Meme-Driven Gamification*

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Abstract

We study whether a meme-asset windfall alters investor behavior by exploiting the January 2023 Bonk meme-coin airdrop on the Solana blockchain. Using wallet-level data, we compare wallets that received the airdrop ("treated") to observably similar non-recipients ("controls"). Following the airdrop, treated wallets increase trading volume by roughly 80% and shift toward riskier tokens, with effects stronger among investors with no prior meme-coin experience. Treated wallets subsequently earn lower returns. At the token level, the airdrop triggers greater price and volume run-ups among tokens that are riskier or more correlated with meme coins. Overall, our findings suggest that exogenous meme exposure raises trading intensity and risk-taking and propagates shocks beyond meme assets, consistent with a shift toward gamified trading. Meme episodes, even when originating from a small number of assets, can therefore have broader investor-behavioral and asset-pricing consequences through spillovers.

Keywords: meme; gamification; FinTech; blockchain; investor behavior

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1 Introduction

Meme episodes, exemplified by the GameStop short squeeze of early 2021, have generated significant public and academic interest. Significant concerns have been raised by regulatory bodies worldwide regarding the implications of meme stock trading on investor protection and market stability (Gensler, 2021; U.S. Securities and Exchange Commission, 2021; European Securities and Markets Authority, 2022). Although the meme phenomenon has generated significant public and academic interest, rigorous empirical analysis remains scarce, perhaps due to the lack of a compelling empirical setting and difficulties in accessing data. This paper fills this gap by studying the Bonk airdrop on the Solana blockchain.

Bonk is a meme coin launched in December 2022. Modeled after Dogecoin and Shiba Inu, Bonk was introduced to promote community engagement and meme-driven branding within Solana. A key feature of its launch was a large-scale airdrop, conducted between December 25, 2022 and January 4, 2023, in which about 50% of the 100 trillion Bonk token supply was distributed to investors unaffiliated with the Bonk project. Crucially, the Bonk airdrop was non-opt-in, meaning that wallets did not need to register or act to receive tokens, and among those satisfying the pre-defined, on-chain eligibility criteria, only a subset actually received Bonk tokens. This design addresses the selection biases inherent in opt-in airdrops, where eligibility hinges on voluntary engagement in certain activities that are not recorded on the blockchain. As such, the Bonk airdrop offers a rare natural experiment to study the causal effects of meme windfalls on investor behavior.

Leveraging blockchain data, we compare wallets that received the airdrop ("treated") to observably similar non-recipients ("controls") in standard difference-in-differences tests. To obtain controls, we match each treated wallet on a set of wallet-level characteristics, including wallet age, balance, trading volume, and prior meme coin exposure, all measured using pre-event data. Importantly, for each treated wallet we restrict the pool of potential controls to wallets that satisfy the same on-chain eligibility criterion but did not receive Bonk tokens, and then select the closest match in terms of these characteristics. We use wallet-by-month panel data covering six months before and after the airdrop. In our empirical analysis, we exclude the Bonk tokens themselves to avoid mechanical biases.

The baseline results indicate a significant increase in trading activity among treated wallets. Trading volume rose by approximately 80% relative to controls following the airdrop, a finding robust to various sample restrictions, such as excluding small wallets and controlling for potential wealth effects. Both higher trading frequency and bigger trade size contribute to the increased trading volume. Treated wallets also shift to buying more volatile tokens, suggesting that exposure to meme coins not only amplifies trading but also induces riskier investment behavior. Dynamic analyses show that the effects emerge immediately after the airdrop, peak within one month, and gradually decline in the following months, remaining highly economically and statistically significant at the end of our sample period. Pre-event trends run in parallel, supporting the identification assumption.

We find that the treatment effect is largely driven by whether an investor receives the airdrop or not, and the amount of the airdrop does not seem to matter substantially. For example, within the subsample of treatment wallets where the airdropped token value accounts for less than 5% of the total portfolio balance, we find treatment effects that are very similar to those observed in the full sample.

We further explore heterogeneity in responses based on prior experience with meme or speculative assets. Investors who previously traded meme tokens or held some art NFTs, which are often regarded as speculative assets, show substantially smaller increases or no significant changes in trading volume and volatility after the airdrop compared with those without such exposure. These findings suggest that prior experiences with memes or NFTs dampen the marginal effects of the meme airdrop, providing further support that the baseline effects we document are driven by exposure to memes.

Beyond the main findings presented above, we examine three additional dimensions of investor response to the Bonk airdrop. First, consistent with the endowment effect, treated investors who received tokens at no cost were significantly more likely to retain Bonk in their portfolios, both in terms of token balances and holding probability, relative to control investors. Second, although the increases in trading volume and volatility slightly attenuate somewhat after investors liquidate their airdropped Bonk tokens, the effects remain economically meaningful, suggesting lasting changes in behavior. Third, to rule out the possibility that our results merely capture high experienced return (e.g., Imas, 2016), we measure investors' experienced Bonk returns and re-estimate our tests separately for positive and non-positive returns. We find that the effects remain strong even when experienced Bonk returns are non-positive, suggesting that the response is driven primarily by meme exposure rather than by high returns. Finally, despite this heightened engagement, treated wallets underperform across multiple return measures, implying that the additional trading induced by the airdrop did not lead to improved performance.

We then examine the impact of meme exposure on financial markets. Our cross-exchange volume analysis reveals that decentralized exchanges (DEXs) on Solana, where the Bonk airdrop occurred, experienced a significant and sustained increase in trading volume relative to other exchanges shortly after the airdrop announcement, even excluding Bonk-related trades. This suggests that meme coin airdrops generate spillover effects, boosting the overall trading activity on affected platforms.

Next, we conduct a token-level analysis by partitioning "Solana-focused tokens," whose trading activity is concentrated on the Solana blockchain rather than other trading venues, according to volatility and their correlation with Dogecoin and a broader meme coin index to capture heterogeneous market responses. High-volatility tokens exhibit substantially greater price appreciation and trading volume increases during and immediately following the airdrop compared to low-volatility tokens. Similarly, tokens more strongly correlated with Dogecoin and the meme index exhibit significantly larger price and volume increases, especially during the airdrop window.

Cross-sectional regressions quantify these effects. A one–standard deviation increase in volatility from its mean is associated with an 11.4% increase in price growth, while a one–standard deviation increase in correlation with Dogecoin and the meme index is associated with price increases of 8.22% and 8.15%, respectively, over the window from one day before the airdrop start date to one day after the airdrop end date. These results are robust to using a wider window from seven days before the announcement through seven days after the airdrop end date. Using days −7 to −1 relative to the announcement as the pre-period and the interval from the airdrop start date through the airdrop end date plus 14 or 28 days as the post-period, we find that a one–standard deviation increase in volatility from its mean is associated with a 21.9%–24.8% increase in post-period trading volume, while a one–standard deviation increase in correlation with Dogecoin is associated with volume increases of 16.3%–18.6%. Collectively, the evidence indicates that meme-coin exposure amplifies both prices and trading activity, particularly for higher-risk (high-volatility) and more meme-linked tokens, highlighting the market impact of meme-driven participation.

Beyond the Bonk airdrop analysis on Solana DEXs, we examine the impact of meme coin listings on centralized exchanges (CEXs) as complementary evidence. Using a difference-in-differences approach on panel data from 2017 to 2025, we compare exchanges that listed major meme coins (e.g., Dogecoin, Shiba Inu, Pepe) with those that did not. Our results indicate that meme coin listings are associated with significant increases in trading volume, approximately 17.5% higher at the exchange level and 7.7% higher for individual tokens on treated exchanges relative to controls. This surge in volume begins immediately, with a sharp increase observed in the first week following the listing. The effect is especially pronounced for tokens that exhibit strong correlations with established meme coins. While we acknowledge the endogeneity of listing decisions and the resulting limitations for causal inference, the immediate post-listing response along with the consistency of these patterns with the plausibly exogenous effects of the Bonk airdrop on DEXs offers supportive evidence that meme coin exposure stimulates investor trading activity.

To the best of our knowledge, this is the first study to examine the causal effect of the meme experience on trading gamification, despite the growing interest in the meme phenomenon (e.g., Bali, Hirshleifer, Peng, and Tang, 2021; Bradley, Hanousek Jr, Jame, and Xiao, 2024). Current discussions have primarily focused on the direct effects of meme trading on meme assets, often overlooking potential spillover effects on other assets. Our findings show that meme episodes, even when originating from a small number of assets, can have broader investor-behavioral and asset-pricing consequences through spillovers. Our asset pricing analyses support Asness (2024)'s conjecture that the stock markets have become less efficient, likely due to the rise of gamified trading, social media coordination, and 24/7 retail trading environments.

The most closely related work is Pedersen (2022), who studies the GameStop episode and provides a theoretical framework of how social interaction among different types of investors can drive extreme trading volume and price dislocations. Using a model of attention-driven trading, he shows that meme dynamics can cause price-volume spirals and high turnover, even in the absence of significant changes to fundamental value. While Pedersen's analysis primarily focuses on the direct effects on meme assets, our study complements his work by documenting sizable spillover effects to other assets—a dimension that, to our knowledge, has not yet been theoretically modeled. We view our empirical evidence as a useful starting point for developing theoretical frameworks that incorporate meme-driven trading and its broader impact on portfolio choice and asset prices.

We also contribute to the nascent literature on the growing role of gamification in financial markets, particularly in the design of trading platforms, which is still in its infancy. Barber, Huang, Odean, and Schwarz (2022) show that Robinhood's interface design increases attention-induced risk-taking among retail investors. Chapkovski, Khapko, and Zoican (2024) provide experimental evidence that gamification elements such as celebratory animations (e.g., confetti) and achievement badges significantly increase trading activity and user engagement. We contribute to this literature by shifting the focus from platform-level design to asset-level gamification. Specifically, we argue that meme assets themselves function as gamified financial products, leveraging humor, internet culture, and playful visual elements to attract attention and engagement.

This form of asset-level gamification is becoming increasingly important with the rise of meme stocks and the recent introduction of meme-coin exchange-traded products (ETPs) in traditional trading venues.[1] Our evidence shows that such gamification exerts significant and persistent spillover effects on investor behavior and financial markets. Introducing meme coins into

[1] Examples include, among others, Dogecoin-based ETFs and ETPs. For instance, in September 2025, REX--Osprey launched the first U.S.-listed spot Dogecoin ETF, marketed as the first meme coin ETF in the U.S. market (see https://www.rexshares.com/rex-osprey-launches-first-u-s-listed-etfs-to-offer-exposure-to-spot-dogecoin-and-spot-xrp/)

traditional venues through ETPs is therefore particularly concerning, as it may transmit these spillovers to a broader set of asset classes. Because ETP listing is subject to regulatory oversight, our results have implications for the evaluation and approval of meme-coin ETPs from an investor-protection perspective.

2 Institutional Background: The Bonk Airdrop

Our primary analysis focuses on the Bonk airdrop. This section provides the institutional background about it. Bonk (BONK) is a meme coin announced on the Solana blockchain on December 8, 2022, by a group identifying as long-standing participants in the Solana ecosystem. Modeled after earlier meme coins such as Dogecoin (DOGE) and Shiba Inu (SHIB), Bonk was designed to promote community engagement and encourage decentralized activity within the Solana network. Figure 1 presents the cover page of the Bonk whitepaper, which features a stylized Shiba Inu dog mascot—an emblematic design choice reflecting the meme-driven branding commonly employed by such tokens.[2] The coin attracted substantial attention from the cryptocurrency community, experiencing rapid growth and ranking as the fifth-largest meme coin with a market capitalization exceeding $2 billion as of August 2025.

A central feature of its launch was a large-scale airdrop, conducted between December 25, 2022, and January 4, 2023, which distributed approximately 50% of its 100 trillion token supply to wallets unaffiliated with the Bonk project. As detailed in Table 1, unaffiliated recipients include categories such as “40 Solana NFT Projects,” “Solana Market Participants,” and “Solana Artists and Collectors,” while affiliated recipients include “Solana Developers,” “Initial Liquidity,”

[2] In this study, we use the terms “token” and “coin” interchangeably, as the distinction between the two is often ambiguous in practice and not central to our analysis.

“Early Contributors,” “Marketing,” and “BonkDAO.” The remaining tokens were allocated to wallets associated with project contributors and affiliated entities.

The Bonk airdrop provides a compelling empirical setting to study the impact of meme coin exposure on investor behavior and asset prices. It is particularly well-suited for causal analysis due to its non-opt-in distribution mechanism. Unlike opt-in airdrops, commonly used in token distributions that require users to register or take specific actions, non-opt-in airdrops automatically allocate tokens to a subset of randomly selected wallets that meet the eligibility criteria without prior consent or intervention. This distinction is crucial for causal identification. Opt-in airdrops pose challenges such as endogenous selection bias, since the decision to participate is voluntary, and difficulties in identifying comparable non-recipient wallets, as registration and activity typically occur off-chain and are not recorded on the blockchain. Conversely, non-opt-in airdrops such as the Bonk airdrop mitigate these concerns by randomly allocating tokens to a subset of wallets among those that meet predefined, on-chain eligibility criteria. This approach enables credible comparisons between recipients and non-recipients within the same eligibility group, thereby addressing potential selection bias.

Moreover, using on-chain transaction data, we identify over 70,000 unique wallets that received Bonk tokens. The decentralized scale of the distribution, combined with the absence of any registration requirement, makes it implausible that the allocation was systematically manipulated or driven by insider control. Anecdotal evidence further supports the random and unexpected nature of the airdrop. For example, on January 17, 2023—roughly two weeks after the airdrop—a Reddit user (“that1prokid”) created a thread titled “This random coin popped up in my Phantom wallet?", accompanied by a screenshot of a Bonk balance, indicating surprise.[3] Similarly, another user ("laskoian") asked, "How did I just get 200M BONK deposited into my wallet on Christmas? Was it given to certain wallets for some reason?", further underscoring that recipients were largely unaware of both the airdrop and its eligibility criteria. Together, these features make the Bonk airdrop a unique setting for examining the impact of meme coin exposure.[4]

3 Data Sources

We collect transaction-level data from the Solana blockchain using two primary sources: Dune Analytics and Solscan API. We extract detailed decentralized exchange (DEX) trading data spanning the period from March 2021 to May 2023 from Dune Analytics. Our dataset encompasses transactions from six major Solana-based DEXs—Lifinity, GooseFX, Meteora, Phoenix, Raydium, and Orca (Whirlpool)—providing a comprehensive view of on-chain trading activity. For each transaction, we observe precise trade timestamps, trade sizes (denominated both in cryptocurrency units and in U.S. dollar terms), wallet addresses, trade direction (buy or sell), and transaction prices. We leverage this granular data to construct wallet-level measures of investor behavior and aggregate it to a daily frequency to examine the effects of meme exposure on token prices and trading volume.[5] In addition, we utilize the Solscan API to collect wallet-level data on NFT holdings and transaction histories for 40 NFT collections, which are used to determine eligibility for the airdrop.

For token-level analysis, in addition to the transaction-level data used to construct token prices and trading volume, we augment our dataset with information from CoinMarketCap to construct a

[3] https://www.reddit.com/r/solana/comments/10ey8x8/this_random_coin_popped_up_in_my_phantom_wallet/
[4] https://www.reddit.com/r/solana/comments/10062bp/bonk/
[5] Leveraging the blockchain's transparent ledger, we construct granular investor-level data to examine investor behaviors. This approach is similar to the methodology employed in Li, Shin, and Wang (2025).

broad cryptocurrency market index and to measure aggregate market-wide trading volume. These variables serve as benchmarks to control for common trends in market-wide price movements and trading activity.

Section A.1 of the Internet Appendix provides a detailed description of our methodology for constructing token-level measures such as prices and trading volume, as well as wallet-level variables such as balances, trading activity, and return volatility, using data collected from Dune Analytics. It also outlines our procedure for identifying investor NFT holdings based on wallet-level data obtained from the Solscan API. In addition, we describe the construction of a broad cryptocurrency market index and aggregated market trading volume using data sourced from CoinMarketCap.

4 Wallet-level Analysis

4.1 Methodology

We identify 70,869 unique wallets that received Bonk coins via an airdrop from the Bonk creator wallet. However, our wallet-level analysis focuses on how meme exposure influences investors' trading behavior. To ensure that we examine active traders, we restrict our sample to wallets that had traded on Solana-based decentralized exchanges (DEXs) prior to the airdrop distribution. Applying this criterion reduces the treatment sample to 31,261 wallets that both received the Bonk airdrop and exhibited prior DEX activity. We further narrow our treatment sample to wallets that executed at least one trade and had non-zero average balance during the pre-treatment period, defined as from June 1, 2022, to November 30, 2022. This additional restriction results in a final treatment sample of 23,586 wallets.

To conduct a matched-sample analysis, we pair each treatment wallet with a closely comparable control wallet that did not receive the airdrop. The matching is based on a set of pre-treatment wallet characteristics defined as follows: We first match exactly pairs of wallets based on pre-meme exposure, defined as whether they hold any meme tokens as of November 30, 2022, the period before the airdrop. Then we apply nearest neighbor matching using the variables defined in equation (1). Specifically, $X_{i,t}^{vol}$ denote the dollar trading volume of wallet *i* in month *t*; $X_{i,t}^{bal}$ represent the dollar-denominated token balance of wallet *i* in month *t*; X_i^{age} is wallet age defined as the number of days from the wallet's creation to November 30, 2022.

Based on these variables, we construct five key metrics used in the matching procedure:

$$M_{i,1} = \left(\frac{X_{i,-6}^{vol} + X_{i,-5}^{vol} + X_{i,-4}^{vol}}{3}\right), M_{i,2} = \left(\frac{X_{i,-3}^{vol} + X_{i,-2}^{vol} + X_{i,-1}^{vol}}{3}\right)$$

$$M_{i,3} = \left(\frac{X_{i,-6}^{bal} + X_{i,-5}^{bal} + X_{i,-4}^{bal}}{3}\right), M_{i,4} = \left(\frac{X_{i,-3}^{bal} + X_{i,-2}^{bal} + X_{i,-1}^{bal}}{3}\right), M_{i,5} = X_i^{age}. \tag{1}$$

Then, we define the distance between a treatment wallet *i* and a control wallet *j* as follows,

$$d_{i,j} = \frac{1}{5}\sum_{s}\left|\log(1 + M_{i,s}) - \log(1 + M_{j,s})\right|, \tag{2}$$

where we employ a standardized version of the log(1 + $M_{i,s}$). Specifically, we winsorize the variable at the 1st and 99th percentiles to mitigate the influence of outliers, and then standardize it by subtracting the sample mean and dividing by the sample standard deviation.

Among the three eligibility criteria for the Bonk airdrop, namely "40 Solana NFT Projects," "Solana Market Participants," and "Solana Artists and Collectors," our analysis utilizes only the first two. We exclude the "Solana Artists and Collectors" category, which is designed to allocate tokens to artists and collectors of 1-of-1 art based on a snapshot from the Collector.sh platform. Unfortunately, the Collector.sh dataset is no longer accessible, precluding our ability to reliably identify recipients under this criterion.

The "40 Solana NFT Projects" criterion specifies that eligible wallets must hold at least one NFT from any of the 40 predefined NFT collections prior to the airdrop. We identify 6,127 wallets out of the 23,586 treatment wallets that satisfy this condition. However, we note that not all eligible wallets received Bonk tokens. This outcome is consistent with standard airdrop practices, in which token distributions are typically randomized among qualifying wallets.[6]

For each treatment wallet that received Bonk tokens under this criterion, we construct a matched control group by identifying wallets that held NFTs from the same collections as those held by the treatment wallet. From this pool, we select control wallets that have the same pre-meme exposure, and choose the wallet with the minimum distance by using a matching procedure with replacement as defined in Equation (2).

The remaining 17,459 wallets in our treatment group are attributed to the "Solana Market Participants" criterion, which allocated 15.8% of the total supply to wallets engaged in decentralized finance (DeFi) activity on Solana. Since inclusion in our sample requires prior trading activity on DEXs, it is reasonable to assume that these wallets qualified under this criterion. While it is possible that some wallets interacted exclusively with non-DEX DeFi protocols (e.g., lending platforms) in their observed history, such behavior is unlikely to be widespread. In practice, many DeFi interactions involve token swaps that are routed through DEXs as part of

[6] For example, Augustin, Chen-Zhang, and Shin (2021) study airdrops by an information platform in which many decentralized finance (DeFi) users satisfy pre-specified eligibility criteria, but only a randomly selected subset of these eligible users actually receive the utility tokens. This randomized allocation among eligible users allows them to examine the impact of information display on yield-chasing behavior in DeFi.

broader transaction chains, making DEX activity a reliable proxy for participation in the DeFi ecosystem.

For each of these 17,459 treatment wallets, we construct a control group from wallets that are not holders of NFTs from any of the 40 NFT collections. We then match each treatment wallet to a control wallet based on pre-meme exposure and a nearest neighbor matching procedure with replacement, as defined in Equation (2).[7]

4.2 Summary Statistics

We present the distribution of airdropped Bonk tokens to 23,586 treatment wallets in Figure 2. Panel A shows the Bonk value, the dollar value of tokens measured on the airdrop date, while Panel B reports the Bonk fraction, defined as the ratio of Bonk value to the wallet balance as of November 30, 2022, the month prior to the airdrop. The most common Bonk values are ≤\$10, \$10-20, and \$70-80, with smaller proportions receiving intermediate or higher amounts. The distribution of Bonk fractions is U-shaped, with many recipients holding either nearly none (0-10%) or almost all (90-100%) of the airdropped tokens.

We then proceed to provide the summary statistics on our sample treatment and control wallets in Table 2. Panel A reports summary statistics for the sample wallets, with all characteristics measured prior to the Bonk airdrop. As of November 30, 2022, the end of the month preceding the airdrop, the average wallet age is approximately 0.624 years. To assess the typical size of investor holdings, we compute the average end-of-month balances over the six months preceding the airdrop. The mean balance is \$1,964.48, while the median is \$243.60, which is broadly consistent

[7] Approximately 50% of the token supply was allocated to insiders, including Solana developers, 22 early contributors to the Bonk project, and designated allocations for marketing, liquidity provision, and governance. The number of wallets in this category is likely small, making it reasonable to expect that they do not materially affect our main findings, which are based on a large sample of over 20,000 wallets.

with account sizes observed on major retail trading platforms such as Robinhood and eToro.8 Also, we find that a significant trading activity is happening on Solana DEXs and the economic magnitude is comparable to that of Robinhood,[9] suggesting the economic relevance of Solana DEXs as a retail trading venue.

To quantify the intensity of investors' trading activity, we compute the average monthly trading volume and number of trades per month over the six months preceding the airdrop. The average (median) monthly trading volume is $1,941.23 ($225.78). The average (median) number of monthly trades is 5.246 (1.833), broadly comparable to figures reported for retail investors on platforms such as eToro.[10]

To assess the riskiness of the assets held by wallets in our sample, we compute the average realized volatility of the cryptocurrencies purchased by each wallet. Specifically, for each month, we calculate the mean absolute daily log return of each purchased token over the next 30 days as a proxy for token volatility, then average these values across the assets traded by the wallet. We then

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[8] According to CNBC (2021, https://www.cnbc.com/2021/07/29/robinhood-stock-nasdaq-ipo-jim-cramer-and-others-share-their-take.html), the median account size of Robinhood users is approximately $240, a figure that closely aligns with the median wallet balance in our sample. This statistic is consistent with findings reported in several academic studies examining retail investor behavior on Robinhood, including Welch (2022), Eaton, Green, Roseman, and Wu (2022), and Bartlett, McCrary, and O'Hara (2024), all of which document comparable account balances. Kogan, Makarov, Niessner, and Schoar (2024) analyze the eToro platform, which enables users to trade both stocks and cryptocurrencies, and report a median account balance of $260 across all asset classes. Given that they estimate the median portfolio share allocated to cryptocurrencies is 0.4, the implied median cryptocurrency balance is comparable to, albeit slightly lower than, the median balance observed in our sample. Collectively, these comparisons suggest that the investor population in our study is representative of retail cryptocurrency investors on other major platforms.

[9] According to Newsroom (2024, https://newsroom.aboutrobinhood.com/robinhood-shares-selec ted-december-2024-month-to-date-trading-volumes/), Robinhood's December 2024 monthly trading volume was $137 billion. Over the same period, Solana-based DEXs recorded an average monthly trading volume of $121 billion, based on data from DeFiLlama (https://defillama.com/dexs/chains/solana), indicating that Solana DEX activity is broadly comparable in scale to that of Robinhood.

[10] According to Kogan, Makarov, Niessner, and Schoar (2024), the average eToro user executed 63.21 trades over an average account age of 489.6 days, implying an average trading frequency of approximately 3.93 trades per month. Using median statistics yields an estimate of 0.52 monthly trades. These calculations assume that a user exhibits the average (or median) number of trades and account age. Despite this simplifying assumption, the implied trading intensity is broadly comparable to that observed among Solana DEX users. We are unable to conduct a comparable analysis for trading volume due to the absence of relevant data in Kogan, Makarov, Niessner, and Schoar (2024).

take the average of these monthly volatilities over the six months preceding the airdrop. The cross-sectional mean and median of this measure across wallets are 0.063 and 0.055, respectively. For comparison, using the same methodology, Bitcoin's average volatility over the same period is 0.023, indicating that traders active on Solana DEXs tend to engage in more volatile assets than Bitcoin.

Finally, to assess investor exposure to meme tokens prior to the airdrop, we define the pre-meme exposure indicator equal to 1 if an investor held any meme coins as of November 30, 2022, and 0 otherwise. To identify meme coins in our sample, we manually verified each SPL token using multiple publicly available sources, including CoinMarketCap, CryptoCompare, X (formerly Twitter), and individual project websites, using the SPL token addresses as identifiers. Then, we searched for instances where a token was explicitly described as a "meme coin." If any credible source characterized a token as such, we classified it as a meme coin. Our results indicate that on average about 14.5% of investors in our sample held meme coins before the airdrop, while the median is zero, suggesting that the meme exposure on the Solana network was limited, but not entirely absent prior to the airdrop.

4.3 Baseline results

Our baseline specification employs a difference-in-differences framework using wallet-by-month panel data spanning the period from June 1, 2022, to May 31, 2023, comprising equal-length periods of six months before and after the airdrop event:

$$y_{i,t} = \alpha + \beta \times Treatment_i \times Post_t + FEs + \epsilon_{i,t}, \quad (3)$$

where i is an index for wallet and t an index for the month. $Treatment_i$ is an indicator equal to one if a wallet received Bonk tokens during the airdrop and zero otherwise. $Post_t$ is an indicator equal to one if t is after November 30, 2022, and zero otherwise. We note that a portion of the post period, specifically, the window from December 1 to December 7, is not exposed to the airdrop, as the airdrop was announced on December 8 and initiated on December 25. As a result, the estimated treatment effect is likely attenuated during December 2022. The specification includes wallet-by-treatment/control pair fixed effects to account for time-invariant wallet-level heterogeneity within matched pairs, and month fixed effects to control for common time trends. Standard errors are clustered at the wallet level to account for autocorrelation in observations within wallets over time. The dependent variables are the natural logarithm of one plus trading volume, log(1 + *Volume*) which accommodates zero trading volume observations, and volatility. An important consideration in this analysis is the exclusion of Bonk tokens when constructing outcome variables to ensure that our results are not mechanically driven by the receipt of airdropped tokens.

Table 3 presents our baseline results. Column (1) of Panel A shows that trading volume among treated wallets increased by approximately 80% relative to matched control wallets following the airdrop, with the effect statistically significant at the 1% level. Columns (2)-(4) report subsample analyses. In columns (2) and (3), we restrict the sample to treated wallets with *Avg. Balance* no more than and more than the median balance, respectively, along with their matched controls. The estimated effect remains similar in magnitude and statistically significant at the 1% level for both small and large wallets, suggesting that the results are not driven by wallet size. Finally, Column (4) focuses on better-matched pairs (i.e., those with matching distances below the median), yielding a coefficient of 0.896, significant at the 1% level. Overall, these results indicate that the treatment effect is robust and not driven by small wallets, wealth effects, or poor match quality.[11]

To understand the impact on the nature of increased trading volume, in Table 3 Panel B, we use an alternative dependent variable, volatility. The column (1) presents that the estimated coefficient is 0.014.[12] Given that the average volatility is 0.063, the treatment wallets experienced an approximately 22% (=0.014/0.063) increase in volatility compared to control wallets following the airdrop, suggesting that investors who were exposed to meme assets not only increase their trading volume but also their trading becomes riskier. We check the robustness of this finding with subsample analyses following the approach of Panel A, and find that the coefficients are comparable and statistically significant across all specifications.

Overall, our baseline results support the hypothesis that exposure to meme assets increases investor trading activity and shifts their portfolios toward riskier assets. These findings suggest that exposure to memes can induce more speculative trading behavior.

4.4 Dynamic effects

To estimate the dynamic effects of the impact of meme asset exposure on investor behavior, we run a regression of the following dynamic difference-in-differences specifications:

$$y_{i,t} = \alpha + \Sigma_{s \neq -1} \beta_s \times Treatment_i \times I[s = t] + FEs + \epsilon_{i,t} \tag{4}$$

[11] As an alternative measure of trading intensity, we consider the number of trades. Table A.1 of the Internet Appendix reports regression results using this measure, which yield qualitatively similar findings. As noted by Chen and Roth (2024), log transformations may introduce bias when the outcome variable includes zeros. Following their recommendation, we estimate Poisson regressions as a robustness check. The results, presented in Table A.2 of the Internet Appendix, are consistent with our baseline results: the Treat × Post coefficients remain positive and statistically significant across both outcome variables.

[12] The number of observations in Table 2, Panel B is smaller than in Panel A because volatility can only be computed for investor-months with at least one trade. In contrast, Panel A includes all investor-months, assigning zero trading volume when no trading activity occurs, thereby retaining a larger sample.

Figure 3, Panel A reports the dynamic treatment effects on trading volume. The estimated coefficients in the pre-treatment period exhibit no discernible trend and are close to zero, providing support for the parallel trend assumption. The treatment effect begins to rise in month 0 (December 2022), peaks in month 1 (January 2023), and gradually declines thereafter, though it remains statistically significant through month 5. Figure A.1 presents analogous results using the number of trades as the dependent variable. The pre-treatment coefficients are again near zero and statistically insignificant, while post-treatment coefficients become significantly positive, mirroring the pattern observed for trading volume.

Figure 3, Panel B examines changes in volatility. The pre-treatment coefficients are close to zero, with the largest treatment effect occurring in January 2023. Although the magnitude decreases in subsequent months, the estimates remain positive and statistically significant at the 5% level in months 1 through 5, with the exception of month 3.

Taken together, these dynamic estimates are consistent with the parallel trends assumption and demonstrate a persistent, though attenuating, treatment effect following meme exposure.

4.5 Extensive and intensive margin

In this subsection, we examine whether the treatment effect is driven by the extensive or intensive margin. Table 4 Column (1) restricts the sample to treatment-control pairs in which the treatment wallet's Bonk fraction, defined as the ratio of the dollar value of Bonk tokens received (measured at the airdrop start date) to the sum of this value and the wallet's balance as of November 30, 2022, is less than or equal to 5%. Following Anagol, Balasubramaniam, and Ramadorai (2021), this restriction limits the influence of large windfall gains and helps address concerns that the observed effects may be driven by wealth shocks. The estimated coefficient increases in magnitude and remains statistically significant relative to the baseline results, indicating that even small token allocations can meaningfully affect behavior. This evidence underscores the importance of extensive margin effects.

To examine intensive-margin effects, we re-estimate the specification in subsamples defined by the *Bonk fraction*: [minimum, 25th percentile), [25th percentile, 50th percentile), [50th percentile, 75th percentile), and [75th percentile, maximum] in columns (2)-(5), respectively. Across all subsamples, the estimated effects remain statistically and economically significant, although the effect on volume (volatility) modestly decreases (increases) as the Bonk fraction rises. Overall, these results reinforce the importance of extensive-margin effects, while the evidence on intensive-margin effects is more mixed.

4.6 The effect of pre-Bonk experience

In this section, we examine cross-sectional heterogeneity in the treatment effect, focusing on variation in wallets' pre-Bonk experience to better understand which subsets of treated wallets are more responsive to meme exposure. Our central hypothesis is that investors with limited prior exposure to meme or meme-like assets are more susceptible to the Bonk airdrop. In contrast, those with prior experience trading assets with similar gamified features may perceive Bonk as less novel and therefore exhibit weaker responses. We begin by partitioning the sample based on whether investors held NFTs of the 40 collections used in the airdrop eligibility criteria prior to the airdrop. These NFTs are classified as "art NFTs," which, unlike utility-based NFTs (e.g., those granting access to services), are likely speculative and serve as a form of gamified trading (e.g., Kong and Lin, 2021; Borri, Liu, and Tsyvinski, 2022). If Bonk tokens share similar characteristics with these art NFTs in terms of valuation opacity and trading motive, we expect the treatment effect to be attenuated among investors who held such NFTs in the pre-airdrop period.

To test this, we estimate a triple difference-in-differences specification using the previously defined Treatment and Post indicators, along with an indicator variable *Pre-NFT Exposure*, equal to 1 if the wallet held NFTs from the 40 collections prior to the airdrop. For brevity, we report only the coefficients on the *Treatment* × *Post* and *Treatment* × *Post* × *Pre-NFT Exposure* in Table 5, Panel A.

In column (1), we find that treatment investors without prior NFT holdings experienced a 94.8% increase in trading volume relative to the control group, whereas treatment investors with prior NFT exposure experienced a smaller increase of approximately 29.7% (i.e., 0.948 - 0.651). A similar pattern emerges when we use volatility as the outcome variable. Treatment investors without prior NFT exposure experienced a 2.9% increase in volatility relative to control investors, while those with NFT exposure showed a 0.4% (i.e., 0.029 - 0.033) decrease.

Next, we examine an alternative proxy for gamified trading experience: prior engagement in meme coin trading. For this analysis, we restrict the sample to investors who did not hold NFTs from the 40 collections, since NFT holders already exhibited weaker effects. Within this subsample, we estimate a triple difference-in-differences model using *Treatment*, *Post*, and *Pre-Meme Exposure*, which indicates whether an investor held any meme tokens prior to the airdrop or not. Results are reported in Table 5, Panel B. Column (1) shows that, treatment investors with prior meme exposure experienced a smaller increase of trading volume by approximately 17.8%. Similarly, column (2) shows that the treatment effect on volatility declines by 2.1% for investors who were exposed to meme before, compared to those who were not exposed to meme before.

Taken together, these results suggest that prior exposure to gamified trading moderates investor responses to meme exposure. Such investors are less likely to exhibit sharp increases in trading activity or risk-taking following the Bonk airdrop. This pattern is consistent with the hypothesis that Bonk exposure drives the increase in gamified trading.

4.7 Endowment effect and liquidation effect

The Bonk airdrop offers a natural setting to examine the endowment effect. Panel A of Figure A.2 plots average Bonk balances, measured by the number of tokens held, across post-event months. Treated investors, who received Bonk without cost or effort, consistently maintain higher token balances over the five-month period following the airdrop. This persistent holding behavior is consistent with the endowment effect, wherein mere ownership increases perceived value. Panel B presents the fraction of investors holding Bonk over time and shows that roughly 30% more treated investors retain the token relative to the control group, further reinforcing the presence of an endowment-driven response.

We next examine how liquidation of Bonk tokens shapes investor behavior. Column (1) of Table 6, Panels A and B, reproduces the baseline estimates from Table 3 as a benchmark. In column (2), we split the post-airdrop period into two subperiods based on the liquidation timing of the treated wallet and estimate separate coefficients for *Treatment* × *Before Liquidation* and *Treatment* × *After Liquidation*, where *Before Liquidation* (*After Liquidation*) is an indicator equal to one for months prior to (after) the full liquidation date of the airdropped Bonk tokens by the treatment wallet in a pair. For both dependent variables, the coefficient on *Treatment* × *After Liquidation* remains statistically significant at the 1% level and is comparable in magnitude to the baseline estimate in column (1), although it is somewhat smaller than the coefficient on *Treatment* × *Before Liquidation*, indicating that the effect of the Bonk airdrop remains strong even after liquidation.

In column (3), we replace month fixed effects with Treatment × Month fixed effects in the baseline specification and estimate the coefficient on *Treatment* × *After Liquidation*. The inclusion of Treatment × Month fixed effects absorb time-varying treatment effects, allowing the remaining variation in *Treatment* × *After Liquidation* to identify the incremental effect of liquidation, rather than the general attenuation of the treatment effect over time. In this specification, we cannot jointly estimate *Treatment* × *Before Liquidation* and *Treatment* × *After Liquidation* because *Before Liquidation* + *After Liquidation* = *Post*, and *Treatment* × *Post* is subsumed by the Treatment × Month fixed effects. The estimated coefficient on *Treatment* × *After Liquidation* is negative but small and statistically insignificant, suggesting that, once time-varying treatment effects are absorbed, liquidation has little additional impact. This finding is consistent with a persistent effect of the airdrop and a lasting shift in investor behavior.

4.8 The effect of experienced Bonk returns

A remaining concern is that the treatment effects documented above may simply reflect investors' high experienced return on the airdropped tokens rather than exposure to the meme asset per se. If the airdrop gives the investors the experience of high returns, treated investors might increase trading volume and risk-taking (e.g., Imas, 2016), even if the asset had no meme features. To separate the role of "experienced returns" from meme exposure, we construct a wallet-level experienced Bonk return measure. Specifically, for each treated wallet-month in the post period, we calculate the cumulative Bonk return up to the prior month based on the Bonk price on the airdrop date and the price on the liquidation date (or, for positions that remain open, the end-of-month unrealized gain).

In column (1) of Table 7, we restrict the sample to treatment-control pairs in which the treatment wallet's Bonk return is less than or equal to 0% in every month of the post-airdrop period. This specification examines whether the treatment effects remain strong even for wallets that do not experience any price appreciation. We find that the average treatment effect for these pairs is comparable in magnitude to the baseline effect in Table 3 and remains statistically and economically significant. In columns (2) and (3), we relax the restriction by imposing higher upper bounds of 200% and 1,000% on the treatment wallet's post-period Bonk return, and the estimated effects are again similar to those in column (1).

Taken together, these patterns indicate that the investor behavior we document is not primarily driven by investors' high experienced Bonk return. Instead, the results are more consistent with the interpretation that it is the meme exposure itself that triggers a persistent shift toward more intensive and speculative trading.

4.9 Performance analysis

Table 8 presents performance comparisons between treatment and control wallets during the post-event period. The four performance measures capture short-term outcomes based on token-level trading behavior: the volume-weighted 14(7)-day return is defined as the volume-weighted average return of purchased tokens minus that of sold tokens over the subsequent 14(7) days, while the equally weighted 14(7)-day return is constructed analogously using equal weights. All returns are expressed in percentage terms.

Across all four return measures-volume-weighted and equally weighted 14-day and 7-day returns-we find that treated wallets underperform relative to control wallets. The return differentials are consistently negative and statistically significant at conventional levels. For instance, the volume-weighted 14-day return is 0.634 percentage points lower for treated wallets, a difference that is statistically significant at the 1% level. All four return measures yield consistent results, with estimated effects ranging from -0.272% to -0.634%.

The consistent underperformance of treated wallets is particularly striking given their increased trading activity and greater exposure to volatile tokens following the airdrop. While treated investors are more active, their returns are lower on average, indicating that greater trading intensity does not translate into improved performance. This suggests that the air-drop spurred trading behavior without an associated improvement in trading performance.

4.10 Validity of the Bonk airdrop as a natural experiment

A key concern regarding the Bonk airdrop as a natural experiment is whether the event was unanticipated. To provide suggestive evidence consistent with an unanticipated shock, Panel A of Figure A.3 in the Internet Appendix plots the Google search intensity for the keyword "Bonk," normalized to its level on the airdrop announcement date, December 8, 2022. Search intensity remains relatively flat around both the announcement date and the airdrop start date, with a noticeable spike emerging only in the later stages of the airdrop period. While not conclusive, this pattern is consistent with the interpretation that the Bonk airdrop was not leaked in advance and was largely unanticipated by the market. Another important concern is the potential influence of confounding events. In the following, we assess the validity of the Bonk airdrop as an exogenous shock to meme exposure and critically evaluate alternative explanations.

We begin by addressing potential sources of bias in our investor-level analysis. First, the non-opt-in nature of the Bonk airdrop substantially reduces concerns about endogenous selection or confounding shocks. The airdrop's allocation mechanism introduces quasi-random variation in meme coin exposure among investors who satisfied the same eligibility criteria. Given this within-group randomness, it is unlikely that an unobserved shock would systematically affect only those wallets that happened to receive the airdrop.

A second identification concern is that, even among wallets meeting the same eligibility criteria, airdrop recipients and non-recipients might differ along unobservable dimensions, particularly if the Bonk team had access to additional, non-public information when selecting recipients. We argue this is unlikely. The Solana blockchain is public and pseudonymous, meaning that all wallet-level data available to the airdrop distributors are equally accessible to researchers and the general public. As such, the selection of recipients must have been based solely on on-chain observable activity. Given the absence of comprehensive off-chain identifiers or metadata, it is implausible that the distributors could condition on private or unobservable wallet characteristics.[13]

To further mitigate selection concerns, we construct a matched sample of recipients and non-recipients based on a rich set of pre-airdrop wallet-level observables, mimicking the approach that airdrop distributors plausibly used given the publicly available on-chain data. The unique transparency of blockchain data, combined with the fact that researchers and airdrop distributors observe the same information set, makes the assumption of selection on observables unusually credible in this context.

The token-level analysis in Section 5 may be susceptible to confounding Solana-specific shocks occurring concurrently with the airdrop. To address this concern, we provide preliminary evidence in Panel B of Figure A.3 of the Internet Appendix, which displays Google Trends search intensity for the terms “Bitcoin,” “Crypto,” “Solana,” and “Doge,” normalized to their values on the airdrop announcement date. The absence of notable shifts in search intensity for “Solana” in the days leading up to the announcement suggests no concurrent Solana-specific shocks that could bias our

[13] This work is not the first to exploit on-chain airdrops for causal identification. For example, Cong, Tang, Wang, and Zhao (2023) use airdrops to study financial inclusion in the Ethereum ecosystem, and Augustin, Chen-Zhang, and Shin (2021) exploit airdrops to causally identify the effect of information display on reaching-for-yield behavior in DeFi.

findings.

A more plausible concern relates to the timing of the FTX collapse, which occurred approximately 1.5 months prior to the Bonk airdrop and may have disproportionately affected certain token categories, such as speculative tokens. However, both empirical and anecdotal evidence indicate that FTX was not disproportionately involved in trading highly speculative assets. Unlike smaller exchanges known for listing illiquid and risky cryptocurrencies, FTX primarily offered a relatively mainstream selection of cryptocurrencies and did not specialize in meme coins or tokens with similar characteristics. Consequently, it is unlikely that any specific subset of tokens was systematically more exposed to the FTX collapse.

To further mitigate this concern, our primary analysis focuses on a subset of “Solana-focused tokens,” whose trading activity is concentrated on the Solana blockchain rather than on centralized exchanges. This restriction enhances our ability to isolate the effects of the Bonk airdrop from potential confounding shocks associated with the FTX event. Additionally, even if an unobserved Solana-specific shock were present, it is unlikely to bias our results. Our primary token-level analysis compares groups of Solana-focused tokens that differ in their exposure to risk or correlation with meme coins. Since both the treatment and control groups are drawn from the same blockchain ecosystem, they are subject to common Solana-related shocks. As a result, any such shocks are effectively differenced out in our empirical specification, mitigating concerns about bias from unobserved Solana-specific factors.

Taken together, these considerations support the validity of the Bonk airdrop as a natural experiment for assessing the impact of meme exposure on investor behavior and market outcomes.

5 Token-level Analysis

5.1 Methodology

For the token-level analysis on the Solana blockchain, we begin with a universe of 1,858 tokens that had been actively traded on Solana DEXs as of December 7, 2022, one day prior to the airdrop announcement. This initial set includes a large number of illiquid cryptocurrencies and cryptocurrencies not primarily associated with the Solana ecosystem. To address this concern, we restrict the sample to tokens whose average daily trading volume exceeds $100 over the prior 30 days (i.e., average 30-day trading volume above $3,000), which reduces the sample to 361 tokens.[14]

Next, to isolate tokens that are primarily associated with the Solana ecosystem, hereafter referred to as "Solana-focused tokens," we exclude the following categories: (i) wrapped tokens (e.g., wrapped BTC), which are representations of assets from other blockchains; (ii) native Solana token, SOL; (iii) liquidity provider (LP) tokens, which represent positions in DEX liquidity pools rather than standalone assets; (iv) stablecoins, due to their price-invariant nature; (v) liquid staking tokens, which are derivative representations of staked assets; (vi) staking rewards, which are typically non-tradable or secondary in nature; and (vii) crypto index tokens, which bundle multiple assets rather than representing individual protocols or projects. After applying these filters, our final sample comprises 230 Solana-focused tokens, which we use for our token-level analysis.

To quantify the correlation between the Solana-focused tokens and major meme coins, we construct a meme coin index comprising Dogecoin (DOGE), Shiba Inu (SHIB), Floki (FLOKI), Bonk (BONK), Pepe (PEPE), and Dogwifhat (WIF). These tokens have consistently ranked among

[14] We also explore alternative thresholds around the $100 benchmark, specifically $50 and $150, and find that our token-level results remain similar.

the highest in market capitalization within the meme coin category from mid-2017, identified as the point at which the cryptocurrency market reached an economically significant scale, through the time of writing, January 2025. The index is weighted by market capitalization and is used to compute correlation and other token-level metrics in this section, as well as in the centralized exchange (CEX) analysis in Section 6.[15]

5.2 Summary statistics

This section presents summary statistics for our sample of 230 Solana-focused cryptocurrencies. We show in Table 9 that on average, each token exhibits a monthly trading volume of approximately $1.1 million, with a median of $33,639, indicating a highly right-skewed distribution. *Token Volatility*, defined as the average absolute daily log return, has a mean of 7.59% and a standard deviation of 9.0%. The mean (standard deviation) of *Corr. with Doge* is 0.165 (0.156), while that for *Corr. with Meme* is 0.171 (0.161). These variables exhibit substantial cross-sectional variation, which we exploit to identify heterogeneous responses to the Bonk airdrop based on volatility and meme-related exposure.

5.3 Empirical findings

Before proceeding to the token-level analysis, we first investigate the aggregate trading volume on Solana DEXs relative to other trading venues. Specifically, we define daily Solana DEX volume as the sum of trading volumes, in U.S. dollar terms, across all tokens traded on Solana DEXs. To construct the volume measure for other exchanges, we subtract the Solana DEX volume from the total daily trading volume, as reported by CoinMarketCap. To facilitate comparison across time,

[15] It is important to note that among prominent meme coins, only Doge (DOGE), Shiba Inu (SHIB), and Floki (FLOKI) were launched prior to the introduction of Bonk. Accordingly, we construct a meme coin index based on these three tokens and use it to compute correlations with Solana-focused tokens in our token-level analysis.

we normalize both volume series by taking the natural logarithm of the ratio of each day's trading volume to the average trading volume over the 14-day pre-announcement window (days -14 to -1, where day 0 denotes the airdrop announcement date).

Figure 4 presents the log-normalized trading volumes for Solana DEXs and for all other exchanges. To ensure our results are not mechanically driven by the introduction of the Bonk token, a newly airdropped meme coin, we exclude Bonk-related trading volume from the Solana DEX volume. Prior to the airdrop, trading volumes on Solana DEXs and other exchanges appear to move in parallel. However, beginning a few days after the airdrop initiation, we observe a significant divergence: Solana DEX volume increases markedly relative to other venues. This pattern is consistent with the hypothesis that meme coin airdrops can generate spillover effects, elevating trading activity.

For token-level analysis, we partition the tokens into two groups: those more likely to be exposed to the Bonk airdrop and those less likely to be exposed. To visualize the effects, we present market-adjusted cumulative returns and market-adjusted normalized trading volumes.

To compute the market-adjusted cumulative return for a given cryptocurrency, we first calculate the natural logarithm of its daily price, normalized by its price on the day prior to the airdrop announcement. We then subtract the corresponding value derived from a cryptocurrency market index to remove common market trends.

For market-adjusted trading volume, we compute the log of one plus a cryptocurrency's daily trading volume divided by one plus its average daily volume over [-7, -1] days relative to the announcement. We add one to accommodate zero trading volume on a given day. We then subtract the corresponding value computed from the aggregate trading volume of the overall cryptocurrency market.[16]

We split the Solana-focused tokens into high- and low-volatility groups based on the median of the volatility measure defined in Section 5.2. Figure 5, Panel A presents the results. We observe that the prices of tokens in the high-volatility group increase substantially relative to those in the low-volatility group beginning a few days after the airdrop start date. The price differential between the two groups peaks around the airdrop end date. Trading volume displays a similar dynamic: the divergence in volume begins during the airdrop period and also reaches its maximum around the airdrop's conclusion.

We further investigate heterogeneity in airdrop effects by splitting Solana-focused tokens into high- and low-correlation groups based on the median correlation with Doge and the meme coin index defined in Section 5.2. In Figure 5, Panels B and C, we show that cryptocurrencies with higher correlations exhibit significantly larger price increases relative to their lower-correlation counterparts. Trading volume follows a similar pattern.

To quantify the impact on prices, we estimate cross-sectional regressions of log price growth adjusted by the cryptocurrency market index return on three explanatory variables: *log(Token Volatility)*, *Corr. with Doge*, and *Corr. with Meme.* The baseline event window spans from one day prior to the airdrop start date to one day after the airdrop end date. As shown in Table 10, Panel A (columns 1-3), all three variables are statistically significant at the 1% level and economically significant. A one standard deviation increase in volatility from the mean is associated with a 11.4% increase in price growth, while a one-unit increase in *Corr. with Doge*

[16] The methodology used to construct both the cryptocurrency market index and aggregate trading volume is described in detail in Section A.1 of the Internet Appendix.

(Corr. with Meme) corresponds to a 8.22% (8.15%) increase. These results are robust to an alternative event window (from seven days before the start date to seven days after the end date), reported in columns (4)–(6), and are particularly pronounced for *Corr. with Doge* and *Corr. with Meme*.

To assess the impact on trading volume, we estimate difference-in-differences regressions with a pre-period defined as days −7 to −1 relative to the airdrop start date and a post-period defined as the interval from the airdrop start date to the airdrop end date plus either 14 or 28 days. We interact each of the three explanatory variables with an indicator for the post period. The dependent variable is a market-adjusted log normalized trading volume measure: within each period, we compute $log \frac{1 + average\ daily\ trading\ volume}{1 + average\ trading\ volume\ in\ the\ pre-period}$ and subtract the analogous quantity constructed from aggregate cryptocurrency market trading volume over the same period. Table 10, Panel B reports the results.

We find statistically and economically significant effects on trading volume. In columns (1) and (2), a one standard deviation increase in volatility from its mean is associated with a 21.9% and 24.8% increase in volume during the post period, defined as the interval from the airdrop start date to the airdrop end date plus 14 and 28 days, respectively. In columns (3) and (5), a one–standard deviation increase in correlation with Doge and the meme index is associated with a 16.3% and 16.6% increase in volume, respectively, when the post period ends at the airdrop end date plus 14 days. In columns (4) and (6), extending the post period to the airdrop end date plus 28 days increases these estimates to 18.1% and 18.6%, with stronger statistical significance.

Overall, the evidence indicates that meme exposure meaningfully shapes market outcomes. Tokens with higher correlations to meme coins and higher volatility exhibit larger increases in both prices and trading activity around the airdrop than tokens with lower meme-coin correlations and lower volatility.

6 Listings of Meme Coins on Centralized Exchanges

6.1 Methodology

In our earlier analysis of the Bonk airdrop at the cryptocurrency level, we documented that, following the airdrop, the trading volume on Solana DEXs increased. Additionally, we observed a disproportionate increase in both the prices and trading volumes of riskier tokens, as well as those more highly correlated with major meme coins, relative to other tokens. In this section, we extend that analysis by examining a different set of events: the listings of meme coins on centralized exchanges (CEXs). For this analysis, we leverage exchange × token × day level panel data obtained from CryptoCompare API.[17]

To implement this analysis, it is essential to identify a sufficiently large set of CEXs that allow us to construct a treatment group—exchanges that listed meme coins, and a control group—exchanges that did not. Furthermore, we require enough meme coin listing events to ensure statistical power. Given that the cryptocurrency market began to experience rapid growth only in the latter half of 2017, and Dogecoin was the only prominent meme coin prior to that period, we restrict our sample to the period from July 2017 to January 2025.

To identify the effects of meme coin listings on CEXs, we focus on a subset of sufficiently large meme coins, as smaller coins are unlikely to generate economically meaningful impacts on market outcomes. Specifically, we select Dogecoin (DOGE), Shiba Inu (SHIB), Floki (FLOKI), Bonk

[17] CryptoCompare API rebranded its name to CoinDesk API in 2024 after CryptoCompare was merged by CoinDesk.

(BONK), Pepe (PEPE), and Dogwifhat (WIF), as these coins consistently rank among the largest meme coins by market capitalization over our sample period, based on data from CoinMarketCap.[18]

For each exchange that listed a meme coin, we identify the first three meme coin listings and define each as a distinct event. We focus on the earliest listings because the marginal impact of subsequent listings on a given exchange is likely attenuated due to diminishing novelty. To accurately determine listing dates, we manually collected data from various sources, including official exchange websites, exchanges' Twitter accounts, historical price data from exchange platforms, and supplementary sources such as news articles and Medium posts.

Our empirical design is based on stacked difference-in-differences. For each event, we construct a corresponding control group composed of exchanges that had not listed any of the selected meme coins up to that date. To ensure comparability and minimize bias, we implement a filtering procedure that excludes very small exchanges and retains only those with trading volumes broadly comparable to those of the treatment exchanges. A detailed description of the filtering criteria is provided in Table A.3. After applying this procedure, we identify a total of 115 meme coin listing events across 64 treatment exchanges and construct a control group consisting of 70 exchanges.

Then, we restrict the sample to trading pairs that had been active for at least four weeks prior to the event date. This restriction addresses the concern that our results could be mechanically influenced by trading activity associated with the newly listed meme coins themselves. For the empirical analysis, we construct trading volume measures at two levels of aggregation, both

[18] Unlike these six meme coins, other smaller ones do not show consistent large market capitalization, and therefore, we focus on these six meme coins in this analysis.

expressed in U.S. dollars: Exchange × Week and Exchange × Token × Week levels.[19]

6.2 Baseline results

Our baseline analysis investigates the impact of meme coin listings on trading volume. In contrast to our earlier analysis on Solana DEXs, we do not examine price effects CEXs. This is because CEXs typically list well-established cryptocurrencies, which are subject to efficient cross-exchange arbitrage. As a result, the price impact of meme coin listings on CEXs is likely minimal.

In Table 11 Panel A, we estimate the following equation at the exchange-week level:

$$y_{i,t} = \alpha + \beta \times Treatment_i \times Post_t + FEs + \epsilon_{i,t}, \quad (5)$$

where i is an index for exchanges and t is the week relative to the listing event. The dependent variable $y_{i,t}$ is the natural logarithm of the weekly trading volume on exchange i in week t, normalized by the volume in the week immediately preceding the event. To control for confounding factors, we include two sets of fixed effects. First, we include *Event*×*Exchange* fixed effects to absorb any time-invariant exchange-specific characteristics within each event. Second, we include *Event* × *Week* fixed effects to control for unobservable, time-varying shocks that are common across exchanges within the same event window.

The estimated result in Panel A of Table 11 indicates that the exchange trading volume increases by 17.5% in the treatment exchanges relative to control exchanges on average, and this effect is statistically significant at the 5% level.

In Panel B, we conduct a similar analysis using a more granular specification at the cryptocurrency-

[19] Section A.2 of the Internet Appendix outlines the detailed procedure for converting trading volume denominated in quote currencies into U.S. dollar equivalents.

exchange-week level. Specifically, we estimate the following equation:

$$y_{i,j,t} = \alpha + \beta \times Treatment_i \times Post_t + FEs + \epsilon_{i,j,t} \tag{6}$$

where i is an index for exchanges, j is an index for cryptocurrencies, and t is the week relative to the event. The dependent variable $y_{i,j,t}$ is the natural logarithm of the weekly trading volume of cryptocurrency j on exchange i during week t, normalized by the volume in the week prior to the event. The primary difference from the specification in Panel A is the inclusion of the cryptocurrency index j, and the replacement of the Event × Exchange fixed effects with Event × Exchange × Crypto fixed effects, which control for any time-invariant cryptocurrency-exchange-specific characteristics within each event.

Column (1) of Table 11, Panel B shows that cryptocurrencies listed on treatment exchanges experience an average increase in trading volume of approximately 7.7% relative to those on control exchanges. This estimate is statistically significant at the 1% level.

6.3 Dynamic effects

Figure 6 presents event-study plots showing dynamic difference-in-differences estimates for both specifications: the exchange-week level analysis (Panel A) and the cryptocurrency-exchange-week level analysis (Panel B). The Week variable is defined such that 0 refers to days 0-6 (from the listing date), 1 to days 7-13, and so on. In both panels, the estimated pre-treatment coefficients are close to zero, while the post-treatment coefficients increase significantly, supporting the validity of the parallel trend assumption.

It is worthwhile to note that the estimated coefficient increases sharply immediately after the listing of meme coins, as evidenced by the high coefficient in Week 0, suggesting that the volume surge occurs too quickly to be fully predicted by the exchange. This immediate response mitigates concerns about endogeneity, as it is unlikely that exchanges can precisely forecast trading volume at such a high (weekly) frequency when making listing decisions.

Taken together, these results suggest that meme coin listings on CEXs lead to increased trading activity by investors exposed to these meme assets. This finding is consistent with our earlier results on DEXs following the Bonk airdrop and supports the broader narrative that meme-driven gamification can stimulate trading behavior.

6.4 Cross-sectional heterogeneity

The cryptocurrency-exchange-week-level difference-in-differences specification allows us to examine cross-sectional heterogeneity in the effects of meme coin listings. In this analysis, we assess how the impact of meme coin listings on CEXs varies with characteristics of individual cryptocurrencies.

To do so, we construct three key variables measured prior to each event. First, *Log(Volatility)* is defined as the natural logarithm of the average absolute daily return for each cryptocurrency. Second, *Corr. with Doge* and *Corr. with Meme* are computed as the Pearson correlations between a cryptocurrency's daily log returns and those of Doge and the Meme index, respectively. The Meme index return is constructed as the market-capitalization-weighted average return of the six major meme coins in our sample. All three variables are estimated using daily return data from January 1, 2016, approximately 1.5 years prior to the first event, through the four weeks preceding each event.

We estimate a triple difference-in-differences (DiD) model in which we interact the Treatment and Post indicators with each of the three continuous variables. The results are presented in Columns

(2)-(4) of Table 11, Panel B. For brevity, we report only the coefficients on the *Treatment* × *Post* interaction and the triple interaction terms.

In Column (2), the coefficient on the triple interaction term *Treatment* × *Post* × *Log(Volatility)* is statistically insignificant, in contrast to our findings in the market-wide analysis of the Bonk airdrop. One possible interpretation is that the information environment differs across settings. In the Bonk airdrop setting, the meme coin, Bonk, did not exist prior to the event, leaving investors with little information about its characteristics. As a result, investors may have perceived Bonk as a generic proxy for risky assets, leading to increased trading in other high-volatility or meme-related cryptocurrencies. In contrast, the meme coins listed on CEXs in our sample, such as DOGE, SHIB, and PEPE, are already well known to market participants. Thus, instead of increasing their activity in riskier assets broadly, investors are more likely to increase trading in cryptocurrencies that are more closely linked to the newly listed meme coins. We view this explanation as only suggestive and cannot directly test the underlying mechanism in our setting.

Consistent with this interpretation, we find that the triple interaction terms involving Corr. with Doge and Corr. with Meme are both positive and statistically significant at the 1% level. The estimated coefficients imply economically meaningful effects: a one-standard-deviation increase in correlation with Doge and the Meme index is associated with a 4.5% and 4.1% increase in trading volume, respectively.

We acknowledge that our CEX-based analysis may be subject to endogeneity concerns, as the decision to list a particular meme coin on a CEX may not be exogenous. However, the surge in trading volume on treatment exchanges relative to control exchanges begins immediately after the listing, with a pronounced increase observed in the first week. It is unlikely that exchanges can anticipate short-term volume spikes at the weekly frequency and time their listing decisions accordingly. Moreover, both the baseline and cross-sectional heterogeneity results are consistent with those from the Bonk airdrop analysis, which offers a plausibly exogenous setting. This alignment of findings supports the interpretation that meme exposure increases investor trading activity, especially in other speculative assets.

7 Conclusion

This paper is the first to examine the impact of meme assets on investor behavior and financial markets using unique data sourced from a public blockchain and experimental settings in cryptocurrency markets. Our findings are consistent with the hypothesis that exposure to meme assets drives more speculative trading and increases demand for riskier investments. These insights have important implications for the operations of trading platforms and retail trading behavior. Trading platform operators, particularly those that promote or randomly distribute meme-like assets to new users, should be mindful of how these practices can encourage unintended excessive risk-taking by retail investors. They may consider implementing clearer risk disclosures and investor education tools to support more informed decision-making. Retail investors, meanwhile, should remain cautious when trading meme-like assets, as their experience with such assets may influence their broader portfolio choices and risk-taking behavior. Given the nascency of the academic literature on meme assets, we view our analysis as a first step toward a more systematic understanding of meme-driven trading and its spillovers to broader financial markets, and we hope that our evidence provides a useful empirical benchmark for future theoretical and empirical work on meme phenomena, the motives for meme investing, and gamified investing.

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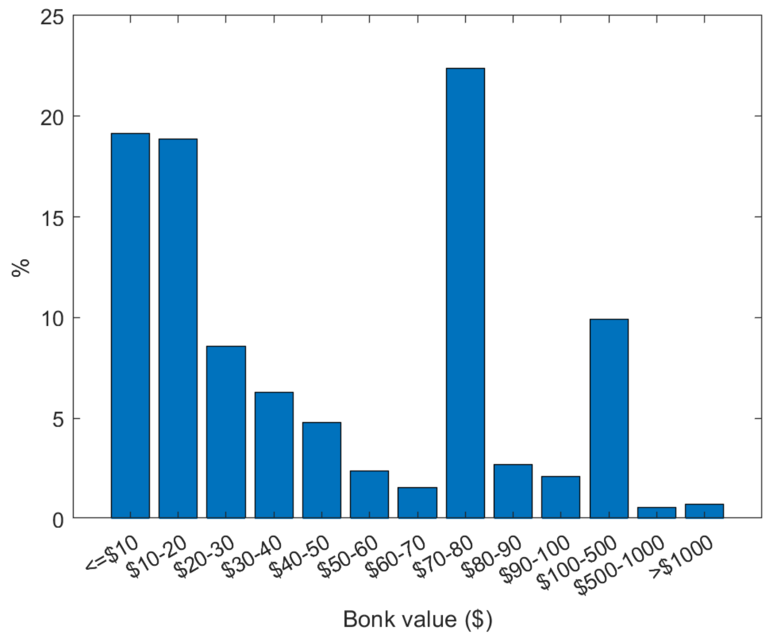
Figure 1: Cover page of the Bonk Whitepaper

This figure displays the cover page of the Bonk whitepaper, which features Bonk's mascot, a Shiba Inu-style dog, symbolizing the meme-inspired nature of the token. The whitepaper outlines the project's core objectives, tokenomics, and airdrop distribution strategy in subsequent sections.

Figure 2: Distribution of Bonk Airdrop

This figure illustrates the distribution of Bonk tokens received during the airdrop. Panel A shows the distribution of investors by Bonk value, the dollar value of Bonk tokens received. Panel B reports the percentage of investors by their Bonk fraction, defined as the value of airdropped Bonk tokens (measured on the airdrop start date) divided by the sum of the wallet's total balance as of November 30, 2022 and the airdropped Bonk value.

(a) Panel A: Distribution of Bonk value



(b) Panel B: Distribution of Bonk fraction

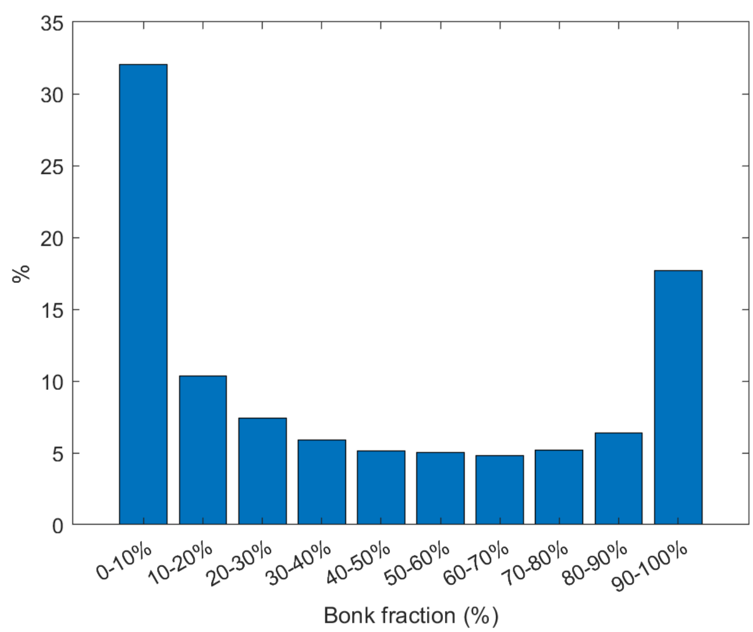
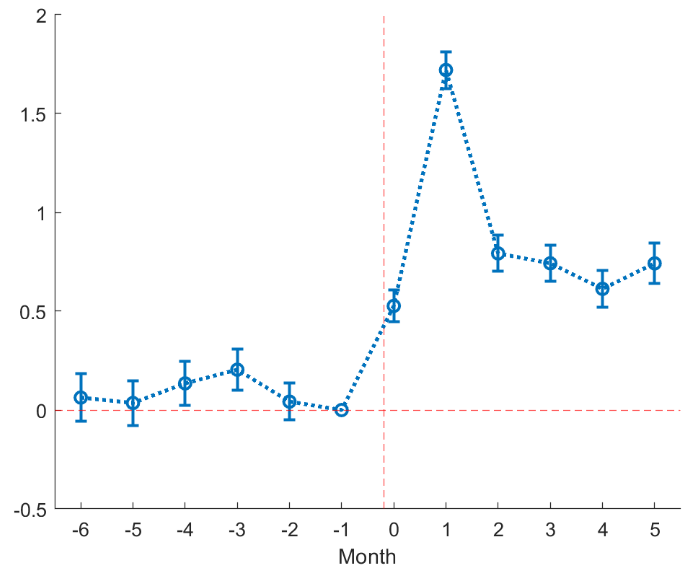


Figure 3: Impact of the Bonk Airdrop on Token Prices and Volume

This figure displays the dynamic treatment effects estimated via difference-in-differences models assessing the impact of the Bonk airdrop on investor behavior. Panel A presents the estimated effects on investor trading volume, measured as Log(1+volume), while Panel B reports the effects on the volatility of tokens purchased by investors. In both panels, the horizontal axis represents months relative to the airdrop event (with zero indicating the airdrop month), and the vertical axis depicts the estimated dynamic coefficients with corresponding confidence intervals.

(a) Panel A: Impact on volume



(b) Panel B: Impact on volatility

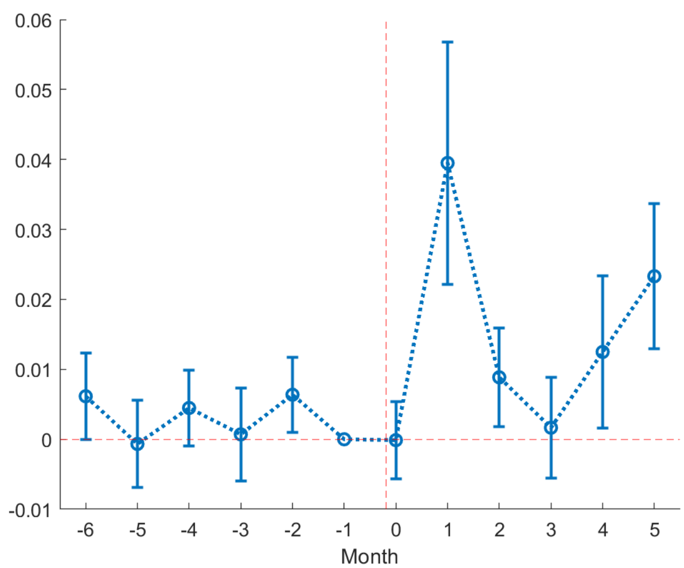


Figure 4: Impact of the Bonk Airdrop on Solana Decentralized Exchanges (DEXs)

This figure compares trading volume on Solana DEXs and other cryptocurrency exchanges surrounding the Bonk airdrop. The horizontal axis is the number of days since the airdrop announcement date, and the vertical axis is the log of daily trading volume normalized by the average daily volume from day -14 to day -1 relative to the announcement date. The solid red vertical line marks the airdrop announcement date; the dashed red line indicates the airdrop start date; and the dotted red line denotes the airdrop end date.

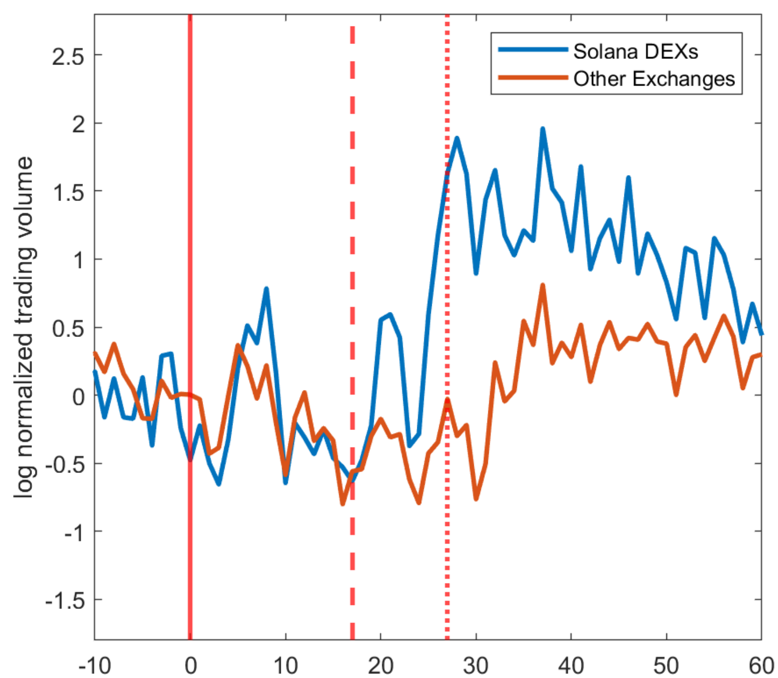
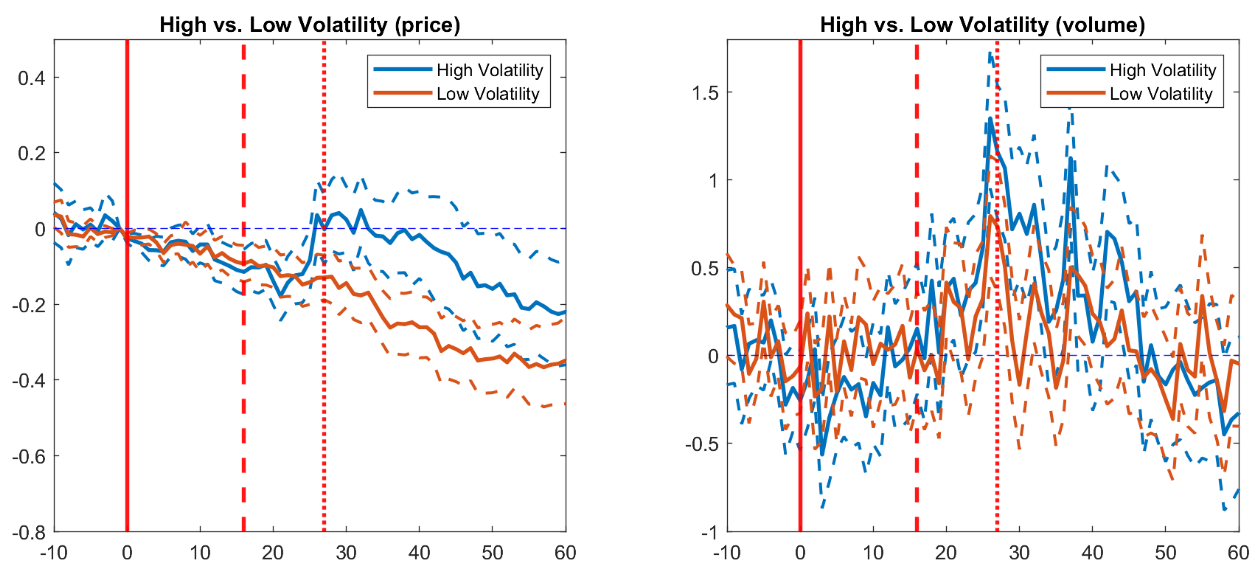


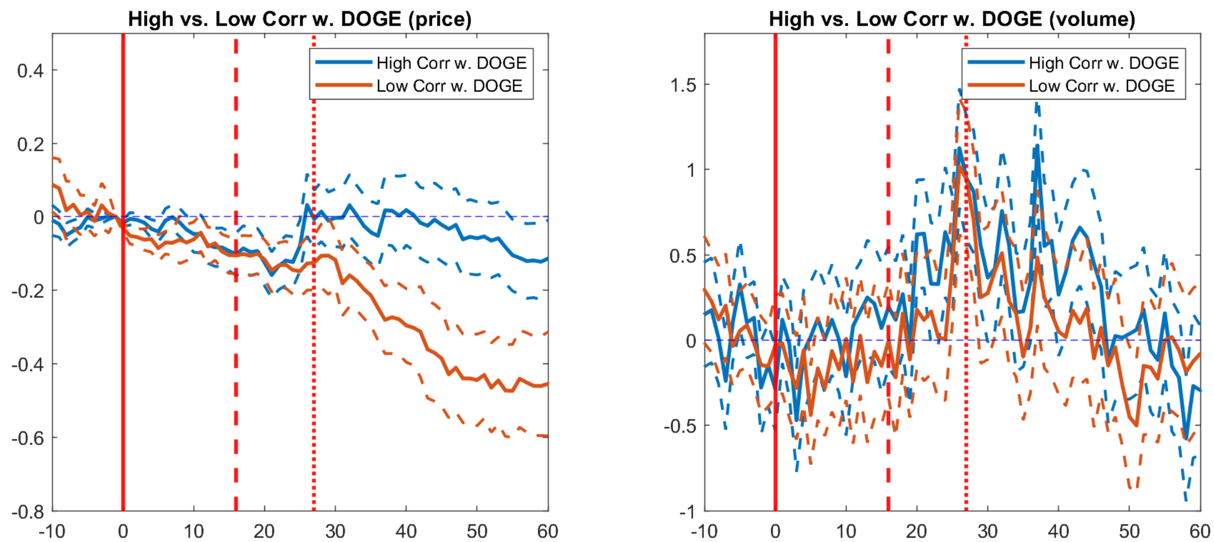
Figure 5: Impact of the Bonk Airdrop on Token Prices and Volume

This figure presents the effects of the Bonk airdrop on the prices and trading volume of Solana-focused tokens, as defined in Section 5.1. Each panel compares two subsets of tokens based on specific characteristics. Panel A contrasts tokens with above-median token volatility (*High Volatility*) and those with below-median token volatility (*Low Volatility*). Panel B compares tokens with high versus low correlations with Doge, while Panel C compares tokens with high versus low correlations with the meme index. In each panel, the left-hand plot reports market-adjusted cumulative log returns. The right-hand plot reports market-adjusted log trading volume, demeaned by its average over the seven days preceding the announcement. The market adjustment procedure is described in Section 5.1. Dashed lines represent 95% confidence intervals. The solid red vertical line marks the airdrop announcement date; the dashed red line marks the airdrop start date; and the dotted red line marks the airdrop end date.

(a) Panel A: Tokens with high volatility vs. low volatility



(b) Panel B: Tokens with high vs. low correlations with Doge



(c) Panel C: Tokens with high vs. low correlations with Meme index

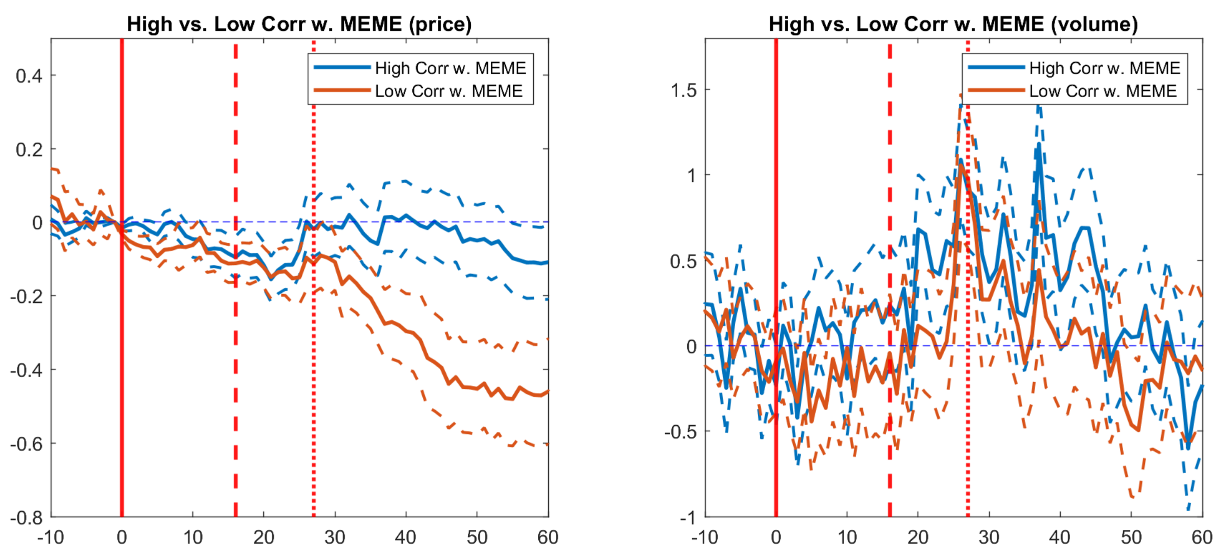
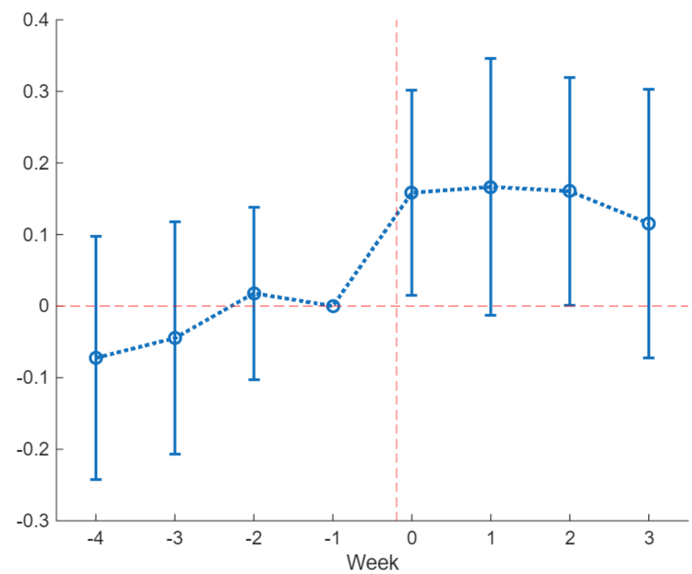


Figure 6: Impact of Meme Coin Listings on Centralized Exchanges: Dynamic Coefficients

This figure presents the dynamic treatment effects from difference-in-differences estimations assessing the market-wide impact of meme coin listings on centralized exchanges. Panel A reports the effect on aggregate exchange trading volume, while Panel B presents the impact on trading volume at the exchange by token level. In both panels, the horizontal axis denotes weeks relative to the listing event, and the vertical axis depicts the estimated dynamic coefficients with corresponding confidence intervals.

(a) Panel A: Exchange × Week Level



(b) Panel B: Exchange × Token × Week Level

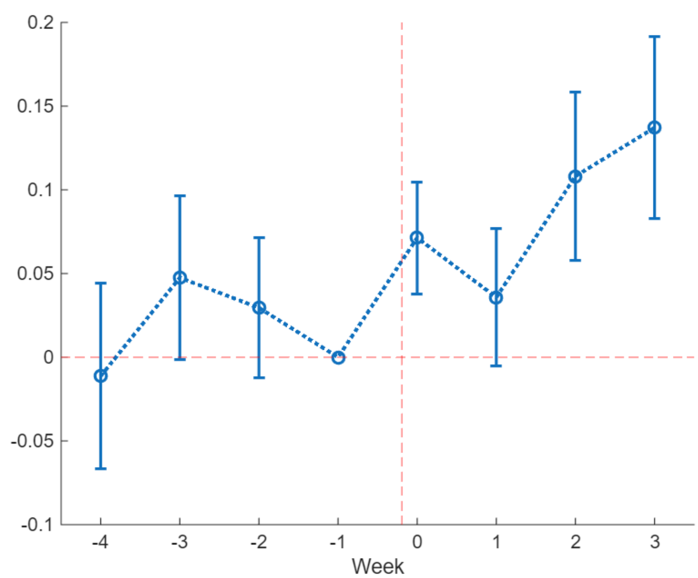


Table 1: Eligibility Criteria of the Bonk Airdrop

This table outlines the allocation of Bonk tokens across different categories, including NFT projects, developers, liquidity, and community initiatives. The distribution aims to promote decentralization and incentivize participation in the Solana ecosystem.

Eligibility Criteria	Detail	Fraction
40 Solana NFT Projects	21% of the coin is being distributed amongst 40 active Solana NFT Projects, representing more than 296,000 individual NFTs. These are spread between high, middle and low cap collections, to prevent the potential accrual of all tokens towards owners of expensive NFTs. This will be based on a snapshot of delisted NFTs, and airdropped via the Famous Fox Federation Tools.	21%
Solana Developers	5.3% of the Supply is going towards Developers on the Solana Chain. It's vital to continue rewarding those who build the programs we need to create things like BONK, and will hopefully be the first of many airdrops towards Solana Developers going forward. This is being facilitated by LamportDAO. BONK is a community coin which may be integrated into Solana dApps across the entire network and serve as a unit of account and user participation reward.	5.3%
Initial Liquidity	5.3% has been allocated towards Initial Liquidity Distribution, this is being used in Raydium, Orca, and Solend, alongside other platforms like FFFlip, Monaco Protocol, and the BoiBook. Any rewards earned via this distribution will be returned to the BONKDAO.	5.3%
Solana Market Participants and DeFi users	15.8% of the Supply is going towards Solana market participants and DeFi users.	15.8%
Early contributors	21% is allocated to the early contributors who helped launch, and continue to advance BONK since its inception. There are 22 individuals included in this allocation, each of whom has provided early support in the form of artwork, token design, marketing, integrations, web development, business development, and provision of early liquidity. Each individual remains dedicated to driving Bonk's long-term success, and this will be reflected with a 3-year linear token vesting period beginning from January 1st, 2023.	21%
Marketing	5.3% will be used for marketing promotions with teams like AssetDash, and further giveaways and incentives for the promotion of BONK.	5.3%
Solana Artist and Collectors	10.5% of the Supply is going towards the Artists and Collectors of 1/1 art, using a snapshot of activity from the Collector.sh team. This will not be proportional to volume traded, but there will be a minimal cutoff.	10.5%
BonkDAO	15.8% will go towards a BONK DAO to be used for initiatives conceived by the wider BONK community. A Realms Instance will be established that will then control further Distribution and allocation of BONK for these initiatives. Program authority over the Token will be delegated to the Realms Instance and guided by token holders going forward.	15.8%
Total		**100%**

Table 2: Descriptive Statistics

This table presents summary statistics for investor characteristics in our sample. Panel A reports statistics for the full sample of investors. Panel B divides the sample into treatment investors—those who received Bonk tokens via the airdrop, and control investors—those who did not. *Wallet age (years)* refers to the time, in years, from the wallet's creation to November 30, 2022. *Avg. balance ($)* denotes the average monthly wallet balance over the pre-period, defined as June 1, 2022 to November 30, 2022. *Avg. volume ($)* represents the average monthly trading volume during the same period. *Avg. # trades* is the average monthly count of executed trades. *Avg. volatility* captures the average monthly volatility of tokens purchased by each investor, where the volatility of a token is defined as the average absolute daily log return within a given month. *Pre-meme exposure* is a dummy variable that equals to 1 if an investor held any meme coins as of November 30, 2022, and 0 otherwise.

Panel A: All Variables								
	N	**Average**	**St. dev.**	**Median**	**p10**	**p25**	**p75**	**p90**
Wallet age (years)	47,172	0.624	0.420	0.586	0.082	0.249	1.000	1.167
Avg. balance ($)	47,172	1,964.48	6,678.02	243.602	12.875	56.617	950.793	3,424.54
Avg. volume ($)	47,172	1,941.23	6,416.27	225.781	5.447	36.012	977.610	3,528.55
Avg. # trades	47,172	5.246	10.718	1.833	0.333	0.667	5.000	12.000
Avg. volatility	35,845	0.063	0.042	0.055	0.029	0.041	0.070	0.100
Pre-meme exposure	47,172	0.145	0.352	0.000	0.000	0.000	0.000	1.000
Panel B: Treatment vs. Control								
Variables	**N**	**Average**	**St. dev.**	**Median**	**p10**	**p25**	**p75**	**p90**
Treatment								
Wallet age (years)	23,586	0.625	0.427	0.586	0.082	0.249	1.000	1.167
Avg. balance ($)	23,586	1,963.51	6,445.65	252.265	13.213	57.357	1,013.53	3,735.91
Avg. volume ($)	23,586	1,963.67	6,376.83	227.505	5.230	35.665	1,005.17	3,698.15
Avg. # trades	23,586	5.222	10.456	1.833	0.333	0.667	5.000	12.167
Avg. volatility	18,361	0.063	0.039	0.055	0.031	0.043	0.069	0.095
Pre-meme exposure	23,586	0.145	0.352	0.000	0.000	0.000	0.000	1.000
Control								
Wallet age (years)	23,586	0.623	0.414	0.586	0.082	0.249	1.000	1.167
Avg. balance ($)	23,586	1,965.45	6,902.70	233.747	12.545	56.099	900.580	3,159.23
Avg. volume ($)	23,586	1,918.79	6,455.53	224.156	5.730	36.544	939.882	3,329.79
Avg. # trades	23,586	5.269	10.974	1.750	0.333	0.667	4.833	12.000
Avg. volatility	17,484	0.064	0.045	0.054	0.027	0.039	0.071	0.104
Pre-meme exposure	23,586	0.145	0.352	0.000	0.000	0.000	0.000	1.000

Table 3: Impact of the Bonk Airdrop on Investor Behavior: Baseline Results

This table presents results from difference-in-differences analyses assessing the impact of the Bonk airdrop. Panel A uses the natural logarithm of one plus monthly dollar trading volume as the dependent variable. Panel B uses monthly volatility, as defined in Table 2, as the dependent variable. Each column reports results for a distinct subsample. Column (1) includes the full sample of wallets defined in Table 2. Column (2) restricts the sample to matched pairs where treatment wallets had a pre-period average balance ≤ median balance, while Column (3) restricts to those with balances > median balance. Column (4) restricts the analysis to matched pairs with a matching distance below the sample median. T-statistics are presented in parentheses below coefficient estimates. Standard errors are clustered at the wallet level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Volume	**(1)**	**(2)**	**(3)**	**(4)**
Treatment × Post	0.778^{***}	0.750^{***}	0.806^{***}	0.896^{***}
	(25.169)	(22.906)	(16.552)	(31.159)
N	516,621	258,323	258,298	258,334
Adjusted R2	0.386	0.354	0.376	0.354
Panel B: Volatility	**(1)**	**(2)**	**(3)**	**(4)**
Treatment × Post	0.014^{***}	0.021^{***}	0.009^{**}	0.028^{***}
	(5.162)	(6.019)	(2.420)	(8.258)
N	150,499	65,497	85,002	51,583
Adjusted R2	0.239	0.260	0.214	0.234
Sample	All	Avg.balance≤ median balance	Avg.balance> median balance	Matching dist. ≤median dist.
Wallet × Pair FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes

Table 4: Impact of the Bonk Airdrop on Investor Behavior: Extensive/Intensive Margins

This table presents results from difference-in-differences analyses assessing the extensive and intensive margin effects of the Bonk airdrop. Panel A uses the natural logarithm of one plus monthly dollar trading volume as the dependent variable. Panel B uses monthly volatility, as defined in Table 2, as the dependent variable. Each column reports results for a distinct subsample. Column (1) includes matched pairs in which the treatment wallet's Bonk fraction, defined as the value of airdropped Bonk tokens measured on the airdrop start date divided by the sum of the wallet's balance on November 30, 2022 and the airdropped Bonk value, is less than 5%. Column (2)-(5) divides the sample into quartiles based on the fraction of Bonk value received during the airdrop by treatment wallets. T-statistics are presented in parentheses below coefficient estimates. Standard errors are clustered at the wallet level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Volume	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Treatment × Post	0.948^{***}	0.930^{***}	0.762^{***}	0.754^{***}	0.667^{***}
	(15.320)	(15.298)	(15.166)	(16.191)	(15.527)
N	121,755	129,165	129,154	129,154	129,148
Adjusted R2	0.372	0.372	0.388	0.386	0.370
Panel B: Volatility	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Treatment × Post	0.009^{**}	0.010^{**}	0.012^{***}	0.016^{***}	0.021^{***}
	(2.092)	(2.375)	(2.762)	(3.902)	(4.701)
N	37,819	40,368	40,923	37,495	31,713
Adjusted R2	0.201	0.203	0.236	0.244	0.271
Sample	Bonk fraction≤5%	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Wallet × Pair FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes

Table 5: Impact of the Bonk Airdrop on Investor Behavior: Heterogeneous Effects

This table reports results from triple difference-in-differences analyses examining heterogeneous effects of the Bonk airdrop. *Pre-NFT Exposure* is an indicator variable equal to 1 if a wallet held NFTs from any of the 40 collections used in the airdrop eligibility criteria immediately before the airdrop, and 0 otherwise. *Pre-Meme Exposure* is defined as 1 if an investor held meme coins as of November 30, 2022, and 0 otherwise. Panel A uses the full sample, while Panel B restricts the sample to wallets not holding any of NFTs in the 40 NFT collections as of November 30, 2022. For brevity, only the *Treatment* × *Post* and triple interaction terms are reported. T-statistics are presented in parentheses below coefficient estimates. Standard errors are clustered at the wallet level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Heterogeneous Effects of Pre-NFT Exposure	**(1)**	**(2)**
	Volume	**Volatility**
Treatment × Post	0.948***	0.029***
	(38.196)	(11.479)
Treatment × Post × Pre-NFT Exposure	-0.651***	-0.033***
	(-7.200)	(-6.019)
N	516,621	150,499
Adjusted R2	0.389	0.239
Sample	All	All
Panel B: Heterogeneous Effects of Pre-Meme Exposure	**(1)**	**(2)**
	Volume	**Volatility**
Treatment × Post	0.976***	0.033***
	(37.031)	(11.876)
Treatment × Post × Pre-Meme Exposure	-0.178**	-0.021***
	(-2.359)	(-3.215)
N	383,186	92,030
Adjusted R2	0.360	0.231
Sample	Non-NFT holders	Non-NFT holders
Wallet × Pair FE	Yes	Yes
Month FE	Yes	Yes

Table 6: Impact of the Bonk Airdrop on Investor Behavior: Bonk Liquidation

This table reports difference-in-differences estimates by the timing of Bonk liquidation for treatment wallets. *Before Liquidation* (*After Liquidation*) is an indicator equal to one for months prior to (after) the full liquidation date of the airdropped Bonk tokens by the treatment wallet in a pair. Column (1) reproduces the baseline estimates from Table 3 as a benchmark. Column (2) splits the post-airdrop period into before and after liquidation and interacts *Treatment* with *Before Liquidation* and *After Liquidation*. Column (3) replaces month fixed effects with Treatment-by-month fixed effects. T-statistics are presented in parentheses below coefficient estimates. Standard errors are clustered at the wallet level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Volume	**(1)**	**(2)**	**(3)**
Treatment × Post	0.778***		
	(25.169)		
Treatment × Before Liquidation		0.848***	
		(26.163)	
Treatment × After Liquidation		0.691***	-0.043
		(18.701)	(-1.156)
N	516,621	516,621	516,621
Adjusted R2	0.386	0.386	0.388
Panel B: Volatility	**(1)**	**(2)**	**(3)**
Treatment × Post	0.014***		
	(5.162)		
Treatment × Before Liquidation		0.016***	
		(4.458)	
Treatment × After Liquidation		0.012***	-0.006
		(3.920)	(-1.330)
N	150,499	150,499	150,499
Adjusted R2	0.239	0.239	0.240
Wallet × Pair FE	Yes	Yes	Yes
Month FE	Yes	Yes	No
Treatment x Month FE	No	No	Yes

Table 7: Impact of the Bonk Airdrop on Investor Behavior: Bonk Return

This table presents results from difference-in-differences analyses using a subsample of matched pairs based on treated wallets' Bonk returns during the post period. In each month, we define Bonk return for treated wallets as the cumulative return on the airdropped Bonk position from the airdrop date to the liquidation date, using Bonk prices observed up to the previous month. If the airdropped Bonk has not been fully liquidated by the end of the sample, we use the unrealized gain as of the last sample month. Column (1) restricts the sample to treatment-control pairs in which the treatment wallet's Bonk return is non-positive (≤ 0%) in every post-period month. Columns (2) and (3) construct analogous subsamples using 200% and 1,000% as thresholds for the treatment wallet's pre-period Bonk return, respectively. T-statistics are presented in parentheses below coefficient estimates. Standard errors are clustered at the wallet level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Volume	(1)	(2)	(3)
Treatment × Post	1.230***	1.187***	1.129***
	(18.007)	(18.490)	(21.062)
Observations	58,494	70,001	109,559
Adjusted R2	0.423	0.419	0.415
Panel B: Volatility	(1)	(2)	(3)
Treat ×Post	0.028***	0.025***	0.025***
	(5.862)	(5.448)	(6.580)
Observations	22,859	27,118	40,549
Adjusted R2	0.260	0.261	0.254
Sample	Bonk return ≤ 0%	Bonk return ≤ 200%	Bonk return ≤ 1,000%
Wallet × Pair FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes

Table 8: Performance Analysis

This table reports performance comparisons between treatment and control wallets during the post-event period. The volume-weighted 14 (7)-day return is defined as the volume-weighted average return of purchased tokens minus that of sold tokens over the subsequent 14 (7) days. The equally weighted 14 (7)-day return is constructed analogously using equal weights. All return variables are expressed in percentage terms. T-statistics are presented in parentheses below coefficient estimates. Standard errors are clustered at the wallet level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Volume-weighted 14-day return	(2) Volume-weighted 7-day return	(3) Equally-weighted 14-day return	(4) Equally-weighted 7-day return
Treatment	-0.634***	-0.272***	-0.558***	-0.326***
	(-4.668)	(-3.627)	(-4.338)	(-3.996)
N	283,032	283,032	283,032	283,032
Adjusted R2	0.0059	0.0028	0.0044	0.0033

Table 9: Summary Statistics for Solana-focused Cryptocurrencies

This table presents summary statistics for Solana-focused cryptocurrencies, as described in Section 5.1. *Avg. monthly volume* is the average trading volume per month computed based on the past 30 days as of the airdrop announcement date. (unit: $1,000) *Token Volatility* is the average absolute daily log return from crypto inception to 30 days prior to the airdrop announcement (December 8, 2022). *Corr. with Doge* is the correlation between a cryptocurrency's daily log returns and those of Doge over the same period. *Corr. with Meme* is computed analogously, using a market-cap-weighted index of major meme coins described in Section 5.1.

Variables	**N**	**Average**	**St. dev**	**Median**	**p10**	**p25**	**p75**	**p90**
Avg. monthly volume ($1,000)	230	1063.715	9137.220	33.639	4.637	7.881	151.873	562.685
Token Volatility	230	0.076	0.090	0.056	0.025	0.038	0.088	0.134
Corr. with Doge	230	0.165	0.156	0.139	-0.002	0.061	0.251	0.370
Corr. with Meme	230	0.171	0.161	0.143	0.006	0.068	0.258	0.402

Table 10: Impact of the Bonk Airdrop on Token Prices and Trading Volume

This table presents regression results analyzing the market-wide impact of the Bonk airdrop. *Token Volatility* is the average absolute daily log return from crypto inception to 30 days prior to the airdrop announcement (December 8, 2022). *Corr with Doge* is the correlation between a token's daily log returns and those of Doge over the same period. *Corr with Meme* is computed analogously using the meme index described in Section 5.1. Panel A reports regressions where the dependent variable in Columns (1)-(3) ((4)-(6)) is the log cumulative return from one (seven) day(s) before the airdrop start date to one (seven) day(s) after the airdrop end date. Panel B presents difference-in-differences treatment effects. The pre-period runs from 7 days before the announcement to 1 day before the airdrop. The post-period runs from the airdrop start date to the airdrop end date plus 2 (4) weeks in odd (even) columns. The dependent variable in Panel B is a market-adjusted log normalized trading-volume measure. Specifically, within each period, we compute log[(1 + average daily trading volume) / (1+average trading volume in the pre-period)] and subtract the analogous quantity constructed from aggregate cryptocurrency market trading volume over the same period. T-statistics are presented in parentheses below coefficient estimates. Standard errors are heteroskedasticity-robust in Panel A and clustered at the cryptocurrency level in Panel B. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Price	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	Market-adjusted Log $\left(\frac{P_{airdrop\ end+1}}{P_{airdrop\ start-1}}\right)$			**Market-adjusted Log** $\left(\frac{P_{airdrop\ end+7}}{P_{airdrop\ start-7}}\right)$		
Log(Token Volatility)	0.146*** (3.091)			0.138** (2.261)		
Corr. with Doge		0.527** (2.582)			0.917*** (4.135)	
Corr. with Meme			0.506** (2.534)			0.880*** (4.036)
Constant	0.350** (2.442)	-0.153*** (-3.521)	-0.153*** (-3.438)	0.318* (1.722)	-0.229*** (-4.337)	-0.228*** (-4.270)
N	230	230	230	230	230	230
Adjusted R2	0.057	0.036	0.035	0.032	0.076	0.075

Panel B: Volume	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	Market-adjusted log normalized trading volume					
Log(Token Volatility) × Post	0.280** (2.292)	0.317** (2.546)				
Corr. with Doge × Post			1.045* (1.818)	1.158** (2.014)		
Corr. with Meme × Post					1.030* (1.832)	1.155** (2.059)
N	460	460	460	460	460	460
Adjusted R2	0.031	0.021	0.026	0.014	0.027	0.015
Cryptocurrency FE	Yes	Yes	Yes	Yes	Yes	Yes
Post Period Window	[airdrop start, airdrop end +2 weeks]	[airdrop start, airdrop end +4 weeks]	[airdrop start, airdrop end +2 weeks]	[airdrop start, airdrop end +4 weeks]	[airdrop start, airdrop end +2 weeks]	[airdrop start, airdrop end +4 weeks]

Table 11: Impact of Meme Coin Listings on Centralized Exchanges

This table reports results from staggered difference-in-differences analyses examining the market-wide impact of meme coin listings on centralized exchanges. The sample includes 115 listing events for six major meme coins—Doge, Shiba Inu, Floki, Bonk, Pepe, and WIF—occurring between June 2017 and January 2025. The dependent variable is the natural logarithm of weekly trading volume, normalized by the volume in the week preceding each event. Panel A uses exchange × week level data, while Panel B uses exchange × token × week level data. *Treatment* is an indicator equal to 1 for exchanges on which the listed meme coin appears, and 0 otherwise. *Post* is an indicator equal to 1 for weeks following the listing event, and 0 otherwise. *Token Volatility* is defined as the average absolute daily log return of a token from January 1, 2016 to four weeks before the event. *Corr. with Doge* is the correlation between a cryptocurrency's daily log returns and those of Doge over the same period. *Corr. with Meme* is computed analogously, using the daily log returns of a market-cap-weighted meme coin index composed of the six major meme coins. T-statistics are presented in parentheses below coefficient estimates. Standard errors are clustered at the event × exchange level in Panel A and at the event × exchange × token level in Panel B. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Exchange × Week Level	**(1)**
	Volume
Treatment × Post	0.175**
	(2.41)
N	6,749
Adjusted R2	0.417
Event × Exchange FE	Yes
Exchange × Week FE	Yes

Panel B: Exchange × Token × Week Level	**(1)**	**(2)**	**(3)**	**(4)**
	Volume			
Treatment × Post	0.077***	0.093	-0.037	-0.026
	(3.585)	(1.137)	(-0.716)	(-0.513)
Treatment × Post × Log(Token Volatility)		0.005		
		(0.196)		
Treatment × Post × Corr. with Doge			0.332**	
			(2.297)	
Treatment × Post × Corr. with Meme				0.307**
				(2.129)
N	52,359	52,359	52,359	52,359
Adjusted R2	0.566	0.566	0.567	0.567
Event × Exchange × Token FE	Yes	Yes	Yes	Yes
Exchange × Week FE	Yes	Yes	Yes	Yes

Internet Appendix of

Meme-Driven Gamification

A. Data construction methodology

A.1. Data construction methodology for Bonk airdrop analysis

We collect Solana blockchain data from Dune Analytics and Solscan for our primary analysis on the Bonk airdrop.

Daily cryptocurrency prices on the Solana blockchain are constructed using two distinct measures: volume-weighted average prices (VWAP) and end-of-day (EOD) closing prices, both referenced to GMT. We employ VWAP to compute token volatility and to analyze token price behavior around the airdrop event, as many Solana-focused tokens exhibit pronounced intraday trading activity. The use of VWAP allows us to capture the intraday distribution of trading volume and prices, which is essential for accurately characterizing short-term price dynamics. In contrast, for balance calculations and cross-token correlations, we use end-of-day (EOD) prices to ensure that observations are time-synchronized, which is essential for consistent measurement.

We observe that most of the trading activity on Solana occurs in stablecoin-denominated trading pairs and pairs involving the native Solana token, SOL. The most frequently used quote assets—serving as numeraires—include: (1) USD Coin (USDC), (2) Tether (USDT), (3) USDH (Hubble's stablecoin), (4) UXD (UXD Protocol's stablecoin), (5) DAI (MakerDAO's stablecoin), and (6) Wrapped SOL (WSOL). We classify a wallet as engaging in a purchase of a given token if it exchanges a numeraire asset for that token. Conversely, a sale is defined as an exchange of the target token for a numeraire.

To construct wallet-level portfolio holdings prior to the Bonk airdrop, we extract end-of-month balances for all SPL tokens—the standard for fungible tokens on the Solana blockchain—from Dune Analytics. We complement this with end-of-month balances for SOL, the native token of the

Solana network. For each wallet, we compute monthly portfolio values in U.S. dollars by multiplying token balances by their corresponding end-of-month market prices. A wallet's total balance is defined as the sum of the U.S. dollar values of its SPL and SOL holdings.[1]

To identify NFT holdings, we utilize the Solscan API. We begin by manually collecting the contract-level addresses for the 40 NFT collections cited in the Bonk airdrop eligibility criteria.[2] Of these, we successfully identify 37 collections; the remaining three—Taiyo Gen1, Helions, and Atadia—are excluded due to the inability to reliably determine their collection-level addresses. For each identified collection, we retrieve the addresses of individual NFTs and download their complete transfer histories via the Solscan API. This enables us to determine whether a wallet held any eligible NFTs immediately prior to the Bonk airdrop. On average, each collection in our final sample contains approximately 5,875 unique NFT items

For the token-level analysis, we obtain data from CoinMarketCap via its public API to construct a cryptocurrency market index and measure aggregate daily trading volume. Specifically, we collect token-day level variables, including closing price, market capitalization, and trading volume. CoinMarketCap classifies tokens as either "tracked" or "untracked," with the latter generally lacking reliable data coverage. Accordingly, we restrict our sample to tokens designated as "tracked."[3]

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[1] We note that this balance measure may underestimate actual wallet wealth as it excludes the value of any non-fungible tokens (NFTs) held.

[2] The 40 NFT collections include ABC, Anon Club, Anybodies, BSL, Aurory, Boogles, Catalina Whales, Claynosaurz, Communi3, Crypto Duck Punkz, Cyber Samurai, DCF, Degen Ape, Degen Trash Pandas, Degods, Elixir Vols, Famous Fox Federation, Frakt, Galactic Geckos, Jelly Beasts, Liberty Squirrels, Lily, Lotus Gang, MBB, OG Pengus, Pengu Bots, Orcanauts, Pawnshop Gnomies, Primates, SAMO DAO, Sol Slugs, SMB, Solgods, Solslimes, SVPLY Chail Male bots, TFF, Y00ts, Taiyo gen1, Helions, Atadia.

[3] As a robustness check, we also construct the cryptocurrency market index and aggregate daily trading volume using the full sample, including tokens labeled as "untracked" by CoinMarketCap. The results of our token-level analyses remain virtually unchanged, indicating that our findings are not sensitive to the exclusion of untracked tokens.

To construct the cryptocurrency market index, we first compute daily simple returns for all sample tokens. On each day, we calculate the prior-day market capitalization-weighted average return across these tokens. The cumulative return of this weighted average return series, beginning on July 1, 2017, defines our cryptocurrency market index. Aggregate daily trading volume is calculated as the sum of trading volumes across all sample tokens for each day.

A.2 Data construction methodology for CEX analysis

Our primary dataset for the centralized exchange (CEX) analysis is structured at the Exchange × Token × Day level and is sourced from the CryptoCompare API. We selected CryptoCompare over Kaiko, a comparable provider, because CryptoCompare typically offers longer time series coverage for identical trading pairs on the same exchange. Moreover, CryptoCompare includes a broader set of cryptocurrency exchanges, allowing us to construct a richer cross-sectional panel dataset. This broader coverage enhances our ability to estimate the effects of meme coin listings with greater precision and to examine heterogeneous treatment effects across exchanges and tokens.[4]

As part of the data preprocessing procedure, we standardize trading volumes by converting them from quote currency units to U.S. dollars to ensure comparability across exchanges and trading pairs. For pairs quoted in tokens, we use daily average token prices provided by the CryptoCompare API, which aggregates prices across exchanges using trading volume as weights. For pairs quoted in fiat currencies, we apply the corresponding daily exchange rates obtained from Refinitiv Eikon.

[4] For a comparison of Kaiko and CryptoCompare coverage, Augustin, Rubtsov, and Shin (2023) provide a comprehensive overview of the coverage of Bitcoin and Ethereum trading pairs in both data providers.

Figure A.1: Impact of Bonk Airdrop on Investor Behavior: Dynamic Coefficients for an Alternative Dependent Variable

This figure replicates Figure 1 using an alternative dependent variable, # *Trades*, defined as the natural logarithm of one plus the number of monthly trades. It displays dynamic coefficients from the difference-in-differences analysis, with the x-axis indicating months since the Bonk airdrop and the y-axis representing the estimated coefficients.

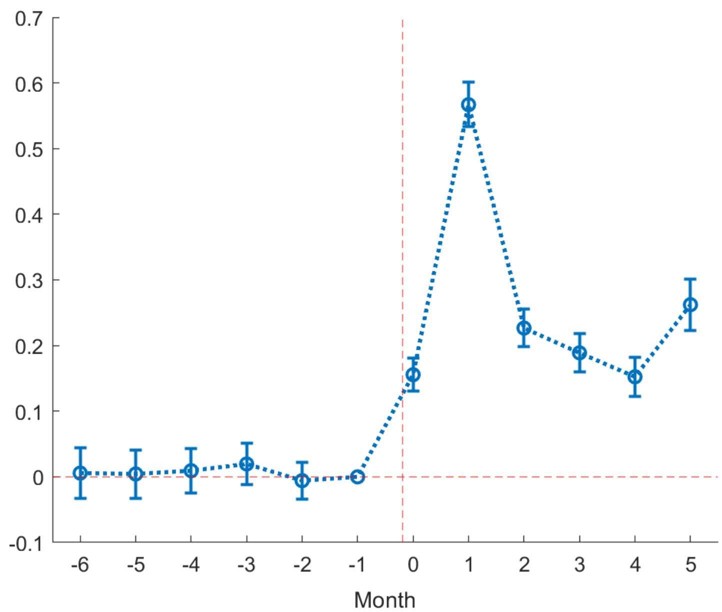
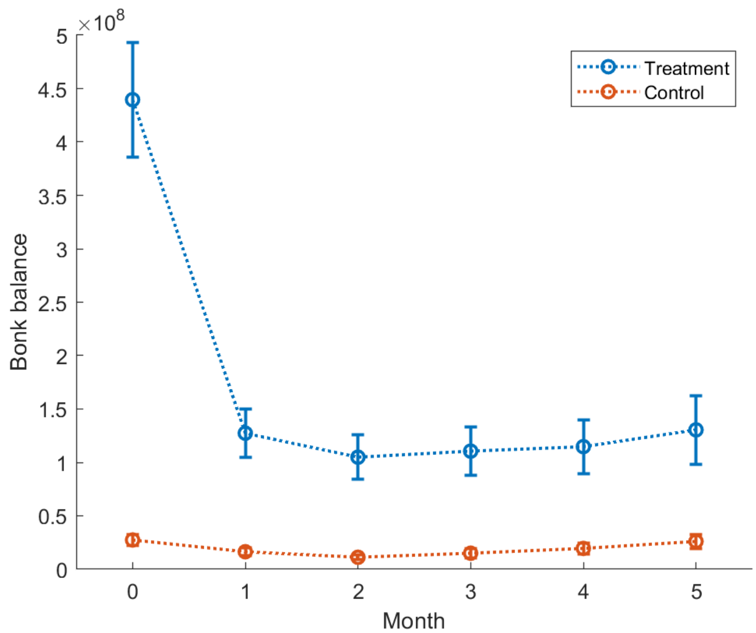


Figure A.2: Endowment Effects

This figure examines whether investors are more likely to retain Bonk tokens following the airdrop. Panel A plots the average number of Bonk tokens held by treatment and control groups, along with corresponding confidence intervals. Panel B shows the monthly fraction of investors in each group who hold Bonk in their wallets.

(a) Panel A: Bonk balance



(b) Panel B: Bonk holders

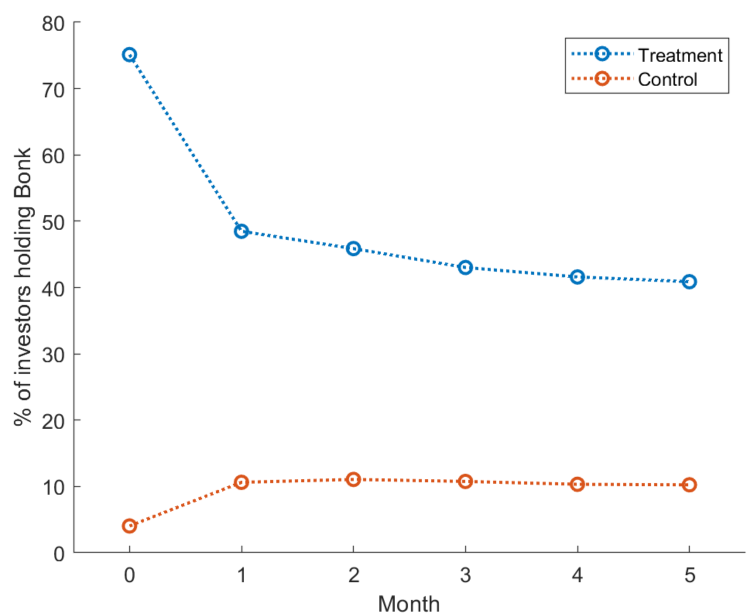
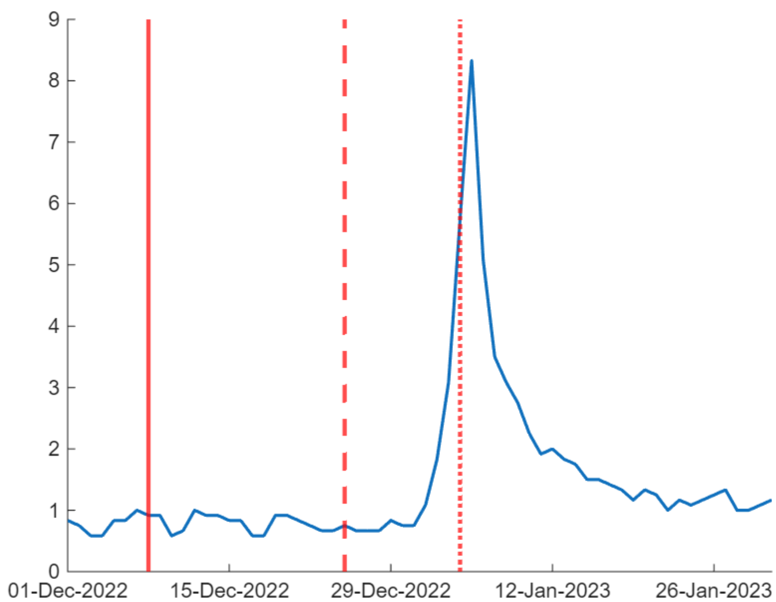


Figure A.3: Google Search Intensity of Crypto-related Keywords

This figure presents Google Trends search intensity for selected keywords around the Bonk airdrop. Panel A plots search intensity for "Bonk," normalized to its level on December 8, 2022, the date of the airdrop announcement. Panel B shows the same for "Bitcoin," "Crypto, "Solana," and "Doge." The three red vertical lines indicate the airdrop announcement, start, and end dates.

(a) Panel A: Google search intensity of "Bonk"



(b) Panel B: "Bitcoin," "Crypto, "Solana," and "Doge"

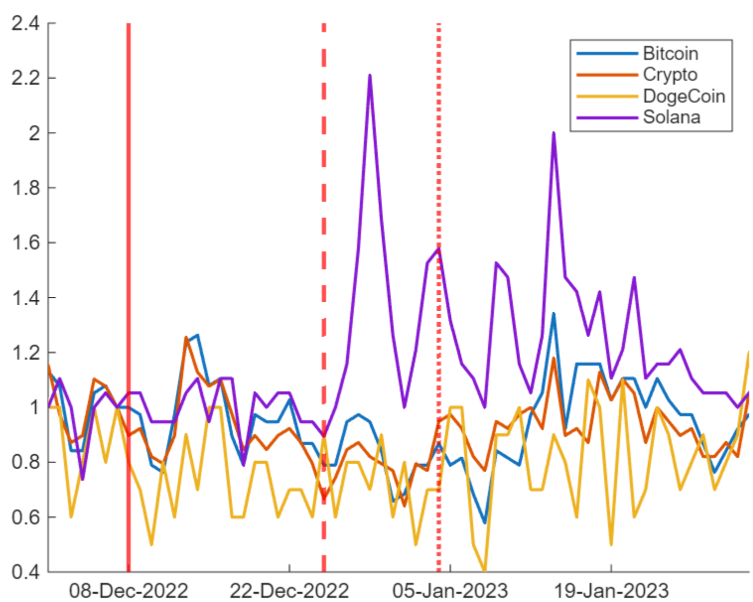


Table A.1: Impact of Bonk Airdrop on Investor Behavior: An Alternative Dependent Variable

This table replicates Table 3 with an alternative dependent variable, # *trades*, which is defined as the logarithm of one plus number of trades in a given month. T-statistics are presented in parentheses below coefficient estimates. Standard errors are clustered at the wallet × treatment-control pair level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

# Trades	**(1)**	**(2)**	**(3)**	**(4)**
Treatment × Post	0.254^{***}	0.263^{***}	0.244^{***}	0.293^{***}
	(20.947)	(19.732)	(13.170)	(30.320)
N	516,621	258,323	258,298	258,334
Adjusted R2	0.469	0.440	0.486	0.444
Sample	All	Avg. balance≤ median balance	Avg. balance> median balance	Matching dist. ≤median dist.
Wallet × Pair FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes

Table A.2: Impact of Bonk Airdrop on Investor Behavior: Poisson Regression

This table presents results from difference-in-differences analyses using the Poisson regressions. Panel A uses monthly dollar trading volume as the dependent variable. Panel B uses the number of trades in a given month as the dependent variable. Each column reports results for a distinct subsample. T-statistics are presented in parentheses below coefficient estimates. Standard errors are clustered at the wallet level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Volume	**(1)**	**(2)**	**(3)**	**(4)**
Treatment × Post	0.378***	0.401***	0.366***	0.716***
	(7.472)	(4.916)	(6.476)	(11.397)
N	516,621	258,323	258,298	258,334
Adjusted R2	0.620	0.634	0.579	0.686
Panel B: # Trades	**(1)**	**(2)**	**(3)**	**(4)**
Treatment × Post	0.319***	0.364***	0.282***	0.642***
	(9.705)	(8.707)	(6.635)	(17.138)
N	516,621	258,323	258,298	258,334
Adjusted R2	0.581	0.576	0.582	0.601
Sample	All	Avg.balance≤ median balance	Avg.balance> median balance	Matching dist. ≤median dist.
Wallet × Pair FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes

Table A.3: Filtering Process for Event–Exchange Sample

Filtering Process	Reasoning	# Events	# Exchanges in Treatment	# Exchanges in Control	#(Event, Exch) Pairs
0. Starting sample (Exchange age ≥8 weeks as of an event date.)	This process ensures that the effect is not driven by the initial launch of exchanges.	148	71	133	5731
1. Exclude control exchanges whose absolute log volume difference from the treatment exchange during days –28 to –1 relative to the event date exceeds 100%.	This process ensures that control exchanges are comparable to treatment exchanges in terms of prior trading volume.	128	68	76	903
2. Select control exchanges with total trading volume during the period from day –28 to day –1 relative to the event exceeding $100,000.	This process ensures that the results are not driven by exchanges with minimal trading activity.	123	64	70	887
3. Exclude duplicate events in which multiple meme coins are listed on the same exchange on the same date.	This process ensures that our results are not inflated by the inclusion of duplicate events.	117	64	70	853
4. Exclude duplicate events where another meme coin was listed on the same exchange within the preceding four weeks.	This process ensures that later events are not influenced by earlier events.	115	64	70	846