MORE THAN A PIPE DREAM

HOW PIPE FINANCING SHAPES POST-MERGER PERFORMANCE IN SPACS

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More Than a PIPE Dream: How PIPE Financing Shapes Post-Merger Performance in SPACs

Abstract:

This paper examines whether Private Investment in Public Equity (PIPE) backed De-SPAC mergers generate different returns compared to their non-PIPE counterparts. Using a sample of 210 De-SPACs completed in the United States between 2022 and 2024, we compare buy-and-hold abnormal returns (BHAR) between 126 PIPE-backed and 84 non-PIPE-backed mergers over one-week and one-year horizons. The results confirm that PIPE-backed De-SPACs exhibit mean BHAR of 22.6 and 17.0 percentage points higher than non-PIPE-backed peers at the one-week and one-year horizons respectively. PIPE-backed deals also exhibit systematically lower redemption rates, consistent with the *certification effect* in which PIPE participation signals deal quality to the market. These findings address a gap in prior literature, as existing studies examine external financing as a continuous variable and do not directly test whether the binary presence of PIPE financing is associated with better post-merger returns. Given the consistent underperformance of De-SPACs, and SPACs' large share of recent US IPO activity, the binary presence of PIPE financing offers common shareholders and institutional investors a credible signal of deal quality when evaluating SPAC investments.

Keywords:

SPAC, De-SPAC, PIPE financing, Mergers and acquisitions, Post-merger performance

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1. Introduction

Traditionally, when a private company seeks to raise capital and broaden its access to financing from public investors, it undergoes an Initial Public Offering (IPO). Through this process, the company transitions from private ownership to becoming a publicly traded, listed entity.

A Special Purpose Acquisition Company (SPAC) is a blank check company[1] created by a sponsor with the purpose of acquiring a private company and take it public through a merger. The SPAC first goes public itself through an IPO to raise capital. This capital is raised by selling units, typically priced at $10, which are made up of one redeemable share and one derivative security which gives the holder the option to purchase an additional share at an agreed price at a future date (Banerjee and Szydlowski, 2024). The raised capital is then placed in a trust account where it generates interest up until a merger agreement is reached (Feng, Nohel, Tian, Wang, and Wu, 2026). The SPAC aims to merge with a private company seeking to go public within 18 to 24 months, offering an alternative to the traditional IPO process.

After a SPAC has identified a target company and reached a merger agreement, shareholders vote on whether the proposed merger should be consummated and independently decide on whether they want to participate or redeem their shares. If they decide to redeem their shares they receive their initial investment back with accrued interest. Since investors always can recover their invested capital plus accrued interest, the SPAC unit can be viewed as a "default-free convertible bond with extra warrants" (Gahng, Ritter, and Zhang, 2023, p. 3467).

As investors have the ability to redeem their shares, there exists a risk of exhausting the trust account, which would threaten the success of an SPAC merger. To mitigate this risk, sponsors often seek Private Investment in Public Equity (PIPE) investments, typically from institutional investors, as a part of the merger.

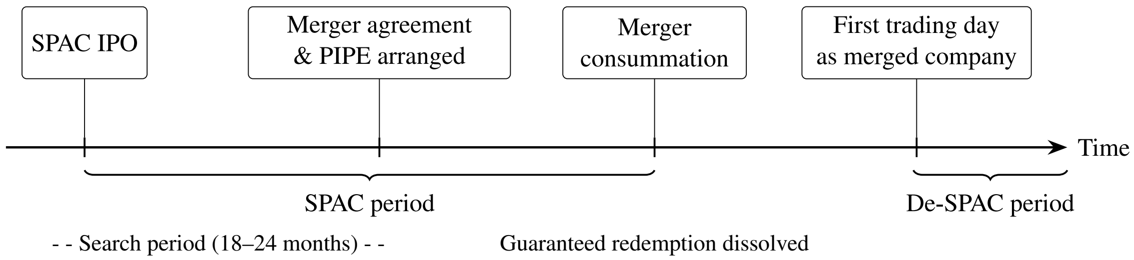
If the proposed merger gets approved by the shareholders, and the SPAC has enough cash to fulfill the terms of the merger agreement, the SPAC starts to trade as a merged entity under a new ticker. If the SPAC fails to consummate a merger within the planned timeline, it must liquidate, returning all raised capital with accrued interest to the investors.

Due to the nature of SPACs, their lifecycle can be divided into two distinct phases which Gahng et al. (2023) refers to as the SPAC Period and De-SPAC period. The SPAC period spans from the SPAC's IPO to either the completion of a merger with the target company or the liquidation of the SPAC. The De-SPAC period commences on the first trading day of the newly merged entity. The lifecycle of SPAC IPOs is illustrated in Figure 1:

Even though it is often more costly to go public via a SPAC merger compared to a traditional IPO process, there are multiple relative advantages with merging with a SPAC. Gahng et al. (2023) identified five advantages that may explain the benefits for companies choosing to go via a SPAC merger.

[1] An entity formed without operations or specific business targets, created solely to raise capital for a future acquisition.

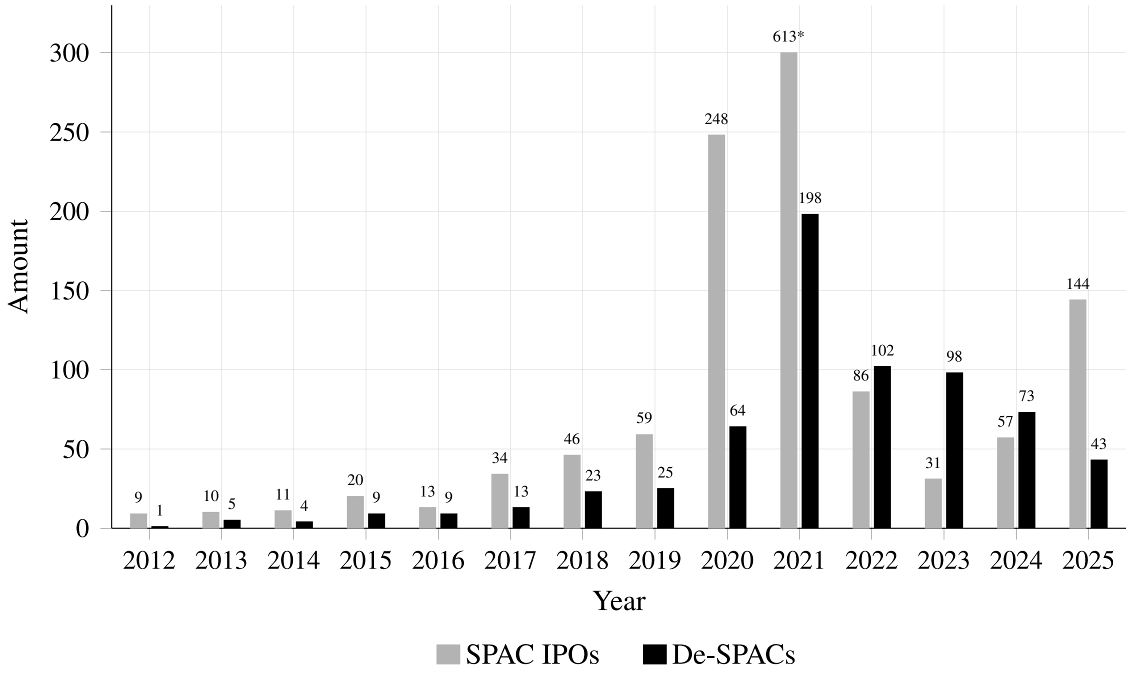
Figure 1: Timeline and Lifecycle of SPACs



Firstly, SPAC sponsors often have deep sector expertise and can, apart from capital, also provide mentorship and business insight. Secondly, SPAC mergers are frequently argued to be faster than traditional IPOs as it avoids the traditional underwriting process. Thirdly, SPAC mergers allow target companies to make more optimistic forecasts than they could in a traditional IPO, as they are shielded from legal liability under merger law. Another benefit is that SPAC mergers offer greater valuation certainty as the merger terms can be agreed before the deal is exposed to the broader market, unlike traditional IPOs that determine the offer price after the traditional underwriting process. Finally, the flexibility in SPAC structures also allows sponsors and underwriters to renegotiate their compensation in order to preserve a deal when faced with adverse conditions that threaten it.

Although SPACs were first introduced in 1993 (Morley, 2022), it was not until 2020 that they gained significant traction. In 2021, SPACs accounted for more than 60% of all IPO activity in the United States, raising close to $220 billion in capital. While the broader equity markets sold off in 2022, De-SPAC completions continued to gain market share. Of the 414 private companies that went public in the US between 2022 and 2023, 200 did so via De-SPAC mergers, underscoring SPACs' continued relevance in the capital markets (Ritter, 2026), see Figure 2. Despite benefits and growing prevalence, De-SPACs have consistently delivered poor post-merger returns to shareholders, with prior studies documenting substantial underperformance compared to broader market benchmarks (Gahng et al., 2023).

Figure 2: SPAC IPOs and De-SPAC mergers in the United States, 2012–2025



** 2021 SPAC IPO bar is capped at 300 for scale; the actual value is 613.*

This paper focuses on De-SPAC mergers completed in the United States between 2022 and 2024. By looking at this window, it captures the most recent De-SPAC activity while also including SPACs that raised capital during the 2021 boom but only completed their mergers in subsequent years. De-SPACs completed in 2021 are deliberately excluded due to the extraordinary market conditions, characterized by PIPE participation rates above 95%. This would introduce deal-specific conditions into the PIPE subsample that would distort the comparison with non-PIPE transactions, regardless of broader market adjustments.

Despite the rapid growth of SPAC activity and their large share of total IPO activity, the post-merger performance of De-SPAC companies for common shareholders remains poorly understood. This paper therefore set out to directly test:

> *Do PIPE-backed De-SPAC mergers generate different post-merger returns compared to their non-PIPE counterparts?*

The hypothesis is that PIPE backed De-SPACs generate post-merger returns higher than their non-PIPE-backed counterparts. This expectation rests on the *certification effect* discussed by Feng et al. (2026). The effect suggests that PIPE investors are incentivized to only invest in companies that they believe will perform strongly, sending a quality signal to the market that both discourages redemption and leads to better post-merger returns. Feng et al. (2026) also identify a competing *financing effect* in which sponsors bring external capital specifically to deals threatened by high redemption rates[2], predicting a

[2]Percentage of shares redeemed for cash from the trust by shareholders rather than retained in the merger.

negative association between external capital and post-merger returns. While both effects may be present, this paper tests whether the net effect of PIPE presence, as a binary distinction, is positive.

Notably Feng et al. (2026) document these effects using total external financing, this paper theorizes that their finding holds for PIPE specifically rather than other external financing sources such as Forward Purchase Agreements (FPAs).

2. Literature Review

Given that SPACs have only gained significant traction in recent years, prior research on the subject is limited. However, two papers are central to the understanding of the field and build a foundation for our research.

The first paper, Gahng et al. (2023), studied 1071 SPAC IPOs listed in the United States between January 2010 and December 2021 and examined the different returns of three key participants in SPACs: sponsors, PIPE investors, and public market investors. The paper found that sponsors received the best returns, secondly PIPE investors, and lastly public market investors. They also found that, on average, investors in the SPAC period received better returns than investors in the De-SPAC period.

Although Gahng et al. (2023) found that a higher PIPE backing is associated with better outcomes for common shareholders, they did not test whether PIPE backing itself improves returns. Moreover, their analysis of PIPE backing isolated PIPE backed deals, excluding all deals without PIPE entirely.

In the second paper, Feng et al. (2026) examined the interactions among sponsors, target companies, and common shareholders in the De-SPAC process. As part of their research, they examined how the amount of external capital raised relates to post-merger returns and identified two opposing channels, a *certification effect* and a *financing effect*. Empirically, the *certification effect* dominates, yielding a net positive correlation between amount of external capital raised and post-merger performance. They treat external capital as a continuous variable and do not address whether PIPE backing itself carries an independent effect on post-merger returns. To address this gap, this paper focuses on the binary question of PIPE presence, which neither prior paper directly tests. The binary comparison between PIPE-backed and non-PIPE-backed De-SPACs is well suited to examine the *certification effect*, as the mere presence of PIPE investors is what drives the quality signal to the market.

Thus, this paper extends previous research in three ways. Firstly, it directly compares post-merger returns between PIPE-backed and non-PIPE-backed De-SPACs by using a binary classification, testing whether the presence of PIPE financing is associated with better post-merger returns. Secondly, this comparison is robustness tested, which neither prior paper applies to the PIPE question. Lastly, the 2022–2024 sample captures more recent De-SPAC activity than either prior study.

The findings from this paper would be directly relevant for common shareholders and institutional investors evaluating SPAC investments. Understanding whether PIPE backing is associated with better post-merger returns can help investors assess deal quality. However, if this research proves that the *certification effect* holds, it carries risk of becoming self-defeating over time. PIPE investors may invest in SPACs regardless of the underlying company quality, anticipating that their participation will generate abnormal returns regardless, and sponsors may seek PIPE backing purely to produce a positive market signal rather than as a genuine endorsement of the target.

3. Data

3.1 Identifying SPACs and Collecting SPAC Data

The De-SPAC data for the period 2022-2024 has been collected from the ListingTrack De-SPAC list (ListingTrack, 2026). Based on this data set, a total of 257 SPAC mergers were completed during this period. While ListingTrack is widely used for SPAC tracking, any independent verification that the database captures the complete list of De-SPAC transactions during this period has not been conducted. Also, as ListingTrack only tracks active listings, delisted De-SPACs at the time of data collection were automatically excluded from the data set. While systematic differences between delisted or omitted transactions, and those in our sample, cannot be ruled out, there is no reason to believe that these differences are large enough to materially affect the results. In addition to the name of the De-SPACs, the data set also provided pro-forma equity value (PFEV)[3] and redemption rates for each merger. Information on whether each company raised PIPE financing was also partially available through ListingTrack but did not provide complete coverage. As a result, the majority of PIPE data was manually verified through S-4 filings in the SEC's EDGAR database (U.S. Securities and Exchange Commission, 2026). Therefore, although the verification process was conducted carefully, it cannot fully rule out small inconsistencies in the final data set.

Closing prices for all relevant dates were sourced from Yahoo Finance's historical stock price database (Yahoo Finance, 2026). Information on the stock exchange listing for each De-SPAC was extracted from the Refinitiv Eikon database (LSEG Workspace, 2026). ListingTrack and Yahoo Finance are freely accessible, while Refinitiv Eikon is a commercial database available through institutional subscription.

3.2 Cleaning the Data

After initial extraction, the sample of 257 De-SPACs was refined through several data-cleaning steps. Six observations could not be matched with PFEV data and one observation lacked redemption rate data, and were therefore excluded from the sample. When collecting closing prices from Yahoo Finance, an additional 27 observations were removed as price data was unavailable for the relevant dates, reducing the sample to 223 observations.

Finally, what we refer to as a *rolling redemption filter*, described in section 4.2, was applied to identify companies whose redemption date preceded T = -30. The filter flags observations exhibiting unusually large price movements at this time, as prices tend to move drastically once the redemption date has passed. Of the flagged observations, 13 were manually confirmed, through verification against SEC Form 8-K filings, to have redemption dates before the baseline and were subsequently removed. This resulted in a final sample of 210 observations, of which 126 are PIPE-backed and 84 are not, see Appendix A.

[3]The estimated equity value of the combined company at the moment the merger closes.

3.3 Descriptive Statistics

Table 1: Descriptive Statistics

Table 1 reports descriptive statistics for the main variables used in the analysis. Panel A presents raw returns for PIPE-backed and non-PIPE-backed firms across both horizons, Panel B reports the deal characteristics used as control variables, and Panel C reports the sample composition for the binary variables PIPE and Nasdaq.

Variable	N	Mean	Median	Std Dev	Min	p25	p75	Max
Panel A: Raw Returns								
1-Week Return (All)	210	-0.122	-0.395	2.106	-0.999	-0.651	-0.086	27.66
1-Week Return (PIPE)	126	-0.131	-0.319	1.028	-0.999	-0.553	0.006	9.03
1-Week Return (No PIPE)	84	-0.107	-0.510	3.094	-0.986	-0.792	-0.201	27.66
1-Year Return (All)	210	-0.438	-0.842	3.608	-1.000	-0.947	-0.547	51.20
1-Year Return (PIPE)	126	-0.628	-0.807	0.509	-1.000	-0.923	-0.514	3.08
1-Year Return (No PIPE)	84	-0.153	-0.886	5.679	-1.000	-0.976	-0.670	51.20
Panel B: Deal Characteristics								
PFEV, $M (All)	210	1,155.8	544.0	2,413.2	2.91	258.8	1,195.0	23,210
PFEV, $M (PIPE)	126	1,396.5	798.5	2,285.3	2.91	357.0	1,597.5	21,250
PFEV, $M (No PIPE)	84	794.6	339.0	2,565.0	54.28	202.3	673.0	23,210
Pct Redeemed, % (All)	210	90.19	96.31	15.88	0.09	88.03	99.07	99.95
Pct Redeemed, % (PIPE)	126	88.98	95.77	16.91	0.09	86.44	98.54	99.87
Pct Redeemed, % (No PIPE)	84	92.01	98.39	14.10	38.52	93.24	99.46	99.95
Panel C: Sample Composition								
PIPE-backed	126	60.0% of sample						
Nasdaq-listed	164	78.1% of sample						

Both the PIPE-backed and non-PIPE-backed subsample exhibit negative mean and median returns over both the one-week and one-year horizon. At the one-week horizon, PIPE-backed firms exhibit a mean return of -13.1% and a median return of -31.9% while firms without PIPE exhibit a mean return of -10.7% and a median return of -51.0%. At the one-year horizon, the PIPE-backed subsample exhibits a mean return of -62.8% and median return of -80.7%, while those without PIPE a mean return of -15.3% and median return of -88.6%. Non-PIPE transactions exhibit higher standard deviation than PIPE-backed transactions, most notable at the one-year horizon. Moreover, the gap between mean and median returns is also more pronounced for non-PIPE transactions over both horizons, indicating extreme observations in the right tail of the distributions, reported in the *Max* column.

Panel B reports systematic differences in deal characteristics between the two groups. PIPE-backed mergers are on average larger than non-PIPE-backed mergers, both looking at mean and median PFEV. The redemption rates for the two groups are both clustered near the upper bound of 100, with PIPE-backed mergers experiencing a lower median of 95.77%. The median across the full sample is equal to 96.3%, reflecting the typical pattern in De-SPAC transactions where most public shareholders choose to redeem their share prior to the merger date.

Panel C summarizes the sample composition, reporting that 60.0% are PIPE-backed firms, and 78.1% of the total sample of 210 firms are listed on Nasdaq.

4. Methodology

4.1 Ideal Methodology

An ideal research design for this study would address three key considerations. Firstly, the preferred baseline for measuring post-merger returns would be the exact redemption date for each SPAC, marking the precise point at which the $10 floor dissolves and the stock begins to trade freely. As redemption dates are not consistently reported across major financial data sources, and manual extraction from SEC Form 8-K filings proved too extensive and time-consuming, this paper has adopted a proxy instead.

Secondly, under ideal conditions a larger sample size would have been studied to increase the statistical power of the study. However, as SPACs only gained significant popularity from 2020 onwards, the availability of recent De-SPAC data is limited. While including 2021 De-SPACs would increase the number of observations, it was excluded due to the risk of including deal-specific conditions mentioned in section 1. The 2022 to 2024 period therefore represents the largest comparable sample within the scope of this study.

Lastly, an ideal study would measure post-merger returns over horizons longer than one year. However, given that our sample extends to the end of 2024, longer return windows are not yet available for a large portion of our observations. Furthermore, many De-SPACs become delisted within a few years after merger completion, which would introduce survivorship bias and either lead to biased results or a further reduced sample size.

4.2 Deciding the Baseline Date

When measuring post-merger returns, this study uses the price 30 days prior to the first trading day as a merged company, T = -30, as the baseline. This date serves as a proxy for the redemption date and is used throughout this paper as the reference point for computing post-merger returns. Prior to the redemption date, SPAC unit prices are supported by a floor through an arbitrage mechanism, as holders have the option to redeem their shares at the redemption date and retrieve their initial investment with interest, regardless of merger outcome. The redemption date therefore marks the point at which this $10 guaranteed floor dissolves and the stock begins reflecting market expectations of the merged company's true value.

The baseline, T = -30, was decided through a *rolling redemption filter*, constructed by comparing price movements across windows prior to the first trading day, with the goal of isolating a period where most companies have not had their redemption date. The method exploits the observation that SPAC prices move drastically after the redemption date has passed, and minimal movement is interpreted as a confirmation that the price floor remains active. The threshold applied to identify large price movements is ±5% as it should capture all large movements following the passed redemption date, see Appendix B.

As shown in Table 2, the filter was first applied to the T = -7 to T = -14 window and rolled back one week at a time. Large price movements were observed across a significant

Table 2: Rolling Redemption Filter

Detection window	Flagged outliers	Confirmed	False	Observation
$T = -7$ to $T = -14$	82			Large movements
$T = -14$ to $T = -21$	40			Large movements
$T = -21$ to $T = -28$	33			Large movements
$T = -28$ to $T = -35$	27			Large movements
$T = -30$ to $T = -35$	**20**	**13**	**7**	**Chosen baseline**

portion of the sample in each window indicating that redemption periods had already occurred. Given that T = -28 to T = -35 indicated a lower number of outliers, the close-end of the window was narrowed by two days, from T = -28 to T = -30. This adjustment produced a sharp drop in flagged observations from 27 to 20, implying that 7 companies in the sample have their redemption date falling between T = -28 and T = -30. Therefore, T = -30 is selected as the baseline, representing a clean one-month proxy substantiated by the low number of flagged outliers.

Of the 20 observations flagged in the T = -30 to T = -35 window, the redemption date was manually verified against SEC Form 8-K filings for each SPAC. 13 were confirmed to have a redemption date preceding T = -30 and were thus removed from the sample. The remaining 7 showed price movements caused by noise rather than redemption and were retained in the data set. This process resulted in a final sample of 210 observations.

4.3 Calculating the De-SPAC Returns

The post-merger stock performance is measured over two horizons: one week and one year after the merged entity's first trading day. The one week return reflects how investors price the company in the period directly following the start of public trading. The one year horizon captures an intermediate-term post-merger performance, allowing us to assess whether one-week reactions are reversed or sustained after short-term noise subsides. For each transaction, the post-merger returns are calculated as:

$$R_{i,h} = \frac{P_{i,h}}{P_{i,-30}} - 1, \tag{1}$$

where $P_{i,h}$ is the closing price of firm i at time h, where $h \in \{7, 365\}$, and $P_{i,-30}$ is our baseline ($T = -30$) price as established in Section 4.2.

4.4 Computing Buy-and-Hold Abnormal Returns

The raw returns calculated in 4.3 do not account for concurrent market movements. In order to isolate the firm-specific movements from the broader market movements, buy-and-hold abnormal returns (BHAR) is used. BHAR captures the difference between the realized return of the stock and the return of a benchmark over the same horizon

(Barber and Lyon, 1997).

4.4.1 Choice of Performance Measure

The choice of BHAR as performance measure is grounded in structural features of De-SPAC transaction design. The merged company does not have pre-merger trading history as the SPAC trades as a shell with a price floor. This makes the pre-merger stock price uninformative and rules out traditional event-study methods requiring a pre-event window. Additionally, an exact redemption date was not obtainable for our sample, precluding the use of an exact event date required by standard event study methodology. BHAR addresses these constraints by comparing realized post-merger returns to a benchmark over the evaluation period. Furthermore, as the study also evaluates post-merger performance over a longer horizon, not only short-term market reactions, BHAR is well-suited for this setting.

The benchmark return $R_{m,h}$ and BHAR are computed as follows, with the firm return $R_{i,h}$ defined in equation (1):

$$R_{m,h} = \frac{P_{m,h}}{P_{m,-30}} - 1, \tag{2}$$

$$\mathrm{BHAR}_{i,h} = R_{i,h} - R_{m,h}. \tag{3}$$

4.4.2 Choice of Benchmark

The benchmark return $R_{m,h}$ in equation (2) is computed using the Nasdaq Composite Index (.IXIC), motivated by the sample composition, where 78.1% of De-SPAC transactions are listed on the Nasdaq exchange. By using this benchmark we capture the most representative estimate to the market environment in which most De-SPACs trade. While Feng et al. (2026) use the Renaissance IPO ETF as their benchmark, arguing that newly listed firms provide a more natural comparison group, we deliberately avoid this approach. Given that a large proportion of newly listed companies during our sample period were themselves De-SPACs, using the Renaissance IPO ETF benchmark would be contaminated by the same population we are studying, undermining the validity of the benchmark adjustment.

4.4.3 Winsorization

Following the computation of the BHARs, they are winsorized at the 1st and 99th percentiles. BHARs are known to be positively skewed, as the downside is limited to -100% while the upside is unbounded (Barber and Lyon, 1997). The winsorization is only applied to the BHAR variable, as the independent variables are not subject to the same distributional concerns. All analyses presented in the following sections are conducted on the winsorized data.

4.5 Univariate Tests of Subsample Differences

The first way this study tests whether there is a difference between PIPE-financed and non-PIPE-financed post-merger abnormal returns is through a Welch two-sample t-test. It compares the mean BHAR, and accounts for the potentially unequal variances across our subsamples. However, as mentioned in section 4.4, the BHAR variable has a tendency to be right-skewed, and under such circumstances a small number of large positive observations can inflate the mean, rendering the mean-based comparison unreliable.

To account for the potential skewness in the data, the t-test is complemented with a non-parametric Mann-Whitney U test, comparing the rank distributions of the BHARs across the two groups. With our expectation that PIPE-backed transactions outperform non-PIPE backed transactions, both tests are specified as one-sided and estimated separately over a one-week and one-year horizon.

4.6 OLS Regression Model

To assess whether the association between PIPE financing and post-merger returns holds independently of deal-specific characteristics, ordinary least squares (OLS) regressions of the post-merger returns are estimated. The BHAR for transaction *i* over estimation window [-30, h], defined in equation (3), is the dependent variable. Separate regressions are estimated for the one-week window [-30, 7] and the one-year window [-30, 365]. The regression model is specified as follows:

$$\text{BHAR}_{i,h} = \beta_0 + \beta_1 \text{PIPE}_i + \beta_2 \text{PFEV}_i + \beta_3 \text{PctRedeemed}_i + \beta_4 \text{Nasdaq}_i + \varepsilon_i, \qquad (4)$$

where ε_i denotes the error term. We are primarily interested in β_1, capturing the marginal effect that PIPE financing has on abnormal returns after the business combination. The binary variable PIPE is the independent variable, equal to 1 if transaction *i* is PIPE-financed and 0 otherwise. The regression is estimated using HC3 heteroscedasticity-robust standard errors, as it outperforms alternative robust estimators in finite samples where $N < 250$ (Long and Ervin, 2000). Three control variables are included to account for deal-specific characteristics that may also independently influence the post-merger abnormal returns.

4.6.1 Control Variables

4.6.1.1 Pro-Forma Equity Value

Pro-Forma Equity Value measures the total equity value of the combined De-SPAC at merger consummation, expressed in intervals of $100 million, which may independently influence post-merger performance. Klausner, Ohlrogge, and Ruan (2022) documented that larger SPACs, measured by IPO size, tend to exhibit better post-merger returns compared to their smaller counterparts. Their finding suggests that scale, even when measured at an earlier stage, is a meaningful predictor of post-merger outcomes. Thus, controlling for PFEV isolates the marginal abnormal return of the PIPE variable from any variation attributable to overall deal size.

4.6.1.2 Redemption Rate

The redemption rate independently influences post-merger returns through its effect on share dilution. High redemptions spread the fixed costs of the SPAC structure, such as sponsor compensation and underwriting fees, across fewer shares which amplifies the per-share dilution. Klausner et al. (2022) document that this dilution is directly associated with worse post-merger returns. Controlling for the redemption rate therefore isolates the standalone PIPE effect from this independent influence on returns.

At the same time, PIPE financing is often raised in anticipation of redemptions, serving to offset the trust cash that sponsors expect to be depleted (Klausner et al., 2022), linking PIPE participation directly to expected redemption levels. Moreover, if PIPE backing operates as a certification mechanism, in which PIPE investors signal deal quality, PIPE may be correlated with lower redemption rate.

4.6.1.3 Nasdaq

Nasdaq is a binary variable, equal to 1 if the De-SPAC is listed on Nasdaq and 0 if listed on the New York Stock Exchange (NYSE). The stock exchanges are structurally different in terms of, among other things, listing requirements, fee structures, and firm composition, with Nasdaq historically attracting more growth-oriented and technology firms (Loughran, 1993). With sector-specific controls being difficult to obtain for our data set, the exchange indicator captures part of any sector-related variation in the post-merger abnormal returns.

4.7 Correlation Between PIPE and Redemption Rate

The certification hypothesis, introduced in section 1, suggests that the presence of PIPE financing discourages public shareholders from redeeming their shares before the merger by signaling deal quality. If this relationship operates as theorized, PIPE-financed De-SPACs should have systematically lower redemption rates compared to their non-PIPE counterparts. Testing this serves two purposes. First, it provides evidence on whether the certification channel operates through variation in redemption rates, complementing the main analysis of post-merger abnormal returns. Second, it addresses a potential concern in the main OLS regression, where redemption rate is controlled for. If there is a strong correlation between PIPE and redemption rate, it would raise concerns whether the PIPE coefficient captures a standalone PIPE *certification effect* or partly reflects redemption rate variation.

Testing this, two non-parametric methods relying on rank distributions are computed. The redemption rate is bounded between 0 and 100, and most observations are clustered near the upper bound, see Appendix C. Therefore, rank-based tests are more appropriate than mean-based tests as they are more robust to skewness and bounded distributions. A Spearman rank correlation is used to assess the strength and direction of any monotonic relationship between the PIPE variable and redemption rate. A negative correlation between the two variables would indicate that PIPE-backed transactions are associated with lower redemption rates across the distribution.

Moreover, a Mann-Whitney U test is used to assess whether the difference in redemption rates between the two groups is systematic across the full distribution rather than driven by a small number of extreme observations. Together, a negative Spearman correlation and a significant Mann-Whitney result would be consistent with the *certification effect*.

4.8 Robustness Tests

4.8.1 Unwinsorized Data

As discussed in section 4.4, the main results are winsorized at the 1st and 99th percentile. To evaluate whether our findings are largely affected by this choice, we re-run the regression in equation (4) on the unwinsorized BHAR, providing an assessment of the influence of outliers on our results.

4.8.2 Alternative Outlier Treatment

As an alternative to winsorization, observations with BHARs above three standard deviations from the mean are excluded from the sample. While winsorization caps extreme values, this alternative approach removes them entirely, providing a stricter test of whether our findings are influenced by the small number of extreme observations in the non-PIPE subsample. If the PIPE effect persists or strengthens under this specification, it indicates that the main results are not an artifact of our chosen outlier treatment.

4.8.3 Quantile Regression at the 50th Percentile

Given the right-skewness documented in the BHAR distribution, particularly within the non-PIPE subsample, a quantile regression is estimated as an alternative to the OLS specification. While OLS estimates the conditional mean of BHAR by minimizing the sum of squared residuals, quantile regression estimates the conditional median by minimizing the sum of absolute residuals. This enables an alternative way of assessing the effect of PIPE financing on post-merger returns that is less sensitive to outliers and skewness of the data.

4.8.4 Alternative Baseline Date

To assess the sensitivity of the results to our choice of baseline date, T = -30, the OLS regression is re-estimated using T = -44 as baseline. The baseline of 30 days already includes firm-specific noise for the transactions where the redemption date falls closer to the merger closing. By pushing the baseline to T = -44, we deliberately introduce additional firm-specific noise to the return calculations not attributable to the business combination. If the findings remain significant using the alternative baseline, it strengthens the conclusion that the PIPE effect is robust to our choice of baseline date.

4.8.5 Testing for Multicollinearity

To ensure that multicollinearity does not influence our results, variance inflation factors (VIF) are computed for all independent variables.

5. Empirical Results

In this section, the empirical findings of this paper are presented. Following the methodology outlined in section 4, the main analysis is divided into several steps. First, univariate tests compare the distribution of post-merger abnormal returns between PIPE-backed and non-PIPE transactions. Second, a cross-sectional OLS regression is estimated to investigate whether the PIPE effect persists after controlling for certain deal-specific characteristics. Third, the relationship between PIPE backing and redemption rates is examined to assess whether the results are consistent with the *certification effect*. Subsequently, several robustness checks are conducted to evaluate the sensitivity of the results to alternative analytical choices and sample specifications.

5.1 Univariate Tests Results

Table 3: Univariate Tests: PIPE vs Non-PIPE Backed De-SPACs

Table 3 reports the results of the Welch two-sample t-test and Mann-Whitney U test comparing the BHAR for PIPE-backed against non-PIPE-backed De-SPACs. The BHARs are winsorized at the 1st and 99th percentile as described in Section 4.4.3. Both tests are specified as one-sided, with the alternative hypothesis that PIPE-backed transactions exhibit higher De-SPAC abnormal returns.

	1-Week BHAR	**1-Year BHAR**
Mean PIPE	-0.1710	-0.8187
Mean Non-PIPE	-0.3972	-0.9883
Welch t-statistic	2.5457^{***}	2.6746^{***}
p-value	(0.0058)	(0.0041)
Mann-Whitney U statistic	$6{,}785^{***}$	$6{,}836^{***}$
p-value	(0.0003)	(0.0002)
# PIPE	126	126
# Non-PIPE	84	84

$^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.10$

What is observable is that both groups exhibit negative mean abnormal returns over both horizons, indicating that the De-SPAC transactions on average underperform relative to the market. However, PIPE-backed De-SPACs underperform by considerably less than the non-PIPE De-SPACs. Over the one-week horizon, PIPE-backed De-SPACs exhibit a mean BHAR of -17.1% compared to the non-PIPE abnormal returns of -39.7%, a gap of 22.6 percentage points. Over the one-year horizon, the PIPE-backed firms exhibit a mean BHAR of -81.9% compared to -98.8% for the non-PIPE firms, a gap of 17 percentage points. The magnitude of these differences is economically substantial, with the PIPE advantage attenuating from the one-week to the one-year horizon. The Welch t-test rejects the null hypothesis at the 1% level over both horizons.

The significant results are confirmed over both horizons through the Mann-Whitney U test, rejecting the null at the 1% level. The fact that both tests yield significant results indicates that the difference between PIPE-financed and non PIPE-financed De-SPACs is not solely driven by extreme observations but suggests that the difference is a systematic pattern across the full distribution. This provides initial evidence in line with our hypothesis that PIPE-backed De-SPACs are associated with better performance compared to De-SPACs without PIPE financing.

5.2 Main Analysis Results

Table 4: OLS Regression: PIPE Financing and Post-merger Returns

Table 4 reports ordinary least squares (OLS) regression examining the relation between PIPE financing and post-merger abnormal returns. The dependent variable is the buy-and-hold abnormal return (BHAR) measured over 1 week [-30, 7] and 1 year [-30, 365] horizon, benchmarked against the Nasdaq Composite Index (.IXIC). The dependent variable is winsorized at the 1st and 99th percentile to mitigate the influence of extreme observations. The PIPE variable is binary and serves as the main independent variable equal to 1 if PIPE financing exists and 0 otherwise. The PIPE coefficient measures the difference in BHAR between transactions with PIPE financing and those without. Control variables include PFEV, redemption rate, and the binary variable Nasdaq. All estimations are measured on the full sample of 210 firms. The magnitude of each variable is reported with heteroscedasticity-robust HC3 standard errors in parentheses below. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	1-Week BHAR	**1-Year BHAR**
Constant	-0.428^{**}	-0.251
	(0.189)	(0.282)
PIPE	0.186^{**}	0.112^{*}
	(0.090)	(0.063)
PFEV (100M)	0.005^{*}	0.002
	(0.003)	(0.002)
Redemption Rate	0.001	-0.006^{**}
	(0.002)	(0.003)
Nasdaq	-0.092	-0.235^{***}
	(0.088)	(0.081)
R^2	0.065	0.139
Adj. R^2	0.047	0.122
F-statistic	3.835	5.506
#	210	210

HC3 standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Consistent with the univariate tests performed in Section 5.1, observing the OLS regression results, PIPE-backed De-SPACs are associated with substantially higher abnormal returns over both horizons. Over one week [-30, 7], PIPE-backed firms are associated with 18.6 percentage points higher buy-and-hold abnormal returns compared to the non-PIPE transactions, significant at the 5% level. While the directional pattern persists for the one-year horizon [-30, 365], it is only significant at the 10% level, and the magnitude attenuates to 11.2 percentage points, indicating a greater and more statistically significant PIPE effect at the shorter horizon, consistent with the pattern in Section 5.1.

Among the controls, PFEV carries a weakly positive and weakly significant association with one-week returns, but loses both effect and significance at the one-year horizon. Redemption rate and Nasdaq listing are both statistically insignificant at the one-week horizon, but at the one-year horizon, they both emerge as negative and significant (at the 5% and 1% level respectively) predictors of abnormal returns. The models for both horizons are jointly significant, indicated by the positive F-statistics, with adjusted R-squared values of 4.68% for the 1 week BHAR and 12.2% for the 1 year BHAR.

5.2.1 Correlation Between PIPE and Redemption Rate

Table 5: PIPE Financing and Redemption Rates

Table 5 examines whether PIPE-backed De-SPACs are systematically associated with lower redemption rates. The first section of the table reports the median redemption rate for each group, alongside the number of observations per group. The second section reports the Spearman rank correlation between the main independent variable, PIPE, and the Redemption rate, and the Mann-Whitney U test reporting the differences in rank distributions across the two groups. A negative statistic on the Spearman rank correlation indicates lower redemption rates for PIPE-financed De-SPACs, with the magnitude indicating its strength. The full sample of 210 observations is used. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Group	**#**	**Median Redeemed (%)**
PIPE-backed	126	95.77
Non-PIPE	84	98.38
Test	**Statistic**	**p-value**
Spearman rank correlation	-0.2334^{***}	0.0007
Mann-Whitney U	$3{,}836.5^{***}$	0.0007

$^{***}\,p < 0.01,\ ^{**}\,p < 0.05,\ ^{*}\,p < 0.10$

The median redemption rate for De-SPACs with PIPE financing is 95.77%, while those without PIPE have a median redemption rate of 98.38%, a gap of approximately 2.6 percentage points. The statistic in the Spearman rank correlation of -0.23 implies a weakly negative and significant correlation, and the Mann-Whitney U test confirms a significant difference in rank between the two groups. These results support the hypothesis that PIPE-backed transactions have a systematically lower redemption rate across the distribution.

The negative association between PIPE financing and redemption rates is consistent with a *certification effect*, in which the participation of PIPE investors serves as a credibility signal to the market, discouraging common shareholders from redeeming their shares.

5.3 Robustness Checks

This section presents three robustness checks to assess whether our main analysis is sensitive to alternative analytical choices. First, we re-estimate the regression through three mechanisms to observe the influence of distributional extremes. Second, we re-estimate the regression using an alternative baseline date of T = -44 instead of T = -30. Lastly, we check for multicollinearity.

5.3.1 Influence of Distributional Extremes

5.3.1.1 OLS Regression on Unwinsorized Data

Table 6: OLS Regression on Unwinsorized Data

Table 6 reports OLS regression results on the unwinsorized sample, providing a robustness check on the influence of extreme observations on the main analysis. All specifications remain identical to the main analysis, except that the buy-and-hold abnormal return (BHAR) is not winsorized at the 1st and 99th percentile. The magnitude of each variable is reported with heteroscedasticity-robust HC3 standard errors in parentheses below. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	1-Week BHAR	**1-Year BHAR**
Constant	−0.612	−0.825
	(0.495)	(0.842)
PIPE	−0.070	−0.493
	(0.373)	(0.671)
PFEV ($100M)	0.012	0.015
	(0.011)	(0.021)
Redemption Rate	0.004	0.004
	(0.007)	(0.012)
Nasdaq	0.045	−0.043
	(0.160)	(0.223)
R^2	0.017	0.013
Adj. R^2	−0.002	−0.006
F-statistic	0.715	1.100
#	210	210

HC3 standard errors in parentheses. $^{***}\ p < 0.01$, $^{**}\ p < 0.05$, $^{*}\ p < 0.10$

In the unwinsorized sample, the PIPE coefficient switches sign and becomes statistically insignificant across both horizons. The adjusted R-squared becomes negative and the control variables Redemption and Nasdaq listing at the one-year horizon, also lose significance on the unwinsorized data. The unwinsorized OLS regression therefore fails to detect the relationships established in the main analysis.

This reversal is mainly attributable to two extreme observations in the non-PIPE subsample, see Appendix D. The most influential exhibits a mean 1 week return of 27.7 (2770%) and a mean 1 year return of 51.2 (5120%), more than 27 standard deviations from the sample mean. This single observation inflates the non-PIPE mean by 32.9 percentage points at the one-week horizon and 61.0 percentage points at the one year horizon, resulting in a flipped sign for the mean BHAR[4].

5.3.1.2 OLS Regression Removing 3SD Outliers

Table 7: OLS Regression Excluding 3SD Outliers

Table 7 reports OLS regression results on unwinsorized data after excluding observations with BHAR falling more than three standard deviations from the sample mean of either the one-week or one-year abnormal return. This resulted in the removal of the same two outliers identified in 5.3.1.1, yielding a total sample of 208 firms. Except for the reduced sample and unwinsorized data, all other specifications are identical to the main analysis. The magnitude of each variable is reported with heteroscedasticity-robust HC3 standard errors in parentheses below. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	1-Week BHAR	**1-Year BHAR**
Constant	-0.440^{***}	-0.252
	(0.169)	(0.288)
PIPE	0.203^{***}	0.149^{**}
	(0.073)	(0.060)
PFEV ($100M)	0.004^{*}	0.001
	(0.002)	(0.002)
Redemption Rate	0.001	-0.006^{**}
	(0.002)	(0.003)
Nasdaq	-0.138^{*}	-0.237^{***}
	(0.081)	(0.085)
R^2	0.086	0.144
Adj. R^2	0.068	0.127
F-statistic	4.989	7.339
#	208	208

HC3 standard errors in parentheses. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$

After excluding the outliers falling more than three standard deviations from the sample mean, the PIPE coefficient recovers its positive sign on both horizons, indicating that

[4]The inflation in the non-PIPE subsample mean caused by the most extreme outlier is computed as

$$\Delta\bar{x} = \bar{x}_{full} - \bar{x}_{excl.\ outlier},$$

where $\bar{x}_{full}$ is the mean of all $n = 84$ non-PIPE observations and $\bar{x}_{excl.\ outlier}$ is the mean of the remaining 83 observations after removing the outlier.

PIPE-backed firms generate higher abnormal returns compared to those without PIPE financing on both the one-week and one-year horizon. At the one-week horizon, PIPE is statistically significant at the 1% level and at the 1 year horizon, significant at the 5% level, with magnitudes exceeding those reported in the main winsorized analysis.

The control variables retain the same patterns as in the main analysis, and the adjusted R-squared increase for both horizons, with the F-statistic also rising for both horizons, remaining highly significant. This confirms that the explanatory power of our model increases once outliers are excluded.

The fact that the magnitude of the PIPE coefficient remains similar under both winsorization at the 1st and 99th percentile and when excluding outliers suggests that both methodologies recover a similar underlying relationship. With both approaches yielding a positive and significant PIPE coefficient, one could argue that the result is not an artifact of a specific methodological choice, but rather a feature of the underlying data.

5.3.1.3 Quantile Regression at the 50th Percentile

Table 8: Quantile Regression at the 50th Percentile

Table 8 reports median (quantile) regression results, estimating the conditional median of BHAR rather than the conditional mean, providing a robustness check that is less sensitive to outliers and distributional skewness. The dependent variable, independent variables, and sample are identical to the unwinsorized OLS regression. Pseudo R-squared values are reported following Koenker and Machado (1999). The full sample of 210 observations is used. The magnitude of each variable is reported with heteroscedasticity-robust HC3 standard errors in parentheses below. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	1-Week BHAR	1-Year BHAR
Constant	-0.226	-0.349^{**}
	(0.207)	(0.171)
PIPE	0.130^{*}	0.146^{***}
	(0.067)	(0.055)
PFEV ($100M)	0.006^{***}	0.001
	(0.001)	(0.001)
Redemption Rate	-0.003	-0.005^{***}
	(0.002)	(0.002)
Nasdaq	-0.135^{*}	-0.284^{***}
	(0.079)	(0.065)
Pseudo R^2	0.053	0.065
#	210	210

HC3 standard errors in parentheses. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$

The main finding is further confirmed using median regression. The PIPE variable is positive and statistically significant at both horizons, with a coefficient of 13.0% at the one-week horizon and a coefficient of 14.6% at the one-year horizon, significant at the 10% and 1% levels respectively. The pattern of significance and magnitude differs from the main winsorized analysis depicted in Section 5.2, as the PIPE coefficient was significant at the 5% level, but is only significant at the 10% level under median regression. Conversely, the results strengthen at the one-year horizon, moving from 10% significance under winsorized OLS regression to 1% significance under median regression.

With regards to the control variables, their directional pattern and magnitude remain similar to the main analysis but with an increased precision. Several controls gain statistical significance: PFEV increasing from significance at the 10% level to 1% at the one week horizon, redemption rate from 5% to 1% at the one-year horizon and Nasdaq at the one-week horizon becoming weakly significant at the 10% level. The Pseudo R-squared values of 5.3% and 6.5% reflect the improvement in fit over a constant-only model. While this is lower than the OLS regression adjusted R-squared, the two metrics are not directly comparable.

5.3.1.4 Synthesis

The influence of distributional extremes has been tested through three different mechanisms. The unwinsorized OLS regression exposes the full influence of extreme observations, the OLS regression with a three standard-deviation filter removes the extreme observations, while unwinsorized median regression makes them irrelevant through the estimator choice. The unwinsorized OLS is the only specification in which the PIPE results do not hold, explained by the extreme observations in the non-PIPE group.

Combined with the winsorized OLS regression presented in the main analysis in section 5.2, the PIPE coefficient is positive and statistically significant under every specification that addresses extreme observations. The level of significance varies across horizons and specifications, ranging from the 10% to the 1% level, but the directional and statistical conclusion that PIPE-backed De-SPACs outperform the De-SPACs without PIPE financing, remains consistent.

5.3.2 Alternative Baseline

Table 9: Robustness: Alternative Baseline (T = -44)

Table 9 reports OLS regression results estimated using an alternative baseline date of 44 calendar days prior to the first trading day. The dependent variable is the buy-and-hold abnormal return (BHAR) measured over the one-week [-44, 7] and one-year [-44, 365] horizons. The regression is run on winsorized BHAR, and all control variables remain identical to those in the main analysis. The sample is reduced to 150 observations due to limited availability of price data 44 days prior to merger consummation. The magnitude of each variable is reported with heteroscedasticity-robust HC3 standard errors in parentheses below. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	1-Week BHAR	**1-Year BHAR**
Constant	2.246	0.339
	(1.861)	(0.613)
PIPE	−0.110	0.057
	(0.550)	(0.137)
PFEV ($100M)	−0.003	−0.001
	(0.007)	(0.003)
Redemption Rate	−0.022	-0.011^{*}
	(0.018)	(0.006)
Nasdaq	−0.063	−0.185
	(0.662)	(0.144)
R^2	0.014	0.086
Adj. R^2	−0.013	0.061
F-statistic	0.442	1.962
#	150	150

HC3 standard errors in parentheses. $^{***} p < 0.01$, $^{**} p < 0.05$, $^{*} p < 0.10$

Under the alternative baseline of T =-44, the PIPE coefficient is statistically insignificant for both the one-week and one-year horizon. Moreover, the coefficients are substantially smaller in magnitude than those reported in the main analysis. The Adjusted R-squared also decreases under this specification, to -1.3% and 6.1%, with an insignificant F-statistic across both horizons.

The control variables also lose significance under the alternative baseline. Only redemption rate remains weakly significant at the 10% level for the one-year horizon, consistent with the main analysis.

These results should be interpreted with caution. The purpose of this robustness test was to introduce additional firm-specific noise to test whether the PIPE effect persists even under noisier conditions. However, price data 44 days prior to merger date is unavailable for 60 of the 210 firms in the original sample. Thus, whether the loss of significance reflects a genuine sensitivity of the PIPE effect to additional noise, reduced statistical power of a smaller sample, or a combination of the both, cannot be determined from this robustness test.

5.3.3 Additional Robustness Checks

Table 10: Variance Inflation Factors

Table 10 reports the variance inflation factors (VIF) computed for each independent variable, assessing whether multicollinearity among the regressors influences the main results.

Variable	VIF
PIPE	1.041
PFEV	1.063
Redemption Rate	1.066
Nasdaq	1.034

All VIF values are around 1, well below the conventional threshold of 5 recommended by James, Witten, Hastie, and Tibshirani (2013). The low VIF indicates that the regressors are largely independent of one another and that multicollinearity does not materially influence the coefficient values or standard errors in the regression.

6. Discussion

6.1 Interpretation

The empirical results of this paper are consistent with the hypothesis that PIPE-backed De-SPACs generate higher abnormal returns than their non-PIPE-backed peers. The univariate tests indicate a meaningful gap between the two groups, confirmed under both a parametric and non-parametric test, and the OLS regression confirms that this difference persists after controlling for deal-specific characteristics. The convergence of results across multiple test-specifications indicates that the finding reflects a robust feature of the underlying data rather than an artifact of methodological choice. These findings extend existing literature on PIPE financing and post-merger returns. While Gahng et al. (2023) and Feng et al. (2026) document positive correlations between external financing and post-merger returns using continuous measures, this paper proves that the binary presence of PIPE financing itself is associated with better outcomes.

The mechanisms behind the outperformance are not fully resolved by the design of this paper. We hypothesize that the explanation lies in the *certification effect*. Through investigation of the relationship between PIPE and redemption rate, we find that PIPE-financed deals exhibit systematically lower redemption rates. This pattern supports that the presence of PIPE financing signals deal quality and discourages common shareholders from redeeming their shares.

This pattern can be interpreted within the broader signaling framework first introduced by Spence (1973), where a costly action that is more difficult for low-quality types than for those of high quality, serves as a credible signal of high quality. As PIPE investors commit private capital to deals they expect to be of high quality, their participation becomes informative to common shareholders. The lower redemption rates in PIPE-financed transactions are consistent with the behavioral response that signaling theory predicts, in which less informed shareholders react positively on observing a credible signal, and act accordingly.

Although there is a significant difference between PIPE and non-PIPE De-SPACs, redemption rates remain very high across the full data set. One plausible explanation is that a significant portion of pre-merger SPAC investors may be attracted by the risk-free return structure described by Gahng et al. (2023). Such investors would redeem their shares regardless of deal quality, compressing redemption rates toward the ceiling across both groups and minimizing the size of the *certification effect*. The 2.6 percentage point difference in redemption rates across the subsamples may therefore understate the true *certification effect*.

These findings are consistent with the certification documented by Feng et al. (2026) as the primary driver of PIPE outperformance. While additional mechanisms such as the *financing effect* may contribute, their relative contributions cannot be determined within the binary design of this paper.

6.2 Limitations and Future Studies

While our empirical results provide useful insights, there are several limitations of our research that should be acknowledged. This paper does not fully resolve the endogeneity concern surrounding the *certification effect*. We theorize that PIPE investors screen for quality, and signal this to the market through participation. However, it cannot be ruled out that PIPE investors invest regardless of underlying company quality, and that their mere participation improves post-merger returns through their market signal rather than through genuine quality selection.

The design also cannot separate how much of PIPE's contribution to returns flows through reduced redemption versus additional liquidity that PIPE capital provides to the merged entity. This would clarify the importance of each mechanism and strengthen our interpretation of the PIPE coefficient. However, a mediation analysis would require strong assumptions such as that no unobserved variables affect both redemption rates and post-merger returns. As we cannot satisfy this assumption, we report the joint evidence as consistent with the *certification effect*, without attempting to quantify the relative contributions of these two channels. Distinguishing between these effects therefore remains an open question that future research with stronger identification strategies could address.

The data set is subject to potential delisting bias and incomplete coverage as discussed in Section 3.1. The data set excludes all De-SPACs that delisted within one year after the first trading date. Given the high delisting rates among De-SPACs, there is a risk that the one-year returns understate the true magnitude of losses for the poorest performing companies.

The sample of 210 observations limits the statistical power, especially in the robustness specifications, such as the T=-44 baseline test, which further reduces the sample. While including 2021 mergers would have increased the statistical power of the study, they were deliberately excluded, driven by concerns that the extraordinary market conditions with PIPE participation rates exceeding 95% would contaminate the PIPE subsample. The chosen 2022-2024 window therefore reflects a prioritization of comparability between PIPE and non-PIPE deals over sample size.

Finally, our binary PIPE classification ignores meaningful variation in deal characteristics that may independently influence post-merger returns, potentially introducing omitted variable bias. PIPE size, sponsor quality and target quality were unobservable, yet each likely correlates with PIPE participation and post-merger performance. As higher quality sponsors and better targets are more likely to secure PIPE financing, our PIPE coefficient may partly reflect the underlying deal quality. Furthermore, systematically classifying all PIPE investors across our full dataset would require extensive manual review of each SPAC's S-4 filings, which would be too demanding given the time frame of this study. This prevented us from distinguishing independent institutional investors who likely carry a strong certification signal. Future research could therefore incorporate PIPE investor identity, PIPE size, sponsor quality and target fundamentals in order to achieve a cleaner identification of the *certification effect*.

We identify three directions for future research. As more post-merger data becomes available, future research in the field will be able to examine longer return horizons beyond one year and allow researchers to test whether any PIPE effect persists over time, something our sample window did not permit. Additionally, a natural extension to our research would be conducting a cross-country analysis comparing post-merger De-SPAC returns across markets such as the US and UK. Finally, future studies could also distinguish between affiliated and unaffiliated PIPE investors and incorporate measures of PIPE size, sponsor quality, and target fundamentals in order to better isolate the PIPE effect from underlying deal quality.

7. Conclusion

This paper examines whether PIPE-backed De-SPAC mergers generate different returns compared to their non-PIPE counterparts by using a sample of 210 completed mergers.

This paper extends prior literature on PIPE financing in De-SPAC transactions. Neither Gahng et al. (2023) nor Feng et al. (2026) directly test whether the presence of PIPE financing itself is associated with better returns. We address this gap through a binary comparison supported by robustness testing, and our 2022–2024 sample captures a post-boom environment in which fewer De-SPACs receive PIPE financing, providing meaningful variation between the two groups.

The results provide statistically significant evidence that PIPE backed De-SPACs outperform their non-PIPE backed counterparts across both a one week and one year horizon. The winsorized Welch t-test and Mann-Whitney U test indicate higher BHARs for PIPE-backed firms, with mean differences of 22.6 percentage points over the one-week horizon and 17.0 percentage points over the one-year horizon. The OLS regression confirms these findings after controlling for PFEV, redemption rate, and stock exchange listing. The PIPE coefficient remains positive and significant across robustness tests addressing the influence of extreme observations.

Examining the mechanism behind this outperformance, this paper finds that PIPE-backed deals exhibit systematically lower redemption rates than non-PIPE-backed deals. This pattern is consistent with the *certification effect* described by Feng et al. (2026), in which higher external financing discourages share redemption. The binary classification in this paper cannot rule out additional channels operating in parallel, but within our sample and model specification, the combination of lower redemption rates and higher abnormal returns suggests that the net effect of PIPE presence on common shareholder returns is positive.

Although this paper concludes that PIPE-backed De-SPACs outperform their non-PIPE-backed counterparts, several limitations apply to this finding and the result should therefore be interpreted with caution. The sample size of 210 observations limits statistical power, and our use of T = –30 as a proxy for the redemption date is not a perfect proxy as it may introduce noise for transactions where the actual redemption date occurs after this baseline. The one-year horizon may also understate long-term losses, given the high delisting rate among De-SPACs. Additionally, the binary PIPE classification cannot account for variation in PIPE investors' identity, PIPE size, sponsor quality, or target quality, which all may independently influence the post-merger returns and contribute to omitted variable bias.

The findings are relevant for common shareholders and institutional investors evaluating SPAC deals, indicating that the presence of PIPE financing is associated with better post-merger returns and may therefore serve as a useful indicator of deal quality. At the same time, the *certification effect* carries the risk of becoming self-defeating as it becomes more widely recognized. If PIPE participation is interpreted as a credible quality signal, sponsors are incentivized to secure PIPE backing purely to generate that signal, regardless of whether there is a need for PIPE financing. PIPE investors may also recognize that quality screening becomes less necessary, as their participation alone attracts common investors and is sufficient to generate abnormal returns. Whether the certification channel will persist as the SPAC market evolves is therefore an open question for the future.

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Appendix

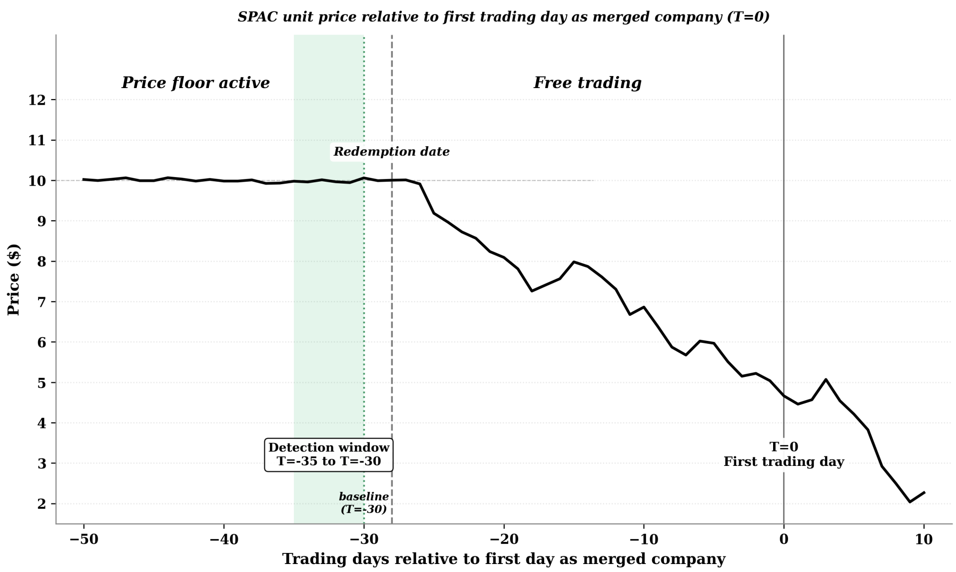
A. Sample Construction

Table 11: Sample Construction

This table reports the sample construction process applied to the initial data set of De-SPAC mergers obtained from the ListingTrack De-SPAC list (ListingTrack, 2026). The starting point is all 257 completed mergers available on ListingTrack in the United States from January 1, 2022 to December 31, 2024. Following this, the sample is refined through several data-cleaning steps. First, observations without IPO size data, and no redemption date were removed. Secondly, observations without closing prices available on Yahoo Finance for the relevant horizons are removed. Finally, observations flagged by the redemption date filter are verified against SEC Form 8-K filings, with confirmed cases removed from the sample. The final sample consists of 210 De-SPAC mergers, of which 126 are PIPE-backed and 84 are non-PIPE-backed.

Sample filter	# of deals
Initial sample: De-SPAC mergers completed 2022–2024	257
Exclude: missing PFEV data	251
Exclude: missing redemption rate data	250
Exclude: missing closing prices	223
Exclude: confirmed redemption date preceding T=−30	210
Final sample	**210**
PIPE-backed	126
Non-PIPE-backed	84

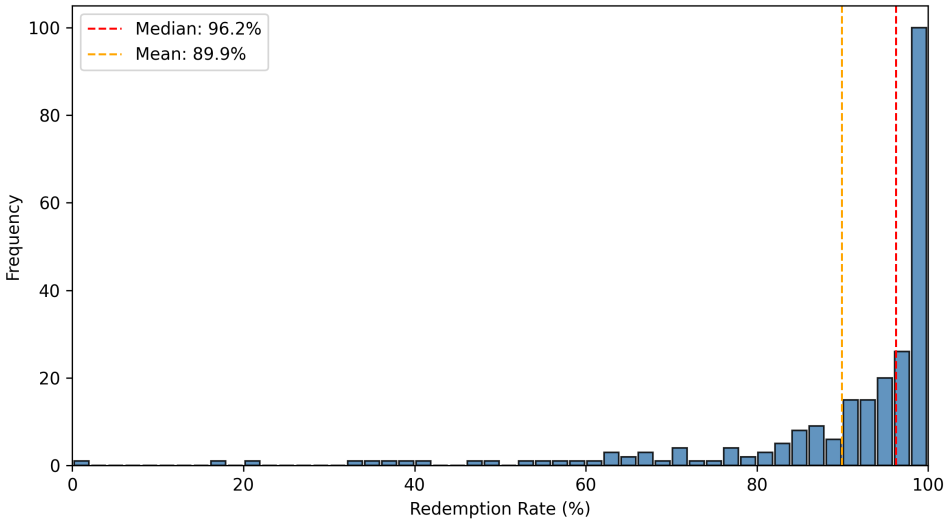
B. Redemption Filter



C. Redemption Rate Distribution

Figure 3: Distribution of Redemption Rates

This figure plots the distribution of shareholder redemption rates across the final sample of 210 De-SPAC mergers. The vertical red dashed line indicates the median of 96.2%, and the orange dashed line the sample mean of 89.9%. The redemption rate is bounded between 0 and 100 and exhibits strong left-skewness, with the majority of observations clustered near the upper bound.



D. Outliers

Table 12: Outliers Identified Beyond Three Standard Deviations

The table reports the observations identified as outliers under the three-standard-deviation filter applied in the robustness check in Section 5.3.1.2. An observation is flagged if its raw return at either the 1-week or 1-year horizon falls more than three standard deviations from the sample mean of that horizon. The 1-week and 1-year returns are computed as the percentage change from the closing price 30 calendar days prior to merger closing to the closing price one week and one year after merger closing, respectively.

Ticker	PIPE-backed	1-Week Return	1-Year Return
NUKK	No	27.66	51.20
RBCN	Yes	9.03	-0.14*

*RBCN's 1-year return falls within the threshold of three standard-deviation but is excluded from both horizons.

AI Declaration

In this paper, AI tools have been used to assist with coding, formatting, and grammar. Specifically, Claude (Anthropic) was used to support Python coding for data collection and analysis, as well as LaTeX formatting including table construction and layout. All AI-generated content and suggestions have been closely reviewed, edited, and assessed by the authors before inclusion.