Journal of
*Risk and Financial Management*

MDPI

*Article*

# A Thin Conduit: Market Segmentation, Credit-Stress Transmission, and Flow Fragility in Tokenized Real-World Assets

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**Abstract**

Tokenized real-world-asset (RWA) markets, in which blockchain tokens grant claims on off-chain loans, Treasury portfolios, and listed securities, reached USD 9.26 billion in May 2026 across twelve public-chain protocols. Using 9081 protocol-day observations from DefiLlama and FRED (December 2020–June 2026), we ask how closely weekly changes in total value locked (TVL), our proxy for net flows, track traditional credit conditions. The evidence is suggestive. A one-percentage-point widening of the Moody's Baa default spread is associated with a 0.36 log-point weekly contraction in TVL, stable in sign across stress measures, TVL floors, and leave-one-protocol-out samples, but imprecise with twelve clusters (wild cluster bootstrap $p = 0.15$) and smaller under outlier treatment. The association is concentrated in the five vehicles holding uncollateralized real-economy loans ($-0.62$; five clusters, bootstrap $p = 0.10$–$0.12$) and small among tokenized-Treasury vehicles, but a formal interaction test places the difference at the margin of significance and finds none within the 2023–2026 window in which both groups coexist. Event studies around four stress episodes show economically large but statistically insignificant contractions after Terra/Luna and FTX and none after the 2024 yen-carry unwind. We offer market segmentation as one interpretation and draw cautious implications for supervision.

**Keywords:** tokenization; private credit; real-world assets; decentralized finance; market segmentation; financial stability; credit spreads

**JEL Classification:** G12; G14; G21; G23; O33

## 1. Introduction

Tokenized real-world assets (RWAs), fungible tokens on a public blockchain that confer a legal claim on an off-chain portfolio of loans, Treasury bills, or listed securities, have become one of the largest non-stablecoin segments of on-chain finance. Across the twelve protocols examined in this paper, aggregate total value locked (TVL) rose from below USD 0.1 billion in mid-2021 to a peak of USD 9.26 billion on 12 May 2026, with USD 8.41 billion outstanding on 30 June 2026. Because the cash flows behind these tokens originate in the traditional economy, the segment sits at the seam between two financial systems, and whether shocks cross that seam motivates the financial-stability agenda on tokenization set out by the Financial Stability Board (FSB, 2024), the BIS Committee on Payments and Market Infrastructures (BIS CPMI, 2024), the Federal Reserve Board (Carapella et al., 2023), and the Bank for International Settlements (BIS, 2025, 2026). It has not, to our knowledge, been examined empirically across protocols.

Check for updates

Academic Editor: Thanasis Stengos

Received: 11 October 2026
Revised: 27 October 2026
Accepted: 28 October 2026
Published: 1 November 2026

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This paper assembles a protocol-day panel of 9081 observations for twelve public-chain RWA credit and yield protocols between December 2020 and June 2026, drawn from the DefiLlama and FRED open-data endpoints and verified observation by observation against the live sources. Five of the protocols (Goldfinch, Clearpool, TrueFi, Credix, and Ribbon Lend) are lending vehicles whose underlying assets are uncollateralized loans to identified real-economy borrowers. One, Maple, lends to institutional borrowers against digital-asset collateral. Six (Ondo Yield Assets, Ondo Global Markets, Anemoy, Centrifuge, Clearpool TPool, and Maple's RWA cash-management pool) are vehicles whose underlying assets are tokenized Treasury portfolios, tokenized funds, or listed securities. The distinction organizes this paper's main result. We complement the panel with protocol characteristics discernible from open sources (underlying-asset type, multi-chain footprint, age, and scale) and pose three questions. First, what fraction of the traditional credit cycle is transmitted into weekly on-chain flows? Second, are crypto-market factors or traditional credit factors more explanatory of these flows? Third, how did protocol-level TVL change around four exogenous stress events: the Terra/Luna collapse of May 2022, the FTX failure of November 2022, the joint Silicon Valley Bank (SVB) failure and USD Coin (USDC) depeg of March 2023, and the yen-carry unwind of August 2024?

Three findings emerge, in order of identification strength, and all three are stated with the caution that twelve protocol clusters impose. First, traditional credit stress is associated with weekly TVL changes through a thin and imprecisely estimated channel. A one-percentage-point widening of the Moody's Baa default spread is associated with a 0.36 log-point contraction in weekly TVL. The sign is stable across alternative stress measures, TVL floors, and leave-one-protocol-out samples, and the response is contemporaneous rather than lagged, but the estimate is not precise: the $p$-value is 0.07 on a t distribution with eleven degrees of freedom and 0.15 under a wild cluster bootstrap over the twelve protocol clusters, and the magnitude falls by a quarter once the weekly log-change is winsorized and by two-thirds once the 26 largest weekly changes are trimmed. The association is concentrated where credit risk sits. Among the five vehicles whose underlying assets are uncollateralized real-economy loans, the coefficient is $-0.62$ (five clusters; $p = 0.05$ under $t(4)$ and 0.10 to 0.12 under the wild cluster bootstrap), its sign survives every outlier treatment, and neither bitcoin nor ether returns displace it; among the six tokenized-Treasury and securities vehicles the baseline coefficient is $-0.10$ ($p = 0.44$). A formal interaction test, however, places the difference between the two groups at the margin of significance over the full window ($p = 0.06$ under $t(10)$, 0.11 under the bootstrap) and finds no difference within the 2023–2026 window in which both groups are observed together. Second, in the full panel weekly bitcoin and ether returns carry comparable, equally marginal explanatory power, and neither factor family dominates. Third, event-study cross-sections of four to five protocols show economically large contractions after the crypto-native shocks (Terra/Luna, FTX), a smaller one after SVB/USDC, and none after the yen-carry unwind, but no event response is statistically distinguishable from zero on a $t$-test, and the Terra/Luna and FTX means each depend on a single protocol.

A fourth result is a null. Cross-sectional differences in weekly TVL volatility are not robustly explained by verifiable protocol characteristics (underlying-asset type, number of settlement chains, age, and scale) once macro conditions or week effects are controlled for; the one stable predictor is the default spread itself.

Throughout, the estimates are framed as associations rather than causal effects: underlying-asset type and distribution choices are made at inception, unobservable sponsor quality plausibly correlates with both those choices and TVL changes, and a cross-section of twelve limits power (Section 6.6). Because inference with so few clusters is approximate, every clustered $p$-value in this paper is computed on a t distribution with $G - 1$ degrees of

freedom, G the number of clusters in the binding dimension, and is reported in addition to a wild cluster bootstrap $p$-value that governs the significance markings (Section 5.2). We also distinguish throughout between changes in TVL, which we observe, and investor flows, which we infer only where revaluation, accrued yield, and reporting-coverage components can be shown to be immaterial or are removed (Section 4.1).

This paper contributes to three bodies of literature. It extends the empirical literature on decentralized-finance (DeFi) lending (Aramonte et al., 2022; Cornelli et al., 2025) from crypto-collateralized money markets to vehicles whose underlying risk is off-chain and whose borrowers are legally identified. It complements the theory of token-based platforms (Cong et al., 2021, 2022) and of DeFi fragility (Chiu et al., 2023) with evidence on how tokenized real-economy claims behave under stress. And it speaks to the official sector's tokenization-risk agenda, whose central question is whether tokenization creates a conduit for shocks between crypto and traditional markets (Carapella et al., 2023); the evidence suggests that any such conduit is at present narrow and imprecisely measured and that the largest disturbances observed in the sample originated inside the crypto ecosystem rather than in traditional credit markets.

Section 2 describes the setting and related work; Section 3 states the hypotheses; Sections 4 and 5 describe data and design; Sections 6 and 7 report results and robustness; and Section 8 concludes.

## 2. Institutional Background and Related Literature

### *2.1. The Market for Tokenized Real-World Asset Credit*

A tokenized RWA vehicle interposes a bankruptcy-remote special-purpose vehicle (SPV) or regulated fund between the underlying asset and the token holder: the SPV holds the assets, and the tokens confer a contractual claim on its cash flows rather than direct ownership of the collateral (S&P Global Ratings, 2024). The underlying exposures are heterogeneous. Goldfinch, Clearpool, TrueFi, Credix, and Ribbon Lend intermediate uncollateralized loans to identified borrowers (emerging-market fintech lenders, trading firms, and middle-market companies) and settle in stablecoins. Maple lends to institutional borrowers against digital-asset collateral at ratios above 150 percent and, since May 2024, accepts permissionless deposits through its Syrup pools alongside its institutional pools, which require know-your-customer (KYC) onboarding; its separately listed RWA pool is a cash-management vehicle invested in U.S. Treasury bills. Ondo Yield Assets provides exposure to short-term U.S. Treasury portfolios; Ondo Global Markets, launched in September 2025, provides exposure to listed equities and exchange-traded funds (ETFs); Anemoy manages tokenized Treasury and credit funds distributed through Centrifuge; Centrifuge hosts tokenized funds spanning Treasuries, collateralized loan obligations, and private credit; and Clearpool's TPool invests in Treasuries. DefiLlama classifies the lending vehicles under its lending categories and the remainder under its RWA category. We retain the market's collective label, "tokenized private credit", but treat the groups separately wherever the distinction matters.

At the aggregate peak on 12 May 2026, four protocols, namely, Ondo Yield Assets (USD 3.31 billion), Maple (USD 2.14 billion), Centrifuge (USD 1.64 billion), and Anemoy (USD 1.14 billion), accounted for USD 8.23 billion, or 89 percent, of the USD 9.26 billion total. For scale, RWA.xyz reported USD 18.91 billion of active on-chain private credit in November 2025, roughly three-quarters of it attributable to Figure's home-equity book on the permissioned Provenance ledger, outside the public-chain universe studied here. Ethereum hosts eleven of the twelve protocols and Solana one (Credix); DefiLlama records TVL for the protocols on between one and twelve settlement chains. Because the underlying-

asset type and multi-chain footprint are fixed at the protocol level, their coefficients are identified from the cross-sectional variation alone.

### 2.2. Related Literature

This paper draws on three bodies of literature. The first studies DeFi lending. Schär (2021) provides an institutional perspective; Aramonte et al. (2022) show that over-collateralization substitutes for borrower information; Chiu et al. (2023) formalize how rigid smart-contract terms lead to self-fulfilling withdrawal equilibria; Cornelli et al. (2025) show with Aave transaction data that deposit supply is primarily yield-seeking; Aquilina et al. (2024) and Makarov and Schoar (2022) survey the sector; and J. Liu et al. (2023) dissect the Terra run that provides one of our stress events. This literature concerns crypto-collateralized lending, where the underlying risk is on-chain and the borrower anonymous.

The second concerns tokenization and stablecoins. Cong and He (2019) and Cong et al. (2021, 2022) provide the theory of smart-contract platforms and token adoption. Griffin and Shams (2020) show that stablecoin issuance moves crypto prices; Anadu et al. (2024) show that stablecoins exhibit money-market-fund-like flights to safety during the 2022 and 2023 runs; Ahmed et al. (2024) show that reserve transparency and reserve quality have distinct effects on run risk; Aldasoro et al. (2025) show that crypto shocks move stablecoin capitalization but not money market funds or traditional financial variables, whereas monetary policy moves both; and Ahmed and Aldasoro (2025) document that stablecoin reserve flows are large enough to move Treasury-bill yields. The settlement layer on which tokenized credit rests is, therefore, itself a channel of stress, already coupled to the safe-asset market. Carapella et al. (2023), the BIS CPMI (2024), the FSB (2024), the IMF (2025), and the BIS (2025, 2026) frame the policy questions; Mafrur (2025) and Krause (2026) examine RWA liquidity and contagion channels, but neither provides cross-protocol evidence on flows.

The third studies private credit and non-bank intermediation, the off-chain benchmark for the vehicles examined here. Block et al. (2024) and the IMF (2024) characterize the market and its risks; Chernenko et al. (2022) and Davydiuk et al. (2024) study direct lending; Aldasoro and Doerr (2025) show that secured direct loans now exceed unsecured ones in the U.S. market, which makes the collateralized/uncollateralized distinction drawn below a live one; Buchak et al. (2018) study fintech origination; and Goldstein et al. (2017) and Falato et al. (2021) show that corporate-bond funds exhibit self-reinforcing outflows under stress. Whether tokenized vehicles inherit those dynamics from their underlying assets or from the crypto ecosystem in which they trade is the empirical question addressed below.

## 3. Hypotheses

Three hypotheses organize the analysis. All are stated as associations, because protocol characteristics are chosen at inception rather than assigned.

**H1** (Credit-stress transmission)**.** *Widening traditional credit spreads are associated with contractions in weekly tokenized-RWA TVL, with the response concentrated in vehicles whose underlying assets carry real-economy credit risk and attenuated relative to what integrated markets would generate.*

**H2** (Factor structure)**.** *Weekly TVL changes co-move with crypto-market factors at least as strongly as with traditional credit factors, consistent with a marginal investor whose stochastic discount factor loads on crypto rather than credit-cycle risk (Y. Liu et al., 2022).*

**H3** (Stress-event asymmetry). *Protocol-level TVL contracts after exogenous stress events, with larger responses to events originating inside the crypto ecosystem (Terra/Luna, FTX, USDC depeg) than to events originating in traditional or global macro markets (SVB, yen carry).*

A fourth conjecture, that weekly TVL volatility varies systematically with observable protocol characteristics, is examined descriptively in Section 6.1 using only open-source characteristics: underlying-asset type, number of settlement chains, age, and scale.

## 4. Data

### 4.1. Sources

Three open sources are combined. DefiLlama's public application programming interface (API; api.llama.fi) provides daily protocol-level TVL; the initial universe comprises the thirteen public-chain RWA credit and yield protocols listed under DefiLlama's RWA, RWA-lending, lending, and uncollateralized-lending categories and cross-referenced against S&P Global Ratings (2024) coverage. TVL is DefiLlama's U.S.-dollar valuation of the assets held by each protocol; for stablecoin-denominated lending pools and tokenized-Treasury products its weekly log-change measures net flows plus accrued yield and write-downs, whereas for Ondo Global Markets it also reflects the prices of the underlying equities; the next paragraph bounds these components and Section 7 removes them. The FRED open endpoint provides the Moody's Baa corporate bond yield less the ten-year Treasury yield (BAA10Y), the Secured Overnight Financing Rate (SOFR), the CBOE Volatility Index (VIXCLS), and the Chicago Fed NFCI. Yahoo Finance provides daily bitcoin and ether closes. Protocol characteristics are restricted to those that can be verified from open sources: the settlement chain and the number of chains on which DefiLlama records positive TVL, protocol age (from the first DefiLlama observation), scale (the protocol-week mean of TVL), and the type of underlying asset, classified from DefiLlama's protocol descriptions and the protocols' own product documentation under the protocol set out in Appendix B. Every downloaded series was re-fetched from its source and matched observation by observation; the only discrepancies are routine NFCI revisions of, at most, 0.014 index points. The data extracts, the classification file, and the code that reproduce every table and figure are provided as Supplementary Materials.

Changes in TVL are not the same as investor flows, and the distinction matters for everything that follows. A weekly change in DefiLlama's TVL can reflect deposits and withdrawals, but also accrued yield, write-downs of defaulted loans, revaluation of non-dollar assets, protocol migrations, and changes in reporting coverage. Four features of the sample bound the non-flow components. First, the five real-economy credit vehicles, Maple's RWA pool, Clearpool TPool, and Ondo Yield Assets are denominated in U.S.-dollar stablecoins or Treasury bills, so price revaluation is absent and accrued yield is small: at an annual yield of 5 to 10 percent, accrual contributes 0.1 to 0.2 log-percent per week, against a median absolute weekly change of 8.0 log-percent for the credit vehicles and 2.0 for the Treasury group; Section 7 nets accrued yield out of the Treasury-group series and finds the estimates unchanged. Second, revaluation through crypto prices, which is relevant for Maple's ether-denominated pools and, in principle, for any pool holding non-stablecoin collateral, is bounded by the coefficients on bitcoin and ether returns in Sections 6.3 and 7, which are small and insignificant for the credit vehicles; Maple is for this reason reported separately as a single-protocol series. Third, Ondo Global Markets reflects the prices of listed equities, which Section 7 removes using the S&P 500 return. Fourth, changes in reporting coverage and protocol migrations generate gaps in the daily series, which Section 4.2 flags and Section 7 excludes. Write-downs of defaulted loans cannot be separated from withdrawals in protocol-level data, and because defaults cluster

in stress periods they bias the credit-stress coefficient toward larger contractions for the credit vehicles. We, therefore, refer to the dependent variable as the change in TVL, state the hypotheses in those terms, and use the word flows only where the components above are immaterial or removed. The title retains the market's own vocabulary.

The credit-stress measure is BAA10Y, the canonical default spread of the asset-pricing literature (Chen et al., 1986; Fama & French, 1989). The ICE BofA option-adjusted spread family, a natural alternative, is freely available from FRED only for a rolling three-year window; over the 703 trading days on which both are observed (September 2023–June 2026), BAA10Y correlates 0.54 in levels and 0.63 in daily changes with the high-yield option-adjusted spread, and all specifications, therefore, use the complete panel.

### 4.2. Sample Construction

Four sequential filters are applied. Protocols with fewer than 30 days of TVL history are dropped; protocol-days with TVL at or below USD 1 million are dropped, which removes dust and post-shutdown periods; daily log-changes in TVL are winsorized at the 1st and 99th percentiles within protocol; and a 30-day rolling volatility is computed requiring at least ten valid observations, each protocol entering the panel on its twentieth day of DefiLlama coverage. The resulting panel contains 9081 protocol-days for twelve protocols between 11 December 2020 and 30 June 2026, aggregating to 1327 protocol-weeks in the volatility panel and 1332 in the panel of weekly TVL changes across 291 calendar weeks. The USD 1 million floor is a judgment; Appendix A re-estimates the main specification under floors between USD 0.1 million and USD 10 million.

Huma Finance, whose DefiLlama coverage begins on 27 May 2026, is excluded by the minimum-history filter at the panel build date; Section 7 shows that a rebuilt panel admitting it leaves the estimates essentially unchanged. Survivorship concerns are mitigated because the panel retains every protocol known to have contracted, restructured, or wound down during 2020–2026: Credix, Ribbon Lend, TrueFi, Maple RWA, and the original Clearpool vehicle. These protocols exit the panel when TVL falls below the floor (Table 1), so the panel is unbalanced through both entry and exit. It also carries interior gaps: the USD 1 million floor removes 1606 protocol-days on which a protocol fell below the threshold and later recovered, concentrated in Credix, TrueFi, Goldfinch, and Clearpool. A weekly difference that spans such a gap measures the change across it rather than a one-week change and is a measurement problem distinct from a large but fully observed weekly change. We, therefore, flag every weekly difference as gap-spanning when the protocol's previous observed week is not the immediately preceding calendar week or when any calendar day inside the two-week span is missing from the daily panel; the protocol's first panel week is not flagged. Of the 1332 weekly differences, 111 are gap-spanning (25 across non-adjacent weeks and 86 with missing days inside the span). Of the 26 weekly differences with $|\Delta \log \text{TVL}| > 1$ that the trimmed specifications remove, 7 are gap-spanning, and none is a protocol's first weekly difference; the remaining 19 are large but fully observed changes, concentrated in Maple (7), Goldfinch (5), and TrueFi (5) during 2021–2022. Section 7 reports a specification that excludes the gap-spanning differences separately from the winsorized and trimmed specifications, and the flag is included in the replication package.

Two limits of the universe should be stated. Centrifuge's DefiLlama listing begins on 24 March 2025 with the protocol's V3 relaunch; its legacy Tinlake pools are listed separately with a DefiLlama peak of only USD 9.0 million and are examined as a robustness check in Section 7. Large permissioned-ledger books such as Figure's fall outside the public-chain universe.

The two halves of the panel are observed on different calendars: TVL on every calendar day, the FRED series on business days only. The last observed close is carried forward across

the 645 of 2028 calendar days without a quote (580 weekend days and 65 weekday holidays; longest run three days), with no backward filling; because every specification uses weekly means, a carried-forward value enters only through its weight in a seven-day average.

**Table 1.** Sample coverage and protocol characteristics.

| Protocol | Type | Chains | Panel Days | Panel Start | Panel End | Peak TVL (USD m) |
|---|---|---|---|---|---|---|
| Ondo Yield Assets | Treasury | 12 | 1220 | 15 February 2023 | 30 June 2026 | 3311.99 |
| Maple | Collateralized lending | 2 | 1830 | 8 June 2021 | 30 June 2026 | 3246.04 |
| Centrifuge | Fund | 9 | 439 | 12 April 2025 | 30 June 2026 | 1990.96 |
| Anemoy | Fund | 3 | 545 | 28 December 2024 | 30 June 2026 | 1518.30 |
| Ondo Global Markets | Securities | 3 | 282 | 22 September 2025 | 30 June 2026 | 1203.09 |
| TrueFi | Credit | 2 | 1022 | 11 December 2020 | 9 October 2024 | 813.68 |
| Goldfinch | Credit | 1 | 1419 | 14 December 2021 | 30 June 2026 | 53.47 |
| Clearpool TPool | Treasury | 2 | 674 | 9 August 2024 | 30 June 2026 | 50.29 |
| Maple RWA | Treasury | 1 | 293 | 13 June 2024 | 1 April 2025 | 45.24 |
| Ribbon Lend | Credit | 1 | 174 | 23 October 2022 | 14 April 2023 | 32.36 |
| Clearpool | Credit | 7 | 1042 | 30 April 2022 | 1 November 2025 | 23.46 |
| Credix | Credit | 1 | 141 | 26 August 2022 | 2 September 2024 | 3.02 |

Notes: Panel days, start, end, and peak TVL refer to the analysis panel after the filters of Section 4.2 (9081 protocol-days in total); raw DefiLlama coverage extends to 30 June 2026 for every protocol but includes post-shutdown days with TVL below USD 1 million. Type classifies the underlying assets under the protocol of Appendix B: Credit = uncollateralized loans to identified real-economy borrowers; Collateralized lending = loans to institutional borrowers against digital-asset collateral; Treasury = short-term U.S. Treasury portfolios (including Maple's cash-management pool); Fund = tokenized funds spanning Treasuries, collateralized loan obligations, and private credit; Securities = listed equities and ETFs. Chains = number of settlement chains on which DefiLlama records positive TVL up to 30 June 2026. All protocols settle primarily on Ethereum except Credix (Solana). Protocol age is measured from the first DefiLlama observation, except for Centrifuge (1 January 2021, since its earlier Tinlake pools predate the current listing). Sources: DefiLlama; protocol documentation.

### 4.3. Descriptive Statistics

Tables 1 and 2 report coverage and estimation moments. Figure 1 plots aggregate TVL against the default spread over the sample period, and Figure 2 plots the daily TVL of the six largest protocols. Scale heterogeneity is extreme, with panel peak TVL ranging from USD 3 million (Credix) to USD 3.31 billion (Ondo Yield Assets), and daily TVL is correspondingly right skewed, with a mean of USD 349 million against a median of USD 32 million. Winsorized daily log-changes are centered on zero but heavy-tailed; the 30-day rolling volatility averages 8.5 percentage points. Panel C of Table 2 shows that, in the raw cross-section, real-economy credit vehicles are more volatile (correlation of 0.65, with mean weekly volatility) and smaller (−0.74 with log scale); because the type and scale are so collinear across twelve protocols, the regressions of Section 6.1 cannot separate them cleanly.

**Table 2.** Descriptive statistics and protocol-level correlations.

| **Panel A. Daily estimation panel ($N$ = 9081 protocol-days)** | | | | | |
|---|---|---|---|---|---|
| **Variable** | **Mean** | **Std. Dev.** | **P25** | **Median** | **P75** |
| TVL (USD millions) | 348.69 | 636.13 | 4.17 | 32.28 | 361.06 |
| Δlog TVL, winsorized (%) | 0.096 | 12.545 | −0.671 | 0.001 | 1.010 |
| 30-day rolling volatility (%) | 8.537 | 9.611 | 2.070 | 4.595 | 10.927 |
| BAA10Y default spread (%) | 1.786 | 0.237 | 1.620 | 1.750 | 1.920 |
| SOFR (%) | 3.799 | 1.638 | 3.630 | 4.310 | 5.050 |
| Protocol age (days) | 684.4 | 512.8 | 251.0 | 572.0 | 993.0 |

**Table 2.** *Cont.*

| Panel B. Protocol-level characteristics (N = 12 protocols) | | | | |
|---|---|---|---|---|
| **Characteristic** | **Mean** | **Std. Dev.** | **Min** | **Max** |
| Real-economy credit vehicle (0/1) | 0.42 | 0.51 | 0 | 1 |
| Number of settlement chains | 3.67 | 3.65 | 1 | 12 |
| Mean TVL (USD millions) | 318.5 | 397.9 | 1.5 | 1121.9 |
| Mean weekly volatility (%) | 6.32 | 5.38 | 1.09 | 13.86 |

| Panel C. Pairwise correlations across protocols (N = 12) | | | | |
|---|---|---|---|---|
| | **Real-Economy Credit** | **Log Chains** | **Log TVL** | **Mean Weekly Vol.** |
| Real-economy credit (0/1) | 1.00 | — | — | — |
| log(number of chains) | −0.38 | 1.00 | — | — |
| log(mean TVL) | −0.74 | 0.62 | 1.00 | — |
| Mean weekly vol. (%) | 0.65 | −0.35 | −0.41 | 1.00 |

Notes: Panel A reports moments of the daily analysis panel; Δlog TVL is computed after the USD 1 million floor is applied and winsorized at the within-protocol 1st and 99th percentiles (the rolling-volatility row covers the 9071 protocol-days with at least ten prior valid observations). Panel B reports the cross-protocol distribution of the characteristics used in Table 3; mean TVL and mean weekly volatility are protocol-level averages over the protocol's weeks in the panel, the latter being the weekly standard deviation of winsorized daily log-changes in percentage points. Panel C reports Pearson correlations across the twelve protocols. Sources: DefiLlama; FRED.




**Figure 1.** Aggregate tokenized-RWA TVL and the default spread, 2020–2026. The navy line and shaded area plot aggregate TVL across the twelve sample protocols (left axis, USD billions); the crimson line plots the Moody's Baa yield less the ten-year Treasury yield (BAA10Y, right axis). Grey bands span the four stress windows of Section 6.4 (three days before to twenty-one days after each event): Terra/Luna (10 May 2022), FTX (8 November 2022), SVB/USDC (10 March 2023), and the yen-carry unwind (5 August 2024). Sources: DefiLlama; FRED.

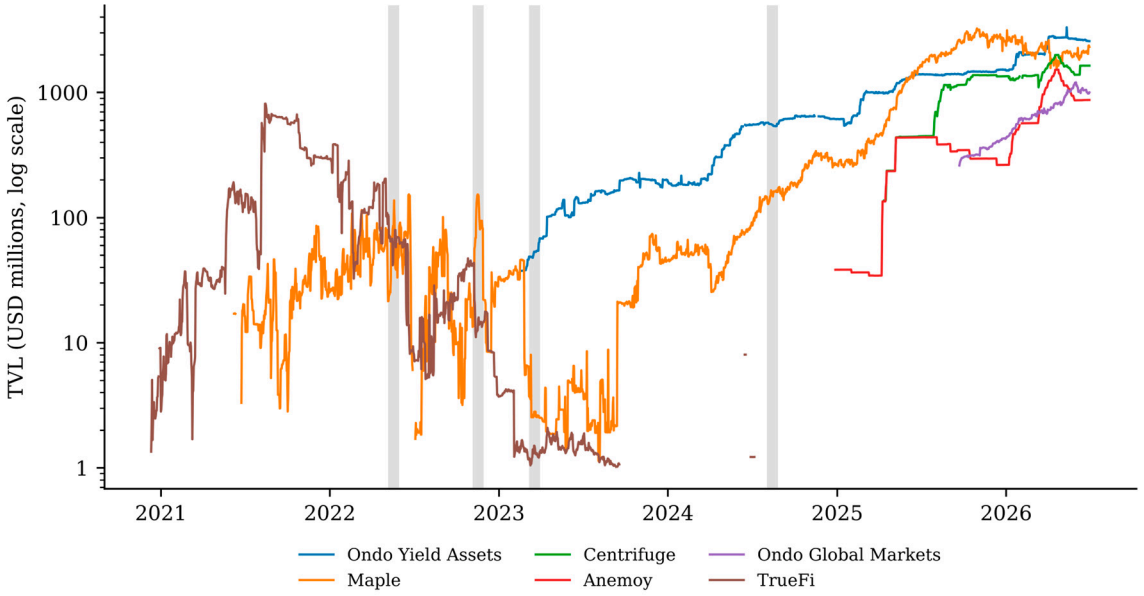



**Figure 2.** TVL trajectories of the six largest protocols. Daily TVL on a logarithmic scale (USD millions) for the six protocols with the highest peak TVL; lines break where a protocol falls below the USD 1 million floor. Grey bands mark the stress windows of Figure 1. Source: DefiLlama.

**Table 3.** Weekly TVL volatility and protocol characteristics.

| Variable | (1) Pooled OLS | (2) +Macro | (3) +Week FE | (4) Random Effects | (5) +Macro, Excl. Maple |
|---|---|---|---|---|---|
| Real-economy credit (0/1) | 0.52 (7.13)<br>[0.943; 0.947] | −5.89 (4.95)<br>[0.259; 0.937] | −7.88 (3.22)<br>[0.032; 0.246] | −1.49 (4.45)<br>[0.744; —] | 7.39 ** (2.80)<br>[0.025; 0.020] |
| log(number of chains) | −0.52 (1.53)<br>[0.741; 0.815] | 0.91 (0.92)<br>[0.343; 0.542] | 0.58 (1.28)<br>[0.659; 0.752] | 0.84 (0.68)<br>[0.243; —] | 1.10 (0.69)<br>[0.144; 0.328] |
| log(Age in days) | −1.11 (0.89)<br>[0.237; 0.279] | 1.38 (1.07)<br>[0.225; 0.593] | 2.34 *** (0.84)<br>[0.017; 0.007] | 0.35 (0.87)<br>[0.698; —] | −0.99 (0.55)<br>[0.100; 0.188] |
| log(TVL) | −0.75 (1.03)<br>[0.480; 0.820] | −1.90 (0.68)<br>[0.017; 0.340] | −1.45 * (0.40)<br>[0.004; 0.094] | −1.73 ** (0.73)<br>[0.038; —] | 0.10 (0.59)<br>[0.874; 0.852] |
| BAA10Y | — | 13.32 ** (3.53)<br>[0.003; 0.010] | — | 8.76 ** (3.16)<br>[0.018; —] | 6.40 ** (2.44)<br>[0.026; 0.030] |
| SOFR | — | −2.41 (0.83)<br>[0.014; 0.216] | — | −1.97 ** (0.89)<br>[0.049; —] | −0.42 (0.84)<br>[0.629; 0.668] |
| Week fixed effects | No | No | Yes | No | No |
| Observations | 1327 | 1327 | 1327 | 1327 | 1063 |
| $R^2$ | 0.060 | 0.272 | 0.517 | — | 0.256 |
| Adjusted $R^2$ | 0.057 | 0.269 | 0.380 | — | 0.252 |

Notes: The dependent variable is the weekly standard deviation of winsorized daily log-changes in TVL, in percentage points, computed after the USD 1 million floor is applied. Real-economy credit equals one for Goldfinch, Clearpool, TrueFi, Credix, and Ribbon Lend (Table 1). Number of chains and age are from DefiLlama; log(TVL) is the log of the protocol-week mean of TVL. Standard errors clustered by protocol (G = 12 clusters; 11 in Column 5) are in parentheses. Brackets report two $p$-values: first on a t distribution with G − 1 degrees of freedom, second from a wild cluster bootstrap by protocol (null imposed, Rademacher weights, 9999 replications); the bootstrap is not available for the random-effects estimator. Significance markings follow the bootstrap $p$-value where available and the t(G − 1) $p$-value in Column (4): *, **, and *** denote 10, 5, and 1 percent. Column (3) includes week fixed effects, which absorb the week-level macro controls. Column (4) uses the random-effects estimator with protocol-clustered standard errors. Sources: DefiLlama; FRED.

## 5. Empirical Design

### 5.1. Weekly Volatility Regressions

The underlying-asset type and the multi-chain footprint do not vary within protocol, so protocol fixed effects would absorb them entirely. We, therefore, report three specifications that preserve their identification: pooled ordinary least squares (OLS) with protocol-clustered standard errors; pooled OLS with week fixed effects, which absorb common time-varying shocks; and a random-effects estimator that accommodates protocol-level unobservables under the assumption that the random component is uncorrelated with the regressors (Baltagi, 2013). The estimating equation is

$$\sigma_{p,w} = \alpha + \beta' \mathbf{X}_p + \gamma' \mathbf{M}_w + \tau_w + \nu_p + \varepsilon_{p,w} \quad (1)$$

where $\sigma$(p,w) is the standard deviation of winsorized daily log-TVL changes for protocol p in week w in percentage points; X(p) collects a real-economy-credit indicator, the log number of settlement chains, log age, and log scale (the protocol-week mean of TVL); M(w) collects week-level macro controls (BAA10Y and SOFR), omitted when week effects $\tau$(w) are included because the latter absorb any week-level regressor; $\nu$(p) is the protocol random component, included in the random-effects specification only; and $\varepsilon$(p,w) is the disturbance. Standard errors are clustered by protocol. In the running text, subscripts are written in parentheses, so that $\sigma$(p,w) denotes the quantity subscripted p,w in Equation (1); the same convention is used for Equations (2) and (3).

### 5.2. Transmission Regression and Inference

The transmission regression is a weekly log-difference specification with protocol fixed effects:

$$\Delta \log(\mathrm{TVL})_{p,w} = \alpha + \beta_1 \Delta \mathrm{BAA10Y}_w + \beta_2 \Delta \mathrm{SOFR}_w + \mu_p + \varepsilon_{p,w} \quad (2)$$

where $\Delta$log TVL(p,w) is the weekly first difference of the protocol-week mean of log TVL, $\Delta$BAA10Y(w) and $\Delta$SOFR(w) are weekly first differences of the default spread and the short rate, and $\mu$(p) absorbs all time-invariant characteristics. The coefficient of interest, $\beta_1$, measures the pass-through of credit stress into weekly TVL changes. Because aggregate TVL is orders of magnitude smaller than the U.S. corporate bond market, reverse causality can be ruled out; the identifying assumption is that weekly spread innovations are exogenous to protocol-level TVL changes. Because the macro regressors do not vary across protocols within a week, protocol-only clustering would understate uncertainty; we report two-way clustered standard errors by protocol and week throughout. Because weekly log-changes contain a few extreme observations associated with launches, vault migrations, and wind-downs, the baseline is reported alongside winsorized (1st/99th percentiles) and trimmed ($|\Delta \log \mathrm{TVL}| \leq 1$) variants.

Turning to inference, the panel has twelve protocol clusters, five in the real-economy credit subgroup and six in the Treasury subgroup, and cluster-robust standard errors are only approximate at these counts (Cameron et al., 2008; MacKinnon & Webb, 2017; MacKinnon et al., 2023). We, therefore, report three devices for every coefficient. The analytic *p*-value uses a t distribution with $G - 1$ degrees of freedom, where G is the number of clusters in the binding dimension (the protocol dimension in every specification). A wild cluster bootstrap imposes the null, resamples protocol clusters with Rademacher weights (9999 replications, seed 42), and, for subsamples with six or fewer clusters, where the Rademacher distribution admits only $2^{G-1}$ distinct draws, additionally with Webb six-point weights. Bootstrap draws that reproduce the sample's test statistic up to floating-point error are counted as rejections, so with five or six clusters the Rademacher *p*-value cannot fall below $2/2^G$, and the Webb variant is the finer instrument. Significance markings in every

table are governed by the bootstrap $p$-value where it is available (the Webb variant when $G \leq 6$) and by the $t(G-1)$ $p$-value otherwise; the convention is stated in each table's notes. Third, because the macro regressors vary only across weeks, we report placebo tests that reassign the weekly macro innovations across weeks. An unrestricted permutation destroys the serial dependence of the innovations (first-order autocorrelations of 0.28 for $\Delta$BAA10Y and 0.36 for $\Delta$SOFR), so we complement it with a block permutation that permutes blocks of eight consecutive weeks (2000 draws) and with the 289 circular shifts of the macro series, both of which preserve the within-block autocorrelation (Künsch, 1989). The permutation tests answer a different question from the cluster bootstrap, namely, whether the time-series alignment of TVL changes with the macro series is unusual, and we do not treat the two as interchangeable.

### 5.3. Event-Study Cross-Sections

For each event date, $T(k)$, and horizon $h \in \{1, 3, 5, 10, 20, 30\}$ days, we compute

$$\Delta \log(\mathrm{TVL})_{p,h,k} = \log(\mathrm{TVL})_{p,T_k+h} - \frac{1}{10}\sum_{s=1}^{10} \log(\mathrm{TVL})_{p,T_k-s} \quad (3)$$

the change in log TVL at horizon h relative to the pre-event mean over days $-10$ to $-1$, in the local-projection spirit of Jordà (2005). We report cross-sectional means and medians, one-sample $t$-statistics with their $p$-values, and 95 percent percentile intervals from 5000 nonparametric bootstrap resamples of the protocol cross-section (seed 42). With four or five protocols per event the bootstrap resamples a tiny cross-section and its percentile intervals are anticonservative; we, therefore, rely on the $t$-test and on the magnitude of the point estimates, report the intervals for completeness only, and re-estimate each cross-section leaving one protocol out. The design compares each protocol with its own pre-event level and does not identify what TVL would have been in the absence of the event; the events are exogenous with respect to protocol design, but contemporaneous protocol-specific developments cannot be ruled out, and the results are descriptive.

## 6. Results

### 6.1. TVL Volatility and Protocol Characteristics

Table 3 reports the volatility regressions, with $t(11)$ and wild cluster bootstrap $p$-values in brackets beneath each coefficient. No protocol characteristic is robustly associated with weekly TVL volatility. The real-economy-credit indicator is +0.5 percentage points in the pooled baseline, $-5.9$ with macro controls, $-7.9$ with week effects, and $-1.5$ under random effects; it is negative at the five percent level under $t(11)$ only with week effects ($p = 0.03$), where the bootstrap $p$-value is 0.25, and positive and significant under both devices only when Maple, the most volatile large vehicle, is excluded (+7.4, bootstrap $p = 0.02$). Scale is the most consistent characteristic: in the full-panel specifications with macro controls or week effects, larger vehicles are calmer by 1.5 to 1.9 percentage points per log unit of TVL ($t(11)$ $p \leq 0.02$), but the bootstrap $p$-values are 0.09 and 0.34, and the estimate vanishes once Maple is excluded. The multi-chain footprint is never significant, and age changes sign across columns. What Table 3 does establish is the volatility counterpart of the result in Section 6.2: a one-percentage-point wider default spread is associated with 6 to 13 percentage points of additional weekly volatility (bootstrap $p = 0.009$ with macro controls, 0.03 excluding Maple), so weekly dispersion in TVL rises with traditional credit stress even where average TVL changes respond only weakly.

Two features of these estimates deserve emphasis. First, with twelve protocols the type indicator and scale are strongly collinear (correlation $-0.74$ in Table 2), and Maple, which is large, volatile, and the only vehicle whose loans are collateralized by digital assets,

sits off the pattern of the other eleven; a single protocol can, therefore, move the type coefficient across zero, and cluster-robust inference with so few clusters is only approximate (Cameron et al., 2008; MacKinnon & Webb, 2017). Appendix A Table A1 confirms that the type coefficient is small under every winsorization rule and sample window and large only when Maple is excluded. Second, a methodological caution. Computing daily log-changes before the USD 1 million floor is applied converts fifteen protocol-days adjacent to zero or near-zero reported TVL (Clearpool TPool nine, Credix two, Maple two, Anemoy one, TrueFi one; six isolated single-day reporting errors) into spurious changes of 17 to 20 log points that survive winsorization and, in this sample, manufacture large spurious volatility differentials between protocols. Applying the floor first, as Section 4.2 specifies, removes them; users of DefiLlama daily histories should screen for isolated zeros before differencing.

### 6.2. Credit-Stress Transmission

Table 4 reports the transmission regression, identified from within-protocol time-series variation, with t(11) and wild cluster bootstrap $p$-values in brackets beneath each coefficient. In the baseline, a one-percentage-point widening of the default spread is associated with a 0.364 log-point contraction in weekly TVL (two-way clustered SE = 0.183), and the short-rate change enters with a coefficient of −0.213 (SE = 0.097). Both signs accord with intuition: tightening credit conditions and rising funding costs draw capital out of these pools, as credit spreads predict activity in traditional markets (Gilchrist & Zakrajšek, 2012). The precision of the estimate depends on the reference distribution. On the normal reference used by the panel estimator the spread coefficient has $p = 0.047$; on t(11) it has $p = 0.073$; under the wild cluster bootstrap over the twelve protocol clusters it has $p = 0.155$, and under a wild bootstrap clustered by week $p = 0.09$. Placebo tests that reassign the weekly macro innovations across weeks return $p = 0.014$ (unrestricted permutation), 0.004 (block permutation of eight-week blocks), and 0.014 (circular shifts); these tests ask whether the alignment of TVL changes with the macro series in time is unusual, and they reject, but they do not address the dependence within protocols that the cluster bootstrap addresses, and we do not read them as overturning it. The defensible statement is that the full-sample association is consistently signed and economically modest but not established at conventional levels once the small number of clusters is respected; we present it as suggestive throughout.

Columns (2) through (4) separate two reasons why the baseline could overstate the association. Winsorization reduces the coefficient to −0.278 (bootstrap $p = 0.20$) and trimming the 26 weekly changes larger than one log point reduces it to −0.116 ($p = 0.59$), so the magnitude depends on a small number of large weekly changes. Column (4) asks whether those changes are mismeasured. Excluding the 111 gap-spanning differences, which include 7 of the 26 trimmed observations, leaves the coefficient at −0.375 (SE = 0.238; t(11) $p = 0.14$; bootstrap $p = 0.22$): the magnitude is intact but the precision is lower, because the gap-spanning weeks are also among the most informative. The attenuation under trimming, therefore, comes from the 19 large but fully observed weekly changes in Maple, Goldfinch, and TrueFi during 2021–2022, which coincide with the crypto-credit stress of that period, not from measurement across coverage gaps. The sign is stable across every treatment; the magnitude is not. The baseline coefficient should not be read as a stable elasticity, and the within-$R^2$ values, between 0.003 and 0.011, are typical of weekly first-differenced financial panels (Cornelli et al., 2025).

Two further results bound the interpretation. Section 7 shows that substituting the VIX or the NFCI for the default spread reproduces a negative coefficient with similar precision (bootstrap $p = 0.22$ and 0.14), so whatever association exists reflects broad traditional stress rather than one index. Section 7 also shows that the association is concentrated in the five vehicles whose underlying assets are uncollateralized real-economy loans (−0.62; five

clusters; t(4) $p = 0.05$; bootstrap $p = 0.10$ to 0.12) and is small for the six tokenized-Treasury and securities vehicles ($-0.10$; $p = 0.44$), with Maple, a single protocol, reported as a memorandum series; Section 7 tests the difference formally. The evidence supports neither the claim that tokenized RWA markets are insulated from traditional credit conditions nor the claim that transmission is established: with the default spread ranging from 1.36 to 2.42 percentage points over the sample, a stable elasticity at the baseline value would imply weekly TVL swings of several percent, and the data show something smaller, noisier, and concentrated in one group of vehicles and one period.

**Table 4.** Credit-stress transmission to weekly Δlog TVL.

| Variable | (1) Baseline | (2) Winsorized 1/99 | (3) Trimmed $\|\Delta\| \leq 1$ | (4) Excl. Gap-Spanning |
|---|---|---|---|---|
| ΔBAA10Y (default spread) | −0.3638 (0.1832)<br>[0.073; 0.155] | −0.2783 (0.1809)<br>[0.152; 0.203] | −0.1157 (0.1854)<br>[0.545; 0.593] | −0.3753 (0.2382)<br>[0.143; 0.215] |
| ΔSOFR | −0.2131 (0.0973)<br>[0.051; 0.150] | −0.1687 (0.0898)<br>[0.087; 0.218] | −0.0801 (0.0662)<br>[0.252; 0.433] | −0.1691 (0.1640)<br>[0.325; 0.534] |
| Protocol fixed effects | Yes | Yes | Yes | Yes |
| Observations | 1332 | 1332 | 1306 | 1221 |
| Clusters (protocols) | 12 | 12 | 12 | 12 |
| Within $R^2$ | 0.0114 | 0.0097 | 0.0027 | 0.0103 |

Notes: The dependent variable is the weekly log-difference of protocol TVL. ΔBAA10Y and ΔSOFR are weekly first differences of the Moody's Baa default spread (FRED BAA10Y) and SOFR. All columns include protocol fixed effects and are estimated on the full panel (December 2020–June 2026). Column (2) winsorizes the weekly log-change at the 1st and 99th percentiles; Column (3) excludes the 26 protocol-weeks with $|\Delta\log \text{TVL}| > 1$; Column (4) excludes the 111 gap-spanning weekly differences defined in Section 4.2 and keeps all other observations. Two-way clustered standard errors (protocol and week; Cameron et al., 2008) are in parentheses. Brackets report the $p$-value on a $t$ distribution with $G - 1 = 11$ degrees of freedom followed by the wild cluster bootstrap $p$-value (null imposed, protocol clusters, Rademacher weights, 9999 replications); no coefficient reaches the 10 percent level according to the bootstrap $p$-value, so no significance markings appear. Sources: DefiLlama; FRED.

### 6.3. Crypto-Market Versus Traditional Factors

Section 6.5 predicts that weekly TVL changes co-move with the crypto-market factors that drive the on-chain investor's wealth at least as strongly as with the credit spread; Table 5 tests this by adding contemporaneous weekly bitcoin and ether log returns to the transmission regression.

The result is weaker than a strict segmentation hypothesis would predict. Using crypto factors alone, the ether coefficient is positive but imprecise (0.264; t(11) $p = 0.08$; bootstrap $p = 0.19$); bitcoin's is not distinguishable from zero. In the joint specification, the ether coefficient (0.246; bootstrap $p = 0.26$) and the default-spread coefficient ($-0.329$; bootstrap $p = 0.17$) are each imprecisely estimated, and the within-$R^2$ rises from 0.011 to 0.015. Weekly TVL changes are weakly related to both traditional and crypto risk, which is consistent with segmentation but is not positive evidence that the marginal investor prices these claims off crypto risk alone. H2, therefore, holds for the panel as a whole in the weak sense that neither factor family dominates; among the real-economy credit vehicles (five clusters), the credit factor retains its magnitude when the crypto factors are added ($-0.600$; bootstrap $p = 0.11$ to 0.12), and neither crypto factor is distinguishable from zero (Section 7).

**Table 5.** Traditional-credit versus crypto-market factors, weekly Δlog TVL.

| Variable | (1) Traditional | (2) Crypto | (3) Joint Specification |
|---|---|---|---|
| ΔBAA10Y (default spread) | −0.3638 (0.1832)<br>[0.073; 0.155] | — | −0.3287 (0.1947)<br>[0.119; 0.167] |
| ΔSOFR | −0.2131 (0.0973)<br>[0.051; 0.150] | — | −0.2081 (0.0923)<br>[0.045; 0.147] |
| r(BTC), weekly | — | −0.0964 (0.1456)<br>[0.521; 0.610] | −0.1303 (0.1166)<br>[0.288; 0.471] |
| r(ETH), weekly | — | 0.2644 (0.1382)<br>[0.082; 0.188] | 0.2458 (0.1441)<br>[0.116; 0.258] |
| Protocol fixed effects | Yes | Yes | Yes |
| Observations | 1332 | 1332 | 1332 |
| Within $R^2$ | 0.0114 | 0.0051 | 0.0150 |

Notes: r(BTC) and r(ETH) are weekly log returns constructed from daily closes (Yahoo Finance, BTC-USD and ETH-USD); other variables as in Table 4. All columns are estimated on the identical panel of 1332 protocol-weeks with protocol fixed effects and two-way clustered standard errors (protocol and week) in parentheses. Brackets report the t(11) $p$-value followed by the wild cluster bootstrap $p$-value (protocol clusters, Rademacher weights, 9999 replications); no coefficient reaches the 10 percent level according to the bootstrap $p$-value, so no significance markings appear. Sources: DefiLlama; FRED; Yahoo Finance.

### *6.4. Event-Study Responses*

Table 6 and Figure 3 report the event studies. The Terra/Luna collapse of 10 May 2022 was followed by a mean three-day log-TVL change of −0.85 across the four protocols with pre-event history and a median of −0.48, with a thirty-day mean of −0.66; the $t$-test $p$-values are 0.13 and 0.19. The three-day mean depends on Goldfinch (−2.07): without it the mean is −0.44, and the only leave-one-out mean with $p < 0.10$ at either horizon is that three-protocol case ($p = 0.03$). The FTX failure (8 November 2022, when the exchange halted withdrawals three days before its bankruptcy filing) was followed by a median three-day change of −0.78 across five protocols, but the mean of −0.52 ($p = 0.34$) conceals a rise of 1.32 at Maple against declines at the other four, and the thirty-day mean of −0.57 has $p = 0.14$. The SVB failure and USDC depeg of 10 March 2023, an episode whose banking mechanics are analyzed by Jiang et al. (2024) and whose stablecoin mechanics are analyzed by Anadu et al. (2024), was followed by changes of roughly −0.25 at three to thirty days ($p = 0.18$ to 0.37). The yen-carry unwind of 5 August 2024 was followed by no change larger than 0.09 log points at any horizon. The bootstrap intervals in Table 6 exclude zero for Terra/Luna at three and thirty days and for FTX at thirty days, whereas the $t$-tests do not reject at any horizon; with four or five observations the percentile bootstrap understates uncertainty, and this paper relies on the $t$-tests and on the magnitudes.

The pattern is consistent with H3 but does not establish it. The two shocks that originated inside the crypto ecosystem and propagated through the stablecoin layer, namely, the run on Terra documented by J. Liu et al. (2023) and the FTX failure, were followed by median contractions of roughly 40 to 80 log-percent in protocol-level TVL, whereas a large dislocation in foreign-exchange and equity-funding markets was followed by no measurable change, and the SVB/USDC episode, which combined a bank failure with a stablecoin depeg, lies between. None of these responses is statistically distinguishable from zero on a $t$-test, the Terra/Luna and FTX means each depend on one protocol, and the design does not separate the event from contemporaneous protocol-specific developments, such as the borrower defaults that several credit vehicles disclosed during 2022. The asymmetry

between crypto-native and traditional-market shocks is the interesting pattern, and we state it as suggestive.

**Table 6.** Event-study cross-sectional TVL responses.

| Event | Horizon (Days) | *N* | Mean Δlog TVL | Bootstrap 95% CI | Median | t-Stat | *p*-Value |
|---|---|---|---|---|---|---|---|
| Terra/Luna | 3 | 4 | −0.847 | [−1.643, −0.361] | −0.480 | −2.06 | 0.13 |
| Terra/Luna | 30 | 4 | −0.658 | [−1.336, −0.082] | −0.421 | −1.68 | 0.19 |
| FTX | 3 | 5 | −0.522 | [−1.178, +0.425] | −0.781 | −1.09 | 0.34 |
| FTX | 30 | 5 | −0.565 | [−1.140, −0.048] | −0.541 | −1.82 | 0.14 |
| SVB/USDC | 3 | 5 | −0.245 | [−0.531, +0.004] | −0.060 | −1.60 | 0.18 |
| SVB/USDC | 10 | 5 | −0.269 | [−0.571, +0.033] | −0.442 | −1.53 | 0.20 |
| SVB/USDC | 30 | 5 | −0.226 | [−0.599, +0.189] | −0.303 | −1.00 | 0.37 |
| Yen carry | 3 | 5 | −0.034 | [−0.106, +0.015] | −0.014 | −0.95 | 0.40 |
| Yen carry | 30 | 5 | −0.090 | [−0.242, +0.034] | −0.050 | −1.12 | 0.32 |

Notes: Δlog TVL is the difference between log TVL at horizon h and the mean of log TVL over days −10 to −1. The t-statistic and *p*-value are from a one-sample *t*-test of the cross-sectional mean against zero with N−1 degrees of freedom. Percentile confidence intervals are from 5000 bootstrap resamples of the protocol cross-section (seed 42) and are reported for completeness; with N = 4 or 5, they are anticonservative and exclude zero in three cases where the *t*-test does not reject, and this paper relies on the *t*-test. N reflects the requirement of a complete pre-event history above USD 1 million. Leave-one-protocol-out estimates for every event and horizon are in the replication package. Source: DefiLlama; authors' calculations.

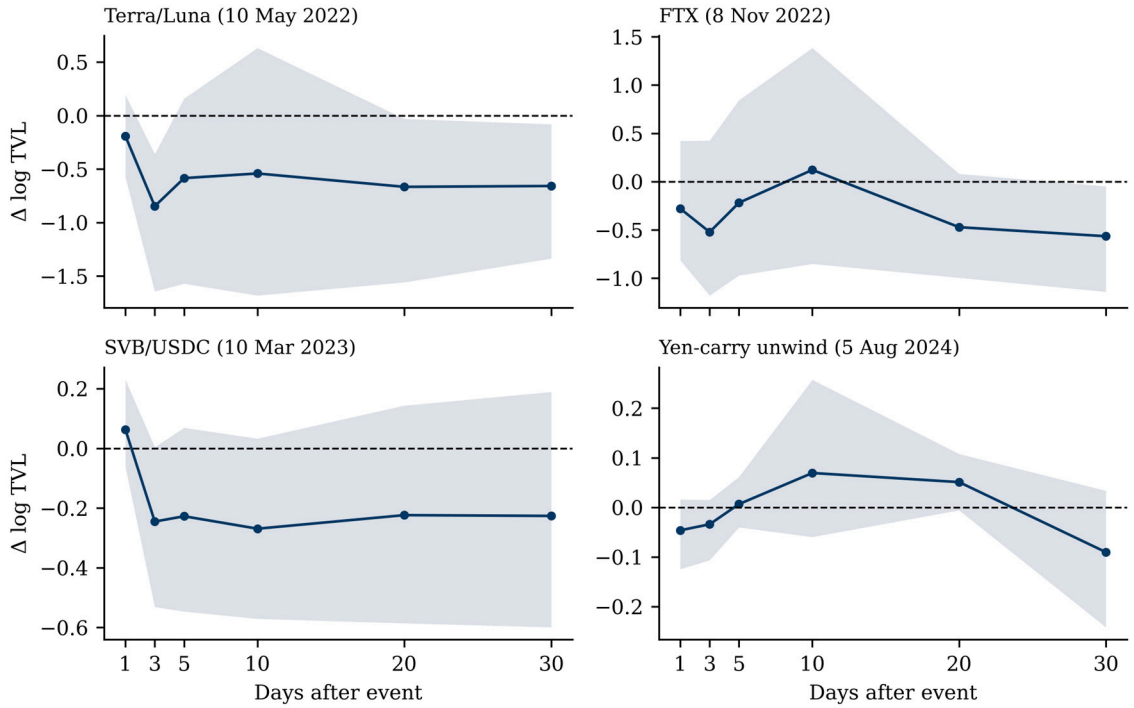



**Figure 3.** Changes in the protocol-level log TVL around four stress episodes, relative to the pre-event mean. Each panel plots the cross-sectional mean of the change in protocol-level log TVL relative to the pre-event window (days −10 to −1), as defined in Equation (3), at horizons of 1, 3, 5, 10, 20, and 30 days; the estimates are descriptive and are not impulse responses to an identified shock; shaded bands are 95 percent bootstrap confidence intervals (5000 resamples, seed 42), and the dashed line marks zero. With four to five protocols per event, the bands are wide by construction and, as percentile intervals of a tiny cross-section, anticonservative; the *t*-test *p*-values in Table 6 govern the inference. Source: DefiLlama; authors' calculations.

### 6.5. Interpretation: The Marginal Investor and Market Segmentation

The imprecision of the transmission estimate, its concentration in vehicles bearing real-economy credit risk, and the asymmetry across stress events admit a common reading rooted in the identity of the marginal investor (Merton, 1987; Gromb & Vayanos, 2010). We offer this reading as one interpretation; investor composition, arbitrage capital, and cross-market frictions are not observed in our data, and the evidence below is consistent with the reading rather than a test of it. The marginal investor pricing traditional credit is an institutional fixed-income allocator benchmarked to a credit index, whose flows respond directly to spread movements; the marginal investor supplying capital to a tokenized pool plausibly differs along four dimensions. Its portfolio loads on bitcoin and ether returns rather than on duration or corporate-credit risk, and cryptocurrency returns are driven by crypto-specific factors largely unrelated to traditional ones (Y. Liu & Tsyvinski, 2021; Y. Liu et al., 2022). Its capital is yield-seeking within a closed ecosystem, so its outside option is another on-chain venue rather than a bond fund (Cornelli et al., 2025). It is demographically distinct: retail crypto participation skews younger, more risk-tolerant, and belief-driven (Hackethal et al., 2022; Aiello et al., 2023; Kogan et al., 2024; Auer et al., 2025). In addition, it faces venue frictions (fiat on- and off-ramping, custody, and, for permissioned pools, KYC onboarding) of the class that sustains persistent cross-venue price gaps in crypto markets (Makarov & Schoar, 2020; Lehar & Parlour, 2025). The arbitrage capital that would integrate the two markets is, in the sense of Gromb and Vayanos (2010), limited.

If default-spread innovations are spanned by traditional-market factors largely orthogonal to the crypto factors in the on-chain investor's information set, the covariance between weekly TVL changes and spread innovations should be small relative to what integrated markets would generate but not zero. The estimates are consistent with that prediction: a coefficient that is consistently signed, modest in the full panel, imprecise once the cluster count is respected, and larger among vehicles whose underlying assets carry real-economy credit risk. The muted response to the yen-carry unwind is also consistent with it. Two alternative readings fit the same facts and cannot be excluded. The first is that the group difference reflects the period rather than the asset: the credit vehicles alone populate 2021–2022, when the largest spread movements and the crypto-native stress episodes coincided, and within the 2023–2026 window in which both groups coexist the difference disappears (Section 7). The second is that write-downs of defaulted loans, which cluster in stress periods and cannot be separated from withdrawals, contribute to the credit vehicles' larger coefficient. The framework's testable prediction is that transmission will strengthen as institutional allocators with traditional mandates enter tokenized markets; the present sample cannot test it.

### 6.6. Identification and Selection

Four considerations limit causal interpretation. First, the type of underlying asset, the settlement chain, and the multi-chain footprint are chosen by the sponsor at inception. Sponsors with stronger backing may jointly select Treasury-backed products, multi-chain distribution, and larger scale, so the Table 3 coefficients partly reflect sponsor quality; sponsor-quality proxies could not be documented to a verifiable source for every protocol and are not used.

Second, the cross-sectional regressions cannot separate type from scale in a sample of twelve; the within-protocol estimates by asset group reported in Section 7 do not depend on that separation and are where the type result should be read. A wider cross-section would permit matching on scale, chain, and launch cohort.

Third, the event studies have cleaner identification because the four episodes are exogenous with respect to protocol design, but four to five protocols per event limit power,

and the design has no counterfactual for what TVL would have done absent the event. Fourth, the two groups of vehicles are observed over different periods: the credit vehicles enter in December 2020 and the first Treasury vehicle in February 2023, so 247 of the 569 credit-vehicle protocol-weeks precede any Treasury observation. Differences in significance between the two subsamples, therefore, do not establish a difference between the two groups, and Section 7 reports a formal interaction test on the full window and on the overlapping window.

## 7. Robustness

Table 7 subjects the transmission estimate to four sets of checks, reporting the $t(G-1)$ and wild cluster bootstrap $p$-values side by side; the formal tests of heterogeneity and the alternative placebo tests are reported later in this section. Panel A of Table 7 replaces the default spread with two alternative stress measures. The VIX yields −0.0031 (SE = 0.0015; bootstrap $p = 0.22$), and the NFCI yields −2.067 (SE = 0.922; bootstrap $p = 0.14$), each reproducing the sign and the imprecision of the spread estimate. Panel B documents the attenuation of the spread coefficient under outlier treatment and subsampling: it falls from −0.364 to between −0.116 and −0.278 under winsorization and trimming, remains at −0.375 when the gap-spanning differences are excluded, and falls to −0.141 in the post-2022 subsample (bootstrap $p = 0.43$). Rebuilding the panel from a later DefiLlama extract that admits Huma Finance (thirteen protocols, 1350 protocol-weeks) yields −0.372 ($t(12)$ $p = 0.05$; bootstrap $p = 0.12$) and further adding Centrifuge's legacy Tinlake pools (782 panel days, peak of USD 9.0 million) yields −0.374 ($t(13)$ $p = 0.02$; bootstrap $p = 0.06$). Appendix A Table A2 varies the USD 1 million floor between USD 0.1 million and USD 10 million: the coefficient is negative at every floor, between −0.21 and −0.74, with bootstrap $p$-values between 0.03 and 0.51, so the sign does not depend on the threshold while the precision does. Appendix A Table A3 reports the leave-one-protocol-out estimates: the full-panel coefficient ranges from −0.26 (excluding TrueFi) to −0.49 (excluding Maple), with $t(G-1)$ $p$-values between 0.02 and 0.23, and the real-economy-credit coefficient from −0.51 to −0.82; no single protocol determines the sign.

**Table 7.** Alternative stress measures, attenuation, heterogeneity by underlying asset, and dynamics.

| Specification | Coefficient | Std. Error | $p$, $t(G-1)$ | $p$, Wild Bootstrap | $N$ |
|---|---|---|---|---|---|
| **Panel A. Alternative stress measures (G = 12)** | | | | | |
| ΔBAA10Y (default spread) | −0.3638 | (0.1832) | 0.073 | 0.155 | 1332 |
| ΔVIX (equity volatility) | −0.0031 | (0.0015) | 0.060 | 0.220 | 1332 |
| ΔNFCI (financial conditions) | −2.0668 | (0.9216) | 0.046 | 0.143 | 1332 |
| **Panel B. Attenuation of ΔBAA10Y (G = 12)** | | | | | |
| Baseline | −0.3638 | (0.1832) | 0.073 | 0.155 | 1332 |
| Winsorized 1/99 | −0.2783 | (0.1809) | 0.152 | 0.203 | 1332 |
| Trimmed $\lvert\Delta\rvert \leq 1$ | −0.1157 | (0.1854) | 0.545 | 0.593 | 1306 |
| Trimmed $\lvert\Delta\rvert \leq 0.5$ | −0.1553 | (0.0905) | 0.114 | 0.160 | 1249 |
| Excluding gap-spanning differences | −0.3753 | (0.2382) | 0.143 | 0.215 | 1221 |
| Post-2022 subsample (from 2023) | −0.1410 | (0.1387) | 0.331 | 0.433 | 1033 |
| Rebuilt panel including Huma (13 protocols) | −0.3720 | (0.1721) | 0.052 | 0.121 | 1350 |
| Rebuilt panel adding Tinlake (14 protocols) | −0.3743 * | (0.1412) | 0.020 | 0.060 | 1474 |
| Yield-adjusted series (Treasury and fund vehicles net of accrued yield) | −0.3617 | (0.1834) | 0.074 | 0.156 | 1332 |

**Table 7.** *Cont.*

| Specification | Coefficient | Std. Error | $p$, $t(G-1)$ | $p$, Wild Bootstrap | $N$ |
|---|---|---|---|---|---|
| **Panel C. ΔBAA10Y by underlying asset (bootstrap: Rademacher/Webb)** | | | | | |
| Real-economy credit vehicles (5): baseline | −0.6159 | (0.2216) | 0.050 | 0.124/0.105 | 569 |
| Real-economy credit vehicles: winsorized 1/99 | −0.5173 | (0.2282) | 0.086 | 0.124/0.121 | 569 |
| Real-economy credit vehicles: trimmed $\|\Delta\| \leq 1$ | −0.2807 | (0.2124) | 0.257 | 0.374/0.352 | 553 |
| Real-economy credit vehicles: excluding gap-spanning differences | −0.8472 ** | (0.2279) | 0.021 | 0.061/0.038 | 499 |
| Real-economy credit vehicles: joint specification, ΔBAA10Y | −0.5995 | (0.2210) | 0.053 | 0.124/0.108 | 569 |
| Real-economy credit vehicles: joint specification, r(BTC) | −0.0639 | (0.1817) | 0.743 | 0.746/0.704 | 569 |
| Real-economy credit vehicles: joint specification, r(ETH) | 0.1597 | (0.1604) | 0.376 | 0.687/0.621 | 569 |
| Treasury, fund, and securities vehicles (6): baseline | −0.0967 | (0.1151) | 0.439 | 0.436/0.427 | 500 |
| Treasury, fund, and securities vehicles: winsorized 1/99 | −0.1595 ** | (0.0887) | 0.132 | 0.032/0.023 | 500 |
| Treasury, fund, and securities vehicles: trimmed $\|\Delta\| \leq 1$ | −0.2373 ** | (0.1417) | 0.155 | 0.032/0.016 | 499 |
| Treasury, fund, and securities vehicles: excluding gap-spanning differences | −0.0918 | (0.1095) | 0.440 | 0.404/0.387 | 468 |
| Treasury, fund, and securities vehicles: yield-adjusted series | −0.0873 | (0.1131) | 0.475 | 0.466/0.476 | 500 |
| Memorandum: Maple (1), single-protocol OLS, Newey–West (4 lags) | 0.1646 | (0.4727) | 0.728 | — | 263 |
| **Panel D. Dynamics of ΔBAA10Y (G = 12)** | | | | | |
| Contemporaneous ΔBAA10Y, with one-week lags of ΔBAA10Y and ΔSOFR | −0.3921 | (0.1733) | 0.045 | 0.121 | 1320 |
| One-week lag of ΔBAA10Y, same specification | 0.1334 | (0.0811) | 0.128 | 0.282 | 1320 |
| One-week lag of ΔBAA10Y only (with ΔSOFR) | 0.0189 | (0.0785) | 0.814 | 0.839 | 1320 |

Notes: Each row reports the coefficient on the indicated stress measure from the specification of Table 4, Column (1), which includes ΔSOFR, protocol fixed effects, and two-way clustered standard errors (in parentheses). Panel A varies the stress measure over the identical full sample. Panel B holds the measure fixed and varies the outlier treatment and the sample; the gap-spanning row excludes the 111 weekly differences flagged in Section 4.2; the Huma and Tinlake rows re-apply the Section 4.2 filters to a later DefiLlama extract that admits Huma Finance, and additionally to Centrifuge's legacy Tinlake listing. Panel C estimates the specification separately for the five vehicles whose underlying assets are uncollateralized real-economy loans (Goldfinch, Clearpool, TrueFi, Credix, and Ribbon Lend; G = 5) and for the six vehicles whose underlying assets are Treasuries, tokenized funds, or listed securities (Ondo Yield Assets, Ondo Global Markets, Anemoy, Centrifuge, Clearpool TPool, and Maple RWA; G = 6); see Table 1 and Appendix B. The joint-specification rows add weekly bitcoin and ether returns. The memorandum row reports Maple, whose loans are collateralized by digital assets, as a single-protocol time series estimated by OLS with Newey–West standard errors (four lags) and a t(n − k) $p$-value; it is not comparable with the panel estimates. Panel D adds one-week lags of ΔBAA10Y and ΔSOFR; the first observed week of each protocol is lost to the lag. Yield-adjusted rows subtract the weekly accrued 3-month Treasury-bill yield (FRED DGS3MO) from the series of the Treasury and fund vehicles and the weekly S&P 500 log return (FRED SP500) from that of Ondo Global Markets. The column $p$, t(G − 1) is the $p$-value on a t distribution with G − 1 degrees of freedom, G the number of protocol clusters; the column $p$, wild bootstrap is from a wild cluster bootstrap by protocol (null imposed, 9999 replications) with Rademacher weights, followed for G ≤ 6 by the Webb six-point variant; * and ** denote significance at the 10 and 5 percent levels according to the bootstrap $p$-value (Webb variant for G ≤ 6; $t$(n − k) for the memorandum row, for which the bootstrap is not available); no coefficient reaches the 1 percent level. Sources: DefiLlama; FRED.

Panel C splits the panel by the nature of the underlying assets. Among the five vehicles whose underlying assets are uncollateralized loans to real-economy borrowers the coefficient is −0.616 (SE = 0.222; t(4) $p = 0.05$; bootstrap $p = 0.12$ with Rademacher and 0.10 with Webb weights), remains −0.517 under winsorization (bootstrap $p = 0.12$), rises to −0.847

when the gap-spanning differences are excluded (bootstrap $p = 0.04$ to 0.06), and in a joint specification with bitcoin and ether returns is −0.600 (bootstrap $p = 0.11$ to 0.12) while neither crypto factor is distinguishable from zero; only the trim to $|\Delta \log \text{TVL}| \leq 1$ attenuates it (−0.281; $p = 0.35$ to 0.37). The block-permutation and circular-shift placebo tests return $p = 0.03$ and 0.01 for this subgroup (Table 8, Panel C). Among the six tokenized-Treasury, fund, and securities vehicles, the baseline coefficient is −0.097 ($p = 0.44$), and the yield-adjusted coefficient is −0.087 ($p = 0.47$); the winsorized and trimmed estimates for this group are −0.160 and −0.237, and they are significant under the bootstrap but not under t(5), so we do not claim a zero response for this group, only a smaller one. Maple, whose loans are collateralized by digital assets, is a single protocol and is reported as a memorandum series estimated by OLS with Newey–West standard errors (+0.165; $p = 0.73$); it is not a third arm of the comparison. Panel D examines timing: with a one-week lag added, the contemporaneous coefficient is −0.392 (t(11) $p = 0.045$; bootstrap $p = 0.121$), while the lag enters at +0.133 (t(11) $p = 0.128$; bootstrap $p = 0.282$), and the lag alone has no explanatory power (0.019; t(11) $p = 0.814$; bootstrap $p = 0.839$); the response is contemporaneous.

Table 8 tests the heterogeneity formally. Panel A interacts the macro innovations with the real-economy-credit indicator in a pooled regression with protocol fixed effects, Treasury, fund, and securities vehicles as the base group, and Maple excluded, so that the interaction coefficient is the difference between the two groups' spread responses. Over the full window, the difference is −0.519 (SE = 0.247; t(10) $p = 0.06$; bootstrap $p = 0.11$); it is −0.755 (bootstrap $p = 0.05$) when the gap-spanning differences are excluded and −0.322 ($p = 0.24$) under winsorization. Restricting both groups to the 153 weeks between February 2023 and June 2026 in which they are observed together, the difference falls to −0.238 ($p = 0.33$ to 0.43), and within that window neither group's own coefficient is distinguishable from zero (Panel B: −0.283 for the credit vehicles, $p = 0.22$ to 0.44; −0.045 for the Treasury group, $p = 0.69$ to 0.72). The credit-vehicle association is, therefore, concentrated in 2021–2022, the period in which the default spread moved most and the crypto-native stress episodes occurred, and the sample cannot separate the asset from the period. Reclassification checks in Panel A leave this picture unchanged: excluding Anemoy and Centrifuge, the two vehicles whose funds mix Treasuries with private credit, gives a difference of −0.457 ($p = 0.10$ to 0.22); excluding Clearpool, whose sponsor also operates a Treasury pool, gives −0.545 ($p = 0.11$ to 0.15); and adding Maple as a third group gives a Maple interaction of +0.261 ($p = 0.06$ to 0.14), consistent with its series being different in kind. Panel C reports the placebo tests described in Section 5.2: the unrestricted permutation, the eight-week block permutation, and the circular shifts return $p = 0.014$, 0.004, and 0.014 for the full panel and 0.022, 0.026, and 0.010 for the credit vehicles, so preserving the autocorrelation of the macro series does not change the placebo verdict; the gap between these $p$-values and the cluster-bootstrap $p$-values reflects the different dependence each device respects, and this paper lets the cluster bootstrap govern its significance claims.

Appendix A Table A1 reports the corresponding perturbations of the real-economy-credit coefficient in the volatility regression of Table 3: tighter and looser winsorization, exclusion of the two largest protocols, and three sample-window restrictions. The coefficient is within one standard error of zero under every winsorization rule and sample window, and is large and significant only when the two largest protocols are excluded, which removes Maple; the cross-sectional volatility result is, therefore, fragile in exactly the way Section 6.1 describes, and this paper rests its type conclusion on the within-protocol estimates of Table 7, Panel C, and on the interaction tests of Table 8.

**Table 8.** Formal tests of heterogeneity by underlying asset, overlapping-window estimates, and placebo tests.

| Specification | Coefficient | Std. Error | $p, t(G-1)$ | $p$, Wild Bootstrap | $N$ |
|---|---|---|---|---|---|
| **Panel A. Interaction ΔBAA10Y × real-economy credit (difference from the Treasury, fund, and securities group)** | | | | | |
| Full window, Maple excluded (11 protocols, 290 weeks) | −0.5193 | (0.2466) | 0.061 | 0.110 | 1069 |
| Overlapping window only (11 protocols, 153 weeks) | −0.2381 | (0.2308) | 0.326 | 0.426 | 726 |
| Excluding gap-spanning differences | −0.7553 ** | (0.2471) | 0.012 | 0.048 | 967 |
| Winsorized 1/99 | −0.3218 | (0.2440) | 0.216 | 0.245 | 1069 |
| Mixed funds (Anemoy, Centrifuge) excluded (9 protocols) | −0.4571 | (0.2430) | 0.097 | 0.219 | 926 |
| Clearpool excluded (10 protocols) | −0.5454 | (0.3104) | 0.113 | 0.151 | 915 |
| Three-group specification (12 protocols): real-economy credit interaction | −0.5193 | (0.2466) | 0.059 | 0.103 | 1332 |
| Three-group specification (12 protocols): Maple interaction | 0.2612 | (0.1247) | 0.060 | 0.137 | 1332 |
| **Panel B. ΔBAA10Y by group within the overlapping window (bootstrap: Rademacher/Webb)** | | | | | |
| Real-economy credit vehicles (5) | −0.2832 | (0.1970) | 0.224 | 0.437/0.385 | 322 |
| Treasury, fund, and securities vehicles (6) | −0.0451 | (0.1176) | 0.717 | 0.686/0.690 | 404 |
| **Panel C. Placebo tests for ΔBAA10Y (placebo $p$-value reported in the bootstrap column)** | | | | | |
| Full panel (Table 4, Column 1): unrestricted permutation (2000 draws) | −0.3638 | | | $p = 0.014$ | 1332 |
| Full panel (Table 4, Column 1): block permutation, eight-week blocks (2000 draws) | −0.3638 | | | $p = 0.004$ | 1332 |
| Full panel (Table 4, Column 1): circular shifts (289 shifts) | −0.3638 | | | $p = 0.014$ | 1332 |
| Real-economy credit vehicles (5): unrestricted permutation (2000 draws) | −0.6159 | | | $p = 0.022$ | 569 |
| Real-economy credit vehicles (5): block permutation, eight-week blocks (2000 draws) | −0.6159 | | | $p = 0.026$ | 569 |
| Real-economy credit vehicles (5): circular shifts (289 shifts) | −0.6159 | | | $p = 0.010$ | 569 |

Notes: Panel A reports the coefficient on the interaction between ΔBAA10Y and the real-economy-credit indicator in a pooled regression of weekly Δlog TVL on ΔBAA10Y, ΔSOFR, their interactions with the indicator, and protocol fixed effects, with two-way clustered standard errors; the Treasury, fund, and securities vehicles are the base group, and Maple is excluded unless stated. The base-group coefficient on ΔBAA10Y is reported in the text. The overlapping window comprises the 153 calendar weeks (20 February 2023 to 29 June 2026) in which at least one vehicle of each group is observed. The mixed-funds row excludes Anemoy and Centrifuge; the Clearpool row excludes Clearpool; the three-group row adds Maple with its own interaction. Panel B estimates the Table 4 specification separately for each group within the overlapping window. Panel C reports placebo $p$-values for the ΔBAA10Y coefficient: the unrestricted permutation reassigns the weekly macro innovations across weeks (2000 draws, seed 42); the block permutation permutes blocks of eight consecutive weeks (2000 draws); the circular-shift test evaluates all 289 non-zero circular shifts of the macro series; $p$-values are the share of the placebo coefficients at least as large in absolute value as the estimate. The $p$-values in Panels A and B are as in Table 7 (t(G − 1) and wild cluster bootstrap by protocol, Rademacher weights, 9999 replications); ** denotes significance at the 5 percent level according to the bootstrap $p$-value; no coefficient falls in the 10 percent (*) or the 1 percent (***) category. Sources: DefiLlama; FRED; Yahoo Finance.

## 8. Conclusions and Policy Implications

Using a protocol-day panel of twelve public-chain tokenized RWA credit and yield protocols (December 2020–June 2026), this paper provides the first cross-protocol evidence on how TVL in these vehicles co-moves with traditional and crypto-native stress. Three findings stand out, each qualified by the inference that twelve clusters permit. Traditional

credit stress is associated with TVL changes through a channel that is consistently signed but thin and imprecisely estimated: a one-percentage-point widening of the default spread is associated with a 0.36 log-point weekly contraction, reproduced by the VIX and the NFCI and stable in sign across TVL floors and leave-one-protocol-out samples, yet with a wild cluster bootstrap $p$-value of 0.15, a magnitude that falls by up to two-thirds under outlier treatment, and a concentration in the five vehicles whose underlying assets are uncollateralized real-economy loans that a formal interaction test places at the margin of significance and that disappears within the window in which both groups of vehicles coexist. In the full panel, weekly crypto returns carry comparable, equally marginal explanatory power, so neither factor family dominates. And event studies show economically large contractions after the crypto-native episodes and none after the yen-carry unwind, a pattern that no $t$-test distinguishes from zero. H1 is, thus, supported only in a suggestive form, H2 only for the aggregate panel, and H3 only qualitatively; tokenized RWA markets sit between the poles the policy debate usually assumes, neither demonstrably integrated with traditional credit markets nor demonstrably sealed from them. A fourth result is a null: cross-sectional differences in weekly volatility are not robustly explained by verifiable protocol characteristics once macro conditions are controlled for, whereas the default spread itself is a strong predictor of weekly volatility.

Three implications follow for authorities weighing the systemic footprint of tokenization, including the public-sector settings the BIS (2025, 2026) envisages, with the caveat that small and imprecise transmission estimates do not establish that tokenized markets are insulated from traditional financial stress. First, at the current scale, the measurable spillover from the traditional credit cycle into tokenized wrappers is limited, a finding that reflects the market's developmental stage and the power of a twelve-protocol panel rather than a structural property and that should be revisited as institutional allocators with traditional mandates enter. Second, the largest disturbances observed in the sample transmitted from within the crypto ecosystem, specifically through the stablecoin settlement layer whose run dynamics Anadu et al. (2024) and J. Liu et al. (2023) document; on the present evidence, supervisory attention belongs there at least as much as with the underlying credit. Third, the transparency of public ledgers is a supervisory asset without traditional analog: every redemption is timestamped and observable, so the coordination dynamics that run theory identifies as decisive (Diamond & Dybvig, 1983) can be monitored in real time. An authority contemplating tokenized public issuance should establish on-chain surveillance (settlement-layer event ingestion, concentration monitoring, and pre-specified thresholds for redemption gates) before issuance and should treat the data and oracle arrangements on which settlement and valuation depend as financial-market infrastructure in the sense of the CPMI–IOSCO Principles (CPMI–IOSCO, 2012), on structural rather than empirical grounds.

Four limitations point to future work. The twelve-protocol cross-section cannot distinguish underlying-asset type from scale or from period, and a wider market observed over a common window will permit matched comparisons across vehicle types. Protocol-level TVL is the outcome, and investor flows are inferred rather than observed; pool-level and address-level histories, which begin in public sources only in 2025, would allow for deposits and withdrawals to be separated from write-downs and revaluation. The event studies lack a counterfactual, which a larger cross-section would supply through untreated vehicles. In addition, the joint dynamics of tokenized private credit and tokenized Treasuries, between which Krause (2026) conjectures cross-segment contagion, are not modeled; the heterogeneity in Tables 7 and 8 suggests they should be.

**Supplementary Materials:** The following supporting information can be downloaded at: https://www.mdpi.com/article/10.3390/jrfm19110869/s1, Replication package (ZIP archive, Version 1.2): LICENSE.txt; data/(eight CSV files: the estimation panel, raw DefiLlama TVL, FRED macro series, Yahoo Finance crypto prices, protocol classification, protocol inception dates, Tinlake TVL, and S&P 500 closes); code/(replication_lib.py, scripts 01–08, run_all.py, requirements.txt); output/(34 CSV files reproducing Tables 1–8 and A1–A3).

**Author Contributions:** Both authors contributed to all aspects of this manuscript, including conceptualization, research design, data collection, data analysis, and interpretation of results. Both authors drafted, reviewed, and finalized the manuscript and approved the final version for publication. The authors take full responsibility for the integrity and accuracy of the work. All authors have read and agreed to the published version of the manuscript.

**Funding:** This work was funded by the Deanship of Scientific Research, Vice Presidency for Graduate Studies and Scientific Research, King Faisal University, Saudi Arabia [Project No. KFU266387].

**Institutional Review Board Statement:** Not applicable.

**Informed Consent Statement:** Not applicable.

**Data Availability Statement:** All source data are publicly available from DefiLlama (protocol-level total value locked), FRED (BAA10Y, SOFR, VIXCLS, NFCI, DGS3MO, and SP500), and Yahoo Finance (BTC-USD and ETH-USD). The estimation panel, the gap-spanning flags, the protocol classification file, and the code reproducing every table and figure, including the revision analyses, are submitted as Supplementary Materials and are deposited as a replication package (Version 1.2) under a CC BY 4.0 license at Zenodo, https://doi.org/10.5281/zenodo.23698715.

**Acknowledgments:** The authors thank the maintainers of the DefiLlama and FRED open-data services. The authors declare that artificial intelligence (AI) tools were used to assist with language refinement, formatting, and the verification of computational reproducibility. No AI tools were used in the generation of the research ideas, the design of the study, the collection of data, or the interpretation of the results. The authors take full responsibility for the originality, accuracy, and integrity of the content presented in this manuscript.

**Conflicts of Interest:** The authors declare no conflict of interest.

## Abbreviations

The following abbreviations are used in this manuscript:

| Abbreviation | Definition |
|---|---|
| API | Application programming interface |
| BAA10Y | Moody's Baa corporate bond yield minus the 10-year Treasury yield (FRED series) |
| BIS | Bank for International Settlements |
| BTC | Bitcoin |
| CBOE | Chicago Board Options Exchange |
| CI | Confidence interval |
| CPMI | Committee on Payments and Market Infrastructures |
| DeFi | Decentralized finance |
| ETF | Exchange-traded fund |
| ETH | Ether |
| FE | Fixed effects |
| FRED | Federal Reserve Economic Data (Federal Reserve Bank of St. Louis) |
| FSB | Financial Stability Board |
| FTX | FTX Trading Ltd., cryptocurrency exchange that failed in November 2022 |
| IMF | International Monetary Fund |
| IOSCO | International Organization of Securities Commissions |
| KYC | Know-your-customer (identity verification) |
| NFCI | Chicago Fed National Financial Conditions Index |
| OLS | Ordinary least squares |

| | |
|---|---|
| RWA | Real-world asset |
| SE | Standard error |
| SOFR | Secured overnight financing rate |
| SPV | Special-purpose vehicle |
| SVB | Silicon Valley Bank |
| TVL | Total value locked |
| USDC | USD Coin (U.S.-dollar stablecoin) |
| VIX | CBOE Volatility Index (FRED series VIXCLS) |

## Appendix A. Sensitivity of the Cross-Sectional Volatility Regression

**Table A1.** Sensitivity of the real-economy-credit coefficient in the volatility regression.

| Specification | Coefficient | Std. Error | t-Statistic | $N$ (Weeks) |
|---|---|---|---|---|
| Baseline (winsorization 1/99) | 0.52 | (7.13) | 0.07 | 1327 |
| Winsorization 0.5/99.5 | 0.08 | (7.85) | 0.01 | 1327 |
| Winsorization 2/98 | 0.20 | (6.14) | 0.03 | 1327 |
| Excluding Ondo Yield Assets and Maple | 10.28 | (2.12) | 4.85 | 886 |
| Post-2022 subsample (from 2023) | −2.59 | (4.90) | −0.53 | 1025 |
| Excluding calendar year 2022 | 0.37 | (5.38) | 0.07 | 1126 |
| From 31 July 2023 | 0.26 | (5.47) | 0.05 | 871 |

Notes: Each row re-estimates Table 3, Column (1), in which the weekly TVL volatility is regressed on the real-economy-credit indicator, log number of chains, log age, and log TVL with protocol-clustered standard errors, under the indicated perturbation and reports the coefficient on the real-economy-credit indicator. The Winsorization rows rebuild the weekly volatility measure at the indicated within-protocol percentiles. Sources: DefiLlama; FRED. The t-statistics are to be read against a t distribution, with G − 1 degrees of freedom (G = 12 protocol clusters; 10 when the two largest protocols are excluded), for which the 10, 5, and 1 percent two-sided critical values are 1.80, 2.20, and 3.11; only the row excluding the two largest protocols exceeds them.

**Table A2.** Credit-stress coefficient under alternative TVL floors.

| Specification | Coefficient | Std. Error | $p$, $t(G-1)$ | $p$, Wild Bootstrap | $N$ |
|---|---|---|---|---|---|
| **Panel A. Full panel (twelve-protocol universe; G = 12, or 11 where stated)** | | | | | |
| Floor USD 0.1 million (12 protocols; 10,815 protocol-days) | −0.5989 | (0.5690) | 0.315 | 0.430 | 1592 |
| Floor USD 0.25 million (12 protocols; 10,263 protocol-days) | −0.7412 * | (0.3038) | 0.033 | 0.100 | 1514 |
| Floor USD 0.5 million (12 protocols; 9912 protocol-days) | −0.4121 | (0.2215) | 0.090 | 0.249 | 1454 |
| Floor USD 1 million (12 protocols; 9161 protocol-days) | −0.3718 | (0.1721) | 0.054 | 0.125 | 1346 |
| Floor USD 2 million (12 protocols; 7855 protocol-days) | −0.6921 ** | (0.2946) | 0.039 | 0.034 | 1159 |
| Floor USD 5 million (11 protocols; 6664 protocol-days) | −0.2858 | (0.3118) | 0.381 | 0.514 | 987 |
| Floor USD 10 million (11 protocols; 5944 protocol-days) | −0.2101 | (0.0492) | 0.002 | 0.125 | 879 |
| **Panel B. Real-economy credit vehicles (G = 5, or 4 where stated; bootstrap: Rademacher/Webb)** | | | | | |
| Floor USD 0.1 million (5 protocols; 5465 protocol-days) | −1.0350 | (0.8119) | 0.271 | 0.371/0.325 | 819 |
| Floor USD 0.25 million (5 protocols; 4922 protocol-days) | −1.1844 * | (0.3671) | 0.032 | 0.121/0.065 | 741 |
| Floor USD 0.5 million (5 protocols; 4572 protocol-days) | −0.7038 | (0.2937) | 0.075 | 0.249/0.210 | 681 |
| Floor USD 1 million (5 protocols; 3824 protocol-days) | −0.6294 * | (0.2034) | 0.036 | 0.124/0.094 | 574 |
| Floor USD 2 million (5 protocols; 2576 protocol-days) | −1.2109 ** | (0.2755) | 0.012 | 0.061/0.025 | 389 |
| Floor USD 5 million (4 protocols; 1538 protocol-days) | −0.9397 * | (0.3475) | 0.073 | 0.120/0.059 | 238 |
| Floor USD 10 million (4 protocols; 1063 protocol-days) | −0.5039 * | (0.1313) | 0.031 | 0.120/0.065 | 166 |

Notes: Each row re-applies the Section 4.2 filters to the raw DefiLlama extract for the twelve sample protocols with the indicated TVL floor in place of USD 1 million and re-estimates the specification of Table 4, Column (1). Because the raw extract post-dates the construction of the estimation panel, the USD 1 million row differs marginally from Table 4 (−0.372 against −0.364). At floors of USD 5 million and above, Credix never exceeds the floor, and the universe contains eleven protocols (four real-economy credit vehicles). The $p$-values follow the conventions in Table 7; * and ** denote significance at the 10 and 5 percent levels according to the bootstrap $p$-value (Webb variant for G ≤ 6), and no coefficient reaches the 1 percent level. Sources: DefiLlama; FRED.

**Table A3.** Leave-one-protocol-out estimates of the credit-stress coefficient.

| Specification | Coefficient | Std. Error | $p, t(G-1)$ | $p$, Wild Bootstrap | $N$ |
|---|---|---|---|---|---|
| **Panel A. Full panel,** Table 4 **Column (1), excluding one protocol (G = 11)** | | | | | |
| Excluding Anemoy | −0.3846 * | (0.1863) | 0.066 | — | 1253 |
| Excluding Centrifuge | −0.3657 * | (0.1861) | 0.078 | — | 1268 |
| Excluding Clearpool | −0.3306 | (0.2165) | 0.158 | — | 1178 |
| Excluding Clearpool TPool | −0.3762 * | (0.1889) | 0.074 | — | 1233 |
| Excluding Credix | −0.4624 ** | (0.1663) | 0.019 | — | 1298 |
| Excluding Goldfinch | −0.2719 | (0.1944) | 0.192 | — | 1127 |
| Excluding Maple | −0.4874 ** | (0.1939) | 0.031 | — | 1069 |
| Excluding Maple RWA | −0.3527 * | (0.1862) | 0.087 | — | 1290 |
| Excluding Ondo Global Markets | −0.3643 * | (0.1845) | 0.077 | — | 1292 |
| Excluding Ondo Yield Assets | −0.3965 * | (0.1980) | 0.073 | — | 1156 |
| Excluding Ribbon Lend | −0.2965 | (0.1820) | 0.134 | — | 1307 |
| Excluding TrueFi | −0.2648 | (0.2061) | 0.228 | — | 1181 |
| **Panel B. Real-economy credit vehicles, excluding one protocol (G = 4)** | | | | | |
| Excluding Goldfinch | −0.5145 | (0.2866) | 0.170 | — | 364 |
| Excluding Clearpool | −0.6421 | (0.2897) | 0.113 | — | 415 |
| Excluding TrueFi | −0.5402 | (0.3021) | 0.172 | — | 418 |
| Excluding Credix | −0.8207 *** | (0.1235) | 0.007 | — | 535 |
| Excluding Ribbon Lend | −0.5200 | (0.2306) | 0.109 | — | 544 |

Notes: Each row re-estimates the specification of Table 4, Column (1), on the panel excluding the indicated protocol, with two-way clustered standard errors in parentheses and the $p$-value on a t distribution, with G − 1 degrees of freedom; the wild cluster bootstrap is not reported for these subsamples, and significance markings (*, **, and ***: 10, 5, and 1 percent) follow the t(G − 1) $p$-value. Event-study leave-one-out estimates: excluding Goldfinch reduces the Terra/Luna three-day mean from −0.85 to −0.44, and excluding Maple, whose TVL rose by 1.32 log points over the three days after the FTX halt, changes the FTX three-day mean from −0.52 to −0.98; across all events and horizons, only three leave-one-out means have $t$-test $p$-values below 0.10: Terra/Luna at three days excluding Goldfinch ($p = 0.03$, three protocols), FTX excluding Maple at three days ($p = 0.01$) and excluding Ribbon Lend at thirty days ($p = 0.09$), each on four protocols. Full results are in the replication package. Sources: DefiLlama; FRED.

## Appendix B. Classification of Underlying Assets

Each protocol is assigned to one of three groups by the underlying asset that generates the token holder's cash flows, using the protocol description and category in DefiLlama's protocol metadata (retrieved 2 September 2026) and the protocol's own product documentation. The rule is mechanical: a vehicle is a real-economy credit vehicle if its DefiLlama category is RWA Lending or Uncollateralized Lending and its documented business is lending to identified off-chain or institutional borrowers without digital-asset collateral; a collateralized lending vehicle if its documented loans are secured by digital assets; and a Treasury, fund, or securities vehicle if its documented underlying assets are government securities, tokenized funds, or listed securities.

Real-economy credit (5). Goldfinch (DefiLlama: RWA Lending; "yields that are generated by real-world economic activity"); Clearpool (Uncollateralized Lending; "a decentralized marketplace for unsecured institutional capital"); TrueFi (Uncollateralized Lending; "borrow capital without collateral"); Credix (RWA Lending; "debt financing and private credit"); and Ribbon Lend (Uncollateralized Lending; unsecured lending to institutional borrowers).

Collateralized lending (1). Maple (DefiLlama: Lending; loans to institutional borrowers secured by digital assets at collateralization ratios above 150 percent, with a permissionless deposit channel, Syrup, since May 2024).

Treasury, fund, or securities (6). Ondo Yield Assets (RWA; "liquid exposure to an ETF of short-term U.S. Treasuries"); Ondo Global Markets (RWA; "stocks on chain"); Anemoy (RWA; tokenized Treasury and credit funds distributed through Centrifuge); Centrifuge (RWA; "tokenize, manage, and distribute tokenized funds onchain"); Clearpool TPool (RWA; "dedicated to investing in or purchasing treasuries"); and Maple RWA (RWA; Maple's cash-management pool, restricted to U.S. Treasury bills and Treasury-collateralized reverse repurchase agreements).

Three protocols have mixed or evolving exposures, and the classification records the dominant exposure over the sample. Anemoy and Centrifuge distribute funds that combine Treasuries with collateralized loan obligations and private credit, and are assigned to the Treasury, fund, or securities group because their flagship products are Treasury funds; Table 8 reports the heterogeneity test with both excluded. Clearpool's sponsor operates both the uncollateralized lending marketplace and, since August 2024, the separately listed TPool Treasury product; the two are separate DefiLlama listings and separate panel units, and Table 8 reports the test with Clearpool excluded. Maple's listing combines institutional pools collateralized by digital assets with, since May 2024, permissionless Syrup pools, and is for that reason reported as a single-protocol memorandum series rather than as a group. No protocol changed its underlying-asset type within the sample.

The number of settlement chains counts the chains for which DefiLlama's per-chain series records positive TVL on any day up to 30 June 2026.

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