Issuer Discretion and Stablecoin Holdings after the Tornado Cash Sanction

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Abstract

On 8 August 2022 the US Treasury sanctioned Tornado Cash and listed 38 addresses. Only one of the two major stablecoin issuers followed suit: Circle froze USD Coin (USDC) at those addresses that day; Tether froze no Tether (USDT) at those addresses then. The question that arises is how users react to the discretionary power to freeze accounts, if they do at all. This can function as a case study for free banking theory and how consumers of competing currencies might react to an issuer's discretion. We have collected Ethereum transfer records for 4,487 past Tornado Cash users, not themselves frozen, and 11,495 matched wallets with no recorded prior Tornado Cash use. We found that past users' USDC holdings minus USDT holdings, scaled by each wallet's usual combined holdings and with extreme values capped, fell by 0.152 relative to matched wallets (wallet-clustered 95% confidence interval: −0.184 to −0.120). The interval lies wholly below −0.10, so the rule fixed in advance says "supported." Their capped combined holdings also fell, by 0.148. We did, however, find that the gap was already falling before the event; with later baselines ending 7 August, the fall is 0.098 to 0.129, with intervals containing −0.10. Without the cap, the gap, combined-holdings and USDC estimates are positive and none is statistically significant at the 5 percent level, so these results do not show a switch from USDC to USDT; nor can they separate reactions to the sanction from reactions to Circle's same-day freeze.

Keywords: stablecoins; issuer discretion; sanctions; Tornado Cash; difference-in-differences; currency competition

JEL classification: E42, G23, G28, K42

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1 Introduction

On 8 August 2022 the US Treasury put sanctions on Tornado Cash, an Ethereum protocol that tries to break the link between a sender and a receiver on the blockchain, making it harder to follow money transfers (U.S. Department of the Treasury, 2022). Following these sanctions, Circle froze USDC held at the 38 sanctioned addresses belonging to Tornado Cash itself (Allaire, 2022; Office of Foreign Assets Control, 2022a). Circle issues USDC and Tether issues USDT, both custodial stablecoins: tokens issued by private firms with a promise of redemption at one dollar per token. While both issuers already had the ability to freeze their own tokens at the listed addresses, only Circle decided to do so. The question we ask is whether past Tornado Cash users whose own addresses were not frozen shifted their holdings from USDC to USDT following these events.

The Treasury stated that Tornado Cash had been used to launder funds, including more than $455 million stolen by North Korea's Lazarus Group (U.S. Department of the Treasury, 2022). At the same time, ordinary users also used the protocol: Ethereum co-founder Vitalik Buterin said he had used it to donate to Ukraine (Beganski, 2022). On 24 August 2022, Tether said that unilateral freezing of certain addresses "could be a highly disruptive and reckless move" (Tether, 2022b). Instead they later adopted a voluntary policy to freeze sanctions-listed wallets, announced on 9 December 2023 (Tether, 2023).[1] In our blockchain data, we observe records of Tether's freezes at all 38 addresses on that date.

Using wallet data from the Ethereum blockchain, we compare past Tornado Cash users against similar wallets with no recorded use of the protocol before the event, over the weeks before and after 8 August 2022. We find a fall in the gap between USDC and USDT holdings of past users, relative to similar wallets, of 0.152 of their usual combined holdings. We did, however, find that the gap was already falling before 8 August, with its largest one-day move from 30 June to 1 July, though with later baselines ending 7 August the fall is still 0.098 to 0.129. We can also see that their combined USDC and USDT holdings fell, while USDC alone, estimated without the cap on extreme values, showed no statistically significant change at the 5 percent level; without the cap, the USDC, gap and combined estimates are all positive. While we cannot tell where they moved their funds, we observe a relative change in holdings.

Looking at the literature, most of it covers cases where one compares issuers that can freeze to issuers who cannot, usually centralized versus decentralized issuers; USDT and USDC are pooled as freezable coins (E. Liu et al., 2026) or compared only at the level of trading pools

[1]The legal status of Tornado Cash changed after the study window, which ends on 12 September 2022. On 8 November 2022 the Office of Foreign Assets Control (OFAC) removed the August entry and re-designated Tornado Cash under its cyber and North Korea authorities (Office of Foreign Assets Control, 2022b). On 26 November 2024 the Fifth Circuit held that Tornado Cash's immutable smart contracts are not the property of a foreign national or entity under the International Emergency Economic Powers Act, so OFAC exceeded its authority in blocking them. The holding covers the immutable contracts only (*Van Loon v. Department of the Treasury*, 2024). On 21 March 2025 the Treasury removed the sanctions against Tornado Cash at its discretion (U.S. Department of the Treasury, 2025).

(Hernandez Cruz et al., 2024). No study, to our knowledge, looks at a case where both issuers have this ability but only one used it, as we do here. One limit is that the sanction and Circle's freeze came on the same day, so a reaction could be to either. It is important to note that the advantage such a study has is that the two largest stablecoins, USDT and USDC, are both centralized (Gorton & Zhang, 2023, p. 923). Furthermore, this topic goes beyond crypto and even beyond free banking theory itself. With continued central bank developments into central bank digital currencies (CBDCs), the effect of programmable currencies has to be examined (Auer et al., 2022). Especially where one public issuer is the only issuer of a digital currency, switching to a competing issuer is not possible. Circle itself warned of a compliance and national-security "kill switch" (Circle, 2022). Hence, a study that analyzed if customers leave a currency due to its issuer acting in a way they disagree with might shed some light on how people actually value programmable currencies.

Past Tornado Cash users who were not frozen themselves during the baseline or the study window may have shifted some USDC toward USDT relative to similar wallets with no Tornado Cash history. We hypothesize that their scaled gap, USDC holdings minus USDT holdings divided by their usual combined holdings of both, falls relative to that comparison group; Section 4 gives the formal definition. We expect this because past Tornado Cash users were able to see the stark difference between the two issuers almost immediately. They saw one issuer freeze addresses on the Treasury's administrative sanctions list, issued without a court ruling, while another declined to do so. Since businesses screen funds by their origin, these users' address histories might lead them to believe that one issuer's token was safer than another's. This belief is not observable in balances, though, and the design cannot separate it from avoiding US-regulated services or from exchanges screening Tornado-linked deposits.

This question also matters for free banking theories, which foresee an economy based on the free exchange and open competition of privately issued currencies. The freedom of consumers to leave one issuer's money for another's should, in theory, put pressure on issuers to keep with consumer demands (Selgin & White, 1994, pp. 1729, 1734). We consider stablecoin use an interesting case study of competition between private issuers. While it is true that these tokens are directly pegged to the US dollar, a fiat currency issued by an actively run central bank, the discretionary powers left to these issuers do exist, and consumers do have a choice and low barriers to switch between issuers or leave the market altogether. What carries over is exit between private issuers of the same unit; these dollar claims do not offer competing monetary standards. Competition among stablecoin issuers is nevertheless limited (Cengiz, 2025). Our claim concerns differential balance adjustment by affected versus comparable holders, without establishing exit behavior or issuer discipline in general.

The paper proceeds as follows. Section 2 explains stablecoins and basic blockchain technology, then the freeze, Tornado Cash and the sanction. Section 3 reviews work on issuer exit, runs and exclusion and identifies the gap. Section 4 explains how we collected the data, the two groups, the measure and the decision rule. Section 5 reports the fall in the gap, the prior trend

and the role of the cap. Section 6 discusses timing, rival explanations, the free-banking interpretation and what our limits were. Section 7 concludes. Appendices A–C provide inference details, design details, and the signed design and placebo record.

2 Background

Circle, like Tether, is a stablecoin issuer, meaning if approved customers give it US dollars it will give them tokens of the equivalent amount, which they can then use to trade on the blockchain and on exchanges. Stablecoins are digital currencies designed to keep a stable value against an external reference, typically the US dollar (Liao & Caramichael, 2022, p. 1). People use these stablecoins to trade against other cryptocurrencies (Liao & Caramichael, 2022, p. 6). Other uses include gaining exposure to the US dollar. We focus on custodial stablecoins, where an intermediary holds reserves and offers redemption on a one-for-one basis (Liao & Caramichael, 2022, p. 3). The two largest are Tether's USDT and Circle's USDC (Gorton & Zhang, 2023, p. 923). On 30 June 2022, about five and a half weeks before the event, Circle reported 55.6 billion USDC in circulation (Grant Thornton LLP, 2022), and Tether reported $66.2 billion in issued tokens, a figure that covers not USDT alone (BDO Italia S.p.A., 2022). Tether also issued stablecoins that were pegged to other currencies, while USDT was the largest by far (Tether, 2022a).

While both issuers offer dollar-pegged money, their legal standing differs. Circle is an American company regulated under the Bank Secrecy Act (Allaire, 2022). Tether, on the other hand, stated that it was not a US person and had no US operations or customers (Tether, 2022b); we do not infer from its statement that it was outside US rules. How holders obtain these issuers' coins also matters for what our data can show. In the primary market, issuers mint and redeem tokens directly with approved, mainly institutional customers (Saggese et al., 2026, p. 7; Watsky et al., 2024). In the secondary market, ordinary holders trade with other holders on centralized exchanges or decentralized trading pools (Watsky et al., 2024). Moving from USDC to USDT therefore means trading one coin for the other, without dealing directly with Circle or Tether. We observe wallet balances, not issuer redemptions. To clarify terms, an address refers to an account on the public ledger, while a wallet is equivalent to one address in this context, and an event log is the public trace left by each transfer. USDC and USDT are the coins people hold; Circle and Tether are the issuers that decide on freezes.

The issuer has a blacklist in the token contract of addresses they freeze. The idea is that these funds cannot be spent, sold or redeemed. While the balance is still there, the contract blocks transfers (Wu et al., 2026, p. 1). From November 2017 to August 2025, Tether blacklisted 2,418 addresses on Ethereum and Circle blacklisted 341 (Wu et al., 2026, pp. 4, 7). The difference in this case is the procedure. In 2022, Tether's stated practice was to freeze a private wallet only on a verifiable law-enforcement request; they said none had been made for the sanctioned addresses

(Tether, 2022b). So on 8 August 2022 only Circle froze USDC at the listed addresses (Allaire, 2022; E. Liu et al., 2026, p. 3511), while Tether held off until December 2023 (Section 1).

The listed addresses belonged to Tornado Cash, where users deposit a fixed sum from one address and withdraw later to a fresh one; both transactions remain visible, but the protocol is designed to hide the match (Nadler & Schär, 2023, pp. 123, 127, 128). In some cases, the match can still be inferred from transaction patterns, for example when the same address both deposits and withdraws (Wang et al., 2023, pp. 2028, 2032). On 8 August 2022, the US Treasury's Office of Foreign Assets Control (OFAC) imposed sanctions on the protocol through an administrative act, without a court decision at the time. They required US persons to block and report covered property and barred dealings with Tornado Cash (U.S. Department of the Treasury, 2022). However, sanctioning an address alone does not change its status on-chain; the issuer must take action to implement the freeze (Wu et al., 2026, p. 1). Tether said that OFAC had not signaled that a freeze was expected (Tether, 2022b).

3 Literature Review

We review work on holders leaving money issuers, covering free banking, bank runs and regulatory exclusion, including the studies closest to ours, before considering which way holders would be expected to move after the freeze. Exit in private money is one of its strongest correction mechanisms. The freedom for consumers to leave one issuer's money for another incentivizes issuers to respect consumer demands, just like in any other competitive market (Hayek, 1976/1990; Klein, 1974; Selgin & White, 1994). The Scottish option clause is the nearest historical counterpart to a freeze that we could find. Introduced by the Bank of Scotland in 1730, later used by other Scottish banks, and banned in 1765, it allowed banks to delay redemption and was invoked against particular holders who presented notes (Gherity, 1995, pp. 717–719; Shah, 1997, pp. 12–16). The clause promised interest for the delay (Gherity, 1995, p. 713), although banks at times refused to pay specie, meaning gold or silver coin (Shah, 1997, pp. 13–16). A stablecoin freeze, on the other hand, promises no compensation and blocks an address whose holder need not have asked to redeem. Users of both USDC and USDT whose addresses are not frozen can change from one to another by trading on exchanges (Watsky et al., 2024), so we want to see if they have actually used this option, and whether past Tornado Cash users adjusted their USDC and USDT balances differently from comparable wallets.

Exit under stress, in turn, is best known from bank runs, one of the oldest problems in modern economies. Depositors exposed to losses leave more readily in a solvency scare (Iyer et al., 2016). Most recently we have seen the case of Silicon Valley Bank (SVB), which failed in March 2023, and what happens if customers lose trust in a bank's ability to pay out their deposits. Some of Circle's reserves were trapped and USDC fell below one dollar (Watsky et al., 2024); outflows from USDC probably went into USDT and a smaller stablecoin (Anadu et al., 2023). In the world of cryptocurrencies and especially stablecoins, the biggest issue is the trust we put in these

issuers to keep their peg. In the case of Terra the peg was supposed to be kept by an algorithm, and it was lost in May 2022 due to reasons outside the scope of this paper; the biggest holders withdrew earliest (J. Liu et al., 2023). Still, it is important to note that just as with traditional banks, a loss in trust leads to something that can very well be described as a kind of bank run. However, while there might have been heightened tension surrounding the question of whether USDC could keep its peg, perhaps because some holders were less trusting, neither coin lost its peg in August 2022. Therefore, in this study we analyze how holders react to an issuer's actions even while choosing between two coins that both keep their one-dollar promise.

Another part of the literature looks at exclusion through regulation. Under the EU's Markets in Crypto-Assets Regulation (MiCA), USDT was delisted on exchanges with a large EU web audience, while USDC was authorized. USDT trading volume was about 20 percent lower there than on global exchanges, and USDC's share of combined USDT–USDC volume rose by 0.82 standard deviations, although USDC volume itself showed no statistically significant change (Borri & Shakhnov, 2026). In our setting, both issuers were able to freeze the sanctioned addresses, but only one did. E. Liu et al. (2026) examine post-sanction Tornado Cash withdrawals and find no statistically significant difference in which kind of token (freezable or not) withdrawers chose over two and a half years. While that study groups USDT and USDC as freezable tokens alongside alternatives without issuer freezing powers, we look at two issuers who both have the technological opportunity. We compare matched wallets around one event, observing the same wallets before and after, keeping the issuers separate and including wallets that did not trade. Other studies examine the sustained reduction in Tornado Cash use following the designation, without studying stablecoins (Brownworth et al., 2024), or how issuers carry out freezes through contract blacklists (Wu et al., 2026). Closest to our design, Tao (2026) matches pre-sanction Tornado Cash depositors to users of other mixers and finds that they cut back on privacy tools overall after the sanction rather than moving to other mixers; that study does not look at stablecoins, while we look at past users' USDC and USDT balances.

Which way holders would move is not obvious. Circle has tried to position itself as the compliant issuer to make itself more attractive. White (2025, pp. 21–22) reports Circle's emphasis on being more law-abiding than Tether. The freeze could make USDC seem a safer choice and raise the USDC–USDT holdings gap, though White (2025, p. 22) doubts that this strategy matters to consumers, given Tether's dominance despite weaker compliance. Conversely, an issuer imposing restrictions holders dislike may lose them to competitors (Dowd, 1991, p. 763; Shah, 1997, p. 8). Holders never frozen may fear being next and hold less USDC, so the gap falls. Switching and coordination costs (Farrell & Klemperer, 2007) and the persistence of incumbent currencies (Luther, 2016, pp. 563, 565) can keep any reaction small. We then have to deal with the fact that either a fall or a rise in the gap can have more than one explanation. While a falling gap could mean fear of a freeze, it could also be a sign that consumers try to avoid US-regulated services. Likewise, a rising gap does not necessarily prove the compliance story right. That reading requires a stronger shift toward USDC among exposed than matched wallets, rather than

a difference in their initial holdings. Either way, the sign gives some insight, while the motive remains unidentified.

4 Data and Methodology

4.1 Hypotheses

We expect consumers will react negatively to the sanction and Circle's decision to freeze addresses. After the event, past Tornado Cash users who were not frozen themselves should therefore show a larger fall in their scaled USDC–USDT holdings gap than similar wallets do (H1), a negative coefficient in Equation (1). Our test cannot tell the sanction and the freeze apart.

H1a checks the combined holdings of both coins to help tell apart potential sources of a fall in the gap, such as USDC being traded for USDT or an outflow from both coins.

Wallets that used Tornado Cash more often should show a larger fall in the scaled holdings gap (H2). This hypothesis is exploratory and concerns a pattern only.

Table 1 gives the formal hypotheses, thresholds and groups.

Table 1: Hypotheses and decision criteria

Hypothesis	Outcome	Criterion	Status
H1	Capped gap D_{it}, Equation (1)	95% confidence interval for β lies entirely below $-\delta$, with $\delta = 0.10$, under both wallet- and stratum-clustered intervals; uncapped veto does not fire; clip flag reported beside the decision (Table 3)	Confirmatory
H1a	Capped combined holdings A_{it}	Own four-way classifier against $\delta_A = 0.05$: negligible, meaningful decline, meaningful increase, inconclusive (Table 3)	Companion test to H1; confirmatory-secondary
H2	Gap D_{it} by number of identified prior Tornado Cash deposits (0 / 1 / 2 or more)	Larger fall in the gap among heavier past users	Exploratory, pattern only

Note: H1a: It uses the sum of the two stablecoins, $A_{it} = (\text{USDC}_{it} + \text{USDT}_{it})/B_i$. Its role is as a gate for any reallocation wording, with the result reported separately, beside H1. For H2, we measure intensity as the count of identified deposits in all 23 pools, grouping them into three categories: 0, 1, or 2 or more. We place 1,818 exposed wallets, or 40.5 percent, in the zero group. Zero means no deposit was identified for the wallet, which includes both withdrawal-only wallets and wallets whose deposits could not be resolved, so the count understates past use. Groups are unequal in size, with ties not split, and the cut-points were chosen with group sizes already known at that point. **Source:** Author's research design and signed pre-specification (28 October 2026). β, δ and D_{it} are defined under Estimating Equation and Decision Rule and Pre-Specification in Section 4.

4.2 Data and Sample

We compare wallets that used and did not use the Tornado Cash protocol. To do so, we built daily balances from Ethereum transfer and freeze-event logs[2] and dropped the 38 frozen addresses. A wallet counts as a past Tornado Cash user if it is an externally owned address (Table 2 note) that

[2]The logs were retrieved through public JSON-RPC endpoints, the standard query interface of an Ethereum node. The freeze events (Circle `Blacklisted`; Tether `AddedBlackList` and `DestroyedBlackFunds`) come from the

deposited into or withdrew from one of the 23 pools listed by Circle before the end of 7 August 2022; activity on 8 August itself does not count, so same-day reactions cannot decide who is in which group.

We took these past users alongside matched wallets, meaning wallets (addresses on the blockchain) with no recorded use of the protocol, matched on size, USDC share, activity, age and ether (ETH) balance (Appendix B; Table A6). After screens such as a floor of $100 in average combined holdings, we found 4,487 past Tornado Cash users and 11,495 matched wallets (Table 2; Appendix B). Then we took the 60 days before the event and 35 days after the event, up to 12 September and so before the Ethereum Merge, and compared how the two groups' holdings moved.

Table 2: Construction of the exposed sample

Step	Addresses
Externally owned addresses that deposited to or withdrew from a Tornado Cash pool up to and including block 15,298,282 (7 August 2022)	86,571
Of which have at least one USDC or USDT Transfer log in the baseline	8,370
Of which pass the holdings and active-day floors	4,502
Of which are eligible (not blacklisted in the baseline)	4,501
Of which have at least one matched control (exposed wallets in the estimation)	4,487

Note: An externally owned address is controlled by a private key held by a user; a contract address holds code and has no private key (Victor, 2020). A transfer log counts for participation even if its value is zero or a third party initiated it. Exposure follows the Tornado Cash pool logs: of 163,696 `Deposit` logs, 138,772 could be tied to a depositing address and 24,924 could not. Of the 4,501 eligible exposed wallets, 109 deposited into a Tornado Cash USDC pool and are flagged. The matched sample holds 4,487 exposed wallets and 11,495 controls (15,982 wallets) in 692 strata. **Source:** Author's calculations from Ethereum event logs (tornado-usdc pipeline, signed run of 28 October 2026).

4.3 Estimating Equation

We use these two groups to filter out the noise of the general crypto market. Just looking at exposed wallets alone would mix in that noise with the actual reaction to the event. The comparison wallets we chose saw the same news as the exposed wallets, so once we take the difference, we will remove its common effect. What remains are shocks that hit one group only, including any difference in how the two groups reacted to the same news. Then we look at changes in the gap rather than its level. If we were to use the levels, we might just see their different holdings, but the changes tell us how they behaved compared with their own usual levels. Furthermore, we look at the scaled gap, since the difference in holdings can be between one hundred and millions of dollars. Using raw dollar amounts would lead to the larger wallets skewing the results. We cap extreme values of the gap and combined holdings at the 1st and 99th percentile of baseline wallet-days (Appendix B); the cap rule was fixed after the capped and uncapped placebo comparison of 15–16 October 2026, before any main post-event contrast was estimated. For our

lifetime freeze files of both token contracts, which were read before signing within the limits listed in the pre-specification. The data-extraction and estimation code was written with the help of an AI coding assistant (Anthropic Claude), to our specification and under our review.

basic assumptions (Roth et al., 2023), we say that nobody reacted before 8 August; without the event, we would have seen the capped gaps of both groups move alike; and we have overlap and use these matched comparison wallets, with the remaining differences in Table A6.

The estimating equation is

$$D_{it} = \beta\,(\text{Exposed}_i \times \text{Post}_t) + \alpha_i + \gamma_t + \varepsilon_{it} \tag{1}$$

with the outcome

$$D_{it} = \frac{\text{USDC}_{it} - \text{USDT}_{it}}{B_i}$$

First we take the daily holdings of each wallet's USDC and then subtract the same but with USDT, then we divide this by the wallet's baseline average combined holdings. We also try to take out each wallet's own usual gap by using wallet effects, and then we use day effects to take out anything that might have moved all wallets on a specific day, for instance market news. Subsequently we have beta in Equation (1), which is the number we actually care about: by how much more has the past user's gap changed compared with a similar user's after the event. It should be noted, however, that this cannot rule out trends specific to one wallet. There is also the limitation that since USDC holdings were greater before, if a wallet cut both holdings by the same share, we would actually see a fall in the gap. So as it stands, the gap mixes "which coin" with "how much in total", and alone it does not show a preference between issuers.[3]

4.4 Decision Rule and Pre-Specification

The bar we set before the main estimate ran was 0.10, a tenth of a wallet's usual combined holdings. At the typical (median) wallet, which held about $7,924, a 0.10 fall in the gap would be a change of about $792, or as if roughly $396 moved from USDC to USDT. The idea was that we wanted to see a clear decrease in the gap. We made this decision on 21 September 2026 and signed and hashed the full rule on 28 October 2026 to make sure the results would not cloud our judgment. To make the bar statistically meaningful, we also said that H1 is supported only if the whole 95 percent interval lies below −0.10; an interval that contains −0.10 is inconclusive. The full rule is in Table 3. The only confirmatory estimate for H1 is the pooled 35-day β of Equation (1); H1a is its companion test (Table 1).

[3]Equation (1) follows from a wallet-coin-day panel with wallet-by-coin, coin-by-day and wallet-by-day effects once the two coins are differenced. Wallet totals and fees drop out only to the extent that they affect both coins equally. In Equation (1), USDC_{it} and USDT_{it} are wallet i's dollar balances at the end of day t, and B_i is its mean combined USDC and USDT holdings over the 60-day baseline. The wallet effect α_i absorbs each wallet's usual gap and the day effect γ_t absorbs what moved the gap for all wallets that day; neither absorbs a trend specific to one wallet. Exposed_i is one for exposed wallets and Post_t is one from 9 August 2022, with 8 August left out. β, the coefficient on their interaction, is read holding the wallet and day effects constant.

Table 3: Decision rules for H1 and H1a

Rule	Condition	Reported as
H1, case 1	$U < -\delta$	Supports H1 (subject to the rows below)
H1, case 2	$L \leq -\delta \leq U$	Inconclusive
H1, case 3	$L > -\delta$ and $U \leq \delta$	Negligible differential response
H1, case 4	$L > \delta$	Meaningful positive differential response
H1, case 5	Otherwise	Meaningful negative response ruled out; size of a positive one open
H1, missing interval	Interval missing or invalid	Not estimable
H1, clusterings	Wallet- and stratum-clustered intervals each classified; both must be in case 1	Disagreement: dependence-sensitive
H1, uncapped veto	Either uncapped-D interval in case 3, 4 or 5 (wholly above $-\delta$)	Cap-sensitive, not supported
H1, clip flag	Exposed-minus-control change in the share of wallet-days beyond a cap exceeds 1 percentage point in either direction, at either tail of either cap	Cap-sensitive flag beside the H1 decision; H1 case unchanged; blocks reallocation wording
H1a	Interval wholly inside $[-\delta_A, \delta_A]$ / wholly below $-\delta_A$ / wholly above δ_A / otherwise	Negligible / meaningful decline / meaningful increase / inconclusive
Reallocation wording	All four: H1 supported (both clusterings, veto silent); capped H1a inside $[-\delta_A, \delta_A]$; uncapped H1a not wholly outside it; no cap-sensitive flag	"Consistent with relative reallocation toward USDT on the observed addresses"; otherwise only whether the gap fell, with H1a separately

Note: $[L, U]$ is the 95% confidence interval for β on capped D_{it}; $\delta = 0.10$, $\delta_A = 0.05$, both fixed in advance and never widened. **Source:** Signed pre-specification (28 October 2026).

5 Results

The question, then, is whether past Tornado Cash users whose own addresses were not frozen shifted their holdings from USDC to USDT. It is neither the case that, without the cap, USDC fell, nor that USDT rose, by an amount statistically significant at the 5 percent level. So what we can actually say is that the gap between the two coins fell compared with similar (matched) wallets, while capped combined holdings also fell relative to them; however, there are no clear signs where they went, why, or whether they preferred one issuer.

Our results show that the gap of wallets that have used the protocol before fell by 0.152 of their usual combined holdings relative to matched non-exposed wallets, with both 95 percent intervals, clustered by wallet and by matching stratum (a group of wallets matched together; Appendix B), wholly below −0.10 (Table 4; Appendix A), so the rule fixed in advance says H1 is supported.[4] None of our safeguards, the uncapped veto and the clip flag (Table 3), were triggered. The data also show that before the date of the event the gap was already decreasing, from +0.146

[4]The rules were pre-specified, signed and hashed privately. The signature of record is dated 28 October 2026; the signed files are public in the code release. Section 0 of the pre-specification lists what we could see before signing: baseline cohort counts, holdings and matching inputs, the issuers' freeze logs including their post-event part, a short post-event canary window, and a pilot. It does not claim that we saw nothing, but no 9 August to 12 September outcome panel had been built and no main post-event contrast had been estimated. An addendum of 28 October 2026 adds three placebo runs without USDC-share matching that were on disk before signing but missing from section 0. It dates the section 0 run of "15 October" to 15 October 23:36 to 16 October 14:50 and hashes the evidence files. The placebo of record does not change (−0.025, case 3).

on 9 June to about zero by late July, and an exploratory joint Wald test rejects parallel pre-trends (Figure 1 and note). But if we use the last days before the event as the baseline, we still have a downward shift of about 0.10 (0.098 to 0.129) in exploratory checks (Table 5). While 0.10 was also our bar as the deciding factor, it does still show a drop, although every interval in the later-baseline rows of Table 5 contains −0.10. Under our rule, that is the inconclusive case: measured from these later baselines, the data cannot tell whether the fall is larger or smaller than our bar.

Within the decline before the event, the largest one-day move came from 30 June to 1 July, when Tether froze 46 other addresses (Figure 1 note), and a fake event date of 9 July on the main sample also gives a fall of 0.045, statistically significant at the 5 percent level (Table 5 note). The signed June placebo does pass (Table A7), but a pass does not establish parallel trends, since its band of plus or minus 0.10 would also let this drift through.

This drift matters for the 0.152 itself: it compares the 35 days after the event with the whole 60-day baseline, and that baseline starts high in early June, which accounts for about a third of the 0.152 (Figure 1 note). The verdict rests on parallel trends, which the exploratory test rejects, so "supported" stands as the output of the rule. That decline is not by itself the size of the event effect; how much of the decline the event accounts for stays open.

Table 4: Headline estimates

Outcome	β	95% CI, wallet-clustered	95% CI, stratum-clustered	Decision
H1: capped gap D	−0.152	[−0.184, −0.120]	[−0.188, −0.116]	Supported (case 1, both clusterings)
H1a: capped combined holdings A	−0.148	[−0.182, −0.114]	[−0.185, −0.110]	Decline (both clusterings)
Uncapped D (veto check)	0.817	[−0.174, 1.808]	[−0.178, 1.812]	Case 2 (both); veto not fired
Uncapped A	0.619	[−0.373, 1.611]	[−0.368, 1.606]	Inconclusive (both)
USDC leg, uncapped (descriptive)	0.72	[−0.26, 1.69]	—	—
USDT leg, uncapped (descriptive)	−0.10	[−0.27, 0.08]	—	—

Note: β in units of baseline combined holdings B. Clip flag FALSE, reported beside the H1 decision as the pre-specification requires. N = 4,487 exposed and 11,495 control wallets in 692 strata (2 to 136 wallets each, median 16); 1,518,290 wallet-days, each wallet in the panel for all 95 days; control effective N after weighting 8,593; the largest single weight is 0.13% of the control total. B is a wallet's average combined USDC + USDT holdings over the 60 baseline days. The legs are USDC/B and USDT/B without caps. The veto would have fired only if the whole uncapped interval lay above −0.10, that is, with an uncapped estimate above about +0.89 (standard error 0.506, about 30 times the capped 0.016); the estimate of 0.817 fell 0.074 short. The clip flag counts days beyond the cap, not dollars, so it cannot detect a few very large wallets. Table A8 reports the gap at other caps, the uncapped estimate without its largest contributors, the two coins under one shared cap and outcomes that need no cap. The same pre-trend qualification applies: an exploratory Wald test rejects parallel pre-trends for combined holdings too, and late baselines ending 7 August give a fall of 0.100 to 0.115 (point estimates; no interval is reported). Raw uncapped means run the other way: exposed wallets' USDC rose from 0.79 to 1.60 of baseline holdings (matched wallets: 0.75 to 0.85), with a few wallets ending far above their own baseline. Those wallets are the case the caps were built for. **Source:** Author's calculations from Ethereum event logs.

The falling gap leaves open whether money moved between the two coins or out of both. We can see that the combined holdings fell as well, by 0.148 of their usual combined holdings relative

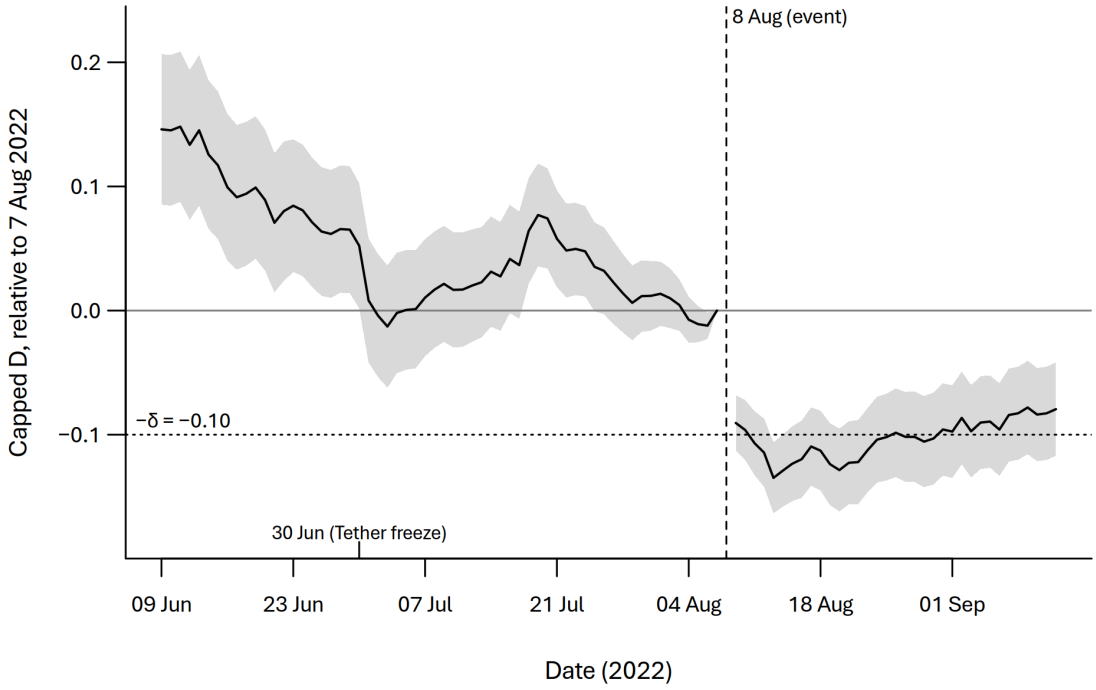

Figure 1: Daily difference-in-differences coefficients for the capped gap D, exposed versus matched non-exposed wallets, 9 June to 12 September 2022, each day relative to 7 August 2022. Shaded band: 95 percent confidence interval, clustered by wallet (stratum-clustered daily intervals are not computed). Dashed line: 8 August 2022 (not in the panel). Dotted line: $-\delta = -0.10$, the pre-specified threshold for a meaningful fall. Tick: 30 June 2022. N = 4,487 exposed and 11,495 control wallets in 692 strata, 1,518,290 wallet-days. **Source:** Author's calculations from Ethereum event logs.

Note: Figure 1 plots the exposed-minus-matched difference in the gap, each day against 7 August: it started at +0.146 on 9 June, fell to −0.013 on 3 July, rebounded to +0.077 on 19 July, and sat about zero from 29 July to 7 August. In exploratory checks, dated 28 October 2026 and run after the results, a joint Wald test rejects parallel pre-trends ($F = 2.06$, $p < 0.001$, by wallet), and the largest one-day pre-event move came from 30 June to 1 July, after Tether froze 46 other addresses on 30 June. About a third of the 0.152 comes from the high early-June baseline. After the event the difference sat near −0.09 on 9 August, reached its lowest point at −0.135 and came back to near −0.08 by 12 September, with a mean over the 35 post-event days of −0.103. All 35 daily intervals lie wholly below zero, but only the one for 13 August lies wholly below −0.10, and the other 34 contain −0.10. These are clustered by wallet only and not adjusted for testing 35 days.

to matched wallets, wholly below their −0.05 bar (Table 4; Figure 2). Both are falls relative to matched wallets, and neither gives a direction for either coin. Without the cap, exposed wallets' USDC even rose from 0.79 to 1.60 of baseline holdings, because a few wallets ended far above their own baseline; this is why the uncapped USDC, gap and combined estimates are positive.

Furthermore, we can see one of the most prominent falls right at the event: from 7 to 9 August the gap dropped by 0.091 relative to matched wallets, [−0.113, −0.068] by wallet and [−0.114, −0.067] by stratum (Figure 1; exploratory, post-results, dated 28 October 2026). Of the two shapes named in the pre-specification, a jump from 9 August or a build-up continuing past 24 August, the path matches the jump. With the cap, this two-day drop is the largest of 158 since

Table 5: The headline against later baselines (exploratory, post-results, dated 28 October 2026)

Baseline	D	95% CI, wallet	95% CI, stratum	A (point estimate)
All 60 days (= headline)	−0.152	[−0.184, −0.120]	[−0.188, −0.116]	−0.148
From 1 July	−0.124	[−0.154, −0.094]	[−0.158, −0.090]	−0.113
From 8 July	−0.129	[−0.159, −0.099]	[−0.162, −0.096]	−0.115
Last 40 days	−0.126	[−0.156, −0.096]	[−0.160, −0.092]	−0.115
Last 20 days	−0.127	[−0.157, −0.098]	[−0.160, −0.095]	−0.112
Last 10 days (29 July to 7 August)	−0.106	[−0.135, −0.076]	[−0.138, −0.074]	−0.100
Last 5 days	−0.098	[−0.127, −0.069]	[−0.130, −0.066]	−0.108
7 August only	−0.103	[−0.133, −0.073]	[−0.135, −0.071]	−0.108
All days, exposed-group linear trend removed	−0.080	[−0.120, −0.040]	[−0.123, −0.037]	−0.046

Note: β in units of baseline combined holdings B. Each entry is the mean of the 35 post-event daily coefficients minus the mean of the pre-event coefficients over the stated baseline. The last row instead adds a linear trend for the exposed group, fitted over all 60 baseline and 35 post-event days, to the estimating equation and reports β net of it (exploratory, 28 October 2026). Ending on 6 August instead (7 August left out), the last 20, 30 and 40 days give −0.131, −0.130 and −0.128 for D; the last-20 wallet-clustered interval lies wholly below −0.10 (upper end −0.1005), its stratum-clustered interval [−0.163, −0.098] does not. A fake event on 9 July 2022 on the main matched sample (days 0–59, own caps; not the signed June placebo of Table A7) gives −0.045, [−0.081, −0.009] by wallet and [−0.083, −0.007] by stratum. Not confirmatory; does not reclassify H1. **Source:** Author's calculations from Ethereum event logs.

March 2022; without it, it ranks 88th, so it does not settle the pre-trend question, and the sensitivity analysis of the post-event mean against the week of 1–7 August cannot clear −0.10 even with no departure from parallel trends (Table A4, Panels A and B; exploratory, post-results).[5]

In a further exploratory check, the trend extension, the post-event mean lies below the extended June–July trend in every fitting window by between about zero and 0.123; the 60-day fit lies inside a rough random-walk range expected with no event (Table A3). The sign is the same in every window, but the size depends on the window, so we adopt no single size.

The capped checks fixed in advance, unlike these exploratory ones, were part of the signed design, and where estimable they all hold, although the main-event rows share the headline's baseline, so their agreement does not test parallel trends (Table 6 and note; June placebo: Table A7).[6] The only check that disagrees with them is the version without the cap on extreme values, but none of our safeguards were triggered, including the veto fixed in advance (Table 4).

Turning to H2, there is a case to be made for a correlation between wallets with more identified deposits into the protocol and a sharper drop in the gap (exploratory; Table 7). The intervals of the low and high groups do not overlap. However, we can also see overlap for neighbouring groups and there has been no formal test run of the difference, so it is a pattern but not proof. Additionally, we see the group of wallets that deposited USDC itself into the Tornado Cash

[5]Two analyses, both exploratory and post-results, are reported in Table A4: a bound on how far parallel trends could be violated (Rambachan & Roth, 2023), and a longer pre-period, from March to June 2022. We hashed the plan for them before reading any of their output, but earlier exploratory previews predate it, and three dated addenda to the plan were written after output had been read (Table A4 note).

[6]Some pre-specified checks are not estimable by design. We deferred the outflow breakdown, and any later version of it is exploratory. The centralized-exchange and Binance flags need a verified source of exchange labels, and so does the screen for reused exchange deposit addresses. The placebo cohort needs a verified list of lending and exchange addresses, and owner-grouped inference needs a defensible partition of addresses into owners.

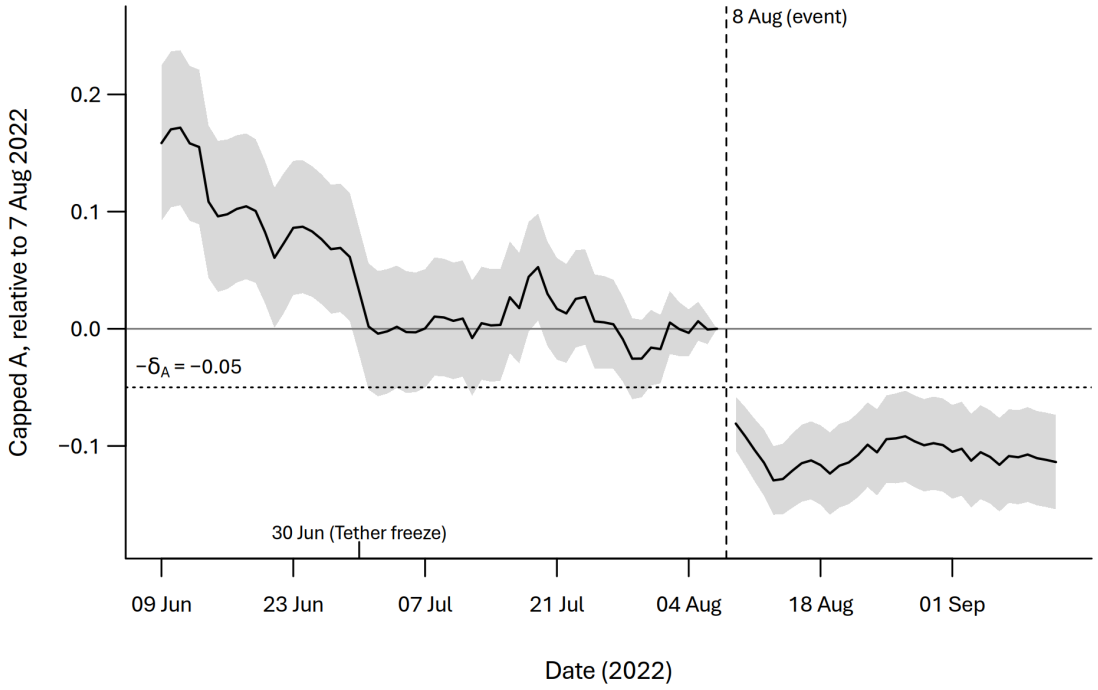

Figure 2: As Figure 1, for capped combined holdings A. Dotted line: $-\delta_A = -0.05$. **Source:** Author's calculations from Ethereum event logs.

USDC pools, later frozen, dropped the most, but with only 109 wallets there is no clear evidence of a difference either.

6 Discussion

As Section 5 reports, we can see a falling trend in the USDC–USDT holdings gap prior to the designation, with its largest one-day move from 30 June to 1 July (Figure 1 note), although the drift had gone flat by late July. Even if we use a later baseline ending 7 August, the estimated fall remains, and every interval in the later-baseline rows of Table 5 contains the decision threshold. Additionally, with the cap we can see a sharp fall within two days of the event, which does not stand out without it (Table A4). So the timing is consistent with users noticing the designation and reacting to it. Still, we cannot say anything after 12 September, when the window ends, nor give a causal size, given the prior trend and the joint event.

Rival explanations start from the fact that the designation and Circle's freeze occurred on the same day. There are a number of ways the sanctions might have hit past Tornado Cash users in addition to the freezes: exchanges screening Tornado-linked deposits, fear of being named, to name a few. Any of these could make them pull money out without Circle's freezes playing any part. More broadly, regulation of this kind might increase overall fear in the market and lead people to move money to less regulated markets, which can mean leaving stablecoins behind altogether. The matched wallets faced the same market, so this explains the relative fall only if Tornado Cash users reacted more strongly than matched wallets. Past Tornado Cash users

Table 6: Robustness checks for H1 (capped gap D), with the status of every pre-specified row

Check (pre-specification section)	β	95% CI, wallet	95% CI, stratum	Case / status
No USDC-share matching (5.6) ‡	−0.215	[−0.247, −0.183]	[−0.254, −0.176]	1
Exposed-quartile-coarsened USDC-share matching (5.6)	−0.138	[−0.171, −0.106]	[−0.175, −0.102]	1
Drop wallets frozen 8 August to 12 September (5.9) †	−0.149	[−0.182, −0.117]	[−0.185, −0.113]	1
One cap for both legs (5.8)	−0.150	[−0.182, −0.118]	[−0.186, −0.115]	1
Uncapped (5.8)	0.817	[−0.174, 1.808]	[−0.178, 1.812]	2 (reported, does not override)
Window ending 4 September (5.4)	−0.157	[−0.190, −0.125]	[−0.194, −0.121]	1; not Binance-sensitive
Doubly robust DiD, unmatched (5.5)	−0.138	[−0.167, −0.109]	n/a	1 (wallet only)
June placebo of record, fake event 9 July (5.2)	−0.025	[−0.055, 0.005]	[−0.058, 0.008]	3 (both), pass
Pretrend plot: daily coefficients relative to 7 August, 8 August excluded; attention to 30 June 2022	—	—	—	Descriptive (Figure 1); a Wald pretrend test added after results (28 October) is exploratory
USDC-pool flag: flagged vs unflagged exposed wallets, rematched (5.10)	—	—	—	Exploratory (Table 7)
Placebo cohort: heavy lending-protocol and exchange users	—	—	—	Not estimable: no verified address manifest; no substitute cohort
Binance counterparty flags and outflow shares	—	—	—	Not estimable
Deposit-address screen: controls sharing an exchange deposit address with an exposed wallet	—	—	—	Not estimable: no verified exchange labels

Note: β in units of baseline combined holdings B. Every row is reported, including rows that are not estimable, with the reason. Numbers in parentheses are sections of the signed pre-specification. † The later-freeze list is derived from the issuers' freeze records.[7] ‡ Placebo runs under this specification before signing are listed in Table A2. All main-event rows share the 60-day baseline of the headline, so their agreement does not test parallel trends; the June placebo uses its own windows (9 June to 8 July against 10 July to 7 August). N exposed: 4,501 (no USDC-share matching), 4,493 (quartile-coarsened), 4,487 (later-freeze row: the 2 later-frozen addresses, both unmatched control candidates, are removed and the sample is rematched with the primary cut-points); doubly robust DiD 4,501 exposed against 71,779 unmatched controls, none trimmed; June placebo 3,007 exposed, 8,381 controls, 508 strata. Ending the window on 4 September, before Binance announced on 5 September that it would convert its users' USDC, gives −0.157. The doubly robust estimator, −0.138, targets a different population, so it serves as a check and the matched estimate stays the arbiter. The row without USDC-share matching gives the largest estimate, but its own placebos before signing fell in case 2 twice.[8] **Source:** Author's calculations from Ethereum event logs.

[7] The list of wallets frozen after the event comes from the issuers' freeze records. A separate on-chain fetch checks that list. Any disagreement makes the row not estimable; the check never adds or removes a wallet. That fetch was not independently repeated.

[8] Before signing, the placebos without USDC-share matching fell in case 3 twice (15 and 16 October) and in case 2 twice (−0.121 on 23 October, −0.111 on 28 October). The placebo of record uses the signed specification, three-bin USDC-share matching on the June population with the participation rule, and gives −0.025 (case 3). Table A2 lists these five and two other pre-signing runs.

Table 7: Exploratory splits fixed in the pre-specification (capped gap D)

Group	N exposed	β	95% CI, wallet	95% CI, stratum
H2: 0 identified Tornado Cash deposits	1,818	−0.098	[−0.146, −0.051]	[−0.152, −0.045]
H2: exactly 1 identified deposit	1,202	−0.170	[−0.223, −0.116]	[−0.226, −0.114]
H2: 2 or more identified deposits	1,467	−0.204	[−0.253, −0.154]	[−0.254, −0.154]
Depositors into the Tornado Cash USDC pools (5.10)	109	−0.387	[−0.605, −0.169]	[−0.620, −0.155]
Other exposed wallets (5.10)	4,378	−0.153	[−0.186, −0.120]	[−0.190, −0.116]

Note: β in units of baseline combined holdings B. Pattern only; no test of the differences between groups was run. Heavy users of Tornado Cash may also differ from light users in other respects. Zero identified deposits (40.5 percent) includes withdrawal-only and unresolved deposits. The H2 cut-points were fixed when the group counts were known. Whether their funds were still in the pools on 8 August cannot be observed, so the flag marks possible direct loss only. **Source:** Author's calculations from Ethereum event logs.

may also move funds to fresh addresses more often than other wallets, which our design cannot exclude and which would look like a fall in holdings of both coins. One possible alternative explanation is a general loss of confidence in Circle's peg. The matched wallets held USDC too, so this explains the relative fall only if past Tornado Cash users were more sensitive or held more USDC. Before matching, the USDC share was 0.784 among 4,501 eligible exposed wallets against 0.352 among 71,779 unmatched control wallets. Matching leaves part of this difference (Table A6), so a market-wide shock to USDC holdings remains a possible explanation. By contrast, the one rival the design can test is Binance's conversion of its users' USDC, announced on 5 September: ending the window on 4 September gives −0.157, and the result is not Binance-sensitive (Table 6).

While stablecoins are not actually independent currencies with independent valuations, since they are tokenized representations of the US dollar, they still offer a valuable case study for what happens if one issuer restricts its users more than another. We regard stablecoins as one of the closest cases we have to privately issued money today, and so as a rough test case for free banking theories. The main idea is that we want the different currency issuers to compete with each other, to cater to the consumer just like any other market would have to. So we would think that if the issuers do something, holders would notice and then they would either leave or stay because of these actions. A freeze is one use of the discretion an issuer holds over its users' balances.

The results show that the past Tornado Cash users, the group that most plausibly worried about being frozen next, saw their USDC–USDT gap fall compared with matched wallets. That fits with our question and with the idea that users notice and react to an issuer's discretion. Nevertheless, our calculations cannot show a fall in USDC holdings themselves without the cap that is statistically significant at the 5 percent level, so switching to a competitor is not shown. The data show relative falls, and the link to exit is our interpretation: it is consistent with holders reacting to the joint sanction-and-freeze event when they have the power to leave or switch to a competitor. This ties in well with free banking theory and having multiple private money issuers (Selgin & White, 1994).

Another important question to ask is how well informed these consumers are. Since the barrier to entry in these spaces is quite high in terms of technological know-how, one might assume that the overall information level both in terms of regulations and ongoing development might be higher than that of an average person. So to use this case study as a proxy for ordinary holders could be misleading, since we cannot assume the average consumer to keep up with regulations and differences in issuers on a daily basis.

We are thus using stablecoins as a very rough stand-in for currency competition, since competition among issuers is limited (Cengiz, 2025). Our measures are rough as well: exposure is measured only through on-chain Tornado Cash use, and one address is not one person (Victor, 2020; Wang et al., 2023). Sanctions bind US persons, but addresses carry no jurisdiction, so exposure measures who is likely to be screened rather than anyone's legal risk. Furthermore, trades inside a centralized exchange appear on-chain only as USDC leaving (Saggese et al., 2026). We value tokens at par and say nothing about welfare, and since we observe wallet balances rather than redemptions at Circle, we cannot show whether any change reached the issuer. Our intervals also leave out the uncertainty from matching, since inference holds the matching weights fixed (Appendix A). Finally, we cannot separate issuer policy and legal regime, but we do not attribute Circle's freeze to regulation: Paxos, supervised by the New York State Department of Financial Services (Anadu et al., 2023), also did not freeze at the sanctioned addresses, per Tether's 24 August 2022 statement (Tether, 2022b).

7 Conclusion

The main findings are consistent with our initial expectations. Relative to matched wallets, past Tornado Cash users' capped combined holdings and their capped USDC–USDT holdings gap both fell after the joint sanction-and-freeze event, with an exploratory check showing a sharp two-day drop in the gap right at the event, which stands out only with the cap. However, the gap was already falling before the event (Figure 1 note), and from later starting points the fall is smaller. We also could not confirm a switch to USDT, since USDC holdings alone, estimated without the cap, showed no statistically significant change at the 5 percent level. Nor is there a way to know whether Circle's choice or the sanction drove it, or even if ordinary holders would react the same way.

Still, within the free-banking framework, our reading is that these results are consistent with holders who can leave responding to the joint sanction-and-freeze event. In future research, we can examine these open questions more closely, mainly by completing the deferred outflow breakdown, verifying exchange and lending labels, and testing how less tech-savvy holders would react to an issuer's discretionary freeze. The question behind the first two is the one our data could not answer: where did their money go?

Statements and Declarations

Competing interests. This research was carried out during a research internship at the American Institute for Economic Research (AIER), Great Barrington, MA, USA. The author has no other relevant financial or non-financial interests to disclose.

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Author contributions. Not applicable (single author).

Data availability. The raw data are public Ethereum mainnet event logs: ERC-20 Transfer events of the USDC and USDT token contracts, the issuers' freeze events (Circle `Blacklisted`; Tether `AddedBlackList` and `DestroyedBlackFunds`), and deposit and withdrawal events of the 23 Tornado Cash pools, retrieved through public JSON-RPC endpoints over pinned block ranges. The pinned block ranges are in the code release (`config/blocks.json`); the derived wallet-day panel is not distributed, and the code in the release rebuilds it from the public logs.

Code availability. Data extraction is written in Python (standard library only) and estimation in R (packages `fixest`, `data.table`, `DRDID`, `HonestDiD`). All code is public at https://github.com/nik110403/tornado-usdc (release v1.0).

Pre-specification. The decision rules were signed and hashed privately on 28 October 2026, before the main post-event contrast was estimated. The signed files and the addenda are in the release (`prespec/`, `addenda/`); the SHA-256 of the signed hash list `prespec/SHA256` is `6bc01a5c9132beceec8289e015ef3393ed83b581074259d6c1e44e8cc1a6516e`.

Ethics approval. Not applicable. The study uses only public blockchain records; no person was contacted and no individual is identified.

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Appendix A. Inference Details

We cluster standard errors by wallet and, separately, by matching stratum, holding matching weights fixed (Bertrand et al., 2004). Wallet clustering allows any correlation across a wallet's days but assumes independence between wallets. Stratum clustering allows correlation between wallets within a stratum but assumes independence between strata. Either independence assumption could fail through common ownership of addresses across these boundaries. We cannot account for this through owner-grouped inference because no defensible owner partition exists. Neither clustering covers common ownership, shocks specific to the exposed or control group, or variance from the matching step (Abadie & Spiess, 2022). On synthetic data, wallet-clustered errors are about 4% too small. We report this discrepancy without correction. We report cluster counts and weight concentration in the note to Table 4.

Our pre-specification included only a pretrend plot, with no gate. We added a Wald pretrend test on 28 October as an exploratory step, without computing its power (Roth, 2022). Our second estimator is a doubly robust difference-in-differences, which models both who is exposed and how the outcome moves among non-exposed wallets. We fit these two working models using DRDID 1.4.0, implementing the improved doubly robust panel estimator of Sant'Anna and Zhao (2020). We run this estimator unweighted, with analytic rather than bootstrap standard errors, and apply a propensity trim at 0.99 that can drop only controls, retaining all exposed wallets. No exposed wallet was above 0.99, and no control was trimmed. The estimator uses collapsed baseline and post means of unmatched screened wallets, with influence-function variance, and it targets the exposed population. This estimate remains a robustness check, since it too is biased when both working models are wrong.

Our power rule, $\Phi(0.05/SE - 1.96)$, uses the larger standard error from the June placebo, 0.0166. This gives power of 0.854, the probability that the interval clears $-\delta$ given $\beta = -0.15$. Since 0.854 exceeds the 0.80 target, we do not enlarge the frame. This decision rests on a planning proxy, not validated main-design power. The proxy implies a minimum detectable effect of 0.1465. Supporting H1 needs more, since the whole interval must lie beyond $-\delta$.

Appendix B. Design Details

Sample Screens

A wallet is exposed if it is an externally owned address that deposits funds into, or withdraws funds from, any of the 23 Tornado Cash pools listed by Circle up to and including the cutoff block 15,298,282 on 7 August 2022, 23:59:57 UTC. This cutoff excludes 8 August 2022 from exposure, so that same-day reactions cannot determine group membership. Activity after the cutoff block leaves group assignments unchanged. We keep two lists separate: the exposure list contains the 23 pools, while the freeze list follows the OFAC designation of 8 August 2022 and contains 38 unique addresses with 45 address tags. These 38 addresses belong to Tornado Cash itself, and we drop them from the sample because funds held there are stuck. For the remaining

sample, we use a baseline window of 60 days, from 9 June to 7 August 2022, and a post-event window of 35 days, from 9 August to 12 September 2022. This post-event window ends before the Ethereum Merge on 15 September 2022, so we keep a single regime throughout the analysis.

We count a wallet as participating if it has at least 1 baseline log of a USDC/USDT Transfer. Among participating wallets, we drop those with average combined USDC plus USDT holdings below $100 or a nonzero balance on fewer than 14 baseline days. We then exclude wallets blacklisted by Circle or Tether during the baseline. We reconstruct this status from the opening blacklist state, updating it with each later addition or removal. A further exclusion removes wallets with a baseline transfer to or from the zero address, the issuers' mint-and-burn point. This screen cannot remove all issuer accounts because no verified treasury list exists. We keep wallets frozen later, from 8 August to 12 September, and report their number per group; none appear in the matched sample. Among the 4,501 eligible exposed wallets, we flag 109 USDC-pool depositors for possible direct loss, since their deposits may have been frozen with the pool. We compare these flagged wallets with unflagged wallets to explore differences. Table 2 summarizes how the screens and subsequent matching take the sample from 86,571 externally owned addresses to 4,487 matched exposed wallets.

Matching

We draw non-exposed candidates from a frame of 172,296 addresses found in 30 random 400-block windows within the baseline. The frame includes addresses sending or receiving USDC/USDT and excludes contracts and addresses appearing in Tornado Cash logs before the cutoff. This frame favors active addresses, so we narrow the activity gap by requiring a baseline transfer in both groups. In plain terms, we divide each trait into a few bands, such as small, medium or large holdings, and compare each exposed wallet only with candidates that share all its bands, which we call a stratum. This procedure is coarsened exact matching (Iacus et al., 2012). We form four bands for each of the four continuous traits, holdings, transfers, age and ether balance, using the 25th, 50th, and 75th percentile among eligible exposed wallets. These cuts divide each trait into low, moderate, high and very high bands, with ties assigned to the higher band and duplicate cut-points removed. For age, never-sent wallets receive a separate band. For the USDC share, we divide the baseline USDC sum by the baseline sum of USDC and USDT, placing values of exactly 0, interior values and exactly 1 in distinct bands. Within each resulting stratum, we draw up to 3 controls per exposed wallet without replacement, sorting addresses with seed 20261015. Matching yields 11,495 controls in 692 strata; we drop and count the 14 exposed wallets without a control. Control counts differ across strata, so equal weighting would let crowded strata dominate the analysis. We therefore use matching weights in all estimates except the doubly robust check, which we estimate on the unmatched sample, unweighted. The matching weights make each stratum's weighted controls proportional to its exposed wallets, so the target population is the matched exposed wallets.

The USDC share lies close to the outcome's baseline level, so matching on it can induce regression toward the mean (Daw & Hatfield, 2018). We keep the share in primary matching and report 2 baseline diagnostics (Table A5), using neither for selection. The research proposal of 29 September 2026 planned a screen that identifies controls sharing an exchange deposit address with an exposed wallet. An exchange deposit address is the per-customer address an exchange assigns, as noted by Victor (2020). Identifying these shared addresses requires verified exchange labels, which we lack, so the screen is not estimable. We reserve any later label list for exploratory analysis only.

Outcome Caps and Clip Incidence

We cap D_{it} and A_{it} at the 1st and 99th percentiles of pooled baseline wallet-days in the matched sample, unweighted. These 958,920 wallet-days give caps set to identical values on every day: D in $[-3.0927, 4.4768]$ and A in $[0, 5.3170]$. We use capped D_{it} to decide H1 and capped A_{it} to decide H1a.

We then check clip incidence, the weighted share of wallet-days past each cap, by group, tail and period. An exposed-minus-control change in that share that exceeds 1 percentage point in either direction flags the cap as sensitive beside the H1 decision; the H1 case and its wording stay unchanged.

Identifying Assumptions

Formally, the three assumptions we make are no anticipation, parallel trends for capped outcomes in the matched sample, and overlap, as stated in Roth et al. (2023). Under these assumptions, β estimates the difference in the event's effect between exposed and non-exposed groups, since the event is assumed to have reached every wallet, with only exposure to Tornado Cash varying. This difference remains vulnerable to a group-specific shock, which would bias β and threaten the validity of our estimates. The interval does not capture that bias. Nor does β measure the average effect of past Tornado Cash use, a reading that requires the stronger assumption of factorial parallel trends (Xu et al., 2026). We write β_D and β_A for the β of Equation (1) on uncapped D_{it} and A_{it}, since the coin-by-coin split below is exact only without caps. For this split, suppose $\beta_D = -0.10$ and $\beta_A = 0$. These hypothetical coefficients imply that, after the event, exposed wallets show a 0.05 rise in their USDT balance and a 0.05 fall in their USDC balance relative to non-exposed wallets, in units of their usual holdings. This split is arithmetic on hypothetical values; it provides no guaranteed USDT gain, no tested bound, and no traced purchase.

Signing and Design-Stage Access

We signed and hashed the pre-specification on 28 October 2026, after substantial design-stage access, including issuers' lifetime freeze logs, a short sample of post-event transfer logs covering 200 blocks, and the baseline transfer logs. This access means that the signed document makes no claim of complete blindness to post-event information. An addendum dated 28 October records

omissions from that document, including three placebo runs without USDC-share matching that were on disk before signing but missing from section 0. The signed document fixes all eight choices left open in the research proposal (Table A1). Our only confirmatory estimate for H1 is the pooled 35-day β, with no split at 24 August, the date of Tether's statement. Alongside this pooled estimate, we report daily coefficients relative to 7 August, excluding 8 August. We expect this daily path to show a jump from 9 August, or a build-up past 24 August. Once post-event data was opened, we kept the rule that no new outcomes, windows or subgroups enter the confirmatory analysis; later analyses are labeled exploratory and dated separately.

Robustness Rows

A pretrend plot shows the day-by-day coefficients over the 60 baseline days, including 30 June 2022, when Tether froze 46 addresses in one day. The largest one-day pre-event drop in the capped-D coefficient occurs from 30 June to 1 July, as the note to Figure 1 shows. We rebuild the placebo using only June data, with an exposure cutoff of 30 June, frame windows within 9–30 June, and floors of \$100 and a nonzero balance on 6 of 22 days. In this placebo, a fake event on 9 July, itself excluded, lets us compare 9 June–8 July with 10 July–7 August. The placebo effect is −0.0249 and passes its rule, with 3,007 exposed wallets, 8,381 controls and 508 strata. This pass does not prove parallel trends. An earlier run on 15–16 October was used for development only. All robustness rows share one baseline, so their agreement is no test of parallel trends. We also consider Binance's announcement on 5 September 2022 that it would convert its users' USDC, USDP and TUSD balances into its branded stablecoin, BUSD, issued by Paxos (Anadu et al., 2023), while leaving USDT alone. The balances were converted on 29 September (Binance, 2022). Unequal Binance use by group may lead to a biased β, with an unknown sign, and Binance use itself cannot be measured without verified exchange labels. We therefore estimate a second β over a window ending on 4 September. If this shorter run falls in a different case of Table 3 than the main run, we label the result Binance-sensitive. Alongside this window check, we use a second estimator, a doubly robust difference-in-differences (DiD), on the unmatched sample, screened to floor-passing wallets and without weighting (Sant'Anna & Zhao, 2020). Table 6 presents the status of every robustness check and the reason for any rows that are not estimable.

Appendix C. Signed Design, Placebo Record and Exploratory Checks

Table A1: Signed design parameters that govern the Results

Parameter	Signed pre-specification (governs)
Status	Signed and hashed privately, 28 October 2026 (signing.json, private analysis repository commit 87c33de), after the design-stage access listed in its section 0; post-event estimation run afterwards on 28 October
Exposed sample	86,571 exposed externally owned addresses; 8,370 with a baseline USDC/USDT Transfer; 4,502 pass the floors; 4,501 eligible (1 blacklisted in baseline); 4,487 matched (14 without a control)
Controls and strata	11,495 controls in 692 strata
Participation rule	Both groups need at least one USDC or USDT Transfer log in the baseline

Table A1 (continued)

Parameter	Signed pre-specification (governs)
Floors and screens	Floors on combined USDC + USDT; baseline blacklist reconstructed from opening state and changes; zero-address issuer screen only; 38 frozen addresses excluded
Wallets frozen by either issuer	Frozen in the baseline: excluded; frozen 8 August–12 September (other than the 38 sanctioned addresses): kept and counted per group, with a drop-and-rematch robustness row
Exposure cutoff	Block 15,298,282 (7 August 2022, 23:59:57 UTC); externally owned addresses only
Matching covariates	Mean combined holdings; USDC share in three bins (exactly 0, strictly between 0 and 1, exactly 1); transfer count; wallet age; ETH balance. Exchange share dropped (no verified labels)
H1 outcome	Capped D; caps at pooled baseline 1st/99th percentiles: D [−3.0927, 4.4768], A [0, 5.3170]
Uncapped veto	H1 supported only if neither uncapped-D interval lies wholly above $-\delta$
Clip flag	Cap-sensitive if the exposed-minus-control change in the clipped share exceeds 1 percentage point in either direction; reported beside H1
Clustering	Wallet and matching stratum; both must be in case 1
Case 3 boundary	Lower bound above $-\delta$ and upper bound at most δ
H1a	Confirmatory-secondary companion test, $\delta_A = 0.05$, four-way classifier
Coin legs	Uncapped USDC/B and USDT/B, descriptive only
H2	Identified deposits, groups 0 / 1 / 2 or more; exploratory, pattern only
Wallets with USDC in Tornado Cash USDC pools	Flagged (possible direct loss), kept; flagged vs unflagged comparison exploratory
Placebo of record	June-only rebuild, fake event 9 July; pass if both clusterings in case 3
Power (planning figure)	Probability 0.854 that the interval clears −0.10 if the true effect is −0.15 (June-placebo standard error 0.0166, stratum); minimum detectable effect 0.1465; frame not enlarged
Doubly robust DiD	Unweighted, on the unmatched screened population, trimming at 0.99; a check, not an arbiter
Not estimable	Placebo cohort; Binance and exchange flags; deposit-address reuse; owner-grouped inference. Outflow breakdown deferred, exploratory if run

Source: Signed pre-specification of 28 October 2026.

Table A2: Placebo runs before signing, by specification (fake event 9 July, capped D)

Run	Specification	β	95% CI, wallet	Case (wallet / stratum)
15 October (listed in section 0)	No USDC-share matching	−0.022	[−0.057, 0.013]	3 (wallet; no stratum figure in section 0)
15 October (listed in section 0)	Three-bin USDC-share matching, development population (the 15 October build, 3,785 exposed)	0.00094	[−0.0366, 0.0385]	3 (wallet; no stratum figure in section 0)
16 October	No USDC-share matching	−0.068	[−0.094, −0.042]	3 / 3
23 October	No USDC-share matching	−0.121	[−0.150, −0.092]	2 / 2
23 October (listed in section 0)	June population before the participation rule	−0.0351	[−0.0648, −0.0053]	3 / 3
28 October	No USDC-share matching	−0.111	[−0.141, −0.082]	2 / 2
Of record	Three-bin USDC-share matching, June population with the participation rule	−0.025	[−0.055, 0.005]	3 / 3, pass

Note: The 16, 23 and 28 October runs were on disk before signing and are listed in the 28 October 2026 addendum, which also dates the 15 October run to 15 October 23:36 to 16 October 14:50. The 15 October run's third USDC-share matching variant (USDC share coarsened at exposed-wallet quartiles) was also inspected; section 0 gives no figure for it. The 23 October run before the participation rule (3,007 exposed, 8,495 controls, 367 strata) was seen before the population of record was chosen. **Source:** Signed pre-specification and its addendum of 28 October 2026.

Table A3: Heuristic random-walk band (exploratory, post-results, dated 28 October 2026)

Outcome	Detrended lag-1 autocorrelation of the daily path	Band for the 35-day post-event mean
Capped gap D	0.917	±0.076 (standard deviation 0.039)
Capped combined holdings A	0.875	±0.093

Note: A heuristic for how far the 35-day post-event mean could drift if the daily path were a random walk with no event. It applies to the post-event mean, not to the two-day move at the event, and it does not reclassify H1. Trend extension: In trend extrapolations (exploratory, post-results, dated 28 October 2026), run with one committed script and leaving the H1 classification unchanged, we extended the June to July trend of the gap past the designation of 8 August 2022. The post-event mean lies below the extension in every window used to fit it. The shortfall is 0.003 to 0.123 across 12 named start windows, 9 of them distinct; the 0.055 for the trend fitted over all 60 baseline days is one of the 12. The sign is the same in every window, but the size depends on the window. We adopt no single size, since picking a window after the results would fit the choice to them. **Source:** Author's calculations from Ethereum event logs.

Table A4: Exploratory sensitivity checks run after the signed results: extended pre-period and sensitivity of the post-event mean to departures from parallel trends (HonestDiD package), capped D and A

Quantity	D
Panel A. Extended pre-period, 1 March 2022 to 7 August 2022, capped, full sample (15,982 matched wallets)	
7→9 Aug two-day move	−0.091
Rank of that move by absolute size among 158 two-day moves	1 of 158
Uncapped 7→9 Aug move and rank (descriptive only)	−0.066, rank 88 of 158
Mean daily coefficient, 1 Mar – 8 May 2022 (L)	0.153
L, 95% interval, wallet-clustered	[0.101, 0.206]
L, 95% interval, stratum-clustered	[0.093, 0.213]
Mean daily coefficient, 9 Jun – 7 Aug (m)	0.049
Level of L (high / low / between), before tie-break	between
Level of L, reading of record	**between**
Pre-set Terra trigger (11→13 May move)	not fired

Coverage and clipping, March 2022 (exposed / control): valid-balance share 100% in both arms; zero combined holdings 55.855% / 54.453%; D upper cap clips 4.384% / 2.637%; D lower 2.088% / 2.635%; A upper 5.653% / 4.225%; A lower 0% / 0%. Reported without a threshold.

Quantity	D	A
Panel B. HonestDiD relative-magnitudes sensitivity, weekly calendar, post mean against the 1–7 Aug reference week		
Target: post mean minus 1–7 Aug week	−0.1027	−0.1061
$\bar{M} = 0$ interval, wallet-clustered	[−0.132, −0.074]	[−0.136, −0.076]
$\bar{M} = 0$ interval, stratum-clustered	[−0.134, −0.071]	[−0.138, −0.074]
Breakdown $\bar{M}$ for $\tau = 0$, wallet	[0.427, 0.427]	[0.392, 0.392]
Breakdown $\bar{M}$ for $\tau = 0$, joint (binding: stratum)	[0.400, 0.400]	[0.371, 0.371]
Breakdown $\bar{M}$ for $\tau = -0.05$, wallet	not run (planned for A only)	[0.160, 0.160]

Table A4 (continued)

Quantity	D	A
Breakdown $\bar{M}$ for $\tau = -0.05$, joint (binding: stratum)	not run (planned for A only)	[0.142, 0.142]
$\tau = -0.10$	no initial clearance (not cleared even at $\bar{M} = 0$)	no initial clearance (not cleared even at $\bar{M} = 0$)

Note: Exploratory, computed after the signed results were known; none of this reclassifies H1 or H1a. The plan for both checks was committed on 28 October 2026, before any of their output. Of three dated addenda to that plan, the first was written after the HonestDiD output was read and before any extended-path coefficient, and the other two after every output had been read; runs are dated 29–30 October, and figures were corrected on 1 November. Panel A extends the same event-study gap back to 1 March 2022 for the same signed wallets, weights and caps, with nothing re-matched; eligibility, scaling and caps were fixed on 9 June – 7 August information, so a March path is conditioned on later eligibility and later scaling. The first two rows give the 7→9 August two-day move and its rank by absolute size among all two-day moves on the 1 March – 7 August path; it describes one move on one path and is not a p-value, and the uncapped rank is shown only to make clear that the rank is a capped reading. The uncapped value (−0.066, rank 88 of 158) is recorded in an addendum to the exploratory plan, not in an output file. L is the mean gap over 1 March – 8 May 2022 and m the mean over the 60-day baseline, both relative to 7 August; the "between" reading supports nothing beyond the paths and these quantities, and L cannot separate a drift from a driftless random walk. The Terra trigger checks whether the Terra collapse (11→13 May 2022) moved the path more than the event did. Bands for L were fixed in the plan: high if L ≥ +0.20, low if L ≤ +0.10, between otherwise (here L = 0.153, between). The band before tie-break is the band of the full-sample L; the tie-break only downgrades, reading between if a restricted-sample row of the plan's variant run falls in a different band; the reading of record is the band after the tie-break. In Panel B, the relative-magnitudes restriction was computed on the weekly calendar only; the smoothness restriction exists only on the daily calendar and only for stratum clustering, where the joint wallet-and-stratum result was not determined, so it is not reported. $\bar{M}$ is a per-step allowance: the largest permitted post-period step in the departure from parallel trends, as a multiple of the largest pre-period step. It is not a bound on the level, because departures accumulate across steps. Breakdown is the smallest $\bar{M}$ at which the robust interval's upper bound is no longer strictly below τ; brackets are the conservative bisection brackets. **Source:** Author's calculations from Ethereum event logs.

Table A5: Pre-specified reporting: cap-clip incidence and baseline USDC-share diagnostics

Outcome	Tail	Exposed, baseline	Exposed, post	Control, baseline	Control, post	Exposed change (pp)	Control change (pp)	Exposed minus control (pp)	Cap-sensitive flag
Panel A. Cap-clip incidence, share of wallet-days strictly beyond the cap, weighted (percent)									
D	lower	0.938	1.176	0.889	1.274	0.238	0.384	−0.146	FALSE
D	upper	1.174	1.938	0.852	2.003	0.765	1.151	−0.387	FALSE
A	lower	0.000	0.000	0.000	0.000	0.000	0.000	0.000	FALSE
A	upper	1.122	2.190	0.859	2.253	1.068	1.394	−0.326	FALSE

Rule: H1 is flagged cap-sensitive if, at either tail of either cap, the exposed-minus-control difference in the post-minus-baseline change of the clip share exceeds 1 percentage point in either direction. No cell exceeds it (largest absolute difference 0.387 pp, D upper).

Diagnostic	Quantity	Value
Panel B. Baseline USDC-share diagnostics (descriptive; no test statistic, alpha or threshold; select no matching rule)		
(a) Mean USDC share, 60 baseline days	Eligible exposed wallets (n = 4,501)	0.784
	Unmatched control wallets (n = 71,779)	0.352
	Difference	0.432
	Only USDC (share = 1): exposed / control	0.586 / 0.246
	No USDC (share = 0): exposed / control	0.138 / 0.559

Table A5 (continued)

Diagnostic	Quantity	Value
(b) Within-wallet persistence, unmatched control wallets with both half-period shares defined (n = 63,322; 8,457 excluded)	Slope of second-half-minus-first-half change on first-half share	−0.070
	Correlation	−0.181

Note: Panel A gives, for each cap, the weighted share of wallet-days strictly beyond it by arm, tail and period, as the pre-specification requires; caps are $D \in [-3.093, 4.477]$ and $A \in [0, 5.317]$. A cap-sensitive flag would be reported beside the H1 decision and would block any reallocation wording; it would not change the H1 case. Panel B: diagnostic (a) compares the USDC share, USDC ÷ (USDC + USDT) as a ratio of sums over the 60 baseline days, between eligible exposed wallets and the unmatched controls; diagnostic (b) splits the 60 baseline days into days 0–29 and 30–59 and regresses the change in share on the first-half share. Both were fixed in the pre-specification, are computed and reported, and select nothing. **Source:** Author's calculations from Ethereum event logs.

Table A6: Baseline balance, matched sample (descriptive, computed 5 November 2026 after the signed results)

Variable	Exposed mean	Exposed SD	Exposed median	Matched control mean	Matched control SD	Matched control median	SMD	Unmatched control mean
Mean holdings B (USD)	133,554	1,334,784	7,924	349,520	13,032,310	7,925	−0.023	184,270
USDC share	0.785	0.380	1.000	0.753	0.395	1.000	0.082	0.280
Transfer count (baseline)	11.512	23.888	5.000	81.171	2,025.063	5.000	−0.049	84.648
Wallet age (blocks; sent-wallets only)	3,138,192	2,911,513	2,374,759	3,133,875	2,945,095	2,375,788	0.001	1,572,427
Never-sent (share)	0.003	0.058	0.000	0.003	0.058	0.000	0.000	0.060
ETH balance (ETH)	24.417	253.593	0.561	44.283	1,420.940	0.559	−0.019	58.370
Baseline D (capped)	0.551	0.715	—	0.500	0.752	—	0.069	−0.378
Baseline A (capped)	0.953	0.148	—	0.968	0.117	—	−0.111	0.947

Variable	Exposed mean	Exposed median	Trimmed control mean	Trimmed control median	SMD
Panel B. Matched controls trimmed to the exposed range (exploratory, 10 November 2026)					
Mean holdings B (USD)	133,554	7,924	130,970	7,762	0.002
USDC share	0.785	1.000	0.755	1.000	0.077
Transfer count (baseline)	11.512	5.000	16.118	5.000	−0.146
Wallet age (blocks; sent-wallets only)	3,138,192	2,374,759	3,125,569	2,365,989	0.004

Table A6 (continued)

Variable	Exposed mean	Exposed median	Trimmed control mean	Trimmed control median	SMD
Never-sent (share)	0.003	0.000	0.003	0.000	−0.001
ETH balance (ETH)	24.417	0.561	20.258	0.546	0.017

Note: Matched exposed N = 4,487; matched controls N = 11,495 (weights sum to 11,495.000); unmatched control-frame wallets N = 60,284 (unweighted, for reference): the frame wallets left over after matching (71,779 − 11,495), not the full 71,779-wallet control frame that Table A5 Panel B and Section 6 compare before matching. Exposed wallets carry weight 1; each matched control carries its signed coarsened exact matching (CEM) stratum weight (pre-specification section 1). Means and SDs are weighted (SD uses the weight-normalised variance, divisor Σw). Baseline window 9 Jun – 7 Aug 2022 (60 days; 8 Aug excluded). B is the baseline mean combined USDC + USDT holdings in USD. Baseline D and A are each wallet's 60-day mean of the capped daily D = (USDC − USDT)/B and A = (USDC + USDT)/B, with the signed caps (D [−3.093, 4.477], A [0, 5.317]). Wallet age is for wallets with at least one send; the never-sent row gives the share without a send. Five CEM matching traits: B, USDC share (three bins), transfer count, wallet age, ETH balance; D, A and never-sent are not matched on directly. SMD = (exposed mean − matched control mean) / $\sqrt{((SD^2 \text{ exposed} + SD^2 \text{ control})/2)}$. Medians are weighted. Panel B drops the 122 matched controls that lie outside the exposed wallets' minimum-to-maximum range on B, transfer count, ETH balance or wallet age (11,373 controls kept); it is a diagnostic run after the signed results and changes no estimate. Descriptive; no test; selects nothing.

Table A7: June placebo of record (fake event 9 July 2022; three-bin USDC-share matching; 3,007 exposed, 8,381 controls, 508 strata): capped and uncapped outcomes and cap-clip incidence by arm

Outcome	β	SE, wallet	SE, stratum	95% CI, wallet	95% CI, stratum	Reading
Panel A. Estimates						
Capped gap D (H1 placebo of record)	−0.0249	0.0152	0.0166	[−0.0547, 0.0049]	[−0.0575, 0.0077]	Case 3 (negligible) / 3 (negligible); pass
Uncapped gap D	0.0235	0.3618	0.3619	[−0.6856, 0.7326]	[−0.6874, 0.7345]	Case 2 (inconclusive) / 2 (inconclusive)
Capped combined holdings A (H1a placebo)	−0.0192	0.0161	0.0172	[−0.0506, 0.0123]	n/a	Inconclusive (wallet)
Uncapped combined holdings A	0.4856	0.3621	0.3663	[−0.2243, 1.1955]	n/a	Inconclusive (wallet)
USDC leg, uncapped, scaled by B (descriptive)	0.2545	0.2641 †	—	—	—	—
USDT leg, uncapped, scaled by B (descriptive)	0.2310	0.2475 †	—	—	—	—

Table A7 (continued)

Outcome	Tail	Exposed, baseline	Exposed, post	Control, baseline	Control, post	Exposed change (pp)	Control change (pp)	Exposed minus control (pp)	Cap-sensitive flag
Panel B. Cap-clip incidence, share of wallet-days strictly beyond the cap, weighted (percent)									
D	lower	0.818	1.135	1.099	1.508	0.317	0.408	−0.091	FALSE
D	upper	1.417	3.237	1.172	2.963	1.821	1.791	0.029	FALSE
A	lower	0.000	0.000	0.000	0.000	0.000	0.000	0.000	FALSE
A	upper	1.440	3.565	1.317	3.575	2.125	2.258	−0.133	FALSE

Note: Prespecified reporting (section 5 item 2 and 5a). Placebo of record: June-only rebuild with the baseline participation rule over 9–30 June; contrast 9 June–8 July with 10 July–7 August; caps from 9–30 June information. Uncapped rows are descriptive and do not override the capped placebo. Clip rule as in Table A5: flag if the exposed-minus-control difference in the post-minus-baseline change exceeds 1 percentage point; the largest absolute difference is 0.133 pp. † Wallet-clustered standard error, as recorded in the pipeline manifest. Stratum-clustered intervals for A are not written by the pipeline. **Source:** Author's calculations from Ethereum event logs (placebo-of-record run of the signed pipeline).

Table A8: Cap sensitivity, single coins and cap-free outcomes (exploratory, post-results, dated 12 November 2026)

Row	β	95% CI, wallet	95% CI, stratum
Panel A. Capped gap D at other caps (own cut-points)			
0.1 / 99.9 percentile	−0.151	[−0.197, −0.105]	[−0.201, −0.101]
0.5 / 99.5 percentile	−0.155	[−0.191, −0.120]	[−0.195, −0.116]
1 / 99 percentile (signed cap)	−0.152	[−0.184, −0.120]	[−0.188, −0.116]
2.5 / 97.5 percentile	−0.147	[−0.175, −0.119]	[−0.179, −0.115]
5 / 95 percentile	−0.140	[−0.164, −0.116]	[−0.168, −0.112]
No cap	0.817	[−0.174, 1.808]	[−0.178, 1.812]
Panel B. Uncapped D, leaving out the wallets with the largest contribution			
Leave out 1 wallet	0.386	[−0.131, 0.902]	[−0.138, 0.909]
Leave out 5 wallets	−0.053	[−0.197, 0.092]	[−0.203, 0.098]
Leave out 10 wallets	−0.127	[−0.237, −0.016]	[−0.245, −0.008]
Leave out 20 wallets	−0.156	[−0.235, −0.077]	[−0.242, −0.070]
Panel C. Single coins, one shared cap (pooled 1 / 99 percentile)			
USDC / B, capped	−0.147	[−0.174, −0.120]	[−0.177, −0.117]
USDT / B, capped	0.003	[−0.012, 0.019]	[−0.015, 0.022]
Panel D. Outcomes that need no cap			
Holds any USDC (share of wallets)	−0.094	[−0.108, −0.081]	[−0.110, −0.078]
Address emptied of both coins (share of wallets)	0.066	[0.052, 0.079]	[0.047, 0.084]

Note: β in units of baseline combined holdings B (Panels A to C) and in shares of wallets (Panel D), relative to matched wallets, 35 post-event days against the 60-day baseline. Panel A: the two-day move from 7 to 9 August lies between −0.080 and −0.105 at every cap, every interval below zero (without a cap: −0.066, interval contains zero). Panel B: contributions are each wallet's share of the uncapped β; the 10 largest are 9 exposed wallets and 1 control. Panel C: the single coins are relative changes; the USDT interval contains zero, so no change in USDT holdings is shown; the capped USDC fall is exploratory and does not show a switch. Without caps the legs are +0.72 (USDC) and −0.10 (USDT), neither statistically significant at the 5 percent level (Table 4). Shifting the event date to each of 65 fake dates from 30 April to 3 July 2022 (each with its own 60-day baseline and 35 following days, all before the event) gives contrasts between −0.113 and +0.006; the real −0.152 is the most negative of the 66, but the windows overlap, so this ranking is descriptive and not a test (path and caveats as in Table A4, Panel A). The plan for these checks was hashed before any of their output was read; they were run after the results, are not confirmatory and do not reclassify H1. **Source:** Author's calculations from Ethereum event logs.