Retail Response to a Public Wealth Shock in a Decentralized Financial Market:

Evidence from a Cryptocurrency Airdrop

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Abstract

How do retail investors respond to a public exogenous wealth shock in a digital financial market, and how does that response vary with prior experience? I study this question using Uniswap's September 2020 retroactive UNI airdrop, an unanticipated distribution of approximately $1,200 to each Ethereum wallet that had interacted with the protocol before a previously undisclosed cutoff date, and full on-chain behavioral histories for 26,150 wallets observed over 165 weeks. Identification uses a local-randomization regression discontinuity around the retroactive eligibility cutoff. Retail response is structurally different across the two engagement margins. Trading frequency rises by approximately 3% in the activation year uniformly across the prior-experience distribution and is moderated by platform visibility, narrowing as public attention to the platform rises. Trading volume traces an inverted-U in prior experience: inexperienced wallets contract per-trade dollar value by 16%, intermediate-experience wallets (≈2.4 years of prior Ethereum activity) expand it by 29%, and the most experienced wallets show no significant response. Both effects dissipate fully by Year 2. The trading-volume pattern does not align with the predictions of the linear-sophistication or attention-driven-trading frameworks that dominate the retail-investor literature. The findings are most parsimoniously explained by a saturation mechanism: converting an exogenous windfall into scaled financial-market participation requires accumulated ecosystem capability that inexperienced participants have not yet developed and that the most experienced participants have already deployed.

Keywords: retail investors; cryptocurrency; decentralized finance; wealth shocks; heterogeneous treatment effects; regression discontinuity

1. Introduction

In September 2020, the decentralized exchange Uniswap distributed approximately $1,200 in governance tokens to each of roughly 250,000 wallets that had used the protocol before a previously undisclosed cutoff date. Uniswap is a cryptocurrency trading platform that runs on the Ethereum blockchain, executing trades through smart contracts rather than through a central intermediary. The distribution, known in cryptocurrency markets as an airdrop, was retroactive, rule-based, and unanticipated: the cutoff fell on September 1, 2020 at 00:00 UTC, but Uniswap disclosed it only on September 16, so wallets active in the days around the cutoff could not have positioned themselves strategically. Eligibility was deterministic and verifiable on the public blockchain, and the tokens conferred voting rights over the protocol's governance while trading as financial assets. The public-ledger structure of the underlying blockchain makes the full behavioral history of every wallet, recipients and non-recipient near-miss controls alike, observable at transaction-level resolution. This paper uses the Uniswap airdrop to study how retail investors respond to a public wealth shock in a digital financial market, and how that response varies with prior experience in the ecosystem.

The pattern is non-monotonic. Trading frequency rises by 3% in the activation year, uniformly across the experience distribution. Trading volume traces an inverted-U: inexperienced wallets contract their per-trade dollar value by 16%, intermediate-experience wallets (with roughly 2.4 years of prior Ethereum activity) expand it by 29%, and the most experienced wallets show no significant response. Both effects fade by Year 2. The pattern does not fit the standard frameworks for retail-investor heterogeneity. Barber and Odean (2000, 2008) predict that more experienced retail investors deploy windfalls more effectively, suggesting a monotone-positive relationship that the volume inverted-U does not exhibit. Barber, Huang,

Odean, and Schwarz (2022) predict that inexperienced retail investors expand speculative activity when drawn into a market by salient events, yet the inexperienced wallets here contract per-trade dollar value rather than expand. The household-finance windfall literature (Imbens, Rubin, and Sacerdote 2001; Cesarini, Lindqvist, Notowidigdo, and Östling 2017; Briggs, Cesarini, Lindqvist, and Östling 2021) implies that recipients with greater pre-shock engagement should respond more strongly, again predicting monotonicity the data do not show. A saturation mechanism is the most parsimonious explanation: converting a windfall into scaled trading requires capability that recipients accumulate through prior experience. Inexperienced wallets have not yet developed this capability, the most experienced wallets have already deployed their participation to a saturated level, and intermediate-experience wallets are uniquely positioned to convert the shock into scaled action. Attention drives the frequency response while experience drives the volume response, two structurally different forms of moderation that operate on different engagement margins.

The Uniswap airdrop has three features that make it well-suited to identifying retail response to a public wealth shock. First, treatment is sharply exogenous to prior behavior, and the deterministic on-chain eligibility rule eliminates any measurement uncertainty about treatment status, together approximating the ideal randomization that household-finance researchers typically have to instrument toward. Second, the data are unusually granular. Every wallet's complete transaction history, including activity on every other Ethereum-based protocol, is observable at timestamp-level resolution before and after the shock. This combination, an exogenous wealth shock to a heterogeneous retail population, with the population's prior and subsequent behavior fully observable, is rare in the household-finance and retail-investor literatures, where windfall response is typically measured through tax records or surveys and

retail-investor behavior is typically observed at coarser granularity. Third, the setting is itself a leading example of the digitization of financial markets. The combination of public-blockchain infrastructure, smart-contract trade execution, and rule-based distribution makes both promotional events and retail responses observable at a granularity that conventional intermediated markets cannot match. The Uniswap setting therefore provides what neither the windfall literature nor the retail-investor literature has had access to: a clean exogenous wealth shock at the individual user level, observed alongside the complete pre- and post-shock trading behavior of the entire affected population.

The paper connects most directly to two strands of recent finance research. The first studies retail behavior on digital trading platforms. Barber, Huang, Odean, and Schwarz (2022) show that Robinhood users engage in particularly attention-driven trading, with herding behavior concentrated in stocks experiencing extreme returns or media coverage. This work identifies retail behavior at the individual level but lacks a clean exogenous wealth shock, the variation it exploits is in attention to particular stocks, not in unanticipated changes to wealth at the individual user level. The second strand studies how households respond to exogenous wealth shocks. Briggs, Cesarini, Lindqvist, and Östling (2021) use Swedish lottery winnings to show that windfalls increase the probability of stock-market participation, with effects concentrated among households with prior savings. This work identifies wealth shocks cleanly but observes outcomes through tax records and survey data, at a granularity that obscures the transaction-level trading dynamics that emerge in the present setting. The Uniswap airdrop combines what the existing data structures in each strand have not allowed: an exogenous, publicly visible wealth shock at the individual user level, observed alongside full longitudinal trading histories. The non-monotonic volume response documented here, with inexperienced wallets contracting and the

most experienced wallets showing no response, is a pattern that neither strand's data structure would naturally surface.

Five additional results strengthen the interpretation. First, the average treatment effect on trading volume is positive but statistically weak (+3.1% in the activation year, $p = 0.22$), while the average effect on trading frequency is precisely estimated (+3.0%, $p < 0.001$). The volume null at the population mean masks substantial offsetting heterogeneity that the interaction analysis recovers. Second, the volume inverted-U is robust to how prior experience is measured. Replacing wallet age with log pre-period transaction count, log pre-period maximum balance, or a composite index of accumulated participation yields significant inverted-U interactions in every case, with peak marginal effects ranging from +26% to +38%. The four measures are weakly correlated at the wallet level (pairwise Pearson r between 0.14 and 0.16), so the replication is independent evidence rather than four restatements of the same variable. Third, platform visibility moderates the frequency response but not the volume response. Adding the interaction PROMO × BUZZ, where BUZZ is the weekly Google Trends search index for "Uniswap," shows that the recipient-versus-control frequency gap narrows by approximately 45% as visibility moves from the 10th to the 90th percentile, with no parallel effect on volume. Tenure moderates volume; visibility moderates frequency. Fourth, both effects fade fully by Year 2. The Years 2–3 estimates of the tenure interaction on volume and the average treatment effect on frequency are both statistically indistinguishable from zero. The dynamic fade rules out learning- and commitment-based mechanisms, which would predict growing or persistent effects rather than the observed contraction-then-null pattern. Fifth, finite-sample permutation tests (Cattaneo, Titiunik, and Vázquez-Bare 2017; 500 permutations) yield exact p-values below

1/501 ≈ 0.002 for the headline interaction coefficients, indicating that the asymptotic significance is not driven by cluster-robust-variance assumptions or distributional misspecification.

The paper makes three contributions. To the retail-investor heterogeneity literature in behavioral finance, the paper documents a non-monotonic experience-response relationship on the trading-volume margin that the linear-sophistication and attention-driven-trading frameworks do not predict. The novice-contraction finding in particular pushes against the prevailing characterization of inexperienced cryptocurrency retail investors as attention-driven speculators on the volume margin: when an exogenous windfall arrives, newly active wallets respond by contracting per-trade volume rather than expanding it. The visibility-moderation finding shows that attention-driven trading is not absent from this setting, it operates on the frequency margin. Attention and experience are structurally different moderators that govern different engagement margins, a distinction the prior literature has not made directly. To the household-finance windfall-response literature, the paper extends the heterogeneity dimensions examined to include accumulated trading-platform experience and shows that experience-conditional response in a digital financial-market setting is non-monotonic, a pattern that the demographic-and-wealth heterogeneity examined in lottery and inheritance settings has not surfaced. Beyond these two literature contributions, the findings demonstrate how the digitization of financial markets, specifically, the user-level public observability of every transaction, enables empirical questions about retail behavior that conventional intermediated markets cannot address. The transaction-level data structure that decentralized exchanges generate is the substrate on which these patterns become visible. Retail entry into digital financial markets has been particularly rapid in emerging-market economies, including India and South Asia, where mobile-first financial-platform adoption has accelerated retail participation in markets that previously had high access

frictions. The patterns documented here, non-monotonic windfall response on the volume margin, attention-moderated response on the frequency margin, speak to questions about retail behavior in decentralized financial markets globally, including the growing retail crypto populations in emerging-market economies.

The remainder of the paper proceeds as follows. Section 2 reviews the related literature. Section 3 describes the institutional setting, the data, and the empirical strategy. Section 4 reports the results. Section 5 discusses the saturation interpretation, scope conditions, and limitations.

2. Related Literature

This paper engages with three lineages of finance research: the retail-investor heterogeneity tradition in behavioral finance, the household-finance literature on response to exogenous wealth shocks, and the empirical work on retail behavior in cryptocurrency and decentralized financial markets. Each lineage has addressed a piece of the question this paper takes up, how retail investors respond to a public wealth shock in a digital financial market, and how that response varies with prior experience. None has addressed the question with the specific combination of features the Uniswap setting makes available: a clean exogenous wealth shock at the individual user level, full longitudinal trading histories on the same population, and clean separation of trading frequency from trading volume in the outcome variables. The three literatures share a common data-structural limitation that the digitization of financial markets, and specifically the public-ledger architecture of decentralized exchanges, relaxes for the first time.

2.1 Retail-investor heterogeneity in behavioral finance

The behavioral-finance literature on retail-investor behavior has developed two principal organizing constructs: sophistication and attention. The sophistication strand, building on Barber and Odean (2000), documents that retail investors systematically underperform passive

benchmarks, with the underperformance concentrated among less sophisticated traders. The implicit prediction of this literature, when extended to windfall response, is that more sophisticated retail investors should deploy windfalls more effectively than less sophisticated ones. The attention strand, building on Barber and Odean (2008), documents that retail investors are net buyers of attention-grabbing stocks and that this attention-driven buying generates systematic patterns in retail order flow. Recent work by Barber, Huang, Odean, and Schwarz (2022) extends the attention strand to digital-platform retail trading specifically, showing that Robinhood users engage in particularly attention-driven trading with herding behavior concentrated in stocks experiencing extreme returns or media coverage. Extended to a public wealth shock, the attention strand predicts that inexperienced retail investors drawn into a market by salient promotional events would expand activity following such an event.

This literature identifies retail-investor heterogeneity at the individual user level, Robinhood account-level data, transaction-level brokerage data, but the identification strategies operate at the aggregate level. The variation exploited is in attention to particular stocks, in media coverage, or in account-opening dynamics, not in unanticipated changes to wealth at the individual user level. No paper in this literature has identified retail response to a publicly visible exogenous wealth shock at the individual user level with full longitudinal histories around the shock. The pattern documented in the present setting suggests a structural refinement of the two-construct framework: attention and experience appear to operate on different engagement margins, with attention moderating whether participants act and experience moderating how much value they deploy.

2.2 Response to exogenous wealth shocks in household finance

A substantial household-finance literature has used exogenous wealth shocks, primarily lottery winnings and inheritance receipts, to identify how unanticipated wealth affects household economic behavior. Imbens, Rubin, and Sacerdote (2001) established the foundational design using U.S. lottery winners. Cesarini, Lindqvist, Notowidigdo, and Östling (2017) significantly improved the design using Swedish administrative data covering the entire population of lottery winners and matched non-winners. The closest precedent to the present paper is Briggs, Cesarini, Lindqvist, and Östling (2021), who extend the Swedish-lottery design specifically to financial-market participation. They show that lottery winnings raise the probability of stock-market participation, with effects concentrated among households with prior savings, a finding that establishes the importance of pre-shock financial-market engagement as a moderator of windfall response. The present paper takes this dimension further by examining the structure of that moderation at the trading-decision level, in a setting where transaction-level outcomes are observable rather than annual or binary outcomes.

What this literature has not been able to examine, given its data infrastructure, is the within-recipient trading-behavior response: how recipients of an unanticipated wealth shock alter the frequency and dollar magnitude of their individual trading decisions in the weeks and months after the shock. Tax records and survey data observe stock-market participation as a binary outcome or at low frequency. Even the Swedish administrative data, which is the gold standard of the windfall literature in terms of population coverage, does not record trades at transaction-level resolution. The implicit prediction from this literature is that windfall response should vary monotonically with prior wealth and prior financial-market engagement; the inverted-U on the volume margin documented here is not what this prediction implies. The digital structure of the decentralized financial markets where the present shock occurs is what makes the within-

recipient trading response observable, and what makes the non-monotonic moderation pattern detectable.

2.3 Retail behavior in cryptocurrency and decentralized financial markets

A growing empirical literature has begun to characterize retail participation in cryptocurrency and decentralized financial markets. The work that engages most directly with the present setting comes from two strands. The first studies initial coin offerings and token-distribution events from a corporate-finance perspective. Howell, Niessner, and Yermack (2020) provides the foundational empirical analysis of ICOs as a fundraising mechanism, examining the determinants of token-sale success and post-distribution token performance. Li, Wan, Cheng, and Zhao (2024) examines dual-incentive ICOs in which token issuers distribute tokens both to investors and to early users, finding that the design of the distribution affects investor participation and post-distribution outcomes. Both papers analyze token-distribution events at the protocol or campaign level, their outcome variables are token prices, fundraising amounts, and aggregate adoption metrics rather than individual user behavior.

The second strand studies retail behavior on cryptocurrency exchanges and DeFi protocols. Makridis, Fröwis, Sridhar, and Böhme (2023) examines the rise of decentralized exchanges and the role of airdrops and governance tokens in driving DEX growth, documenting that token-distribution events coincide with increased DEX activity and with shifts in trading volume between centralized and decentralized venues. Their analysis uses protocol-level aggregated data and does not decompose the population response across recipient heterogeneity. Cong, He, and Li (2023), examining decentralized mining incentives in pool structures, contributes to the broader literature on protocol-level economic design rather than to user-level retail-behavior analysis specifically.

What none of these strands has provided is user-level analysis of retail response to a public token-distribution event using full longitudinal on-chain behavioral histories. The protocol-level studies see token-distribution effects in aggregate but cannot decompose them across the recipient population. The user-level cryptocurrency studies that exist focus on cross-platform retail behavior or on adoption dynamics rather than on response to an exogenous shock at the individual level. The Uniswap setting, public, rule-based, exogenously-assigned wealth shock; on-chain longitudinal observability of every recipient, is the specific configuration of features that allows the user-level identification.

The three literatures reviewed above each address a piece of the question this paper takes up, but each is limited by a data-structural constraint that the present setting overcomes. The retail-investor literature has user-level granularity but no clean exogenous wealth shock at the individual level. The household-finance windfall literature has clean shocks but observes outcomes at coarser granularity than transaction-level trading. The cryptocurrency-finance literature has the relevant market structure but has analyzed retail response primarily at the protocol or aggregated level. The digitization of financial markets, specifically, the public-blockchain architecture that records every wallet's transactions in real time, is what makes the three sets of features simultaneously available.

Beyond the data-structural opportunity, however, the three literatures share a substantive implicit prediction: that retail response to an exogenous wealth shock should vary monotonically with prior experience or engagement. The non-monotonic volume response documented in this paper is not what any of these literatures predicts. Inexperienced retail participants contract per-trade dollar value rather than expanding it. The most experienced participants show no incremental response despite their accumulated trading sophistication. Intermediate-experience

participants are the ones who scale up. Separately, the trading-frequency response is approximately uniform across the experience distribution but is moderated by platform visibility, a structural asymmetry between the two engagement margins that the three-literature framework has not made explicit. The paper's contribution is therefore both empirical (a setting that allows the question to be addressed) and substantive (a pattern that requires the three literatures to revise their implicit monotonicity assumption, and a margin-level asymmetry that suggests attention and experience are structurally different moderators operating on different aspects of retail trading).

3. Setting, Data, and Empirical Strategy

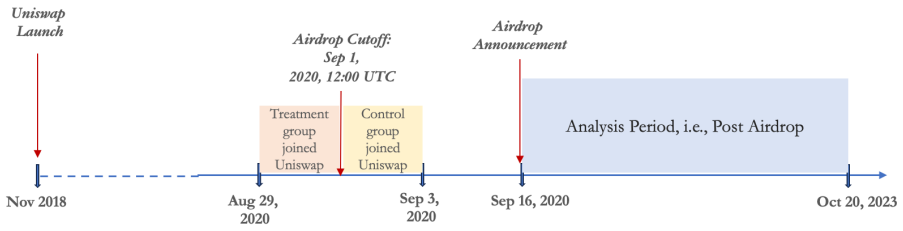
3.1 Institutional setting

Uniswap is a decentralized exchange (DEX) operating on the Ethereum blockchain. Launched in November 2018, the protocol functions as an automated market maker: trades execute directly against liquidity pools maintained by independent liquidity providers through smart contracts, with no centralized order book or intermediary. By the middle of 2020, Uniswap had become the dominant venue for decentralized cryptocurrency trading on Ethereum, with daily trading volumes in the hundreds of millions of dollars.

On September 16, 2020, Uniswap announced a retroactive distribution of 400 UNI tokens to every Ethereum wallet that had interacted with the protocol prior to September 1, 2020 at 00:00 UTC. UNI is Uniswap's governance token; recipients gained voting rights over protocol upgrades and treasury allocation in addition to receiving the tokens as transferable financial assets. At the announcement price, 400 UNI was worth approximately $1,200, a substantial wealth shock for most recipients. The distribution allocated approximately $1.4 billion in tokens to roughly 250,000 wallets in aggregate.

Figure 1 summarizes the institutional timeline. The SushiSwap launch (August 28, 2020), the Uniswap eligibility cutoff (September 1, 2020), the ±3-day sample window (August 29 – September 3, 2020), the announcement of the airdrop and disclosure of the cutoff rule (September 16, 2020), the actual token distribution (September 17, 2020), and the 165-week post-airdrop observation window are shown on a single time axis. The retroactive structure of the eligibility rule is visible directly: the cutoff falls 15 days before its disclosure, with no information about the rule available to any participant during the sample window.

Figure 1. Uniswap timeline: airdrop cutoff, announcement, and analysis period.



Treatment group users are those who registered on Uniswap, i.e., interacted with Uniswap, for the first time between Aug 29, 2020, and Aug 31, 2020.
Control group users are those who registered on Uniswap, i.e., interacted with Uniswap, for the first time between Sept 1, 2020, and Sep 3, 2020.

Key institutional events relevant to identification: SushiSwap launch (August 28, 2020); Uniswap eligibility cutoff (September 1, 2020, 00:00 UTC); ±3-day sample window (August 29 – September 3); airdrop announcement and disclosure of the cutoff rule (September 16); UNI token distribution (September 17). The 15-day gap between the cutoff and its disclosure structurally precludes anticipatory manipulation.

Three institutional features of this event are central to the identification strategy developed in Section 3.4. First, eligibility was retroactive: the September 1 cutoff was determined and disclosed only on September 16, after the date had already passed. Wallets active in the days surrounding the cutoff could not have positioned their first interaction strategically because no eligibility rule existed in their information set at the time. Second, the eligibility rule is fully deterministic and observable on-chain. The Ethereum ledger provides a complete record of every wallet's interactions with Uniswap prior to the cutoff, eliminating any measurement uncertainty

about treatment status. Third, eligibility itself does not depend on any subsequent action by the recipient, supporting a clean intent-to-treat estimand from on-chain history alone.

3.2 Sample construction

I construct a wallet-level panel of users whose first observed interaction with Uniswap occurred within ±3 days of the September 1 cutoff (August 29 – September 3, 2020), yielding 26,150 wallets after dropping 13 with first-interaction dates outside the window. The treatment indicator equals 1 for wallets whose first interaction occurred August 29–31 (the eligible side; n = 11,536) and 0 for wallets whose first interaction occurred September 1–3 (the control side; n = 14,614).

The choice of a ±3-day window is the central bandwidth choice in this design. The window must be narrow enough that the wallet populations on the two sides of the cutoff are comparable on observables and unobservables, and wide enough that the resulting sample provides statistical power for the heterogeneous-treatment-effect analysis. The ±3-day window achieves both: it generates approximately 26,000 wallets across two sides while remaining short enough that calendar effects (day-of-week, holiday-week dynamics) and contemporaneous market events affect both sides similarly. Section 3.6 reports bandwidth sensitivity at ±1d and ±2d, which yields the same qualitative conclusions on a 2.5× smaller sample.

For each wallet, I observe weekly trading activity from September 17, 2020 (the day after the airdrop announcement) through October 22, 2023, yielding 478,168 wallet-week observations across 165 weeks. The median wallet contributes 10 weekly observations and the mean is 18.3; the panel is unbalanced because a wallet appears in the data only in weeks during which it was active on Uniswap. The 165-week observation window is sufficient to detect both short-run (activation year) and longer-run (Years 2–3) treatment effects, and to characterize the dynamic pattern of the response.

Table 1 reports descriptive statistics and pre-treatment balance. Eligible wallets show a mild imbalance on pre-airdrop SushiSwap activity (25.6% vs. 22.3%; standardized mean difference 0.077), which is addressed by the pre-Sushi cohort restriction reported in Section 3.6. All other pre-period covariates exhibit standardized mean differences below 0.05, consistent with the local-randomization assumption.

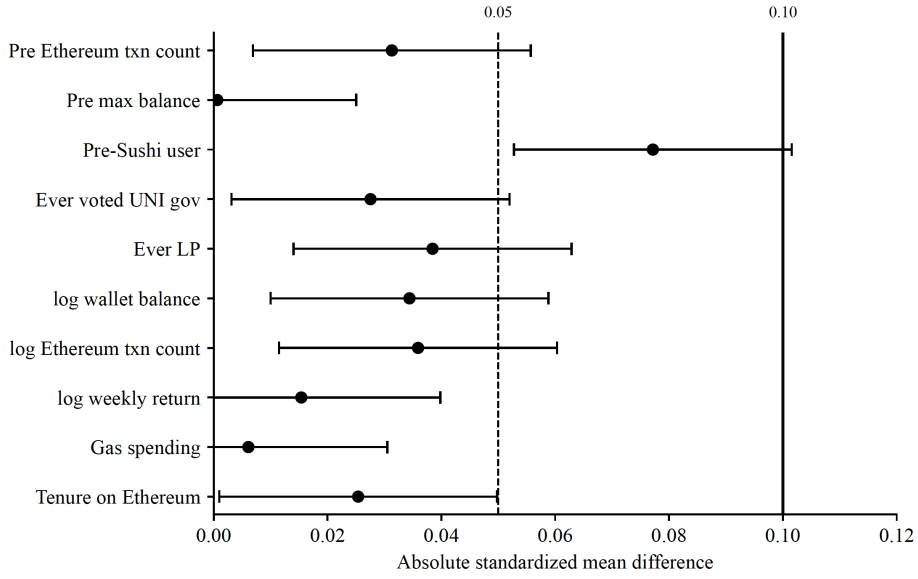
Table 1. Descriptive statistics.

Variable	**All wallets**	**Control**	**Treated**
Wallet-level (N = 26,150)			
Pre Ethereum txn count	39.98 (159.16)	37.78 (178.34)	42.77 (13.84)
Pre max balance ($K)	13.27 (182.98)	13.22 (178.14)	13.34 (188.93)
Pre-Sushi user (0/1)	.24 (.43)	.22 (.42)	.26 (.44)
Ever voted UNI gov (0/1)	.001 (.04)	.001 (.03)	.002 (.04)
Ever LP (0/1)	.11 (.31)	.10 (.30)	.11 (.32)
Wallet-week panel (N = 478,168)			
Weekly swap count	1.86 (4.26)	1.78 (4.15)	1.93 (4.35)
Weekly USD value swapped	2,460 (57,441)	2,724 (72,366)	2,199 (37,146)
log(1+SWAPS) (FREQ)	.40 (.78)	.39 (.76)	.42 (.79)
log(1+USD) (VOL)	1.83 (3.27)	1.81 (3.28)	1.85 (3.26)
Tenure on Ethereum, weeks (EXP)	53.23 (61.40)	54.02 (62.06)	52.46 (6.73)
log wallet balance	5.49 (2.76)	5.53 (2.81)	5.44 (2.71)
log Ethereum txn count	1.28 (1.18)	1.26 (1.17)	1.30 (1.18)
log weekly return	−0.03 (1.80)	−0.04 (1.86)	−0.01 (1.75)
Gas spending ($)	95,411 (137,663)	95,833 (163,462)	94,993 (106,083)
Time-only variables (165 weeks)			
Visibility, Google Trends (BUZZ)	16.61 (12.39)	—	—
UNI daily active users	70,617 (30,976)	—	—
UNI liquidity	.40 (.34)	—	—
Weekly ETH/USD change	8.42 (231.86)	—	—

Standard deviations in parentheses. Pre-period covariates measured prior to the September 16, 2020 airdrop announcement. Eligible wallets show a mild imbalance on pre-airdrop SushiSwap activity (SMD = 0.077), addressed by the pre-Sushi cohort restriction in Table 2.

Figure 2 summarizes pre-treatment balance, plotting the standardized mean difference (with 95% confidence intervals) for each pre-period covariate against reference lines at the 0.05 and 0.10 thresholds. Nine of the ten covariates fall below the conservative 0.05 threshold. The lone exception is the pre-Sushi indicator, at 0.077: it is the only covariate exceeding 0.05, though its point estimate remains below the 0.10 unconfoundedness threshold (Rosenbaum and Rubin 1985; Stuart 2010). This single imbalance, the contemporaneous SushiSwap launch three days before the cutoff, is the most substantive identification challenge in the design, and I engage it directly through the pre-Sushi cohort restriction in Section 3.6.

Figure 2. Covariate balance: standardized mean differences between treated and control wallets.



Absolute standardized mean differences for ten pre-period covariates between treated and control wallets, with 95% confidence intervals. Reference lines: dashed at SMD = 0.05 (conservative balance threshold) and solid at SMD = 0.10 (unconfoundedness threshold; Rosenbaum and Rubin 1985; Stuart 2010). Nine of ten covariates fall below the 0.05 threshold; the pre-Sushi indicator at 0.077 is the only above-0.05 imbalance and is addressed in Section 3.6.

3.3 Outcome and control variables

Two trading outcomes are measured at the wallet-week level. Trading frequency is the natural log of one plus the count of swap transactions executed by the wallet in the focal week. Trading volume is the natural log of one plus the USD value of those swaps, computed using transaction-time exchange rates. The log-plus-one transformation is standard for trading-outcome variables with many zero observations and substantial right-skew when nonzero.

The heterogeneity variable, prior ecosystem experience (EXP), is the age of the wallet on Ethereum in weeks at observation time, computed as elapsed weeks since the wallet's first observed Ethereum transaction. This measure captures accumulated experience in the broader Ethereum ecosystem rather than experience with Uniswap specifically, which is held approximately constant across the sample by the sample-construction rule (all wallets had first Uniswap interaction within a six-day window). For robustness, three alternative operationalizations of prior experience are constructed: log pre-period Ethereum transaction count, log pre-period maximum wallet balance, and a composite z-score index. The four measures are weakly correlated at the wallet level (pairwise Pearson r between EXP and the others ranges from 0.14 to 0.16), so the four operationalizations represent largely independent measurements of underlying participation.

A separate time-varying moderator, platform visibility (BUZZ), is the weekly Google Trends search index for "Uniswap" and varies across the 165 weeks of the post-airdrop panel. BUZZ enters Section 4.4 as a moderator of the trading-frequency response.

Wallet-level controls include the contemporaneous (weekly) log balance, log Ethereum-wide transaction count, log weekly return, USD gas spending, an indicator for ever providing liquidity to Uniswap pools, an indicator for prior SushiSwap activity, and an indicator for ever exercising UNI governance voting rights. These controls absorb wallet-specific time-varying characteristics

that affect trading behavior; the identification of treatment effects does not rely on them but they improve precision.

3.4 Identification strategy

The treatment of interest is exogenous airdrop eligibility, assignment to receive approximately $1,200 in UNI tokens as a function of a single observable rule (interaction with Uniswap prior to September 1, 2020). The identification challenge is to separate the causal effect of this assignment from confounding sources of correlation between treatment status and post-airdrop outcomes. This subsection articulates the identification challenges I confront and the strategy I use to address each.

3.4.1. Identification challenges

Four threats to causal identification arise in this setting. The first threat is anticipatory manipulation. If wallets active near the cutoff could have positioned their first Uniswap interaction strategically to gain eligibility, the treatment indicator would not be exogenous to wallet-level decision-making, and any post-airdrop outcome difference between treated and control wallets could reflect compositional effects of strategic positioning rather than the causal effect of treatment. The second threat is sorting on observables or unobservables. Even absent strategic manipulation, the populations of wallets that happened to first use Uniswap in the days before versus after September 1, 2020 may differ systematically in ways that affect post-airdrop trading behavior. Unobserved differences in wallet-holder characteristics, risk tolerance, financial sophistication, time available for crypto-related activity, could be correlated with the calendar date of first Uniswap interaction in ways that confound the treatment-effect estimate.

The third threat is contemporaneous confounding events. The August 28, 2020 launch of SushiSwap, a competing decentralized exchange that distributed its own governance token

(SUSHI) and was widely interpreted at the time as a competitive challenge to Uniswap, falls within the sample window. SushiSwap-related attention could have asymmetrically drawn certain wallet types to first interact with Uniswap on certain days surrounding the cutoff, generating differential composition between treated and control wallets that does not reflect the airdrop itself. The fourth threat is time-varying composition of the broader cryptocurrency market. Post-airdrop outcomes are observed over 165 weeks, during which Ethereum and the broader cryptocurrency market experienced substantial volatility, regulatory developments, and shifts in macroeconomic conditions. Any difference in how these dynamics affect treated versus control wallets could confound the treatment-effect estimates over the observation window.

3.4.2. Identification Strategy

I address these four challenges through four layers of design, each layer addressing one or more of the challenges identified above. The first layer is the retroactive structure of the eligibility rule. The single feature of the Uniswap airdrop that does the most identification work is the retroactive disclosure of the eligibility cutoff. The September 1 cutoff was determined and made public only on September 16, 2020, fifteen days after it had already passed. At no point between September 1 and September 16 did any wallet have information that an eligibility rule existed, that it would apply retroactively, or that the cutoff date was September 1. Wallets that first interacted with Uniswap on August 29, 30, or 31 did so without knowing they were about to be classified as eligible; wallets that first interacted on September 1, 2, or 3 did so without knowing they were about to be classified as ineligible. This structurally precludes the first threat, anticipatory manipulation, because no participant could position itself with respect to an eligibility rule that did not yet exist. The retroactive structure is unusual among token-distribution events in cryptocurrency markets, where eligibility rules are typically announced ex

ante and participants can position strategically to qualify. The Uniswap airdrop is, in this sense, exceptionally clean among naturally-occurring wealth shocks.

The second layer is the local-randomization framework applied within a narrow window around the cutoff. Standard regression-discontinuity estimators (Hahn, Todd, and Van der Klaauw 2001; Imbens and Lemieux 2008) rely on a continuously varying running variable, with identification coming from local-polynomial fits that compare units just above and just below the cutoff. The running variable in the present setting, the calendar date of first Uniswap interaction, is discrete, with at most six distinct values in the ±3-day window I use. Local-polynomial fits are not informative in this setting, and the appropriate inference framework is the local-randomization approach developed by Cattaneo, Frandsen, and Titiunik (2015) and refined by Cattaneo, Titiunik, and Vázquez-Bare (2017). The framework's central assumption is that, within a sufficiently narrow window around the cutoff, treatment can be treated as if randomly assigned conditional on observed covariates. The narrow window restriction is what makes this assumption credible: as the window shrinks, the units on either side become increasingly similar on observables and unobservables alike. The ±3-day window I use yields 26,150 wallets; Table 1 shows that all pre-period covariates other than SushiSwap activity exhibit standardized mean differences below 0.05, well within the 0.10 threshold conventional for unconfoundedness in observational settings. This layer addresses the second threat, sorting on observables and, indirectly, on unobservables that are likely correlated with observables.

The third layer is the explicit engagement with the SushiSwap launch as a contemporaneous confounder (Figure 2). SushiSwap is the only contemporaneous event within the sample window that could plausibly drive differential composition between treated and control wallets. I address this threat in three ways. First, the pre-Sushi cohort restriction (reported in Section 3.6) limits the

sample to wallets created on Ethereum before the SushiSwap launch, wallets that, by construction, could not have been brought to Uniswap by SushiSwap-related attention. The inverted-U interaction holds on this restricted sample at approximately three-quarters of its baseline magnitude, with reduced precision attributable to the smaller sample size but the same qualitative pattern. Second, an indicator for pre-cutoff SushiSwap activity is included as a control in all specifications, absorbing wallet-level variation in SushiSwap exposure. Third, the treatment effect of interest is the interaction of treatment with prior Ethereum experience (EXP), not the average treatment effect. Even if SushiSwap-related composition differences exist on the cutoff boundary, they would need to vary systematically with prior experience to bias the interaction estimate, a stronger composition-effect requirement than would arise for average treatment effects alone.

The fourth threat, time-varying market conditions, is addressed structurally by the design rather than by an explicit robustness check. Week fixed effects absorb all market-level variation that affects all wallets in a given week. The treatment-effect estimates rely on between-wallet variation within each week, not on time-series variation in market conditions. Additionally, the dynamic-fade pattern observed in the results (Section 4.5) provides indirect evidence that broader market conditions are not driving the headline treatment effect. If the headline coefficients were artifacts of post-airdrop crypto-market dynamics rather than the airdrop itself, the heterogeneity pattern should persist or evolve with market conditions, not fade cleanly to zero by Year 2. The clean fade is hard to generate from market-level confounders, which would not naturally produce a sharp twelve-month-bounded effect.

The four-layer strategy, retroactive disclosure as the structural foundation, local-randomization within a narrow window as the empirical foundation, explicit engagement with

SushiSwap as the dominant contemporaneous threat, and week-fixed-effects absorption of time-varying conditions, provides a structured response to each of the four identification challenges. Section 3.6 reports the battery of robustness specifications that empirically validates this strategy.

3.5 Empirical specification

I estimate two nested specifications. The first quantifies the average treatment effect of airdrop eligibility:

$$Y_{it} = \alpha + \beta \cdot TREAT_i + \Gamma \cdot X_{it} + \delta_t + \varepsilon_{it} \quad (1)$$

where Y_{it} is the outcome (trading frequency or trading volume), $TREAT_i$ is the treatment indicator, X_{it} is the wallet-level control vector, δ_t is a week fixed effect, and ε_{it} is an error term clustered at the wallet level. The second specification introduces experience-moderated heterogeneity through linear and quadratic interactions:

$$Y_{it} = \alpha + \beta_1 \cdot TREAT_i + \beta_2 \cdot EXP_{it} + \beta_3 \cdot EXP^2_{it} + \rho_1 \cdot (TREAT_i \times EXP_{it}) + \rho_2 \cdot (TREAT_i \times EXP^2_{it}) + \Gamma \cdot X_{it} + \delta_t + \varepsilon_{it} \quad (2)$$

The marginal effect of treatment at experience level e is $\beta_1 + \rho_1 \cdot e + \rho_2 \cdot e^2$. When $\rho_1 > 0$ and $\rho_2 < 0$, the marginal effect traces an inverted-U with peak at $EXP^* = -\rho_1/(2\rho_2)$.

Section 4.4 introduces a parallel specification with TREAT × BUZZ to test whether platform visibility moderates the response. I estimate equations (1) and (2) on three samples: the full post-airdrop window (165 weeks), the activation year (52 weeks), and Years 2-3 (113 weeks). Inference uses cluster-robust standard errors at the wallet level. For the headline interaction terms, I supplement the asymptotic inference with finite-sample permutation tests (Cattaneo, Titiunik, and Vázquez-Bare 2017; 500 permutations).

3.6 Identification threats and robustness

Section 3.4 articulated the identification strategy at the framework level. This subsection reports a battery of robustness specifications, each addressing a specific class of threat. Table 2 summarizes the activation-year volume interaction coefficient under each specification alongside the baseline.

Table 2. Robustness battery: activation-year volume tenure interaction.

Test	PROMO × EXP (SE)	PROMO × EXP² (SE)	N	Wallets
Baseline (±3d, full sample)	+.0073*** (.0015)	−2.96e-05*** (7.9e-06)	282,565	26,148
Donut: drop cutoff day (Sep 1)	+.0061*** (.0016)	−2.39e-05** (9.2e-06)	227,069	20,343
Donut: drop boundary days (Aug 31, Sep 1)	+.0071*** (.0018)	−2.72e-05** (1.0e-05)	172,435	15,756
Bandwidth ±1d	+.0060** (.0022)	−2.61e-05* (1.3e-05)	110,130	10,392
Bandwidth ±2d	+.0079*** (.0017)	−3.29e-05*** (9.2e-06)	213,503	19,777
Pre-Sushi cohort (≤ Aug 28)	+.0054† (.0029)	−2.19e-05 (1.3e-05)	81,156	5,964

**** p < .001, ** p < .01, * p < .05, † p < .1. Each row reports OLS estimates of equation (2) on the activation-year subsample under the indicated robustness specification. Cluster-robust standard errors in parentheses.*

Donut RD specifications. Donut-RD drops observations immediately adjacent to the cutoff before estimating the treatment effect. The motivation is that observations precisely at the cutoff are most susceptible to measurement error or local sorting. Dropping the cutoff day alone yields a treatment-interaction coefficient of +0.0061 ($p < .001$); dropping the two days immediately adjacent to the cutoff boundary (Aug 31 and Sep 1) yields +0.0071 ($p < .001$). Both estimates remain close to the baseline +0.0073 and remain highly statistically significant. The donut-RD evidence rules out the hypothesis that the headline result is driven by anomalous behavior precisely at the cutoff boundary.

Bandwidth sensitivity. Bandwidth choices in local-randomization RD trade off bias (wider windows include less-comparable units) against precision (narrower windows have fewer

observations). I estimate the treatment-interaction coefficient at the baseline ±3-day window and at ±1d and ±2d alternatives. The estimates are +0.0060 (±1d, $p = .007$), +0.0079 (±2d, $p < .001$), and +0.0073 (±3d, $p < .001$), within 25% of one another across a 2.5× range of sample size. The result is not sensitive to bandwidth choice.

Pre-Sushi cohort restriction. Restricting the sample to wallets created on Ethereum before the August 28, 2020 SushiSwap launch yields PROMO × EXP = +0.0054 ($p = .064$) on 5,964 wallets. The point estimate is approximately three-quarters of the baseline magnitude, and the marginal significance reflects the smaller sample. The result confirms that the inverted-U interaction is present in the sub-population that cannot have been brought to Uniswap by SushiSwap-related attention, supporting the interpretation that SushiSwap-induced composition does not drive the headline finding.

Density test. I conduct a discrete-running-variable analog of the McCrary (2008) density test by examining the distribution of first-interaction dates across the cutoff. Figure 3 plots the density. The test rejects uniformity ($\chi^2 = 1414$, $p < 10^{-300}$), but the entry density peaks on the control side of the cutoff (Tuesday September 1) rather than the treated side. Strategic manipulation, if it were occurring, would generate a density peak on the treated side; the observed pattern is the opposite of strategic sorting. The Tuesday September 1 peak is consistent with calendar effects, Tuesday is the first business day after the Labor Day weekend in the United States (September 5–7 in 2020), rather than anticipatory manipulation around the cutoff. The retroactive disclosure structurally precludes anticipatory manipulation in any case; the density-test result confirms this empirically.

Figure 3. Entry density around the eligibility cutoff.

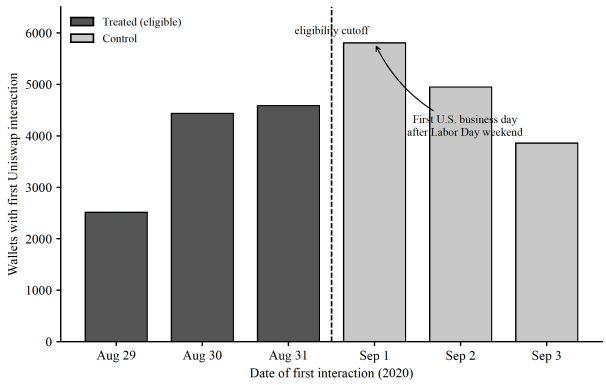


Figure 3 plots the number of wallets entering on each date in the ±3-day window, shaded by treatment status. If wallets had strategically accelerated their first interaction to fall on the eligible side of the cutoff, entry density would bunch just before September 1. It does not. The density instead peaks on Tuesday, September 1 (5,807 wallets), squarely on the control side, and the three control days collectively outnumber the three treated days (14,614 versus 11,536). The pattern is the opposite of what manipulation would produce: mass accumulates just after the cutoff, among wallets that forfeited eligibility. September 1 is the first U.S. business day following the Labor Day weekend, and the elevated entry is consistent with a calendar-driven return to activity rather than sorting on the airdrop threshold.

Placebo cutoffs. I conduct two placebo cutoff exercises. First, I impose a placebo treatment-control boundary within the all-treated sub-sample (August 30) and within the all-control sub-sample (September 2), and re-estimate the heterogeneous treatment effect specification. The placebo coefficients are statistically null ($p = .27$ for the August 30 placebo; $p = .33$ for the September 2 placebo for the headline volume interaction). The result rules out the hypothesis that spurious discontinuities exist within the same-treatment-status sub-populations.

Pre-period covariate placebos. A second placebo exercise uses pre-period covariates as the dependent variable in equation (1). If treatment were correlated with pre-period outcomes (which by construction it cannot have caused), this would indicate that treated and control wallets differ on pre-period characteristics in ways that might confound post-period outcome estimates. The placebo PROMO coefficients are statistically null on both pre-period transaction count ($p = .38$) and pre-period maximum balance ($p = .45$). The result confirms that treatment does not predict pre-period outcomes, supporting the as-if-random assignment assumption.

Oster bounds. Following Oster (2019), I compute the δ that would be required for selection on unobservables to fully explain the treatment-effect estimates, given the patterns of selection on observables already observed. The δ values for the average treatment effects of equation (1) range from 5.9 to 9.5, exceeding the standard threshold $|\delta| \geq 1$ by an order of magnitude. The result indicates that selection on unobservables would have to be implausibly strong (5 to 10 times stronger than selection on observables) to explain the documented effects.

Inverse-probability weighting. As a final methodological robustness check, I re-estimate equation (2) using inverse-probability-weighted regression adjustment (Słoczyński, Uysal, and Wooldridge 2022). The propensity model is a wallet-level logit of PROMO on pre-period covariates. Full results are reported in Section 4.7.2. The IPW estimates closely track the OLS estimates, with the headline interaction terms differing by approximately 1–2% (Table 11), and the propensity model itself has McFadden's pseudo-R^2 of 0.0002, indicating that pre-period covariates do not separate eligible from near-miss control wallets, a balance-test result that further supports the local-randomization assumption.

The battery of robustness checks, taken together, supports the central identification claim. Boundary contamination, bandwidth choice, SushiSwap composition, strategic manipulation, spurious discontinuities, pre-period confounding, selection on unobservables, and weighting-scheme choice each fail to overturn the headline inverted-U finding. The retroactive-disclosure foundation laid in Section 3.4 is empirically validated by this collection of tests.

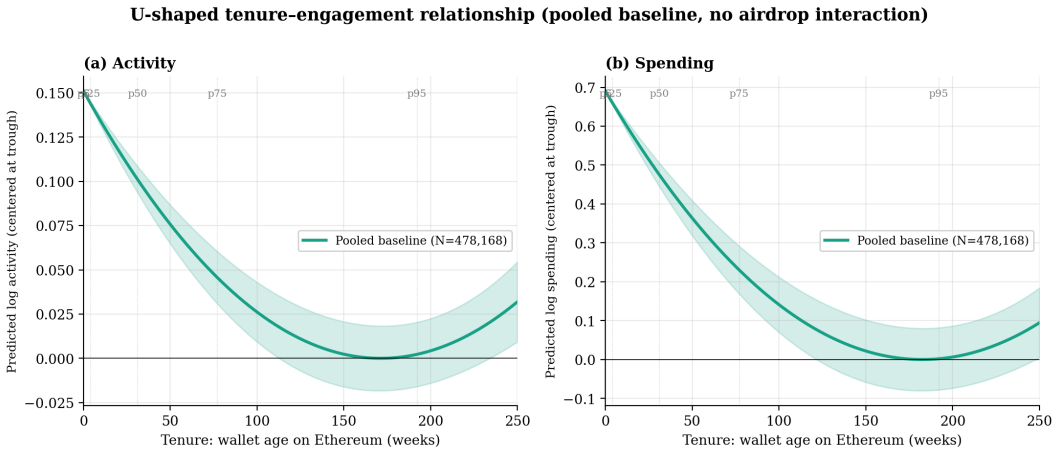
4. Results

4.1 Baseline relationship between experience and trading behavior

Before turning to treatment effects, I document the baseline relationship between prior ecosystem experience and trading behavior in the pre-treatment data, holding the airdrop aside.

Estimating the regression in equation (2) with the TREAT terms omitted, both trading outcomes are U-shaped in prior experience. For trading frequency, the linear coefficient on EXP is −.00176 ($p < .001$) and the quadratic coefficient on EXP^2 is $+5.15 \times 10^{-6}$ ($p < .001$), implying a trough at EXP* = 171 weeks (≈3.3 years). For trading volume, the corresponding coefficients are −.00756 and $+2.07 \times 10^{-5}$ (both $p < .001$), with a trough at EXP* = 183 weeks. Baseline trading on both outcomes is highest among newly created wallets and the most experienced wallets, and lowest at intermediate experience (Figure 4).

Figure 4. Baseline U-shape in trading frequency and trading volume (pooled sample, no airdrop interaction).



Panel (a): trading frequency (log of weekly swap count). Panel (b): trading volume (log of weekly USD value). Curves centered at the within-outcome trough; shaded regions are 95% confidence bands. Vertical dotted lines mark the 5th, 25th, 50th, 75th, and 95th percentiles of EXP in the full panel.

Intermediate-experience wallets have, on average, lower baseline trading engagement than either novices or veterans. For the treatment-effect analysis that follows, this baseline pattern has direct implications: intermediate-experience wallets have the most slack in their pre-treatment trading and are therefore the cohort with the largest scope to scale up in response to an exogenous wealth shock.

4.2 Average treatment effects

Table 3 reports estimates of equation (1) in two specifications. Three patterns are evident. First, airdrop eligibility raises trading frequency, and the effect is concentrated in the activation year. The covariate-adjusted estimate is +.022 ($p < .001$) in the full sample, +.030 ($p < .001$) in the activation year, and +.002 (n.s.) in Years 2–3. Second, the average treatment effect on trading volume is positive but statistically weak: +.028 ($p = .118$) in the full sample and +.031 ($p = .220$) in the activation year. Third, both effects dissipate fully by Year 2.

Table 3. Average treatment effect of airdrop eligibility on trading frequency and trading volume.

Period	Outcome	Spec A: β (SE)	Spec B: β (SE)	N
Overall	Trading Frequency	+.0363*** (.0058)	+.0221*** (.0041)	478,168
Overall	Trading Volume	+.0488* (.0210)	+.0284 (.0182)	478,168
Activation Yr	Trading Frequency	+.0609*** (.0085)	+.0296*** (.0055)	282,565
Activation Yr	Trading Volume	+.0888** (.0295)	+.0306 (.0250)	282,565
Years 2–3	Trading Frequency	+.0013 (.0042)	+.0024 (.0037)	195,603
Years 2–3	Trading Volume	−.0080 (.0189)	+.0024 (.0173)	195,603

*OLS estimates of equation (1), cluster-robust standard errors at the wallet level in parentheses. *** $p < .001$, ** $p < .01$, * $p < .05$. All specifications include 165 week fixed effects.*

4.3 Experience-moderated heterogeneous treatment effects

Table 4 reports estimates of equation (2). For trading volume, the linear interaction TREAT × EXP is +.0029 ($p < .001$) and the quadratic interaction TREAT × EXP^2 is -1.06×10^{-5} ($p < .001$) in the full sample, sharpening to +.0073 and -2.96×10^{-5} in the activation year. The signs trace an inverted-U marginal effect of treatment, with peak at EXP* = 123 weeks where the predicted marginal effect on log trading volume is +29.0% (SE 5.0%, $p < .001$). For trading frequency, the interaction terms are statistically indistinguishable from zero, the airdrop's effect on frequency is uniform across the experience distribution.

Table 4. Experience-moderated heterogeneous treatment effects.

Period	Outcome	TREAT	TREAT × EXP	TREAT × EXP²
Overall	Trading Frequency	+.0194***	+.000177	−1.05e-06
Overall	Trading Volume	−.0593**	+.002929***	−1.06e-05***
Activation Yr	Trading Frequency	+.0279***	−.000026	+5.21e-07
Activation Yr	Trading Volume	−.1580***	+.007291***	−2.96e-05***
Years 2–3	Trading Frequency	−.0003	+.000124	−5.67e-07
Years 2–3	Trading Volume	−.0024	+.000099	−5.99e-08

*OLS estimates of equation (2), cluster-robust standard errors at the wallet level. Wallet-level controls and 165-week fixed effects included; full coefficients with all controls in Table 5. *** p < .001, ** p < .01, * p < .05. TREAT × EXP² in scientific notation.*

Table 5 reports the full coefficient set for equation (2) with all wallet-level controls. The control coefficients behave intuitively. log(TXN), the wallet's contemporaneous Ethereum transaction count, dominates the explanatory power: a one-log-point higher count of Ethereum transactions is associated with +.40 log-points more trading frequency and +.91 log-points more trading volume, reflecting strong within-week complementarity of on-chain activity. log(BAL) is a small but precisely estimated positive predictor of both outcomes. Liquidity providers execute substantially more swaps but at smaller dollar values per swap, consistent with inventory-rebalancing behavior. Pre-Sushi users are systematically less active on Uniswap (log frequency coefficient −.07, p < .001), reflecting cross-venue substitution that the controls partial out.

Table 5. Full coefficient table for the experience-moderated specification.

Variable	Year 1: FREQ	Year 1: VOL	Overall: FREQ	Overall: VOL
PROMO (eligibility)	+.0279*** (.0065)	−.1580*** (.0360)	+.0194*** (.0053)	−.0593** (.0225)
EXP (Ethereum tenure)	−.00227*** (1.83e-04)	−.01299*** (8.16e-04)	−.00185*** (1.47e-04)	−.00902*** (6.42e-04)
EXP²	+8.08e-06*** (1.05e-06)	+4.58e-05*** (4.61e-06)	+5.67e-06*** (8.20e-07)	+2.60e-05*** (3.66e-06)
PROMO × EXP	−2.63e-05 (3.21e-04)	+.00729*** (1.50e-03)	+1.77e-04 (1.65e-04)	+.00293*** (5.85e-04)
PROMO × EXP²	+5.21e-07 (1.79e-06)	−2.96e-05*** (7.89e-06)	−1.05e-06 (8.54e-07)	−1.06e-05*** (3.22e-06)

log(BAL) wallet balance	+.0162*** (.001)	+.0691*** (.005)	+.0194*** (.0006)	+.0759*** (.003)
log(TXN) Ethereum txn count	+.397*** (.003)	+.913*** (.012)	+.401*** (.002)	+.945*** (.008)
log(RET) weekly return	−.0117*** (.001)	−.0463*** (.004)	−.0140*** (.001)	−.0618*** (.003)
GAS gas spending	+1.4e-08 (3.7e-08)	+5.7e-08 (1.6e-07)	+3.9e-08 (2.8e-08)	+1.4e-07 (1.2e-07)
GOV ever voted (0/1)	−.0728* (.034)	−.345** (.124)	−.103*** (.029)	−.498*** (.117)
LP ever liquidity provider (0/1)	+.0421*** (.008)	−.077** (.029)	+.0270*** (.005)	−.060*** (.020)
sushi_pre pre-Sushi user (0/1)	−.0696*** (.006)	−.354*** (.022)	−.0670*** (.004)	−.355*** (.016)
Week fixed effects	Yes	Yes	Yes	Yes
N	282,565	282,565	478,168	478,168
Wallets	26,148	26,148	26,150	26,150
R^2	.452	.262	.442	.308

*OLS estimates of equation (2). *** p < .001, ** p < .01, * p < .05. Standard errors clustered at the wallet level in parentheses. Coefficients on EXP², PROMO × EXP², and GAS in scientific notation.*

Table 6 translates the activation-year volume interaction into marginal effects at five percentiles of the prior experience distribution. Among newly created wallets (5th percentile, EXP = 0 weeks), the airdrop reduces weekly trading volume by 15.8% ($p < .001$). At the 25th percentile (EXP = 4 weeks), the contraction remains substantial at 12.9% ($p < .001$). At the median (EXP = 31 weeks), the effect is null. At the 75th percentile (EXP = 77 weeks), the effect rises sharply to +22.8% ($p < .001$). At the inverted-U peak (EXP = 123 weeks), the effect reaches +29.0% ($p < .001$; 95% CI +19.3%, +38.8%). At the 95th percentile (EXP = 192 weeks), the effect remains positive at +14.9% but loses statistical significance.

Table 6. Marginal effect of airdrop eligibility on activation-year trading volume, by experience percentile.

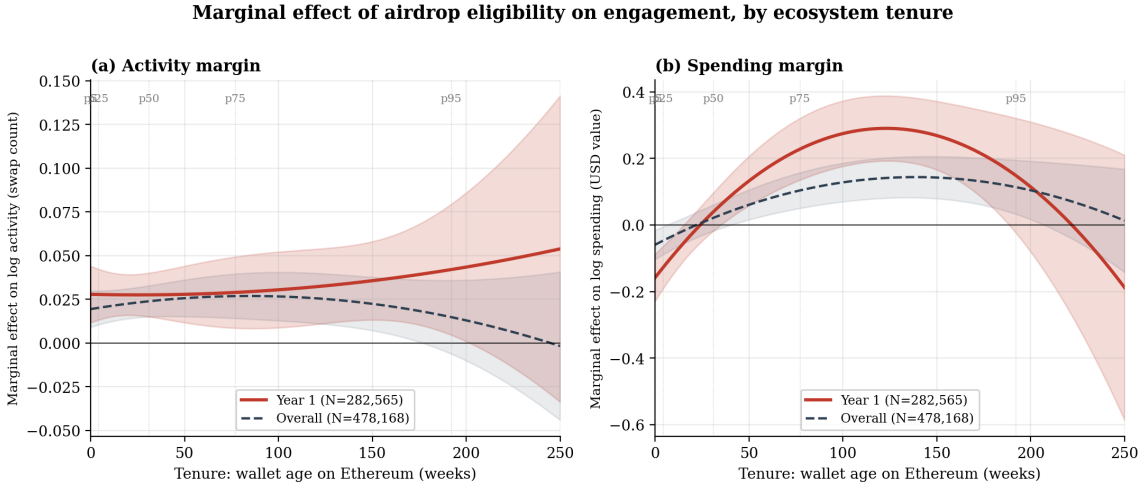
Tenure	**EXP (weeks)**	**Marginal Effect**	**SE**	**95% CI**
p5 (newly created)	0	−.158***	.036	[−.229, −.087]
p25 (novice)	4	−.129***	.033	[−.193, −.065]
p50 (median)	31	+.040	.029	[−.018, +.097]
p75 (mature)	77	+.228***	.047	[+.135, +.320]
Inverted-U peak	123	+.290***	.050	[+.193, +.388]

p95 (veteran)	192	+.149	.088	[−.023, +.321]

*Estimates from equation (2). Marginal effect = $\beta_1 + \rho_1 \cdot EXP + \rho_2 \cdot EXP^2$. Delta-method standard errors via cluster-robust variance estimator. *** $p < .001$.*

Figure 5 plots the full marginal-effect curve for trading volume and frequency with 95% confidence bands. The effect on trading frequency is essentially flat across the same experience range.

Figure 5. Marginal effect of airdrop eligibility on activation-year trading volume, by prior ecosystem experience.



Panel (a): trading frequency (log of weekly swap count). Panel (b): trading volume (log of weekly USD value). Solid (red) curves: Year 1. Dashed (gray) curves: full sample. Shaded regions: 95% confidence bands using cluster-robust standard errors at the wallet level. Vertical dotted lines: 5th, 25th, 50th, 75th, and 95th percentiles of EXP. Horizontal reference at zero. The trading-frequency response is essentially flat across tenure; the trading-volume response in Year 1 traces an inverted-U with peak at EXP ≈ 123 weeks.

For the headline activation-year volume interaction, finite-sample permutation tests (Cattaneo, Titiunik, and Vázquez-Bare 2017; 500 permutations) yield exact p-values below 1/501 ≈ .002 for the TREAT main effect, TREAT × EXP, and TREAT × EXP². The asymptotic significance levels are not artifacts of cluster-robust-variance-estimator assumptions.

4.4 Visibility-moderated response on the trading-frequency margin

While prior ecosystem experience moderates the trading-volume response, a separate question is whether platform visibility moderates the trading-frequency response. The two heterogeneity

dimensions, user-level experience and time-varying visibility, are conceptually distinct: experience captures user state, while visibility captures the public salience of the platform at a given moment. The Barber-Odean attention-driven-trading framework would predict that visibility should matter on the frequency margin in particular, because attention shocks typically draw retail participants into trading at all without necessarily changing the dollar magnitude of their trades.

To test this, I add the interaction PROMO × BUZZ to equation (2), where BUZZ is the weekly Google Trends search index for "Uniswap." Table 7 reports the visibility-moderated interaction coefficients alongside the tenure interactions for comparison.

Table 7. Visibility-moderated heterogeneous treatment effect.

Period	Outcome	PROMO × BUZZ	PROMO × EXP	PROMO × EXP²
Overall	Trading Frequency	−.000440*** (1.78e-04)	+.000259	−1.39e-06
Overall	Trading Volume	+.000114 (8.74e-04)	+.002908***	−1.05e-05***
Activation Yr	Trading Frequency	−.000606** (2.00e-04)	+.000119	−1.68e-07
Activation Yr	Trading Volume	+.000164 (9.93e-04)	+.007252***	−2.95e-05***
Years 2–3	Trading Frequency	−.000203 (2.82e-04)	+.000162	−7.05e-07
Years 2–3	Trading Volume	+.000399 (1.47e-03)	+.000026	+2.11e-07

*OLS estimates with cluster-robust standard errors at the wallet level. Tenure interaction coefficients reproduced for comparison. *** p < .001, ** p < .01, * p < .05.*

The two heterogeneity dimensions sort cleanly across the two outcome margins. In the activation year, PROMO × BUZZ on trading frequency is −.00061 (p = .002): the recipient-versus-control trading-frequency gap narrows as visibility rises. The interaction is statistically null on trading volume (p > .87 in the activation year). Symmetrically, the tenure interaction on volume remains highly significant in the inverted-U pattern, while the tenure interaction on frequency remains null. Tenure moderates volume; visibility moderates frequency. Including the visibility term does

not absorb any of the volume-margin heterogeneity, the tenure interaction coefficients are essentially identical to those in equation (2). Table 8 translates the visibility interaction into marginal effects on activation-year trading frequency at five percentiles of BUZZ, evaluated at sample-mean prior ecosystem experience.

Table 8. Marginal effect of airdrop eligibility on activation-year trading frequency, by visibility (BUZZ) percentile.

Visibility (BUZZ)	BUZZ index	Marginal Effect	SE	95% CI
p10 (low visibility)	8	+.0377***	.0088	[+.0206, +.0549]
p25	9	+.0371***	.0087	[+.0201, +.0541]
p50 (median)	14	+.0341***	.0083	[+.0179, +.0503]
p75	25	+.0274***	.0078	[+.0122, +.0427]
p90 (high visibility)	36	+.0208**	.0079	[+.0053, +.0363]

*Evaluated at sample-mean prior ecosystem experience (EXP = 45.7 weeks). Delta-method standard errors. *** $p < .001$, ** $p < .01$.*

The marginal effect of treatment on activation-year trading frequency declines monotonically from +3.77% at low visibility (10th BUZZ percentile) to +2.08% at high visibility (90th percentile), a 45% relative attenuation, with statistical significance at every percentile. The result is consistent with attention-driven spillovers from a publicly visible promotion: high-visibility weeks draw non-recipients onto the platform, partially closing the gap that the airdrop creates between eligible and near-miss wallets. The narrowing operates only on the trading-frequency margin, non-recipients trade more often during high-attention weeks but do not deploy more dollars per trade.

This visibility result is consistent with the Barber-Odean attention-driven-trading framework operating on the frequency margin. The framework correctly predicts that public salience matters for whether retail participants act. But the framework does not predict the volume-margin pattern, where experience rather than attention is the moderator and the

relationship is non-monotonic. The two findings together suggest that attention-driven trading and experience-conditional deployment operate on different engagement margins, and that a unified account of retail response to a public wealth shock must accommodate both.

4.5 Dynamic fade

The heterogeneity in trading-volume response is concentrated in the activation year and dissipates fully by Year 2. The Years 2–3 estimates of TREAT × EXP and TREAT × EXP² are both statistically indistinguishable from zero (Table 4, last two rows), as is the Years 2–3 TREAT main effect on both outcomes. This dynamic pattern is itself a substantive finding rather than a power problem. It rules out at least two alternatives the literature would naturally consider.

First, a learning-based account, in which the airdrop teaches inexperienced recipients to deepen their trading over time, would predict that the trading-volume response among low-experience wallets should grow over time, not contract immediately and then stay at zero. Inexperienced wallets exhibit a 16% contraction in the activation year and no significant deviation thereafter, which is the opposite of what a learning mechanism would generate. Second, a loyalty- or commitment-based account — in which the airdrop creates platform-specific attachment that translates into sustained higher engagement — would predict that positive treatment effects should persist into Years 2–3. The clean fade observed in both outcomes contradicts this prediction. The dynamic pattern is most consistent with the saturation interpretation introduced in Section 1: the airdrop releases existing capability rather than building it, and once the available deployment capacity is exhausted, the marginal effect of additional liquidity converges to zero. The saturation reading is the most parsimonious explanation consistent with the observed dynamic pattern; the design does not formally adjudicate between it

and other potential explanations of the dynamic fade. What the dynamic pattern does rule out, with high confidence, are learning- and commitment-based accounts.

4.6 Alternative operationalizations of prior experience

The headline specification operationalizes prior experience as wallet age on Ethereum (EXP). Prior participation is multi-dimensional, however, and the moderation result should not depend on a single operational choice. Table 9 reports estimates of equation (2) under three alternative measures: log pre-period transaction count, log pre-period maximum balance, and a composite z-score index combining transaction count, capital scale, and a pre-cutoff cross-venue indicator.

Table 9. Alternative operationalizations of prior ecosystem experience.

Percentile / Term	EXP (baseline)	log(1+pre_txn)	log(1+pre_maxbalk)	Composite index
p5	−.172***	−.484***	−.221***	−.499***
p25	−.144***	−.104***	−.146***	−.165***
p50	+.019	+.114***	−.010	+.094**
p75	+.201***	+.262***	+.147***	+.301***
p95	+.127	+.304***	+.264***	+.346***
Inverted-U peak	+.262***	+.312***	+.266***	+.379***
Underlying interaction terms				
PROMO × measure (linear)	+.0070***	+.512***	+.262***	+.186***
PROMO × measure² (quadratic)	−2.86e-05***	−.051***	−.034**	−.031***
Correlation with EXP				
Pearson r (wallet-level)	1.000	.142	.150	.160

**** p < .001, ** p < .01, * p < .05. All specifications estimated on the activation-year sample (282,565 wallet-week observations, 26,148 wallets). Wallet-level controls and 165 week fixed effects included. Each column corresponds to a separate regression. The composite index uses sushi_pre_dum as a component, so this dummy is dropped from the controls in the composite specification.*

The inverted-U pattern replicates across all four operationalizations of prior experience. The peak marginal effect on trading volume ranges from +26.2% under the EXP baseline to +37.9%

under the composite index, with the activity-based measures showing somewhat sharper effects at the low-experience extreme: novice wallets contract per-trade dollar value by approximately 17% under EXP but by 48–50% under the activity-based measures.

The four measures are weakly correlated at the wallet level (pairwise Pearson r between EXP and the others ranges from .14 to .16). The replication of the inverted-U across measures that are largely orthogonal is independent evidence rather than four restatements of the same variable.

4.7 Additional engagement modes and methodological robustness

4.7.1 Governance voting and liquidity provision

Two additional engagement modes are observable in the data: governance voting and liquidity provision. Both are absorbing states defined at the wallet level. Table 10 reports wallet-level cross-sectional logit estimates for each.

Table 10. Governance voting and liquidity provision as alternative engagement modes.

Variable	Governance voting (logit)	Liquidity provision (logit)
Outcome rate (unconditional)	*.141% (n=37)*	*1.818% (n=2,829)*
PROMO	+.5285 (.343)	−.2647*** (.046)
EXP (Ethereum tenure)	+.0023 (.014)	−.0083*** (.0017)
EXP²	+1.32e-05 (3.5e-05)	+2.13e-05*** (5.3e-06)
log(lifetime swaps)	+.6138*** (.176)	+.8655*** (.020)
log(lifetime trading volume)	−.1277† (.077)	−.0933*** (.0090)
sushi_pre_dum	+.0935 (.420)	+.0419 (.054)
N	26,150	26,150

*Wallet-level cross-sectional logit. *** p < .001, ** p < .01, * p < .05, † p < .1. Pre-period controls (pre_txn, pre_maxbalk) included but suppressed.*

Two findings are notable. First, lifetime swap activity is strongly positively associated with both governance and LP participation (log(lifetime swaps) coefficient +.61 for governance, +.87 for

LP, both $p \leq .001$): higher-frequency traders are also the wallets that vote and provide liquidity. Second, lifetime trading volume is negatively associated with LP participation: wallets that LP execute many small swaps for inventory rebalancing rather than fewer large-volume trades. The airdrop indicator itself is statistically null on governance ($\beta = +.53$, $p = .12$), reflecting low power given the rarity of voting (37 voters in the sample), and modestly negative on LP ($\beta = -.26$, $p < .001$), reflecting alternative UNI-acquisition channels available to control wallets via LP rewards.

4.7.2 IPW-weighted estimation comparison

The local-randomization framework relies on as-if-random assignment within the ±3-day window. As a methodological robustness check, I re-estimate equation (2) using inverse-probability-weighted regression adjustment (IPWRA; Słoczyński, Uysal, and Wooldridge 2022). The propensity model is a wallet-level logit of PROMO on the two pre-period covariates pre_txn and pre_maxbalk. The propensity model has essentially no predictive power: McFadden's pseudo-R^2 is .0002, indicating that pre-period covariates do not separate eligible from near-miss control wallets. The IPW weights are correspondingly tight (mean 2.00, SD .24), and the IPW estimates closely track OLS, with the headline interaction terms differing by approximately 1–2% (Table 11).

Table 11. OLS versus IPW-weighted estimates of the activation-year volume tenure interaction.

Estimator	**PROMO × EXP**	**PROMO × EXP²**
OLS (Table 4)	+.007291***	−2.96e-05***
IPW-weighted	+.007391***	−3.02e-05***

*** *p < .001. The headline estimate is not driven by the choice of weighting scheme. The propensity model has pseudo-R^2 = .0002, itself informative as a balance test.*

5. Discussion

The September 2020 Uniswap airdrop offers a setting in which a publicly visible exogenous wealth shock can be linked to user-level retail trading behavior at transaction-level granularity. Using local-randomization regression discontinuity around the retroactive eligibility cutoff, the paper documents that retail response varies structurally across the two engagement margins. Trading frequency rises by approximately 3% in the activation year uniformly across the prior-experience distribution and is moderated by platform visibility, narrowing as public attention rises. Trading volume traces an inverted-U in prior experience: inexperienced wallets contract per-trade dollar value by 16%, intermediate-experience wallets (≈2.4 years of prior Ethereum activity) expand it by 29%, and the most experienced wallets show no significant response. Both effects dissipate fully by Year 2.

5.1 The two-margin pattern and the standard frameworks

The two-margin pattern engages the dominant frameworks in retail-investor heterogeneity in a structured way. On the trading-frequency margin, the attention-driven-trading framework (Barber and Odean 2008; Barber, Huang, Odean, and Schwarz 2022) finds support: visibility moderates the frequency response in the direction the framework predicts, with high-visibility periods narrowing the recipient-versus-control gap by 45% as BUZZ moves from the 10th to the 90th percentile. On the trading-volume margin, however, neither this framework nor its linear-sophistication counterpart predicts what the data show. A linear-sophistication account in the Barber and Odean (2000) lineage would predict that more experienced retail participants deploy windfalls more effectively, a monotone-positive experience-response relationship on the volume margin. The inverted-U observed here, with novices contracting and veterans showing no incremental response, is not consistent with this prediction. The household-finance windfall-response literature (Imbens, Rubin, and Sacerdote 2001; Briggs, Cesarini, Lindqvist, and Östling

2021) implies a comparable monotone pattern in which recipients with more prior engagement respond more strongly, which the volume-margin inverted-U does not exhibit either.

The novice-contraction finding is the most surprising element of the pattern. The prevailing characterization of inexperienced retail participants in cryptocurrency markets has emphasized attention-driven, speculation-prone behavior. The evidence here suggests this characterization is incomplete on the volume margin. When inexperienced retail participants receive an unanticipated $1,200 windfall, they do not expand per-trade dollar deployment; they contract it, engaging in small-stake exploratory trading rather than scaled-up speculation. They are not immune to attention: the visibility analysis shows that public salience does draw inexperienced participants into more frequent trading. But attention and experience operate on different engagement margins. Attention moves whether retail participants act; experience moves how much value they deploy when they do.

The two-margin asymmetry is itself a structural finding that the prior literature has not made explicit. A unified account of retail response to a public wealth shock in a digital financial market would need to accommodate both: why attention shocks affect frequency uniformly across the experience distribution, and why experience moderates volume non-monotonically. The present paper offers the empirical finding that motivates this distinction.

5.2 The saturation interpretation

Saturation is the most parsimonious explanation for the inverted-U on the volume margin. Under this interpretation, converting an exogenous windfall into scaled financial-market participation requires accumulated ecosystem capability, operational fluency with the protocol's gas dynamics, slippage tolerances, competing venues, and deployment strategies, that inexperienced participants have not yet developed. The most experienced participants have accumulated this

capability and have already deployed their participation to a level that limits further marginal response. Intermediate-experience participants are uniquely positioned to convert the shock into scaled action: they have the capability to deploy and the slack to do so.

Two features of the data are consistent with this reading. First, the dynamic fade by Year 2 is what the interpretation predicts. If the shock releases existing deployment capability rather than building new capability, then once the available capacity is deployed the marginal effect of the additional liquidity should exhaust. Second, the alternative mechanisms most naturally considered, learning and commitment, generate dynamic predictions that are inconsistent with the observed fade. A learning account would predict the response among low-experience wallets to grow over time, not contract immediately and remain null. A commitment account would predict persistent positive effects rather than the clean fade documented.

Saturation is presented as the most parsimonious account consistent with both the cross-sectional inverted-U and the dynamic fade, not as the only logically possible account. Three scope caveats apply. First, ecosystem experience is observed but the capability that saturation invokes is not directly measured; wallet age and pre-period transaction count are familiarity proxies, not capability measures. Second, the design does not rule out alternative mechanisms beyond learning and commitment. Behavioral accounts emphasizing loss aversion at low experience, or portfolio-rebalancing optimization at high experience, are consistent with portions of the pattern and remain open. Third, the saturation interpretation is descriptive rather than a formal structural model. Adjudicating among the remaining candidate mechanisms requires a setting that varies capability orthogonally to experience, which the present design does not.

5.3 The digitization of financial markets and retail behavior

The empirical setting is itself a leading example of the digitization of financial markets. Decentralized exchanges, built on public-blockchain infrastructure and executing transactions through smart contracts, generate user-level transaction histories at a granularity that conventional intermediated financial markets cannot match. The complete pre- and post-shock behavioral history of every wallet, recipients and near-miss control wallets alike, is observable without survey attrition, partial-account sampling, or aggregation. The two-margin distinction documented here is detectable in this setting because the data structure separates the frequency and volume dimensions cleanly at the transaction level; lower-granularity data structures that record stock-market participation as a binary outcome or at annual frequency would not surface the asymmetry.

The digitization angle has implications for the empirical study of retail behavior more broadly. Public-ledger architecture enables a class of questions about individual recipients of publicly visible shocks, how they alter their trading decisions, week by week and trade by trade, over a multi-year window, that the data infrastructure of conventional markets has not made tractable. The methodological approach used here, local-randomization regression discontinuity on a retroactive cutoff paired with on-chain longitudinal observability, is portable to other token-distribution events in decentralized finance with similar structural properties. Retail entry into digital financial markets has been particularly rapid in emerging-market economies, including India and South Asia, where mobile-first financial-platform adoption has lowered access frictions and where cryptocurrency adoption has outpaced advanced economies. Whether the experience-conditional response on the volume margin documented here generalizes to other digital financial-market settings, and to emerging-market retail populations specifically, is an

open question that the data structures of decentralized exchanges are increasingly equipped to answer.

5.4 Scope conditions and limitations

Four scope conditions warrant explicit statement. First, the empirical setting is a single, large, publicly visible wealth shock on a decentralized exchange. The non-monotonic experience-response pattern may not generalize cleanly to smaller, privately-targeted, more frequent, or differently-structured promotional events. Second, the retroactive structure of the Uniswap airdrop is central to the design's identification properties and is unusual among token-distribution events in cryptocurrency markets, where eligibility rules are typically announced ex ante and participants can position strategically to qualify. Third, the experience measure is the age of the recipient's wallet on the Ethereum blockchain, a behaviorally faithful but imperfect proxy for the capability that the saturation interpretation invokes. Transaction-stock and composite measures yield qualitatively similar inverted-U patterns (Table 9), but a direct measure of capability would strengthen the mechanism interpretation. Fourth, the heterogeneous treatment effect dissipates fully by Year 2. The findings describe the activation-window dynamics of retail response to a publicly visible wealth shock, not a permanent reorganization of trading behavior.

The non-monotonic pattern on the volume margin is, to my knowledge, the first user-level evidence of inverted-U experience-response in retail windfall behavior. Together with the visibility-moderated frequency response, the finding constrains the set of plausible mechanisms governing retail behavior in digital financial markets and motivates a research line on how prior experience and platform visibility jointly shape retail response to publicly visible shocks in these settings.

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