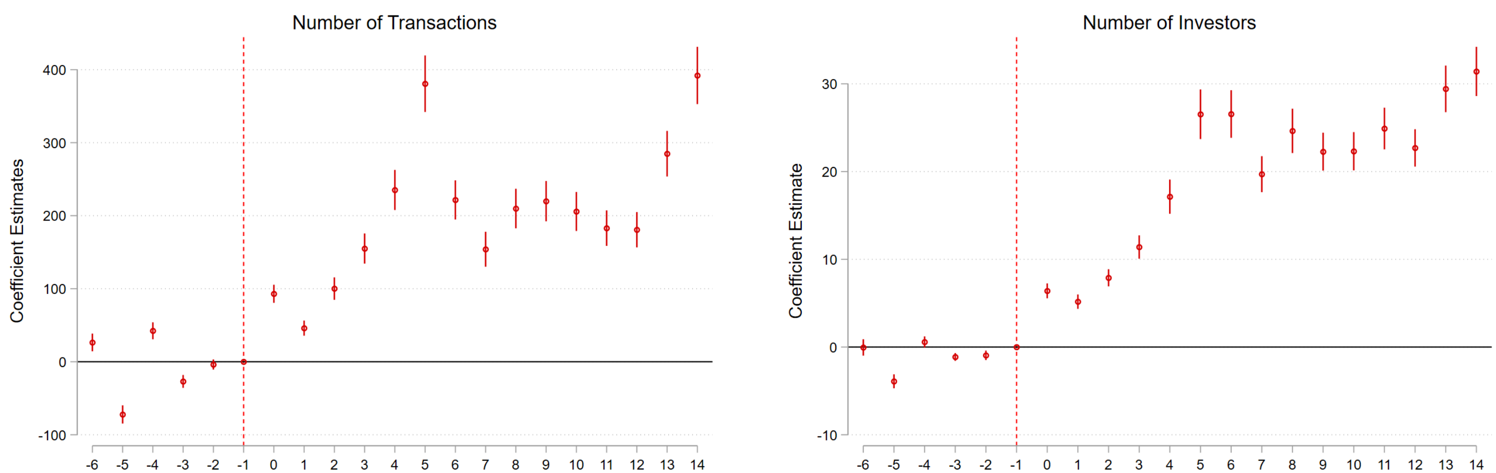
(A) Difference Between Number of Transactions (High-Low)



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

Figure 3
Dynamic Effects of UPI on Stock Market Participation

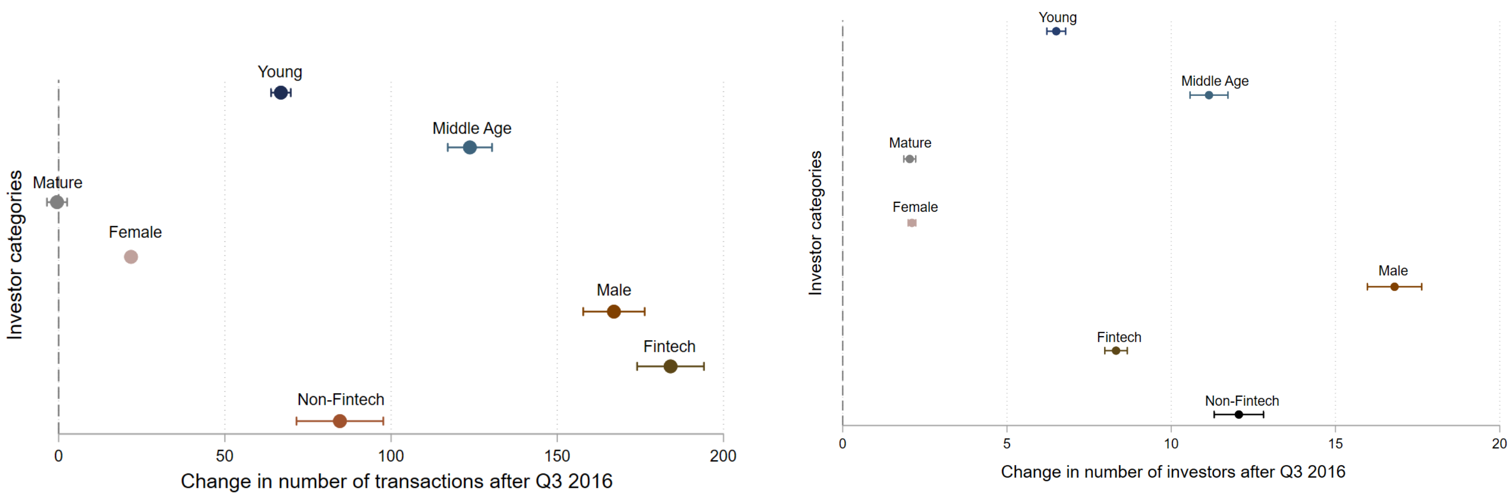
This figure presents treatment dynamics using the specification in equation 9 for the number of retail transactions (left) and retail investors (right). The underlying data, aggregated at the pincode level, is at a quarterly frequency from Q1 2015 to Q1 2020. Each dot represents the point estimate, with vertical lines indicating 95% confidence intervals. The regression includes pincode and district-quarter 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).



(A) Number of Transactions **(B) Number of Investors**

Figure 4
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 $\times$ 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**

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)	131,442,054	-20.515	246.762	-1194.052	-112.621	-16.966	73.292	1377.173
Abnormal Returns (25 days)	131,352,347	-63.103	846.876	-3547.293	-415.955	-29.992	311.939	3519.357
Abnormal Returns (140 days)	123,618,328	-294.966	2022.533	-7584.222	-1201.002	-110.592	641.127	6571.692
Number of Trades	132,900,568	10.395	18.293	1	1	3	10	143
Number of Active Days	133,508,122	4.637	4.999	1	1	2	6	21
Risk Taking	134,242,917	0.634	0.386	0.000	0.333	0.750	1.000	1.000
Portfolio Diversification	134,242,917	0.419	0.347	0.000	0.000	0.491	0.737	0.999
Cross-month Trading Gap	133,072,816	7.937	9.741	0	1.550	4.000	10.667	47.000

This table reports the summary statistics for key variables in our analysis. The first section presents pincode-month-level measures of UPI exposure, the Bartik instrument, the post-UPI indicator, and stock market participation outcomes, namely, *Number of Transactions* and *Number of Investors*. The second section presents investor trading behavior measures, including *Abnormal returns*, *Number of Trades*, *Number of Active Days*, *Risk Taking*, *Portfolio Diversification*, and *Cross-month Trading Gap*. The data is from January 2015 to January 2020.

Table 2
Balance Test by UPI Exposure

Variable	(1) High UPI Exposure N	Mean/(SD)	(2) Low UPI Exposure N	Mean/(SD)	(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)
Age	9,202	42.323	9,200	42.151	0.172**
		(5.002)		(5.166)	(0.075)
Female	9,200	0.045	9,198	0.045	0.000
		(0.053)		(0.059)	(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. The outcomes include economic activity, proxied by pincode-level night-light intensity; stock-market activity, measured at the pincode–month level using the number of transactions and investors and their pre-period growth rates; and investor demographics, including age and gender, averaged across investors within each pincode so that each pincode receives equal weight rather than weight proportional to its investor count. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table 3
UPI and Stock Market Participation

DV	Number of Transactions (1)	Number of Investors (2)	Per Active Investor (3)
UPI Exposure × Post	197.961***	20.310***	0.236
	(11.717)	(1.059)	(0.250)
Pincode FE	Y	Y	Y
District-Month FE	Y	Y	Y
N	1,121,378	1,121,385	1,074,602
Adj. R^2	0.964	0.964	0.513

This table presents the difference-in-differences (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 is the *Number of Transactions* in column 1, the *Number of Investors* in column 2, and the *Number of Transactions per Active Investor* in column 3. 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table 4
Interoperability and Retail Market Participation

Panel A: UPI versus SBI's Proprietary YONO Platform

Yono Measure	Value		Volume	
	Number of Transactions	Number of Investors	Number of Transactions	Number of Investors
	(1)	(2)	(3)	(4)
UPI Exposure	11.959***	1.163***	13.258***	1.246***
	(1.436)	(0.126)	(0.800)	(0.070)
Yono Exposure	3.369*	0.259	0.019***	0.002***
	(1.992)	(0.176)	(0.006)	(0.001)
District-Month FE	Y	Y	Y	Y
N	495,749	495,409	495,749	495,409
Adj. R^2	0.243	0.302	0.219	0.281

Panel B: Early and Late Post-UPI Effects

DV	Number of Transactions	Number of Investors
	(1)	(2)
UPI Exposure $\times$ Early Post	118.798***	9.525***
	(8.275)	(0.631)
UPI Exposure $\times$ Late Post	223.853***	23.837***
	(13.302)	(1.215)
Pincode FE	Y	Y
District-Month FE	Y	Y
N	1,526,871	1,527,536
Adj. R^2	0.967	0.968
T-test	Late Post − Early Post	Late Post − Early Post
	105.054***	14.312***
	(8.329)	(0.711)

This table presents complementary tests of the role of interoperability in the effect of UPI exposure on stock market participation. Panel A compares UPI, an open payment system, with YONO, SBI's closed digital banking platform. The sample covers November 2017 to January 2020, when both UPI and YONO are present. YONO exposure is measured using transaction value in columns 1–2 and transaction volume in columns 3–4. Panel B estimates the effect of UPI exposure separately for the early post-UPI period, November 2016 to August 2017, and the late post-UPI period beginning in September 2017. The estimation sample stacks the pre-November 2016 period separately with each post period, and the reported t-test compares the late- and early-period coefficients. Observations in both panels are at the pincode-month level. The dependent variable is the *Number of Transactions* in columns 1 and 3 of Panel A and column 1 of Panel B, and the *Number of Investors* in columns 2 and 4 of Panel A and column 2 of Panel B. UPI exposure is a continuous measure of regional exposure, as defined in Equation 1. Panel A includes district-month fixed effects, while Panel B includes stack-specific pincode and district-month fixed effects. Standard errors are clustered at the pincode level and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

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-differences (DiD) estimates for the impact of UPI adoption on stock market participation. Observations are at the investor-broker-month level for the period January 2015 to January 2020. The dependent variable is *Number of Transactions* executed by an investor through a given broker in a month. Column 1 is restricted to the sample of investors with two or more brokerage accounts during the sample period. Columns 2 and 3 are 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

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	-0.992***	-0.566***		
	(0.130)	(0.040)		
Post × UPI Exposure	10.282***	3.421***	10.260***	3.424***
	(0.677)	(0.209)	(0.680)	(0.211)
Bank Holiday × UPI Exposure	-1.818***	-0.650***	-2.040***	-0.681***
	(0.191)	(0.057)	(0.208)	(0.063)
Post × Bank Holiday	1.915***	0.608***		
	(0.291)	(0.088)		
Post × Bank Holiday × UPI Exposure	-1.589***	-0.824***	-1.653***	-0.911***
	(0.411)	(0.123)	(0.464)	(0.138)
Pincode FE	Y	Y	Y	Y
Day FE	Y	Y		
State-Day FE			Y	Y
District-Month FE	Y	Y	Y	Y
N	2,698,265	2,698,257	2,698,201	2,698,193
Adj. R^2	0.951	0.961	0.951	0.961

This table presents the triple difference-in-differences (DiD) estimates for the impact of UPI adoption on stock market participation at the pincode level. The unit of observation is the pincode-day, covering 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table 7
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-differences (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 columns 1 and 2 is the *Number of Transactions* in each pincode-month, and the dependent variable in columns 3 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table 8
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	-2.398***	-1.032	-16.481***
	(0.406)	(1.318)	(3.437)
Investor FE	Y	Y	Y
District-Month FE	Y	Y	Y
N	128,491,871	128,404,541	120,990,787
Adj. R^2	0.064	0.047	0.079

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	-0.004***	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	131,281,827	131,281,827	130,101,352	130,544,232	129,937,319
Adj. R^2	0.338	0.383	0.155	0.497	0.460

This table presents the difference-in-differences (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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table 9
Consequences: Heterogeneity by 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	-4.665***	-7.582***	-27.945***
	(0.634)	(2.114)	(5.178)
Post × UPI Exposure × Q2 Trading Experience	-0.265	7.638***	14.726**
	(0.790)	(2.665)	(6.262)
Post × UPI Exposure × Q3 Trading Experience	2.808***	10.854***	18.051***
	(0.767)	(2.625)	(6.326)
Post × UPI Exposure × Q4 Trading Experience	5.490***	9.617***	21.485***
	(0.835)	(2.798)	(7.094)
Investor FE	Y	Y	Y
District-Month FE	Y	Y	Y
N	128,491,871	128,404,541	120,990,787
Adj. R^2	0.064	0.047	0.079
1 SD exposure effect (bps):			
Q1: Post × Exposure	-0.875***	-1.422***	-5.225***
	(0.119)	(0.396)	(0.968)
Q2: Post × Exposure + Post × Exposure × Q2	-0.925***	0.010	-2.472**
	(0.133)	(0.445)	(1.101)
Q3: Post × Exposure + Post × Exposure × Q3	-0.348***	0.614	-1.850*
	(0.119)	(0.403)	(1.016)
Q4: Post × Exposure + Post × Exposure × Q4	0.155	0.382	-1.208
	(0.122)	(0.402)	(1.070)

Table 9
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	0.001	0.052***	0.001	-0.037
	(0.001)	(0.001)	(0.017)	(0.010)	(0.040)
Post × UPI Exposure × Q2 Trading Experience	-0.001	0.003***	-0.189***	0.042***	0.105**
	(0.001)	(0.001)	(0.032)	(0.013)	(0.052)
Post × UPI Exposure × Q3 Trading Experience	-0.004***	0.004***	-0.184***	0.057***	0.210***
	(0.001)	(0.001)	(0.031)	(0.015)	(0.062)
Post × UPI Exposure × Q4 Trading Experience	-0.003**	0.003**	-0.139***	0.098***	0.516***
	(0.001)	(0.001)	(0.034)	(0.025)	(0.120)
Investor FE	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y
N	131,281,827	131,281,827	130,101,352	130,544,232	129,937,319
Adj. R^2	0.340	0.398	0.358	0.516	0.468
1 SD exposure effect (in units of dependent variable):					
Q1: Post × Exposure	-0.0002	0.0002	0.0098	0.0002	-0.0070
Q2: Post × Exposure + Post × Exposure × Q2	-0.0004	0.0008	-0.0258	0.0081	0.0128
Q3: Post × Exposure + Post × Exposure × Q3	-0.0009	0.0009	-0.0248	0.0109	0.0325
Q4: Post × Exposure + Post × Exposure × Q4	-0.0008	0.0008	-0.0164	0.0186	0.0901

This table presents the difference-in-differences (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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table 10
Stock Level Consequences

DV	Trading Volume (1)	Annualized Volatility (2)	Amihud Illiquidity (3)	Return Reversal (4)	Forward Monthly Excess Returns (5)
UPI Intensity	-700.557	-26.005***	0.042	0.048	0.003
	(601.505)	(4.519)	(0.026)	(0.113)	(0.024)
Post $\times$ UPI Intensity	4743.742***	2.485	0.019	-0.077	0.017
	(863.720)	(5.066)	(0.026)	(0.114)	(0.024)
Security FE	Y	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y	Y
Additional Controls	N	N	N	N	Y
N	77,948	77,163	77,682	30,518	61,055
Adj. R^2	0.785	0.313	0.602	0.056	0.162

This table presents difference-in-differences (DiD) estimates for the association between *UPI Intensity* and stock-level trading measures. The unit of observation is the stock–month, covering the period from January 2015 to January 2020. The dependent variable is trading volume in column 1, annualized volatility in column 2, Amihud illiquidity in column 3, return reversal in column 4, and forward monthly excess returns in column 5. UPI Intensity is the share of each stock's monthly trades originating from above-median-exposure pincodes, as defined in equation 15. The variable *Post* is a dummy variable equal to 1 from November 2016 and later. All regressions include security and year–month fixed effects. The specification for forward monthly excess returns additionally controls for lagged returns, lagged trade imbalance, lagged market capitalization, book-to-market, and lagged return skewness. Standard errors are clustered at the security level and are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table 11
Mechanism: Funding Immediacy and Trading Responses to Market Crashes

Panel A: Aggregate Trading Response

Event	2019 & 2020		2019		2020	
DV	Number of Transactions	Number of Tickers Traded	Number of Transactions	Number of Tickers Traded	Number of Transactions	Number of Tickers Traded
	(1)	(2)	(3)	(4)	(5)	(6)
Post Crash × 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

Panel B: Buy versus Sell Trading Response

Event	2019 & 2020		2019		2020	
DV	Number of Buy Transactions	Number of Sell Transactions	Number of Buy Transactions	Number of Sell Transactions	Number of Buy Transactions	Number of Sell Transactions
	(1)	(2)	(3)	(4)	(5)	(6)
Post Crash × UPI Exposure	0.006***	0.003**	0.006**	0.002	0.006***	0.003**
	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)
Investor FE	Y	Y	Y	Y	Y	Y
Hour FE	Y	Y	Y	Y	Y	Y
N	14,487,223	14,478,678	5,443,216	5,434,850	9,043,994	9,043,815
Adj. R^2	0.080	0.099	0.116	0.119	0.080	0.109
Buy - Sell	(1)-(2)		(3)-(4)		(5)-(6)	
(stacked)	0.003*		0.004		0.002	
	(0.002)		(0.003)		(0.002)	

This table presents the difference-in-differences (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. Panel A reports aggregate trading activity. The dependent variables are the *Number of Transactions* in columns 1, 3, and 5 and the *Number of Tickers* in columns 2, 4, and 6. Panel B decomposes the trading response by transaction direction. The dependent variable in columns 1, 3, and 5 is the *Number of Buy Transactions* by each investor-hour, while the dependent variable in columns 2, 4, and 6 is the *Number of Sell Transactions*. Columns 1–2 pool both crashes, while columns 3–4 and 5–6 correspond to 2019 and 2020, respectively. UPI exposure is a continuous regional exposure variable, defined in equation 1. *Post Crash* equals one during the 12 trading hours following the crash. All regressions include investor and hour effects as indicated. Standard errors are clustered at the investor level and reported in parentheses. The Buy – Sell row is estimated from a separate stacked specification with buy and sell-side observations pooled and interacted with a buy indicator, allowing investor and hour fixed effects to vary by side. This accounts for the covariance between the two estimates, since both sides can be observed for the same investors. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table 12
Mechanism: Reduction in Fixed Costs of Small-Value Trading

Panel A: Number of Transactions

DV	Number of Transactions								
Cut-Off	Pooled	Trading Value – 1,000		Trading Value – 2,000		Trading Value – 5,000		Trading Value – 10,000	
Small Value Trades		Y	N	Y	N	Y	N	Y	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
UPI Exposure X Post	314.731***	33.261***	281.451***	51.826***	263.183***	81.050***	235.646***	110.397***	208.410***
	(17.856)	(1.714)	(16.187)	(2.727)	(15.168)	(4.466)	(13.403)	(6.300)	(11.575)
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	1,121,383	1,121,378	1,121,381	1,121,387	1,121,379	1,121,377	1,121,382	1,121,386	1,121,376
Adj. R^2	0.958	0.860	0.960	0.893	0.960	0.928	0.958	0.946	0.956
T-test: Small − Large (%)		26.644***		18.939***		7.274***		0.147	
Scaled by Pre-UPI Mean		(1.821)		(1.479)		(1.050)		(0.860)	
Pre-UPI Mean	1,252.092	65.923	1,182.028	122.491	1,126.107	260.399	987.989	430.601	817.593
Change Relative to Pre-UPI Mean (%)	25.136	50.455	23.811	42.311	23.371	31.125	23.851	25.638	25.491

Panel B: Number of Investors

DV	Number of Investors								
Cut-Off	Pooled	Trading Value – 1,000		Trading Value – 2,000		Trading Value – 5,000		Trading Value – 10,000	
Small Value Investors		Y	N	Y	N	Y	N	Y	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
UPI Exposure X Post	20.310***	6.458***	17.688***	8.532***	16.626***	11.128***	14.982***	13.492***	13.260***
	(1.059)	(0.271)	(0.956)	(0.370)	(0.909)	(0.507)	(0.831)	(0.643)	(0.742)
Pincode FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	1,121,385	1,121,355	1,121,386	1,121,356	1,121,387	1,121,375	1,121,388	1,121,376	1,121,386
Adj. R^2	0.964	0.913	0.966	0.934	0.967	0.955	0.966	0.962	0.963
T-test: Small − Large (%)		22.332***		15.373***		6.897***		3.040***	
Scaled by Pre-UPI Mean		(1.133)		(0.884)		(0.632)		(0.582)	
Pre-UPI Mean	66.875	12.806	62.955	19.886	60.395	32.476	54.743	43.359	47.228
Change Relative to Pre-UPI Mean (%)	30.371	50.428	28.096	42.902	27.529	34.265	27.368	31.116	28.076

This table reports difference-in-differences estimates of the effect of UPI exposure on stock market participation at the pincode-month level from January 2015 to January 2020. Panel A reports the number of transaction sides and Panel B the number of investors. The pooled column reports the overall outcome without imposing a trade-size cutoff. The remaining columns split trading activity into small- and large-value categories using cutoffs of Rs. 1,000, Rs. 2,000, Rs. 5,000, and Rs. 10,000. Buy and sell sides are classified separately by their transaction value. 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table 13
Mechanism: Liquidity Reallocation

DV	Number of Transactions (1)	Number of Investors (2)
$\mathbb{1}_{Top\ Tercile}$ × UPI Exposure × Post	313.647***	36.791***
	(39.712)	(3.529)
$\mathbb{1}_{Top\ Tercile}$ × Post	298.278***	30.895***
	(27.960)	(2.451)
UPI Exposure × Post	62.911***	5.201***
	(10.519)	(0.945)
Pincode FE	Y	Y
District-Month FE	Y	Y
N	1,121,378	1,121,385
Adj. R^2	0.965	0.967

This table presents the difference-in-differences (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 within 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Funding the Trade: Payment Interoperability and Retail Investors

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 two-to-three-hour interval shown refers to NEFT batch settlement; because brokers credit funds and release margin only after settlement clears, the effective delay before an investor can trade extends to one to two business days, as described in Section 2. The comparison bar at the bottom highlights that UPI transfers are cheaper, available 24/7, and take less time compared to traditional methods.

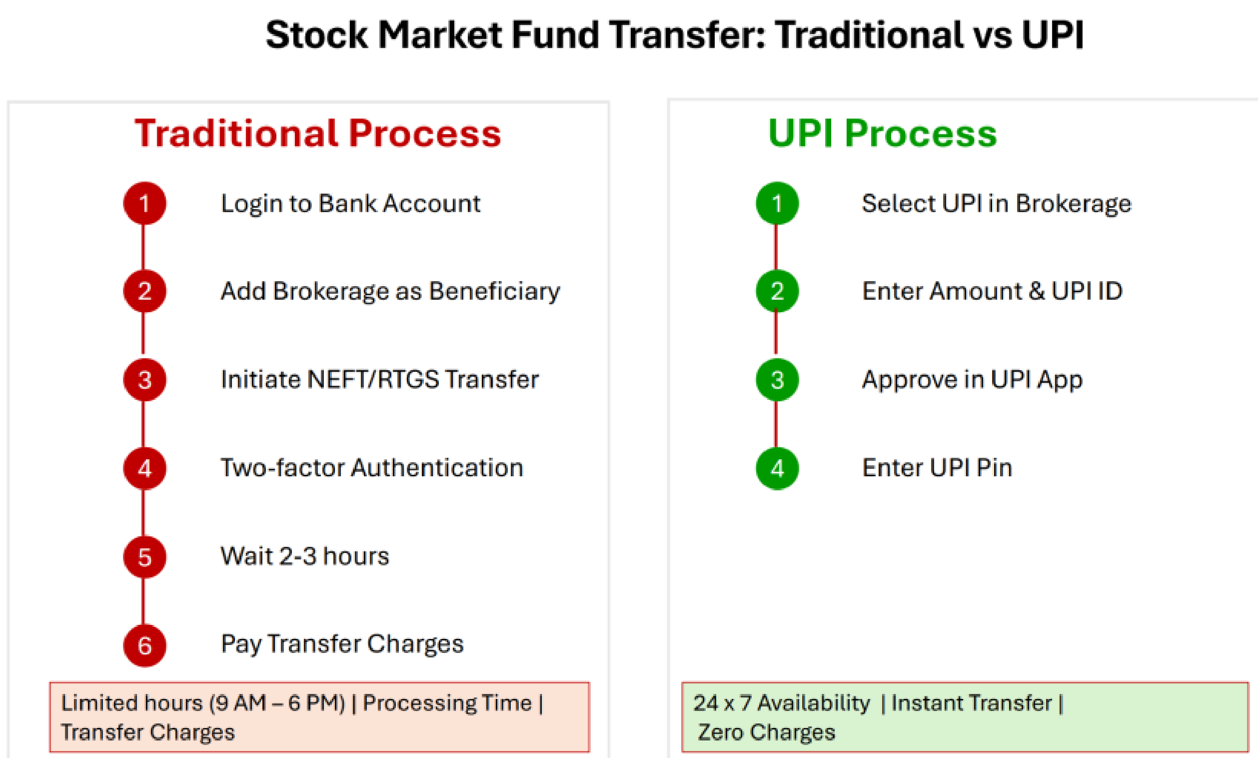


Figure A2
Geographic Variation in UPI Exposure

The map plots the geographic distribution of UPI exposure 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 Exposure is time-invariant and ranges from 0 to 1 by construction.

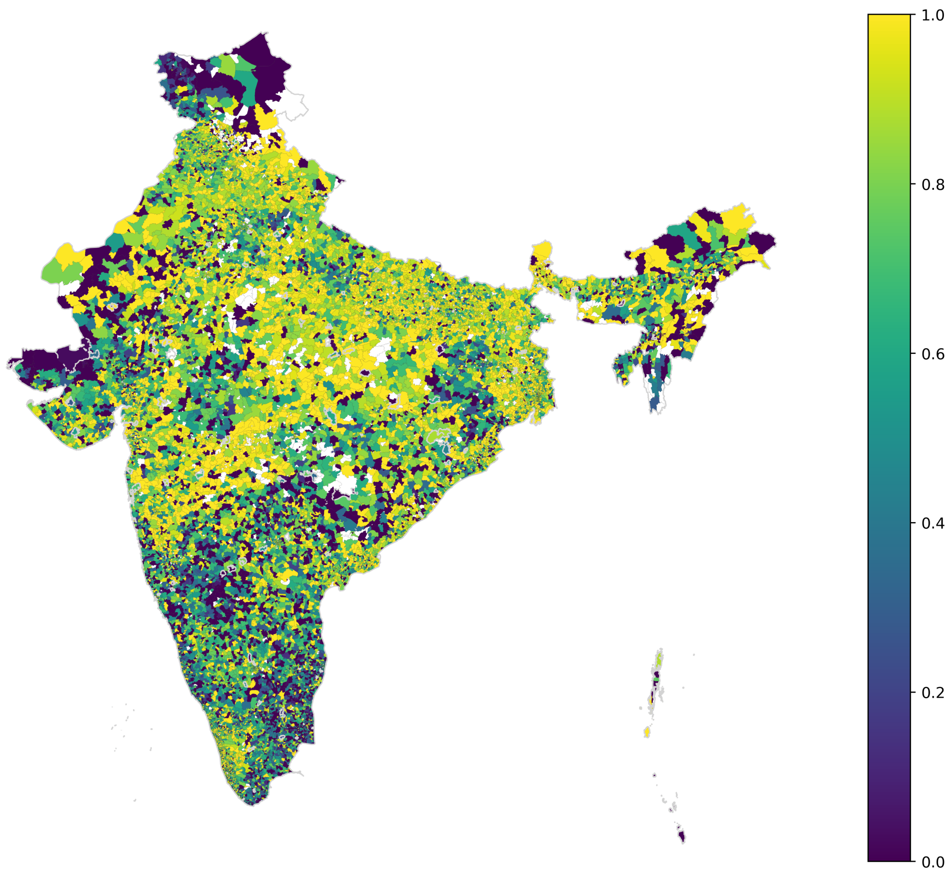


Table A1
UPI Exposure and Transaction Intensity

DV	UPI Transactions per 1,000 residents as of September 2017
	(1)
UPI Exposure	7.394***
	(0.745)
Night Lights	0.267***
	(0.029)
District FE	Y
Night Lights Control	Y
N	10,873
Adj. R^2	0.139

This table presents cross-sectional estimates of the relationship between UPI Exposure and subsequent UPI transaction intensity. Observations are at the pincode level for the month of September 2017. The dependent variable is the total number of UPI transactions in a given pincode as of September 2017, scaled by pincode population and expressed per 1,000 residents. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. The regression controls for night-light intensity and includes district fixed effects, as indicated. Standard errors are clustered at the district level and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A2
UPI and Stock Market Participation: Investor Entry and Reactivation

	First Appearances	Reactivations			
DV	First Appearance	≥1 Month	≥3 Months	≥6 Months	≥12 Months
	(1)	(2)	(3)	(4)	(5)
UPI Exposure × Post	1.600***	5.053***	2.263***	1.239***	0.613***
	(0.069)	(0.276)	(0.120)	(0.063)	(0.030)
Pincode FE	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y
N	1,121,477	1,121,391	1,121,390	1,121,373	1,121,447
Adj. R^2	0.883	0.950	0.940	0.934	0.921

This table presents the difference-in-differences (DiD) estimates for the impact of UPI exposure on investors' first observed trading appearances and reactivation. The dependent variable in column 1 is the number of investors making their first observed appearance in the transaction data. To identify first appearances more reliably, investor trading histories are extended back to 2010, providing a five-year burn-in period before the January 2015–January 2020 regression sample. The dependent variables in columns 2–5 are the numbers of previously observed investors returning after at least 1, 3, 6, or 12 complete months of inactivity, respectively. Observations are at the pincode-month level for the period January 2015 to January 2020. 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A3
UPI and Stock Market Participation: Time-Varying Bartik-Style Exposure

DV	Number of Transactions (1)	Number of Investors (2)
UPI Bartik	113.260***	13.399***
	(4.263)	(0.365)
Pincode FE	Y	Y
District-Month FE	Y	Y
N	1,110,925	1,110,994
Adj. R^2	0.965	0.967

This table presents difference-in-differences (DiD) estimates of the impact of UPI exposure on stock market participation, replacing the time-invariant exposure measure with the time-varying UPI Bartik measure defined in Equation (2). Observations are at the pincode-month level for the period January 2015 to January 2020. The dependent variable is the *Number of Transactions* in column 1 and the *Number of Investors* in column 2. UPI Bartik is calculated as the product of the national UPI over time and the pincode-level UPI-GDP measured as September 2017. All regressions include pincode and district-month fixed effects, as indicated. Standard errors are clustered at the pincode level and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A4
UPI and Stock Market Participation: Baseline Financial Access from CMIE People of India

DV	Number of Transactions		Number of Investors	
	(1)	(2)	(3)	(4)
UPI Exposure × Post	279.348***	271.854***	30.801***	29.968***
	(30.636)	(30.496)	(2.713)	(2.703)
Pincode FE	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y
Post × Bank Account	N	Y	N	Y
Post × Credit Card	N	Y	N	Y
Post × Demat Account	N	Y	N	Y
N	445,718	441,114	445,748	441,159
Adj. R^2	0.966	0.967	0.968	0.968

This table presents the difference-in-differences (DiD) estimates for the impact of UPI exposure on stock market participation, controlling for baseline financial access and digital readiness. Observations are at the pincode-month level for the period January 2015 to January 2020. The dependent variable in columns 1 and 2 is the *Number of Transactions* and the dependent variable in columns 3 and 4 is the *Number of Investors*. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. Columns 1 and 3 present the baseline results. Columns 2 and 4 additionally control for pincode-level averages of individual bank-account, credit-card, and demat-account indicators from the January–October 2016 CMIE People of India database, each interacted with Post. 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A5
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 difference-in-differences (DiD) estimates of the impact of UPI exposure on stock market participation using a balanced pincode-month panel for the period January 2015 to January 2020. In the baseline specification, observations outside the 1st and 99th percentiles are trimmed and therefore set to missing, which results in an unbalanced estimation sample. Panel A retains the full sample, including these outliers, while Panel B caps outlying values at the 1st and 99th percentiles rather than setting them to missing, thereby preserving the balanced panel. 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A6
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-differences (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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A7
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-differences (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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A8
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-differences (DiD) estimates for the impact of UPI exposure on stock market participation after transforming the outcomes using the inverse hyperbolic sine transformation. 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A9
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 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A10
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-differences (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. The reported coefficient is the mean estimated placebo effect across the randomization runs, and parentheses report the standard deviation of the estimated coefficients across those runs. All regressions include pincode and district-month fixed effects, as indicated. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A11
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. R^2	0.913	0.943

This table presents the results of a placebo test using difference-in-differences (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 column 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A12
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-differences (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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A13
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-differences (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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A14
Consequences: Raw Returns by Cumulative Trading Months

DV	Buy-and-Hold Returns (bps)		
Holding Period	1 Trading Day	25 Trading Days	140 Trading Days
	(1)	(2)	(3)
Post × UPI Exposure	-4.419***	-5.565**	-19.933***
	(0.643)	(2.259)	(5.306)
Post × UPI Exposure × Q2 Trading Experience	-0.725	7.057**	15.748**
	(0.803)	(2.875)	(6.565)
Post × UPI Exposure × Q3 Trading Experience	2.502***	12.059***	6.113
	(0.780)	(2.830)	(6.587)
Post × UPI Exposure × Q4 Trading Experience	5.537***	10.541***	-11.716
	(0.847)	(2.979)	(7.410)
Investor FE	Y	Y	Y
District-Month FE	Y	Y	Y
N	128,491,956	128,405,440	120,990,351
Adj. R^2	0.068	0.047	0.076
1 SD exposure effect (bps):			
Q1: Post × Exposure	-0.831	-1.046	-3.727
Q2: Post × Exposure + Post × Exposure × Q2	-0.967	0.280	-0.783
Q3: Post × Exposure + Post × Exposure × Q3	-0.360	1.221	-2.584
Q4: Post × Exposure + Post × Exposure × Q4	0.210	0.935	-5.918

This table presents the difference-in-differences (DiD) estimates for the impact of UPI exposure on raw returns 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. The dependent variables in columns 1,2, and 3 represent raw buy-and-hold returns measured over 1, 25, and 140 trading days 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. 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A15
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	-15.256***	-24.211***	-102.391***
	(2.105)	(6.610)	(15.865)
Post × UPI Exposure × Q2 Trading Experience	6.742***	20.093***	63.788***
	(2.333)	(7.370)	(17.832)
Post × UPI Exposure × Q3 Trading Experience	11.786***	25.095***	95.133***
	(2.223)	(7.015)	(16.963)
Post × UPI Exposure × Q4 Trading Experience	15.038***	24.641***	94.831***
	(2.129)	(6.700)	(16.136)
Investor FE	Y	Y	Y
District-Month FE	Y	Y	Y
N	90,659,938	90,634,940	87,344,374
Adj. R^2	0.066	0.035	0.056
1 SD exposure effect (bps):			
Q1: Post × Exposure	-2.743***	-4.351***	-18.410***
	(0.378)	(1.188)	(2.852)
Q2: Post × Exposure + Post × Exposure × Q2	-1.531***	-0.740	-6.941***
	(0.191)	(0.618)	(1.549)
Q3: Post × Exposure + Post × Exposure × Q3	-0.624***	0.159	-1.305
	(0.141)	(0.462)	(1.184)
Q4: Post × Exposure + Post × Exposure × Q4	-0.039	0.077	-1.359*
	(0.083)	(0.271)	(0.719)

Table A15
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	-0.010***	0.005**	-0.052	-0.021	-0.109*
	(0.003)	(0.002)	(0.041)	(0.018)	(0.063)
Post × UPI Exposure × Q2 Trading Experience	0.004	-0.002	0.212***	0.026	0.058
	(0.003)	(0.002)	(0.054)	(0.023)	(0.078)
Post × UPI Exposure × Q3 Trading Experience	0.007**	0.000	0.085*	0.036	0.077
	(0.003)	(0.002)	(0.051)	(0.024)	(0.083)
Post × UPI Exposure × Q4 Trading Experience	0.008***	-0.001	-0.016	0.103***	0.408***
	(0.003)	(0.002)	(0.045)	(0.023)	(0.090)
Investor FE	Y	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y	Y
N	92,447,729	92,447,729	91,598,047	91,908,495	91,308,910
Adj. R^2	0.317	0.377	0.149	0.509	0.472
1 SD exposure effect (in units of dependent variable):					
Q1: Post × Exposure	-0.0018	0.0009	-0.0094	-0.0038	-0.0196
Q2: Post × Exposure + Post × Exposure × Q2	-0.0011	0.0005	0.0288	0.0009	-0.0092
Q3: Post × Exposure + Post × Exposure × Q3	-0.0005	0.0009	0.0059	0.0027	-0.0058
Q4: Post × Exposure + Post × Exposure × Q4	-0.0004	0.0007	-0.0122	0.0148	0.0538

This table presents the difference-in-differences (DiD) estimates for the impact of UPI exposure on abnormal returns and trading behavior 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 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A16
Summary Statistics: Stock-Level Measures

Variable	N	Mean	S.D.	Min	P25	Median	P75	Max
UPI Intensity	79,310	0.570	0.045	0.000	0.549	0.573	0.593	1.000
Trading Volume	77,980	2104.868	5882.520	0.200	25.500	166.200	876.700	51297.300
Annualized Volatility	77,200	41.394	21.853	11.998	25.857	35.671	50.782	140.549
Amihud Illiquidity	77,722	0.033	0.079	0.000	0.000	0.002	0.019	0.670
Return Reversal	30,558	-0.046	0.332	-1.000	-0.285	-0.057	0.180	1.000
Forward Monthly Excess Return	94,707	-0.009	0.117	-0.349	-0.077	-0.017	0.048	0.506
Forward Monthly Excess Return (Observed UPI Intensity)	76,540	-0.008	0.114	-0.349	-0.075	-0.017	0.048	0.506
Institutional Ownership (Raw)	76,544	12.662	13.984	0.000	0.908	8.649	19.310	88.215
Institutional Ownership (Pre-period Standardized)	73,979	0.059	1.024	-0.886	-0.808	-0.231	0.550	5.548

This table reports summary statistics for the stock-month-level variables used in the analysis. The variables include *UPI Intensity*, *Trading Volume*, *Annualized Volatility*, *Amihud Illiquidity*, *Return Reversal*, and *Forward Monthly Excess Return*. UPI Stock Intensity measures the share of a stock's monthly trades associated with investors residing in above-median UPI-exposure pincodes. Annualized Volatility is the within-month standard deviation of daily market-adjusted returns, annualized and expressed in percentage terms. Amihud Illiquidity is the absolute daily market-adjusted return per unit of traded value, while Return Reversal is the within-month correlation between current and lagged daily market-adjusted returns. Forward Monthly Excess Return is the following month's market-adjusted stock return. The table also reports institutional ownership in raw form and as a pre-period standardized measure. The data span from January 2015 to January 2020.

Table A17
Stock Level Consequences: Interaction with Institutional Ownership

DV	Trading Volume (1)	Annualized Volatility (2)	Amihud Illiquidity (3)	Return Reversal (4)	Forward Monthly Excess Returns (5)
UPI Intensity	449.717	-5.864***	0.003	0.094	-0.005
	(956.634)	(1.016)	(0.023)	(0.112)	(0.023)
Post × UPI Intensity	7,013.601***	0.891	0.022	-0.041	0.020
	(1,180.066)	(1.121)	(0.024)	(0.111)	(0.023)
UPI Intensity × Institutional Ownership	943.521	-0.888	-0.095***	-0.019	-0.023
	(1,491.455)	(1.119)	(0.022)	(0.107)	(0.024)
Post × Institutional Ownership	-3,586.960***	-0.389	0.005	-0.108**	0.002
	(1,014.414)	(0.668)	(0.011)	(0.052)	(0.013)
Post × UPI Intensity × Institutional Ownership	7,912.856***	1.478	0.002	0.177*	0.008
	(1,875.593)	(1.175)	(0.019)	(0.091)	(0.022)
Security FE	Y	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y	Y
Additional Controls	N	N	N	N	Y
N	72,682	71,975	72,488	28,518	58,390
Adj. R^2	0.794	0.316	0.602	0.055	0.161

This table presents difference-in-differences (DiD) estimates for the association between *UPI Intensity* and stock-level trading measures. The unit of observation is the stock–month, covering the period from January 2015 to January 2020. The dependent variable is trading volume in column 1, annualized volatility in column 2, Amihud illiquidity in column 3, return reversal in column 4, and forward monthly excess returns in column 5. UPI Intensity is the share of each stock's monthly trades originating from above-median-exposure pincodes, as defined in equation 15. Institutional Ownership is each stock's average institutional ownership over the pre-UPI period, standardized using the cross-sectional mean and standard deviation of these stock-level pre-period averages. UPI Intensity enters on its natural [0,1] scale, as in Table 10, so the coefficients are reported in the same units, although the estimation sample is restricted to stocks with available institutional-ownership data. The variable *Post* is a dummy variable equal to 1 from November 2016 and later. All regressions include security and year–month fixed effects. The specification for forward monthly excess returns additionally controls for lagged returns, lagged trade imbalance, lagged market capitalization, book-to-market, and lagged return skewness. Standard errors are clustered at the security level and are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Table A18
Mechanism: Phone and Internet Access

DV	Number of Transactions		Number of Investors	
	(1)	(2)	(3)	(4)
Post × UPI Exposure	274.725***	288.389***	30.439***	31.797***
	(30.938)	(30.911)	(2.748)	(2.744)
Post × Moderator	-159.038	-113.561	-13.167	-11.930
	(113.315)	(196.608)	(10.504)	(20.671)
Post × UPI Exposure × Moderator	-105.829	1795.046***	-10.371	189.816***
	(147.015)	(343.225)	(13.222)	(35.260)
Pincode FE	Y	Y	Y	Y
District-Month FE	Y	Y	Y	Y
Moderator	Phone	Internet	Phone	Internet
N	445,901	445,901	445,931	445,931
Adj. R^2	0.966	0.966	0.968	0.968

This table presents the difference-in-differences (DiD) estimates for the impact of UPI exposure on stock market participation, allowing the effect to vary with household access to phone and internet services. Observations are at the pincode-month for the period January 2015 to January 2020. The dependent variable in columns 1 and 2 is the *Number of Transactions* and the dependent variable in columns 3 and 4 is the *Number of Investors*. UPI exposure is a continuous variable that measures the regional exposure, as defined in equation 1. The moderator in columns 1 and 3 measures phone access, while that in columns 2 and 4 measures internet access. Both are constructed as pre-period (January-October 2016) pincode-level averages of indicators for positive household expenditure on the respective service using the CMIE Consumer Pyramids Household Survey and are demeaned before constructing the interaction terms. *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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.