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Do Private Equity Managers Raise Funds on (Sur)real Returns? Evidence from Deal-Level Data

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Abstract

Recent studies on agency problems in private equity fueled the suspicion that fund managers strategically manipulate performance estimates around fundraising times. While these studies use aggregated portfolio data, this paper offers the first analysis of "window dressing" in private equity based on deal-level performance. In contrast to previous findings of a smoking gun at the fund level, I do not find any evidence of inflated performance at the deal level. Fund performance peaks are driven by a cohort effect whereby late investments are made under pressure before fundraising and have lower returns than those made earlier in the fund's life.

I. Introduction

Private equity reported net asset values (NAVs) have come under increasing scrutiny in recent years. Industry observers and academics have called for regulatory changes to the private equity industry, raising concerns that NAVs are inflated around periods of fundraising, particularly in the case of low-reputation funds where the ability to perform has yet to be proven. A statement by Pennsylvania State Treasurer Joe Torsella summarizes this increasing urge to overhaul regulation of private equity fund's disclosure: "Standardization would help prevent inflated or overstated successes through metrics that are known to be easily manipulated." (WSJ, Jan. 2020)

Empirical evidence suggests that funds raise new capital after good performance. At the time a new fund is raised, existing investments are not necessarily liquidated, so current fund performance relies heavily on NAVs of their investments. Fund managers who act as general partners (GPs) have a lot of discretion in

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reporting NAVs to their investors who are referred to as limited partners (LPs). This discretion and the evidence of peak performance around fundraising events in low-reputation funds has led to the speculation that reported performance data in private equity are manipulated. While this has been widely alleged, data limitations have prevented previous studies to make definitive conclusions about manipulation. Considering the high level of sophistication among groups of LPs, manipulating NAVs may be a difficult endeavor. Measuring the extent of any NAV manipulation requires more granular data than available in previous studies, but these data are available to LPs. Thus, the aim of this study is to determine the degree of manipulation that an LP could potentially experience and to reconcile the results with the prior literature.

The striking peak performance observed by academics in fund-level data shortly before fundraising times is consistent with two hypotheses that frame my analysis. One is that GPs may advertise strong current performance by inflating true estimates of current asset values. Alternatively, GPs may raise funds around true estimates of high current NAVs but undertake bad deals shortly before fundraising with increased pressure to invest unused cash from LPs. I label this a "cohort effect" or a forced-buyer hypothesis. It occurs because of the time low-reputation funds take to identify good deals. After the earliest investments are made, the following investments are of lower quality. Funds essentially become forced buyers because of the impending start of raising a new fund.

The forced-buyer hypothesis implies that the pattern of high NAVs followed by low NAVs is due to deal timing rather than inflated valuations. GPs have incentives to invest unused cash to increase their chances of raising a new fund. The incentive to raise new funds in private equity stems from the typical 10-year limited partnership life which is broken down into an investment period and a liquidation period. If GPs want to make new investments after the investment period of their current fund has expired, they need to raise a new fund. Various studies have focused on agency problems around fundraising, centering on the question of whether GPs inflate portfolio values when marketing a new fund. The collection of evidence indicates that performance estimates are inflated particularly for investments in low-reputation funds (Barber and Yasuda (2017), Chakraborty and Ewens (2018), and Brown, Gredil, and Kaplan (2019)).[1] The empirical findings of these papers mainly suggest that less reputable funds face lower costs of manipulation as they need to signal quality to be able to raise a new fund and are therefore more likely to strategically inflate valuation estimates compared to high-reputation funds. However, in a market with sophisticated LPs, the cost of manipulation could be high for low-reputation funds. Unlike academic studies, LPs have access to quarterly NAVs of individual portfolio holdings and have the incentive to look for red flags in deal valuations before deciding to commit to a new fund. In that regard, the data underlying these studies are aggregated too coarsely to attribute performance peaks to inflation of underlying asset values. To better estimate the degree of manipulation, I use a novel database of quarterly deal valuations in U.S. buyout funds.

[1]Investing in private equity requires investing in low-reputation funds, at least initially, to gain a "seat at the table" once their GPs become more reputable (see, e.g., Lerner, Schoar, and Wongsunwai (2007)).

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With the aim of revisiting the fund-level findings suggesting NAV inflation, my analysis consists of several steps. First, I replicate the primary results of Barber and Yasuda (2017) and examine the importance of deal-level data in empirical tests using NAV markdowns and performance after the fundraising event. If the documented erosion of fund performance after fundraising is attributable to inflated NAVs, this should be reflected on the deal level in higher NAV markdowns during that time and a drop in performance post fundraising.

I find that deal NAV markdowns are the largest in the first 1.5 years after investment and are independent of closeness to fundraising. GPs mark down investments that were on average stated at cost, and markdowns are larger in later investments of low-reputation funds. While this leads to higher aggregated markdowns post fundraising in low-reputation funds, it is a very different finding than inflating deal NAVs before fundraising as the literature suggests. Defined on a \$10,000 investment in a fund, all else being equal, aggregated markdowns post fundraising in low-reputation funds drop by roughly \$150 and become insignificant when deals made within a year before fundraising are excluded from the portfolio. While this number is in line with the mean markdown of \$162 on a \$10,000 fund shown by Barber and Yasuda (2017) the economic magnitude of the drop increases when taking into account that late investments have notable markdowns prior to fundraising. For example, the markdowns in low-reputation funds drop by about twice as much when adding three quarters leading up to the fundraising event to the post-fundraising period. Once these late deals are excluded from the portfolio, the drop in portfolio markdowns again becomes insignificant.

Next, I test whether the effect on markdowns stemming from the timing of investments is sufficient to explain lower post-fundraising performance of fundraisers compared to non-fundraisers, as shown by Barber and Yasuda (2017). I find no evidence that post-fundraising-event performance is noticeably different in deals made more than a year before fundraising compared to cohort deals that did not raise a new fund that quarter. In line with the NAV markdown results, deals that are made shortly before fundraising show a significant drop in returns after fundraising in funds that lack reputation. The post-fundraising-event value multiple (assuming a fund invests in a company at the fundraising event quarter NAV and holds the deal to realization) is around 26 percentage points lower in deals made less than a year prior to fundraising compared to non-fundraising cohort deals (37 percentage points lower in deals realized by the end of the sample period).[2] This difference in late investments explains the performance drop post fundraising first reported by Barber and Yasuda (2017). Post-fundraising performance turns out to no longer be significantly different from non-fundraising cohort funds after dropping deals made shortly before fundraising from the portfolio.

The evidence of underperforming later deals after the fundraising event begs the question of what happened to these investments prior to the fundraising event. Looking at valuation multiples (VMs) in event time, similar to Barber and Yasuda (2017) and Jenkinson, Sousa, and Stucke (2013), reveals that deals are stated at cost for the first two quarters. When broken down into subgroups, I find strong evidence

[2]The importance of cohorts in fundraising has also been documented by Chakraborty and Ewens (2018), Barber and Yasuda (2017), and Brown et al. (2019).

that low-reputation funds have, on average, more successful investments well ahead of fundraising compared to deals made shortly before fundraising.[3] The difference is not absorbed by investment year, geographic region, or industry. This finding is not consistent with the NAV manipulation hypothesis since high valuations in early deals around fundraising are supported by high realized exit multiples.

After reconciling with Barber and Yasuda (2017), my analysis focuses on mapping differences on the deal level into the compelling evidence at the fund level presented by Brown et al. (2019). They develop a theory to guide their empirical work and find their results to be consistent with a "costly signaling equilibrium" where inflating NAVs is associated with a lower probability of a successful fundraising but nevertheless is pursued by low-reputation funds. Following their design, which uses excess returns over the public market index as an outcome variable, I analyze how aggregating deal excess returns in event time muddles their picture. Consistent with previous analyses, I find that the documented drop in fund-level excess returns shortly before the fundraising event quarter originates from investments made within a year prior to fundraising. That means, Brown et al.'s (2019) "costly signaling equilibrium" can be explained by a simple cohort effect which does not require high reputation funds to engage in costly actions to distinguish themselves from low reputation funds.

Besides excess returns, Brown et al. (2019) focus on the widely recognized importance of performance relative to peers to market a new fund. They find that reported fund returns revert more strongly to those of their peers the longer it takes to raise the next fund. According to their interpretation, these funds have a higher incentive to manipulate reported returns; thus they inflate NAVs and in consequence report lower interim returns subsequently. In fact, I find that investments that are, on average, stated at cost only to drop in valuation thereafter are simply made at a time where fund performance is at its best compared to peers. In short, Brown et al.'s (2019) finding is not actually an inflation of NAVs relative to the costs of the investments but rather evidence of rushed investments in order to meet thresholds that allow for fundraising of a new fund.

While finding no evidence of inflated NAVs by mapping deal-level results into documented patterns from other studies, I apply an additional test considering club deals. In particular, I compare the valuation of the same deal across two GPs that hold this deal in their portfolio, where one is fundraising and the other one is not.[4] In line with previous results, I find no systematic differences in valuations of the same deal across different GPs, casting further doubt on the notion of systematic NAV inflation around the fundraising event.

To explain the mechanism that is driving the result regarding bad deals undertaken before fundraising, in the last step I analyze whether particularly low-reputation funds are under more pressure to deploy cash not used in early deals, referred to as "dry powder," closer to fundraising. The idea is that investors are unlikely to commit

[3]In the future, investors may measure performance by computing Public Market Equivalents (PMEs), Generalized Public Market Equivalents (GPMEs) developed by Korteweg and Nagel (2016), or Credit Market Equivalents (CMEs) developed by Hüther, Schmid, and Steri (2022), while this is not common practice as of 2022.

[4]According to Phalippou (2013), these valuations could be substantially different.

capital to a new fund if the current fund has a substantial amount of unspent capital near the fundraising quarter. Low-reputation funds may lack the network among private equity firms or the deal flow and thus need more time in finding good investment opportunities compared to high-reputation funds, which puts them under investment pressure closer to fundraising. To explore this channel, I model drawdown times as a function of reputation and other fund and market characteristics. Indeed, I find strong evidence that low-reputation funds draw down the first 35% of their committed capital more slowly, while the next 35% of their capital is drawn down quicker compared to high-reputation funds. The contractual threshold to raise a new fund is typically 70% of committed capital (this is also in line with reported results by Chakraborty and Ewens (2018)). The richness of the data allows me to control for overlapping quarters with the investment period of the previous fund, which is strongly related to an increase in holding time for the first 35% of committed capital drawdowns.[5] Considering that a new fund needs to be raised, at the latest, by the end of the current fund's investment period if the GP wants to make new investments, and that fundraising is more successful in good market times, GPs are limited in the time they can wait to attempt to raise new funds.

I find that funds under pressure pay a premium for buyouts made shortly before fundraising. In particular, the premium for investments made within 1 year before fundraising increases the purchase multiple (relative to comparable mergers and acquisitions transactions) by about 35% more in low-reputation funds as compared to high-reputation funds. This premium is in line with the approximately 35% drop in post-fundraising realized deal performance of low-reputation funds made less than 1 year ahead of fundraising as compared to non-fundraising cohort deals.

To summarize, I have evidence that what other studies conclude is manipulation is likely a cohort effect driven by the desire for future funding. In the cohort effect, low-reputation funds make their best investments early, and the investments made just prior to fundraising are on average consistently worse due to the pressure to use leftover cash. My findings are consistent with the model proposed by Shin (2003), showing that it is optimal for a manager to disclose the best possible outcome to interested parties by verifiable disclosures. Shin (2003) argues that an effective penalty will dissuade funds from reporting false evidence. While my evidence certainly does not rule out all types of manipulation, the patterns found by other studies are not sufficient to conclude that GPs are manipulating NAVs. The seemingly compelling evidence of manipulation of NAVs is in fact consistent with poor investment decisions resulting from contract designs.

The mirror image of fire sales ("fire purchases") relates to the budget lapsing literature where unspent funds do not carry over from one budgeting period to the next. Due to contract design this "spend it or lose it" creates a natural incentive for managers to exhaust the budget each year. In this field it has been shown (e.g., Douglas and Franklin (2006)) that a policy to carry over year-end surpluses restructures the contract between the principal (legislature) and its agents (state agencies)

[5]Since low-reputation funds have fewer overlapping quarters (because they are not able to raise new funds that frequently), omitting the overlap dummy would impart a negative bias in the coefficient estimate for low-reputation funds.

so that their interests are better aligned, regarding end-of-the-year surpluses. Extending the investment period in private equity might have a similar effect but could potentially slow down investments early on.

It is important to note that my sample represents less than half of the industry, and the investments in the sample were made by the most sophisticated LPs. It is certainly possible that NAV manipulation occurs outside of these firms. However, the finding of high NAVs before fundraising is not itself evidence of NAV manipulation.

A. Contribution to the Literature

I make three main contributions to the literature. First, I contribute to previously discussed literature on private equity performance management and reputation (Jenkinson et al. (2013), Barber and Yasuda (2017), Chakraborty and Ewens (2018), and Brown et al. (2019)). My paper is novel in using deal-level data rather than fund-level data used in these previous studies, with the exception of Chakraborty and Ewens (2018). However, Chakraborty and Ewens (2018) focus on delays of negative information in VC deals rather than inflated NAVs. This is most likely the case because NAVs in venture capital are tied to a transaction price in a financing round which does not provide scope for performance inflation. Supporting that notion, Hüther, Robinson, Sievers, and Hartman-Wendels (2020) find that fundraising events are linked to spikes in observable portfolio exits in venture capital. I analyze buyout deal performance that is based on subjective valuation estimates that allow for the possibility of inflated NAVs.

Second, I contribute to the literature studying the relation of private equity fund performance to capital flows (Kaplan and Schoar (2005), Phalippou (2010), Chung, Sensoy, Stern, and Weisbach (2012), Hochberg, Ljungqvist, and Vissing-Jørgensen (2014), and Phalippou, Rauch, and Umber (2018)). While these studies support the importance of current performance on the ability to raise follow-on funds, and also show that fundraising is more dependent on performance for GPs without track records (Kaplan and Schoar (2005), Chung et al. (2012)), none of these studies investigate the verifiability of reported returns. The need to address verifiability, especially for funds that expect difficulties in fundraising, is underlined by Braun, Jenkinson, and Stoff (2017) who find that performance persistence is associated with poorly performing GPs. Chung et al. (2012) document that pay for current performance is stronger for buyout funds than for venture capital funds. This implies that performance manipulation is a stronger concern in buyout funds as compared to venture capital funds. That being said, no prior study has access to quarterly buyout deal valuations. Robinson and Sensoy (2016) state that self-reported NAVs of buyout funds seem to be informative since none of their performance assessment is sensitive to the inclusion of non-liquidated funds. This is consistent with my results of unbiased performance estimates. At the same time, my data allows me to investigate NAV manipulation around fundraising events where performance manipulation is of highest concern.

Third, I contribute to the literature on investment behavior of buyout funds (Axelson, Strömberg, and Weisbach (2009), Arcot, Fluck, Gaspar, and Hege (2015), Lopez-de-Silanes, Phalippou, and Gottschalg (2015), Degeorge, Martin, and Phalippou (2016), and Ljungqvist, Richardson, and Wolfenzon (2020); for recent work on investment behavior in VC, see, e.g., Ewens, Nanda, Rhodes-Kropf (2018)). Ljungqvist et al. (2020) find that investment opportunities, managers' bargaining power, and credit conditions affect the speed with which funds draw down committed capital. I expand their analysis by controlling for reputation, the remaining time before fundraising, and overlapping funds (motivated by Lopez-de-Silanes et al. (2015)). Arcot et al. (2015) and Degeorge et al. (2016) investigate investment behavior of funds under pressure, but their work does not focus on the pressure to buy before fundraising. The model of Axelson et al. (2009) predicts that buyout funds with substantial dry powder late in their investment period are more likely to invest in less attractive projects, which is consistent with my empirical findings.

The rest of the paper proceeds as follows: Section II discusses the sample. Section III explores whether net asset values of underlying investments are strategically manipulated and reconciles the deal-level results with the prior literature. Section IV investigates the mechanism driving the result of bad deals undertaken before fundraising. Section V concludes the article.

II. Data

The data in this study were provided by one of the largest international LPs in the world on an anonymous and confidential basis. Although the data source is a large, global investor who invests in various private equity asset classes, the analysis is restricted to U.S. buyout funds to narrow the scope of investment focus with regard to the research question.

There are several reasons to focus on buyouts rather than venture capital deals with regard to this research question. Based on my conversations with investors, buyout funds are generally understood to provide more scope for manipulation than venture capital funds, since valuations are not tied to a per share price that a VC investor is willing to pay in a financing round. In addition, valuation estimates of mature private companies are typically viewed to be more informative and relevant to potential investors compared to valuation of start-ups with limited track records (see, e.g., Jenkinson, Landsman, Rountree, and Soonawalla (2020)). This view is also consistent with Barber and Yasuda (2017), who find that the current performance of buyout funds has roughly twice the impact on fundraising as compared to VC funds. Thus, GPs should have a stronger incentive to inflate valuations in buyout deals. Although researchers and practitioners highlight the importance of a better understanding of interim valuations especially for buyout funds, to the best of my knowledge, no study has access to quarterly valuations of buyouts. This fine-grained data is necessary to investigate the hypothesis of window dressing as a consequence of agency frictions around fundraising.

The data set comprises 2,776 fund-investment pairs of 136 funds raised between 1996 and 2010. About 90% (121) of these funds engaged in marketing to raise a new fund, whereas 15 did not try to advertise a follow-on fund (by sending out due diligence information to the institutional investor). I disregard funds that raised a new fund after Dec. 31, 2013, the last date for which I observe NAVs and cash flows. Considering that existing fund investors still observe performance after the report date of the current fund's performance in the due diligence package, I define the fundraising event for these 121 sample funds as the first quarter after closing (expected closing) of the next fund.[6] Since the analyses require a fundraising date, the final sample contains 2,451 fund-investment pairs of 121 funds with vintage years between 1996 and 2010.

About 40% of their exited investments are realized below cost, and 17% are completely written off.[7] The fund-investment pairs correspond to 2,100 unique sample portfolio companies. Being able to differentiate between investments held by several funds, I can identify 239 (127) companies in the portfolios of at least two different funds (at least two different GPs).

A. Sample Representativeness and Basic Summary Statistics

I assess the representativeness of my sample by comparing it with publicly available data collected by Preqin and the other related studies, in particular, with Braun et al. (2017) and Brown, Harris, Hu, Jenkinson, Kaplan, and Robinson (2020) who also have deal-level information. The latter study is based on Burgiss data.

While Panel A of Table 1 shows that my sample is smaller compared to the number of funds in Preqin and the number of portfolio companies in Braun et al. (2017) as well as in Brown et al. (2020), my data allow me to analyze cash flows and reported NAVs on the individual portfolio company level. Braun et al. (2017) also have deal-level cash flows, but they are missing quarterly deal NAVs to address the manipulation question.

Splitting the number of deals by investment year shows that my sample size increases over time, which is in line with the sample of Brown et al. (2020). Braun et al. (2017) have slightly older investments which were predominately made in the early 2000s. In addition, their sample is more tilted toward European deals, whereas by far the majority of my sample deals are located in the USA, the same as in Brown et al. (2020). The distribution across industries differs compared to Braun et al. (2017), with more consumer discretionary-oriented companies and fewer deals in consumer staples. The median equity investment is slightly higher in my sample as compared to Braun et al. (2017) but similar compared to Brown et al. (2020). This could be due to observing more and larger deals after 2005 in both my sample as well as in Brown et al. (2020).

To identify funds with potentially strong incentives to strategically manipulate valuations, I split the sample into high-reputation and low-reputation funds. Low-reputation funds must place more emphasis on convincing prospective LPs that they are skilled. These funds lack a strong past track record and need more time to reach a first close compared to vintage year cohorts.[8] Thus, I define low-reputation funds as having no prior top quartile performing funds that are more than 5 years old from the inception of the sample fund (obtained from the LP's due diligence

[6]For funds that advertised a new fund but ultimately did not fundraise, I use the expected first closing from the PPMs/due diligence documents.

[7]This is consistent with Lopez-de-Silanes et al. (2015) who report 15% of complete write-offs in their sample.

[8]"First close" means the first time that investors commit to making their investment in the fund. While "final close" refers to the time where the last investors commit to making their investments.

TABLE 1

Sample Representativeness and Summary Statistics

Table 1 shows sample representativeness by comparing my data set to Braun et al. (2017), Brown et al. (2020), and Preqin as of Dec. 31, 2013, with vintage years between 1996 and 2010. Panel B presents the *p*-values of *t*-tests and Wilcoxon rank-sum tests (in brackets) for the comparison with Preqin. YRS_BEFORE_NEXT_FUND_RAISED describes the timing of subsequent buyout funds. SIZE_OF_GP expresses the size of the GPs in questions (across its previous funds of the last 10 years) as a fraction of the total capital it raised relative to the total amount raised by all GPs (i.e., investors' commitments) over the 10 years preceding each fund; AGE_OF_GP shows the age of the GP, that is, the time of the closing of the first partnership that the GP raised to the closing of this fund; #_OF_PAST_FUNDS gives the number of past funds of the GP.

Panel A. Sample Representativeness

	Sample: Fund and Deal Data	Preqin: Excl. Sample Data	Braun et al.	Brown et al.
Fund Data				
No. of funds	121	851	865	
High-reputation funds	78			
Low-reputation funds	43			
No. of corresponding GPs	75	426	269	
Portfolio Company Data				
No. of fund-portfolio-comp pairs	2,451		12,541	15,095
Fully realized	1,187		7,568	8,461
Not fully realized	1,264		4,973	6,634
Inv. times				
1996–1999	241		2,410	392
2000–2004	628		2,267	1,495
2005–2013	1,582		793	>3,500
Region				
Asia/Pacific	362		193	691
Europe	514		3,718	1,875
North America	1,498		3,121	4,052
Other	77		536	5,895
Industry				
Industrials	462		1,885	1,703
Consumer staples	55		1,191	
Consumer discretionary	666		1,038	
Technology	481		945	1,269
Other	787		2,509	991
Equity investment				
No. of deals with size > 300 m	247			
No. of deals with 50 m < size ≤ 300 m	833			
No. of deals with 10 m < size ≤ 50 m	819			
No. of deals with size < 10 m	552			

Panel B. Summary Statistics

	Sample: Fund and GP Data	Preqin: Excluding Sample Data	*p*-Values Testing for Diff. Between Sample & Preqin
VINTAGE_YEAR	2004.165	2005.632	0.026
	(2006)	(2007)	(0.000)
FUND_SIZE (m)	3,069.223	1,440.550	0.000
	(1,500)	(650)	(0.000)
YRS_BEFORE_NEXT_FUND_RAISED	3.314	3.544	0.347
	(3)	(3.250)	0.756
SIZE_OF_GP (% of industry $)	0.051	0.028	0.019
	(0.024)	(0.001)	(0.000)
AGE_OF_GP (years)	15.95	10.157	0.000
	(12)	(6)	(0.000)
#_OF_PAST_FUNDS	5.092	2.002	0.000
	(3)	(1)	(0.000)

documents) and where the time to fundraise from sending out due diligence material to LPs to first closing of the sample funds is above the sample median. As a result of multiple conversations with different GPs and LPs, the time to fundraise in comparison to vintage year cohorts is claimed to be a strong indicator of GPs' reputation (this is also in line with reports by Preqin). In robustness tests, I use alternative proxies of anticipated problems in fundraising. I use an indicator of whether a fund had unsuccessful exits prior to fundraising (see Supplementary Tables A5–A12), and an alternative reputation measure as defined in Barber and Yasuda (2017) (see Supplementary Tables A13–A20). Measures of reputation are defined ex ante, whether a sample fund is ultimately successful is not taken into consideration at the time of fundraising (except for the excess return analysis based on Brown et al. (2019), see Supplementary Figure A1). In that regard, the measurement of low reputation does not have any biases in terms of post-fundraising performance.

Descriptive statistics of the funds and their GPs are reported in Panel B of Table 1. The average (median) vintage year in my sample is 2004 (2006), which is comparable to Preqin. However, I find that my sample consists of statistically and economically larger funds that are raised by more established GPs than the average and median counterparts from Preqin. This is partly attributable to the fact that the large size of the investor in question precluded them from investing in small funds. Recent studies, such as Metrick and Yasuda (2010), also using data from a large independent LP, report similar average fund sizes (1,238 million USD) to those of Preqin (Barber and Yasuda (2017), Brown et al. (2019)). Considering that Metrick and Yasuda (2010) only have data through 2006, which leaves out the heyday of fundraising in the U.S. buyout market from 2006 to 2008, my average fund size is reasonably comparable.

In relation to Preqin's data universe, my GPs are, on average, older and larger in terms of previous investment activity. To capture this, I compute the size of previous funds operated by the same GP as a fraction of the total investment activity in the sector over the previous 10 years. My sample GPs appear more experienced as active investors, rather than displaying inactive firms with an average of five funds for a lifetime of 15 years versus an average of two funds in 11 years, as in Preqin.

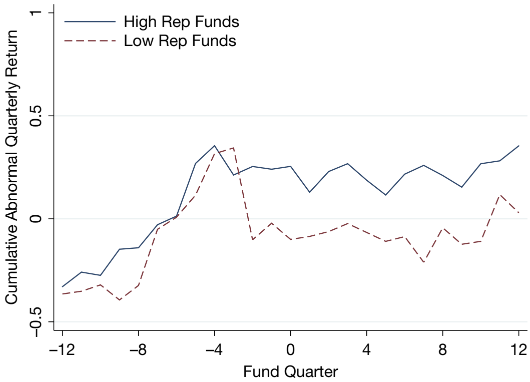
A data set of generally more experienced GPs might raise concerns that these GPs might have a low incentive to strategically manipulate NAVs. However, I find clear evidence of performance peaks before fundraising in my sample, in line with previous studies (see Figure 1 and Supplementary Table A2), which has led to speculations of NAV manipulation. In addition, 26% of the initial 136 funds did not raise a follow-on fund. These are funds that have not been able to raise a follow-on fund for at least 9 years since initiation (as of June 30, 2019 with 2010 as the last vintage year in my sample). For the subsample of funds that advertised a new fund and which are used in this paper, 17% failed in their attempt to fundraise (funds without a first closing *at least* 6 years after due diligence information was sent out). This fraction is similar to related studies (31% as in Chakraborty and Evens (2018), 30% as in Barber and Yasuda (2017), and 14% as in Brown et al. (2019)). Brown et al. (2019) find that more experienced GPs are more likely to have a follow-on fund, although the relationship is not monotonic.[9] Overall while the sample is composed of more experienced and successful GPs on average, it still exhibits similar characteristics to the prior literature.

[9]A typical example, discussed in the media, is the failed fundraising attempt of the fourth U.S. buyout fund by J. W. Childs Associates in 2007. The private equity firm was in business for 12 years at that time and had around $3 billion under management.

FIGURE 1

Average Fund Performance by Fundraising Event Quarter

Figure 1 displays the average cumulative reported fund returns by fund quarter in excess of the cumulative returns for funds of the same vintage year but different fundraising quarter based on Preqin, ($CAR_t^{NAV} = \prod_{s=1}^{t}(1+r_s^{NAV}) - \prod_{s=1}^{t}(1+r_s^{preqin})$, with $r_t^{NAV}, r_t^{preqin} = (NAV_t + CO_t - CI_t)/NAV_{t-1} - 1)$, where CO_t and CI_t are fund cash inflows and cash outflows, respectively, in quarter t (for a similar way to capture changes in NAVs, see, e.g., Phalippou and Gottschalg (2009) or Ewens, Jones, and Rhodes-Kropf (2013)).



B. Deal Performance

As pointed out in the introduction, various challenges exist using fund-level data to study performance inflation around fundraising. Investment times and deal performance are heterogeneous; however, fund-level data does not allow us to distinguish between inflated deal performance and a simple cohort/forced-buyer story. In addition, deal-level purchase multiples are necessary to test whether buyers are pressured close to fundraising and actually pay a premium for later deals. Table 2 provides summary performance information on the mean and median investment years of the 2,451 fund-deal pairs in my sample. I report VMs based on the full sample and differentiate between realized and unrealized investments. Unrealized investments are investments that are still in the fund's portfolio at the end of the sample period as of Dec. 31, 2013, with valuations based on reported NAVs.[10]

For the 2,451 fund-deal pairs, the mean (median) VM is 1.83 (1.4). For the subsample of realized deals, the mean (median) is 2.09 (1.61), while for unrealized deals it is 1.53 (1.11). These numbers parallel those from the sample by Braun et al. (2017) and Brown et al. (2020), finding a median VM of 1.5 [1.55] for their full sample, 1.9 [2.10] for realized deals, and 1.0 [1.21] for unrealized deals. Meanwhile, Lopez-de-Silanes et al. (2015) document a median of 1.7 for a slightly older sample period.

Turning to the distribution of deal performance over initial investment years, Panel A of Table 2 shows the procyclicality of private equity investments (those that are made in up-markets) and the countercyclicality of private equity returns. While investments made between 1996 and 1999 did quite well, those initiated

[10]In the supplementary material, I present summary statistics based on the IRR (see Supplementary Table A1).

TABLE 2

Fund-Deal Pair Performance: Raw Results

Table 2 presents both raw mean and median performance based on value multiples, VM_i ($VM_i = (\sum_{t=0}^{T} CO_{it} + NAV_{iT}) / \sum_{t=0}^{T} CI_{it}$), across years of initial investment for each deal (Panel A), across industries (Panel B), and across geographies of the investments (Panel C). Investment VMs are presented for the full sample, for realized investments and unrealized investments as of Dec. 31, 2013. The data is winsorized at the 1% extremes.

Panel A. Fund-Investment Performance Across Investment Years

		Full Sample		Realized		Unrealized	
Investment Year	No. of Inv.	Mean	Median	Mean	Median	Mean	Median
1996	14	3.12	1.88	3.12	1.88		
1997	18	2.11	1.89	2.21	1.98	1.28	1.28
1998	30	2.64	1.8	2.74	1.84	1.33	1.33
1999	175	2.12	0.6	2.17	0.55	1.29	0.88
2000	196	1.29	0.29	1.29	0.28	1.26	0.4
2001	85	1.74	0.94	1.83	1.03	0.62	0.52
2002	96	2.45	1.99	2.56	2.19	0.9	0.62
2003	100	2.29	2.1	2.29	2.19	2.25	1.33
2004	157	2.44	2.2	2.6	2.31	1.7	1.73
2005	165	2.44	1.85	2.53	2.16	2.25	1.17
2006	241	1.89	1.47	2.36	1.93	1.49	1.08
2007	351	1.46	1.12	1.79	1.4	1.33	1
2008	246	1.56	1.16	1.58	1.46	1.55	1.05
2009	149	1.97	1.35	1.79	1.48	2.04	1.32
2010	243	1.56	1.19	2	1.93	1.5	1.14
2012	14	1.38	1.22			1.38	1.22
2013	16	1.09	1			1.09	1
All deals	2,451	1.83	1.4	2.09	1.61	1.53	1.11

Panel B. Fund-Investment Performance Across Industries

Industry	No. of Inv.	Mean	Median	Mean	Median	Mean	Median
Consumer discretionary	666	1.82	1.41	2.22	1.93	1.48	0.98
Consumer staples	55	2.43	1.77	1.92	1.1	2.89	2.43
Energy	60	2.42	1.49	3.24	2.63	1.82	0.66
Financials	278	1.83	1.41	2.15	2.07	1.51	0.8
Health care	276	1.93	1.46	2.53	1.95	1.28	0.94
Industrials	462	1.88	1.32	2.16	1.82	1.59	0.83
Information technology	481	1.63	1.2	1.69	0.83	1.46	1.44
Materials	121	1.86	1.36	2.37	1.7	1.55	1.18
Telecommunication services	39	0.91	0.59	0.88	0.52	1	1.11
Utilities	13	2.97	1.09	3.8	1.67	1.11	0.44

Panel C. Fund-Investment Performance Across Geographies

Geography	No. of Inv.	Mean	Median	Mean	Median	Mean	Median
USA	1,461	1.93	1.45	2.18	1.68	1.59	1.11
Non-USA	990	1.67	1.28	1.86	1.34	1.44	1.19

between 2000 and 2001 exhibited low VMs. This trend is reversed afterward. In particular, investments that were initiated between 2002 and 2005 experienced high realized VMs given their exits during the heyday of buyout funds and the valuation effect of money-chasing deals (Gompers and Lerner (2000)). Investments made during the financial crisis exhibited high returns as well. The finding of procyclical investment patterns (column 2) and divestment decisions is in line with Robinson and Sensoy (2016). Ewens and Farre-Mensa (2020) find a similar pattern for the change of founder equity in VC deals over time. Additionally, the realization status (which means whether portfolio companies have been exited) reveals that investments made in the late 1990s still had some unrealized deals (i.e., unexcited deals) in 2013, while returns from recent investments have not been harvested 2 years later, at the end of the sample period (in line with Braun et al. (2017)). Furthermore, the distribution of the sample and deal performance by industry and location of investment is presented in Panels B and C of Table 2, respectively. The consistency with descriptive statistics of comparable samples from other studies is reassuring.

III. Revisiting NAV Manipulation at the Deal Level

This section examines the importance of deal-level data in empirical tests using outcome variables that have been examined at the fund level in prior studies: NAV markdowns, valuation multiples, NAV-weighted excess returns, and NAV biases. I basically show how aggregating investment data into a single NAV muddles the picture of inflated NAVs found in these studies.

To set the stage, I begin by verifying the widely documented performance peak of low-reputation funds before fundraising in my sample relative to funds from the same vintage-year cohort that are not fundraising that quarter. For the same vintage year cohort benchmark funds, I use performance data from Preqin. The importance of peer-adjusted performance has been documented by Barber and Yasuda (2017) and Chakraborty and Ewens (2018). This also follows the prevalent industry practice of benchmarking against cohorts as evidence of a good track record when advertising a new fund. Figure 1 displays average cumulative excess returns over fund returns based on Preqin.

Confirming the results of previous studies, Figure 1 shows that cumulative abnormal excess fund returns peak shortly before a new fund is raised. While excess returns of high-reputation funds appear to plateau, they visibly drop for the subsample of low-reputation funds. In line with the extant literature, the drop in excess fund returns occurs shortly before the follow-on fund is closed (e.g., see also Graph A of Figure 6 in Barber and Yasuda (2017) or Graph A of Figure 3 in Brown et al. (2019)). A drop before fundraising is already itself evidence against manipulation, presumably because investors can observe this decline and manipulation would seem to require attempts to deceive that carry through until after fundraising.

In addition to Figure 1 which displays equally-weighted period-to-period NAV-based returns, high NAVs around fundraising, primarily driven by low reputation funds, are also evident in my sample using, for example, excess rank of since-inception returns in line with Barber and Yasuda (2017) (see Supplementary Table A2).

A. NAV Markdowns

Barber and Yasuda (2017) define markdowns in quarters t as $\text{MD}_t = \min(\text{NAV}_t - (\text{NAV}_{t-1} + \text{CI}_t - \text{CO}_t), 0)$, where CO_t and CI_t are the cash outflows and cash inflows, respectively. They find larger downward revisions of NAVs, for low-reputation funds following a fundraising event. This result squares with the existing evidence of performance peaking (also shown in Figure 1 and Supplementary Table A2), which is interpreted as inflation of NAVs at the time of fundraising. In a first step to distinguish inflation of NAVs from a cohort explanation, I analyze MDs of individual deals in event time and show how aggregating deal MDs map into the documented patterns from Barber and Yasuda (2017).

Figure 2 displays value-weighted averages of NAV markdowns (MDs) in quarter t of N_τ deals where each deal i was made 8 quarters ($\tau = -8$, blue bars),

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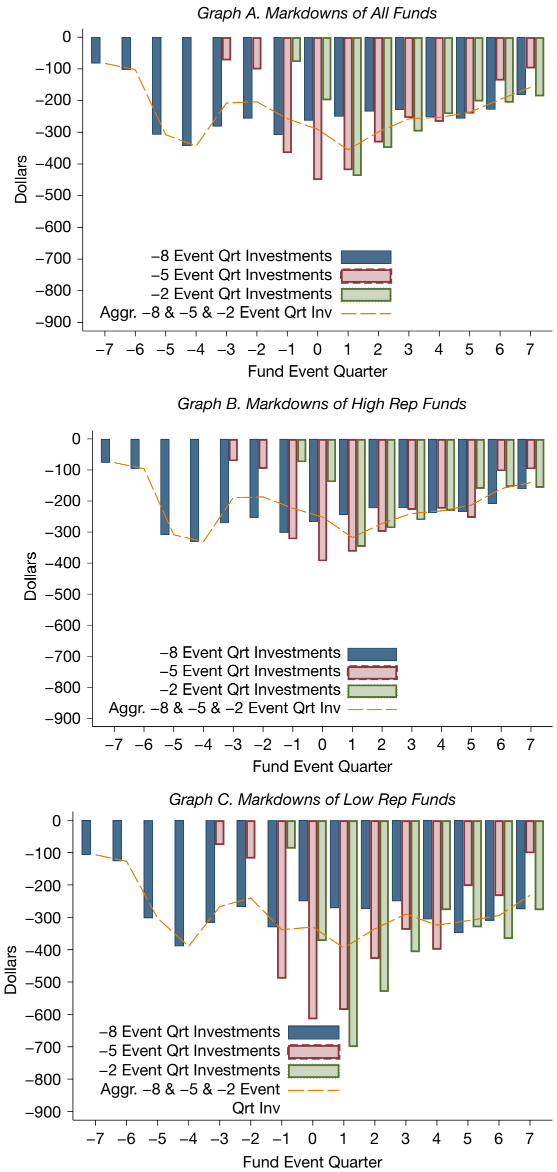
FIGURE 2

Size of NAV Markdowns by Fundraising Event Quarter

In Figure 2, first, I calculate value-weighted averages of NAV markdowns (MDs) in quarter t of N_τ deals where each deal i was made eight quarters ($\tau = -8$, blue bars), four quarters ($\tau = -4$, red bars), and two quarters ($\tau = -2$, green bars) before fundraising, $\text{MD}_{\tau t} = \sum_i^{N_\tau} y_{i\tau t-1} \text{MD}_{i\tau t}$, with $y_{i\tau t-1} = \text{NAV}_{i\tau t-1} / \sum_i^{N_\tau} \text{NAV}_{i\tau t-1}$. I follow the definition of a markdown by Barber and Yasuda's (2017) equation (3), and calculate a markdown on a \$10,000 investment. Second, I calculate average portfolio markdowns of deals with these three event investment times, which are displayed by the dashed orange line. This means, value-weighted averages of deal NAV markdowns ($\text{MD}_{\tau t}$) from step one are weighted by $\sum_i^{N_\tau} \text{NAV}_{i\tau t-1} / \sum_i^{M} \text{NAV}_{i\tau t-1}$, with $M = N_{-8} + N_{-4} + N_{-2}$. Graph A displays deals of high-reputation funds. Graph B displays deals of high-reputation funds, while Graph C shows results for the subsample of low-reputation funds.



4 quarters ($\tau = -4$, red bars), and two quarters ($\tau = -2$, green bars) before fundraising, $\text{MD}_{\tau t} = \sum_i^{N_\tau} y_{i\tau t-1} \text{MD}_{i\tau t}$, with $y_{i\tau t-1} = \text{NAV}_{i\tau t-1} / \sum_i^{N_\tau} \text{NAV}_{i\tau t-1}$.[11] I follow the definition by Barber and Yasuda (2017) (equation (3)) and calculate a markdown on a $10,000 investment. Findings generally reveal that markdown size is the largest in the third to fifth quarter of a deal's life. This shows (in line with the later valuation multiple (VM) analysis; see Table 7, Section III.C) that investments are on average held at cost for the first two quarters and that NAVs are mainly adjusted (either downward or upward) early in a deal's life. The finding does not suggest that markdowns are manipulated around fundraising.

The dashed orange line in Figure 2 displays average portfolio markdowns of deals with these three event investment times. This means, value-weighted averages of deal NAV markdowns ($\text{MD}_{\tau t}$), as shown as bars, are weighted by $\sum_i^{N_\tau} \text{NAV}_{i\tau t-1} / \sum_i^{M} \text{NAV}_{i\tau t-1}$, with $M = N_{-8} + N_{-4} + N_{-2}$. In line with Graph A of Figure 4 in Barber and Yasuda (2017), portfolio MDs peak around fundraising and are higher post-fundraising as compared to pre-fundraising event times in the subsample of low-reputation funds. As Figure 2 reveals, the peak is a result of keeping deals at cost on average for the first two quarters while the deals made closer to fundraising experience a larger drop in markdown size in the subsample of low-reputation funds.

In addition, Table 3 reports the results of a multivariate analysis where deal MDs in quarter *t* are regressed on a dummy variable that takes the value of 1 in periods after the fundraising event interacted with a dummy variable equaling 1 for deals made at most 1 year before fundraising, and other observables. In line with the "visual evidence" of Figure 2, I observe larger increases in markdowns post fundraising for deals made shortly before fundraising, significantly so in low-reputation funds.

Chakraborty and Ewens (2018) find that VC funds delay write-offs past fundraising, which could be a reason why we see higher markdowns post fundraising for deals that are made shortly before the event. In line with the evidence presented in Figure 2, my result of lower valuations for these deals does not change when controlling for the time investments are held at cost nor do they change when controlling for increases in NAVs prior to the fundraising quarter.

Next, I test how differences in deal MDs across investment times connect to the multivariate fund-level results in Barber and Yasuda ((2017), Table 6). Table 4 presents fund-level results based on my sample *with* and *without* deals that were made up to 1 year before fundraising. Markdowns in low-reputation funds drop by roughly $150 and become insignificant when deals made within a year before fundraising are excluded from the portfolio (column 6). Since an MD on the fund level is defined on a $10,000 investment, just as on the deal level, this drop is of about the same magnitude as the change in deal markdowns post-fundraising (see columns 5 and 6 of Table 3).[12]

[11]Deals are only considered that are held in the portfolio post fundraising.

[12]While this number is in line with the mean markdown of $162 on a $10,000 fund shown by Barber and Yasuda (2017) the economic magnitude of the drop increases when taking into account that late investments have notable markdowns prior to fundraising. For example, the markdowns in low-reputation funds drop by about twice as much ($270) when adding three quarters leading up to the fundraising event to the post-fundraising period.

TABLE 3

Deal Markdowns in the Post-Fundraising Period

Table 3 presents estimates of Tobit regressions of deal (i) markdown in quarter t ($\text{MD}_{it} = \min(\text{NAV}_{it} - (\text{NAV}_{it-1} + \text{CI}_{it} - \text{CO}_{it}), 0)$) on investment time and other observables. Deal size (total investment costs) is scaled to be $10,000 for all sample deals. Models in columns 2, 4, and 6 include only deals that have been completely realized (r) at the end of the sample period. POST-FUNDRAISING is a dummy variable that takes a value of 1 for quarters +1 to +14, where 0 is the fundraising event quarter (FRE). Deals are only considered that are held in the portfolio post fundraising. ≤YR_BEFORE_FRE is a dummy variable that takes the value of 1 for investments that are made within 1 year before the FRE, and 0 for investments made more than 1 year before the FRE. DEAL_SIZE denotes the size of the investment at the time of fundraising. TIME_INV_AT_COST is the number of quarters a deal is held at cost since its initial investment. NAV_UPLIFT (t-1) is a dummy variable that takes the value of 1 if the deal increased in NAV 1 quarter before the FRE, and 0 otherwise. Regression estimates are based on models of calendar year, fund quarter, geographic region, and industry fixed effects (FE). Standard errors are in parentheses, clustered at the fund level. *, **, and *** represent 2-tailed significance at the 10%, 5%, and 1% levels, respectively.

	All Funds		High Reputation		Low Reputation	
	$r+u$	r	$r+u$	r	$r+u$	r
	1	2	3	4	5	6
POST-FUNDRAISING	29.605*	21.363	33.226**	29.163	4.322	−6.852
	(15.582)	(22.758)	(16.321)	(24.316)	(33.069)	(50.551)
≤YR_BEFORE_FRE	−24.637	17.704	−28.767	24.006	−63.032*	−31.740*
	(20.383)	(26.542)	(20.194)	(26.385)	(38.663)	(17.329)
≤YR_BEFORE_FRE × POST-FUNDRAISING	−52.978**	−48.800*	−28.829	−33.886	−160.087***	−134.814**
	(24.340)	(25.947)	(26.260)	(34.688)	(60.187)	(65.650)
DEAL_SIZE	−104.019	56.797	98.188	419.915	−118.069	−17.613
	(158.719)	(275.302)	(159.673)	(288.021)	(339.446)	(523.220)
TIME_INV_AT_COST	12.802	9.942	8.219	6.772	34.722	13.463
	(11.216)	(8.411)	(8.243)	(7.426)	(162.189)	(22.845)
NAV_UPLIFT (t-1)	−42.410	−1.087	−1.194	−45.102	−0.871	−21.050
	(34.332)	(14.744)	(15.097)	(32.436)	(32.269)	(48.300)
Fund quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	34,116	17,541	23,621	12,508	10,495	5,033
Adj. R^2	0.021	0.019	0.026	0.022	0.027	0.024

TABLE 4

Fund Markdowns in the Post-Fundraising Period

Table 4 presents estimates of Tobit regressions of fund (j) markdown in quarter t ($\text{MD}_{jt} = \min(\text{NAV}_{jt} - (\text{NAV}_{jt-1} + \text{CI}_{jt} - \text{CO}_{jt}), 0)$). POST-FUNDRAISING is a dummy variable that takes a value of 1 for quarters +1 to +14, where 0 is the fundraising event quarter (FRE). Columns 1, 3, and 5 present fund results including all deals (i), while columns 2, 4, and 6 show fund results excluding investments made within 1 year before fundraising. Regression estimates are based on models of calendar year and fund quarter fixed effects (FE). Standard errors are in parentheses, clustered at the fund level. ** and *** represent 2-tailed significance at the 5% and 1% levels, respectively.

	All Funds		High Reputation		Low Reputation	
		w/o late i		w/o late i		w/o late i
	1	2	3	4	5	6
POST-FUNDRAISING	−118.907**	−53.432	−80.678	−58.794	−199.480***	−45.816
	(53.796)	(36.935)	(49.476)	(44.567)	(68.956)	(29.849)
Fund quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar year FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	3,818	3,764	2,519	2,465	1,299	1,299
Adj. R^2	0.103	0.066	0.143	0.043	0.065	0.019

B. Post-Fundraising Cohort Performance

Based on the MD results, I test whether the differences in deal MDs between investment times are sufficient to explain lower post-fundraising performance of fundraisers compared to cohort non-fundraisers, as documented by Barber and

Yasuda (2017) (Table 7). In line with their study, I calculate deal value multiples using the NAV at the time of a fundraising event (FRE) as investment costs for investment year × geographic region × industry cohort deals. The post-fundraising value multiple (FVM) is defined as

$$\text{FVM}_i = \frac{\sum_{t=\text{FRE}+1}^{T} \text{CO}_{it} + \text{NAV}_{iT}}{\text{NAV}_{i\text{FRE}} + \sum_{t=\text{FRE}+1}^{T} \text{CI}_{it}}, \tag{1}$$

where CO_{it} and CI_{it} are the cash outflows and cash inflows, respectively, for deal i in quarter t and T is the deal's liquidation quarter/last quarter in the sample period. Since benchmarking performance against cohorts is conventional in private equity and carried out to capture the fund-level peer comparison, I calculate cohort value multiples each time there is a fundraising event. For example, for the investment year 2006, there are 99 fundraising event quarters and 1,493 cohort deals of which 177 are fundraising and 1,316 are not.

Figure 3 displays the number of fundraising events by investment year (Panel A) and the number of cohort deals by investment year (Panel B). The previously mentioned procyclicality of private equity investments is reflected in both fundraising events as well as non-fundraising cohort investments. To alleviate potential concerns that some cohorts might be underrepresented, Figure 4 shows that the majority of my sample includes investment year × geographic region × industry cohorts with 50–300 deals.

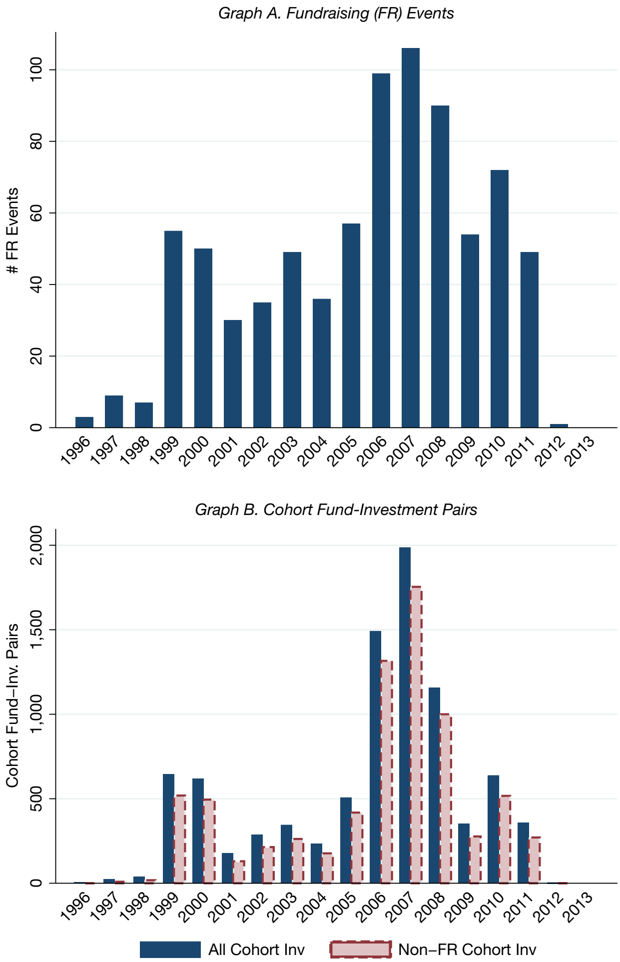
The results of the post-fundraising deal value multiples are presented in Table 5. Among all investment year × geographic region × industry cohort deals, the value multiple of fundraisers is on average not statistically different from their cohorts for deals made at least a year prior to the fundraising event, independent of low- or high-reputation funds. That being said, the average FVM is significantly lower for fundraising deals that are made up to 1 year before the event quarter compared to older cohort deals (ones made more than a year ahead of the event). The difference stems from low-reputation fundraising deals whose post-performance drops on average by 26 percentage points compared to their cohorts (column 5) and 37 percentage points when excluding deals that are unrealized at the end of the sample period (column 6). Larger deals seem to be valued more conservatively as deal size is associated with higher FVMs. This is consistent with Jenkinson et al. (2020) who find that NAVs reported by larger funds seem to be more conservative. For an additional robustness test, I exclude neighborhood cohort portfolio companies, defined as those with a fundraising quarter between −4 and +4 in event time (see Supplementary Table A3). Results remain qualitatively similar.

Results of Table 5 also address the concern that GPs may nurture deals from their newest fund and may ration their effort in monitoring deals from their previous fund. If that was the case, one would expect to see deals from fundraisers to drop in performance more than cohort deals post-fundraising event (regardless of whether deals were made shortly or long before the fundraising event). However, the first row of Table 5 shows no significantly or economically lower post-fundraising value multiples of deals made by fundraisers more than a year prior to fundraising.

FIGURE 3

Fundraising Events and Cohort Fund-Investment Pairs

Figure 3 displays the number of fundraising events by fund-investment year (Graph A) and number of cohort observations by fund-investment year (Graph B). Cohort observations include all fund-investment pairs that are held at the quarter of fundraising (blue solid bars) and only fund-investment pairs that are held in fundraising quarters of non-fundraising funds for the same investment year, geographic region, and industry (red dashed bars).



To reconcile these findings with the fund-level results of Barber and Yasuda (2017), I estimate models on the fund level on my sample and exclude investments made within 1 year before fundraising. Table 6 shows that the mean FVM is 31 percentage points less than non-fundraisers' (column 5), while this difference shrinks by 21 percentage points and becomes insignificant when excluding deals that were made at least 1 quarter before fundraising (column 6). This drop is roughly in line with the 26 percentage points lower FVM of deals made shortly before fundraising compared to cohort deals, displayed in column 5 of Table 5.

FIGURE 4

Distribution of Cohort Deals

Figure 4 displays the distribution of cohort observations including all fund-investment pairs that are held at the quarter of fundraising. A cohort consists of deals of same investment year, geographic region, and industry.

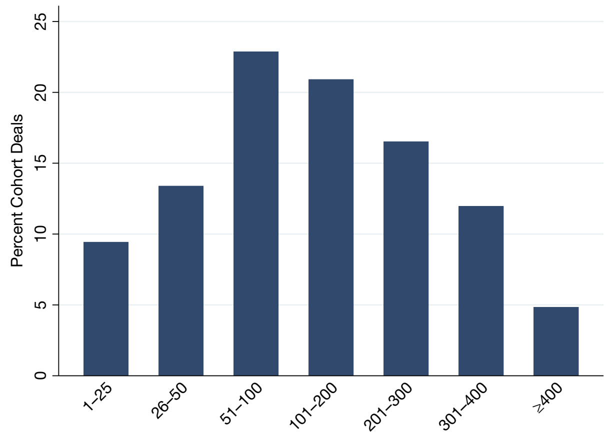


TABLE 5

Post-Fundraising Performance Across Deal Cohorts

Table 5 presents estimates of OLS regressions of post-fundraising performance of portfolio holdings (i) based on the value multiple at the fundraising event quarter (FRE), FVM_i ($\text{FVM}_i = (\sum_{t=\text{FRE}+1}^{T} \text{CO}_{it} + \text{NAV}_{iT})/(\text{NAV}_{i\text{FRE}} + \sum_{t=\text{FRE}+1}^{T} \text{CI}_{it})$), on investment time, and other observables. Thus, the unit of observation is a portfolio company × FRE quarter. I calculate a FVM for each investment year × geographic region × industry cohort portfolio company i assuming an investor made an investment at the stated net asset value (NAV) in the FRE and held the investment to liquidation (or the last quarter in which I observe an NAV). Models in columns 2, 4, and 6 include only deals that have been completely realized (r) at the end of the sample period. FR_INVESTMENT is a dummy variable that takes the value of 1 if a fund is raising a new fund that quarter, and 0 otherwise. I exclude cohort portfolio companies with the same fundraising quarter. ≤YR_BEFORE_FRE is a dummy variable that takes the value of 1 for investments that are made within 1 year before the FRE, and 0 for investments made more than 1 year before the FRE. DEAL_SIZE denotes the size of the investment at the time of fundraising. TIME_INV_AT_COST is the number of quarters a deal is held at cost since its initial investment. NAV_UPLIFT (t-1) is a dummy variable that takes the value of 1 if the deal increased in NAV 1 quarter before the FRE, and 0 otherwise. Regression estimates are based on models of investment year, geographic region, and industry fixed effects (FE). Standard errors are in parentheses, clustered at the fund level. *, **, and *** represent 2-tailed significance at the 10%, 5%, and 1% levels, respectively.

	All Funds		High Reputation		Low Reputation	
	$r+u$	r	$r+u$	r	$r+u$	r
	1	2	3	4	5	6
FR_INVESTMENT	−0.017 (0.035)	0.010 (0.066)	0.031 (0.047)	0.047 (0.086)	−0.055 (0.054)	0.016 (0.099)
≤YR_BEFORE_FRE	−0.003 (0.079)	−0.108 (0.151)	−0.010 (0.092)	−0.133 (0.174)	0.039 (0.207)	0.009 (0.448)
≤YR_BEFORE_FRE × FR_INVESTMENT	−0.132** (0.065)	−0.237* (0.130)	−0.072 (0.079)	−0.140 (0.157)	−0.261*** (0.090)	−0.373** (0.175)
DEAL_SIZE	1.686* (0.923)	0.023 (1.515)	0.929 (0.905)	−0.436 (1.910)	3.591** (1.671)	0.786 (2.699)
TIME_INV_AT_COST	0.012 (0.008)	0.017 (0.012)	0.010 (0.009)	0.010 (0.012)	0.158 (0.252)	0.030 (0.035)
NAV_UPLIFT (t-1)	0.133* (0.071)	0.173 (0.150)	0.047 (0.075)	0.232 (0.186)	0.173 (0.201)	0.084 (0.294)
No. of obs.	8,697	3,511	6,032	2,548	2,665	963
Adj. R^2	0.048	0.058	0.052	0.065	0.112	0.105

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TABLE 6

Post-Fundraising Performance Across Deal Cohorts

Table 6 presents estimates of OLS regressions of post-fundraising performance based on the fund (j) value multiple at the fundraising event quarter (FRE), FVM_j $((\text{FVM}_j = \sum_{t=\text{FRE}+1}^{T} \text{CO}_{jt} + \text{NAV}_{jT})/(\text{NAV}_{j\text{FRE}} + \sum_{t=\text{FRE}+1}^{T} \text{CI}_{jt}))$. Thus, the unit of observation is a fund × FRE quarter. I calculate a FVM for each vintage year cohort fund j assuming an investor purchased the fund at the stated net asset value (NAV) in the FRE and held the fund to liquidation (or the last quarter in which I observe an NAV). Columns 1, 3, and 5 present fund results including all deals, while columns 2, 4, and 6 show fund results excluding investments (i) made within 1 year before fundraising. FUNDRAISER is a dummy variable that takes the value of 1 if a fund is raising a new fund that quarter, and 0 otherwise. I exclude cohort funds with the same fundraising quarter. Regression estimates are based on models of event-vintage year fixed effects (FE). Standard errors are in parentheses, clustered at the fund level. * represents 2-tailed significance at the 10% level.

	All Funds		High Reputation		Low Reputation	
		w/o late i		w/o late i		w/o late i
	1	2	3	4	5	6
FUNDRAISER	−0.117*	−0.067	−0.064	−0.048	−0.312*	−0.099
	(0.057)	(0.040)	(0.041)	(0.042)	(0.163)	(0.123)
Event-vintage year FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	872	872	597	597	275	275
Adj. R^2	0.200	0.213	0.228	0.209	0.196	0.236

C. Performance Across Investment Time

The evidence of underperforming later deals after the fundraising event brings us to the question of what happened to these investments prior to the fundraising event. To test whether NAVs for these deals are moving up and down and are initially being stated at cost, I conduct a subsample analysis of valuation multiples. I run t-tests of mean deal valuation multiples ± 7quarters surrounding the fundraising event along the lines of Table 5 of Barber and Yasuda (2017) and Table 4 of Jenkinson et al. (2013). Table 7 displays results by event quarter for investments made eight quarters prior to the fundraising event, four quarters and two quarters prior to fundraising.[13]

As results in Table 7 show, independent of investment time, deals are stated at cost, on average, up to two quarters after the initial investment. I observe the largest average change in return multiples around the third to fifth quarter post initial investment, independent of closeness to the fundraising event (this is in line with NAV markdowns which are the largest in the third to fifth quarters of a deal's life; see Figure 2). Only investments of high-reputation funds, made −8/−4/−2 quarters before the fundraising event, as well as investments of low-reputation funds, made in event quarter −8, increase on average in valuation multiples (see columns 4, 5, and 7 of Table 7). Investments of low-reputation funds, made within a year prior to fundraising, decrease on average in valuation multiples (see columns 8 and 9 of Table 7). Increases and decreases in deal valuations are mostly monotonic and statistically significant.

Table 8 presents a multivariate analysis of deal performance across investment time. Overall, the results indicate that peak performance on the fund level is associated with early investments having higher returns as compared to investments

[13]Deal valuation multiples are defined as (cumulative deal cash outflows to date + NAV of the deal to date, if unrealized)/cumulative investments into the deal to date.

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TABLE 7

Valuation Multiples in Event Time

Table 7 presents mean deal valuation multiples by event quarter, where $t = 0$ is the quarter of a fundraising event. Deal valuation multiples are defined as (cumulative deal cash outflows to date + NAV of the deal to date, if unrealized)/cumulative investments into the deal to date. Columns 1, 4, and 7 (2, 5, and 8) [3, 6, and 9] only include investments made 8 (4) [2] quarters prior to the fundraising event. High-reputation funds are the complements of low-reputation funds. I run t-test of whether valuation multiples are statistically different from 1. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively, for tests of the 2-sided null hypothesis that valuation multiples are 1.

	All Funds			High Reputation			Low Reputation		
	1	2	3	4	5	6	7	8	9
−7	1.028			1.015			1.054		
−6	1.025			1.022			1.031		
−5	1.178*			1.208**			1.121*		
−4	1.264***			1.259***			1.273**		
−3	1.379***	1.004		1.324***	1.007		1.487**	0.996	
−2	1.422***	1.030		1.369***	1.051		1.524***	0.982	
−1	1.484***	1.006	1.008	1.488***	1.115*	1.019	1.476***	0.750***	0.981
0	1.513***	1.068*	1.008	1.548***	1.253**	1.068	1.446**	0.651***	0.866**
1	1.499***	1.129**	1.081*	1.515***	1.389**	1.166**	1.468***	0.544***	0.786***
2	1.516***	1.273**	1.280**	1.503***	1.572***	1.512***	1.541***	0.599***	0.728***
3	1.629***	1.275**	1.354**	1.693***	1.591***	1.606***	1.506***	0.564***	0.755**
4	1.736***	1.312**	1.371***	1.818***	1.655***	1.637***	1.576***	0.539***	0.739**
5	1.722***	1.373***	1.372***	1.784***	1.728***	1.658***	1.601**	0.574***	0.692**
6	1.871***	1.478***	1.403***	1.856***	1.887***	1.711***	1.899**	0.557***	0.671**
7	1.889***	1.533***	1.404***	1.886***	1.959***	1.719***	1.896*	0.571**	0.652**
No. of deals	97	166	142	64	115	100	33	51	42

TABLE 8

Deal Performance Across Investment Time

Table 8 presents estimates of OLS regressions of deal (i) value multiple at exit or at the end of the sample period (T), VM_i ($\mathrm{VM}_i = (\sum_{t=0}^{T}\mathrm{CO}_{it} + \mathrm{NAV}_{iT})/\sum_{t=0}^{T}\mathrm{CI}_{it}$), on investment time, and other observables. Deals are only considered that are held in the portfolio post fundraising. The unit of observation is a portfolio company. In contrast to Table 5, which looks at the cross section of FRE quarters of cohort investment, this table compares ex post performance of investments across the fund's life time. Models in columns 2, 4, and 6 include only deals that have been completely realized (r) at the end of the sample period. ≤YR_BEFORE_FRE is a dummy variable that takes the value of 1 for investments that are made within 1 year before the FRE, and 0 for investments made more than 1 year before the FRE. DEAL_SIZE denotes the size of the investment at the time of fundraising. Regression estimates are based on models of investment year, geographic region, and industry fixed effects (FE). Standard errors are in parentheses, clustered at the fund level. ** and *** represent 2-tailed significance at the 5% and 1% levels, respectively.

	All Funds		High Reputation		Low Reputation	
	$r+u$	r	$r+u$	r	$r+u$	r
	1	2	3	4	5	6
≤YR_BEFORE_FRE	−0.321***	−0.465**	−0.114	−0.278	−0.736***	−0.847***
	(0.113)	(0.177)	(0.120)	(0.184)	(0.135)	(0.261)
DEAL_SIZE	1.709	−0.268	0.335	−0.397	2.569	0.575
	(1.513)	(2.276)	(1.223)	(2.074)	(2.635)	(3.766)
Inv. year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	1,650	787	1,173	571	477	216
Adj. R^2	0.055	0.111	0.081	0.136	0.123	0.185

made shortly before fundraising.[14] The difference stems from low-reputation funds. The economic effects are also large. Column 5 shows that VMs of deals that are made within 1 year prior to fundraising are on average 74% below deals that are

[14]Deals are only considered that are held in the portfolio post fundraising, and the same is true for Table 7.

made earlier, and 85% lower when excluding deals that are unrealized at the end of the sample period. Considering that deals made shortly before fundraising decline only by about 26 percentage points post fundraising (see Table 5), earlier deals have notably higher valuations at fundraising than later deals, as already shown in univariate results in Table 7. Investment year, geographic region, and industry fixed effects ensure that I compare VMs within the same cohorts as in Table 5.

Sections III.A and III.B in combination with Section III.C show that the fund-level results seem to be due to early investments legitimately having higher NAVs at fundraising time, while later investments are largely held at cost at fundraising that end up being poor investments.

D. Excess Returns

Despite reconciling the deal-level results with Barber and Yasuda (2017), the question remains of how they map into the compelling evidence at the fund level presented by Brown et al. (2019).

Brown et al. (2019) conduct their empirical analysis based on a theoretical framework and find their results to be consistent with a "costly signaling equilibrium" where inflating NAVs is associated with a lower probability of a successful fundraising but nevertheless is pursued by low-reputation funds. Since their theoretical framework calls for a proxy of excess returns, I analyze cumulative NAV-weighted excess returns (or WPMEs) of individual deals in event time and illustrate how aggregating deal WPMEs produces results that at first seem consistent with NAV manipulation, but in fact relate to poor investments made near fundraising events.

To illustrate the effect, Figure 5 presents WPMEs of a subsample related to Sections III.A and III.C. First, I calculate changes in PME for each investment i which was made either eight quarters ($\tau = -8$), four quarters ($\tau = -4$), or two quarters ($\tau = -2$) before fundraising, following equation (15) in Brown et al. (2019). A change in PME in quarter t to the previous quarter is defined as excess return over the public equity benchmark, normalized by the ratio of time $t-1$ NAV to the time t future value of all cash inflows:

$$\Delta\mathrm{PME}_{i\tau t} = \left(R_{i\tau t}^{\mathrm{NAV}} - R_{i\tau t}^{mkt}\right)\frac{\mathrm{NAV}_{i\tau t-1}}{\sum\limits_{k=\tau}^{t}\mathrm{CI}_{ik}R_{ik:t}^{mkt}}, \tag{2}$$

where $R_{ik:t}^{mkt}$ is the gross return of deal i between time k and t on the market (CRSP value-weighted index). Second, I calculate cumulative NAV-weighted excess returns based on equation (16) in Brown et al. (2019) for deals with investment date eight quarters (blue bars in Figure 5), four quarters (red bars in Figure 5), and two quarters (green bars in Figure 5) before fundraising. This means that weighted PMEs are defined over a time interval (τ,b) for a cross section of N_τ investments made in event quarter τ:

$$\mathrm{WPME}_{\tau:b} = 1 + \sum_{t=\tau}^{t=b}\left[\sum\nolimits_{i\in N_\tau}\Delta\mathrm{PME}_{i\tau t}\Big/\sum\nolimits_{i\in N_\tau}\frac{\mathrm{NAV}_{i\tau t-1}}{\sum\limits_{t=\tau}^{t=b}CI_{i\tau:b}R_{i\tau:b}^{mkt}}\right]. \tag{3}$$

FIGURE 5

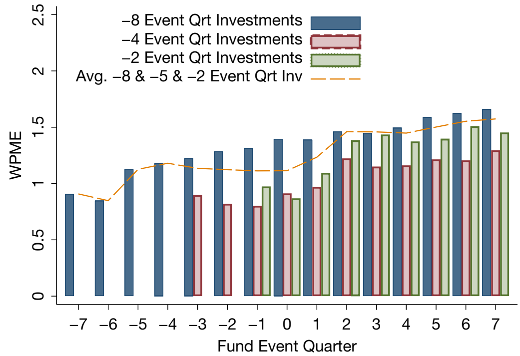
Average Performance by Fundraising Event Quarter

Figure 5 displays cumulative NAV-weighted excess returns of private equity investments over the public market index. First, I calculate changes in PME for each investment i which was made either eight quarters ($\tau = -8$), four quarters ($\tau = -4$), and two quarters ($\tau = -2$) before fundraising. A change in PME in quarter t to the previous quarter is defined as excess return over the public equity benchmark, normalized by the ratio of time $t-1$ NAV to the time t future value of all cash inflows: $\Delta \text{PME}_{i\tau t} = \left(R^{\text{NAV}}_{i\tau t} - R^{mkt}_{i\tau t}\right) \frac{\text{NAV}_{i\tau t-1}}{\sum_{k=\tau}^{t} \text{CI}_{ik} R^{mkt}_{ik:t}}$, where $R^{mkt}_{ik:t}$ is the gross return of deal i between time k and t on the market (CRSP value-weighted index). Second, I calculate cumulative NAV-weighted excess returns for investments made eight quarters (blue bars), four quarters (red bars) and two quarters (green bars) before fundraising. This means that weighted PMEs are defined over a time interval (τ, b) for a cross section of N_τ investments made in event quarter τ:

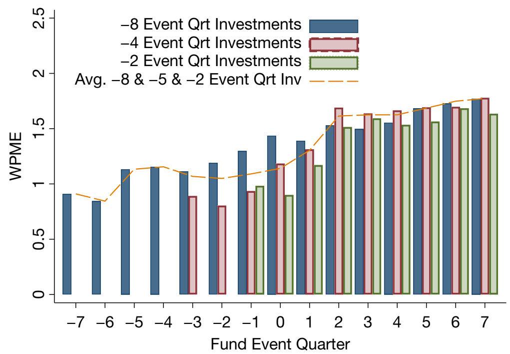
$$\text{WPME}_{\tau:b} = 1 + \sum_{t=\tau}^{t=b} \left[\sum_{i \in N_\tau} \Delta \text{PME}_{i\tau t} \Big/ \sum_{i \in N_\tau} \frac{\text{NAV}_{i\tau t-1}}{\sum_{t=\tau}^{t=b} \text{CI}_{i\tau:b} R^{mkt}_{i\tau:b}} \right].$$

Third, I calculate WPMEs of deals with these three event investment times combined, which are displayed by the dashed orange line. Graph A displays results for all sample funds. Graph B displays deals of high-reputation funds, while Graph C shows results for the subsample of low-reputation funds.

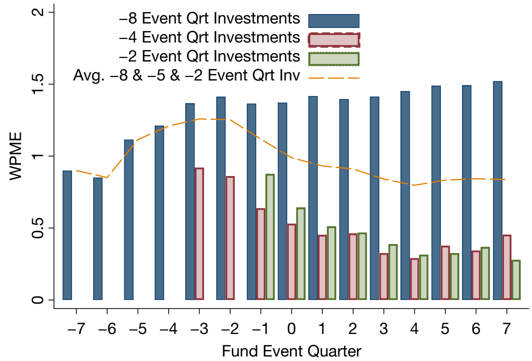
Graph A. Cumulative NAV–Weighted Excess Returns All Funds



Graph B. Cum. NAV–Weighted Excess Returns High Rep Funds



Graph C. Cumulative NAV–Weighted Excess Returns Low Rep Funds



Third, I calculate cumulative value-weighted portfolio excess returns of deals with these three event investment times, which are displayed by the dashed orange line in Figure 5. This means, WPMEs are derived across all three investment times based on equation (3).

Figure 5 shows that WPMEs are slightly below 1 for the first two quarters where deals are held at cost and increase steadily independent of investment time in the overall sample (Graph A). The picture changes when splitting the sample between high- and low-reputation funds. In line with Graph B of Figure 3 in Brown et al. (2019), excess returns of low-reputation funds (orange line, Graph A of Figure 5) drop, and in fact, become negative shortly before fundraising while WPMEs of high-reputation funds steadily increase (Graph B, Figure 5). Taking into account that results remain qualitatively similar when conditioning on fundraising success (see Supplementary Figure A1), the drop is not a result of costly signaling. It rather stems from investments made within a year prior to fundraising and can be explained by a cohort effect.

E. Biases in NAVs and Peer Chasing

The importance of tracking private equity performance relative to peers and the claim that low-reputation funds inflate NAVs accordingly has been addressed on the basis of Barber and Yasuda (2017) in Section III.B. Brown et al.'s (2019) theoretical framework makes similar predictions which they test with an NAV-bias measure. Following their design, I analyze whether NAV biases are related to peer chasing when manipulation incentives are strong or can alternatively be explained with the timing of investments. I define the NAV bias of deal i in quarter t based on equation (19) in Brown et al. (2019) as:

$$(4)\qquad \Delta \mathrm{BIAS}_{it} = \log\left(\mathrm{NAV}_{it}\right) - \log\left(\mathrm{NAV}_{it-1} \times R_t^{mkt} - \left(\mathrm{CO}_{it} - \mathrm{CI}_{it}\right)\right).$$

Thus, $\Delta \mathrm{BIAS}_{it}$ is the market- and cash flow-adjusted NAV growth between $t-1$ and t. Table 9 reports estimates of models with $\Delta \mathrm{BIAS}_{it}$ as the dependent variable and PEER_CHASING, FUND_TIMING as well as ≤YR_BEFORE_FRE are the three main explanatory variables of interest. PEER_CHASING is defined as the difference between a fund's reported IRR-to-date for the $t-1$ quarter of fund life and the median across all funds within 1 year from the fund's vintage year. FUND_TIMING is the natural log of the number of years spent without a follow-on fund.[15] As before, ≤YR_BEFORE_FRE is a dummy variable equaling 1 for deals made up to 1 year before fundraising. In columns 3 and 4, I use a beta of 1.7, and I only include fully realized deals by the end of the sample period in columns 2 and 4.

The first two rows of Table 9 reveal that peer chasing and fund timing are not correlated to NAV bias for investments made more than 1 year prior to fundraising. That being said, I find a significantly positive effect of fund timing on NAV bias in later deals and a significantly negative effect on NAV bias when the fund catches up to the performance of its peers in those later investments. The three-way interaction of PEER_CHASING, FUND_TIMING' and ≤YR_BEFORE_FRE reveals that catching up to the performance of its peers has a stronger effect on the drop in

[15]Both definitions are in line with Brown et al. (2019).

TABLE 9

Biases in Deal NAVs

Table 9 presents estimates of OLS regressions of deal (i) NAV bias in quarter t, ΔBIAS_{it} $\left(\Delta\text{BIAS}_{it} = \log(\text{NAV}_{it}) - \log\left(\text{NAV}_{it-1} \times R_t^{mkt} - (\text{CO}_{it} - \text{CI}_{it})\right)\right)$, on fund timing, peer chasing, and other observables. Deals are only considered that are held in the portfolio post fundraising. FUND_TIMING is the natural log of the number of years (1 plus years after the second) spent without a follow-on fund. PEER_CHASING is defined as difference between a fund's reported IRR-to-date for the $t-1$ quarter of fund's life and the median across all funds within 1 year from the fund's vintage year. Both these regressors are mean-centered. ≤YR_BEFORE_FRE is a dummy variable that takes the value of 1 for investments that are made within 1 year before the FRE, and 0 for investments made more than 1 year before the FRE. Models in columns 2 and 4 include only deals that have been completely realized at the end of the sample period. The market beta of the fund assets is assumed to be 1.7 in models 3 and 4. All models include calendar year, geographic region, and industry fixed effects (FE), as well as deal cash outflows (CO_{it}) and cash inflows (CI_{it}) over the current quarter scaled by the end of quarter deal NAVs. Standard errors are in parentheses, clustered at the fund level. *, **, and *** represent 2-tailed significance at the 10%, 5%, and 1% levels, respectively.

	$\beta = 1.0$		$\beta = 1.7$	
	$r+u$	r	$r+u$	r
	1	2	3	4
FUND_TIMING (FT)	0.011 (0.010)	0.018 (0.015)	0.011 (0.009)	0.021 (0.013)
PEER_CHASING	−0.036 (0.023)	−0.047 (0.034)	−0.025 (0.019)	−0.038 (0.029)
≤YR_BEFORE_FRE	−0.056** (0.027)	−0.025 (0.027)	−0.049* (0.026)	−0.023 (0.022)
FUND_TIMING × PEER_CHASING	0.013 (0.022)	0.037 (0.033)	0.004 (0.019)	0.026 (0.028)
≤YR_BEFORE_FRE × FUND_TIMING	0.092*** (0.016)	0.097*** (0.023)	0.072*** (0.013)	0.075*** (0.020)
≤YR_BEFORE_FRE × PEER_CHASING	−0.064* (0.034)	0.012 (0.050)	−0.052* (0.028)	0.023 (0.042)
≤YR_BEFORE_FRE × FT × PEER_CHASING	−0.100*** (0.032)	−0.070* (0.037)	−0.075*** (0.027)	−0.051** (0.025)
Cash-flow controls	Yes	Yes	Yes	Yes
Calendar year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
No. of obs.	34,116	17,541	34,116	17,541
Adj. R^2	0.206	0.170	0.214	0.181

NAV bias of late investments as compared to early investments the longer it takes to raise the next fund. Brown et al. (2019) interpret the negative coefficient of their 2-way interaction between PEER_CHASING and FUND_TIMING on the fund level as evidence that funds with a stronger incentive to manipulate when chasing peers inflate NAVs so that reported returns revert to those of its peers in the next quarter. Since I find this effect stemming from deals which, on average, are initially stated at cost only to drop in valuations thereafter and are made at a time where fund performance is at its best compared to peers (see Supplementary Table A2), investment timing is a more consistent explanation of the finding.

To tie the cohort effect into the documented patterns in Brown et al. (2019), I turn to the fund-level and analyze ΔBIAS_{jt} for fund j in quarter t on the interaction between PEER_CHASING and FUND_TIMING *with* and *without* late investments. Columns 1 and 2 of Table 10 indicate a negative and significant coefficient on the interaction term. This ~10 percentage points stronger return revision is roughly of the same economic magnitude found in late investments, see column 1 of Table 9, and turns out to vanish when excluding these investments from the fund as shown in columns 3 and 4 of Table 10. The result shows that the cohort effect is economically strong enough to explain the seemingly inflated NAVs on the fund-level.

TABLE 10

Biases in Fund NAVs

Table 10 presents estimates of OLS regressions of fund (j) NAV bias in quarter t, ΔBIAS_{jt} $\left(\Delta\text{BIAS}_{jt} = \log\left(\text{NAV}_{jt}\right) - \log\left(\text{NAV}_{jt-1} \times R_t^{mkt} - \left(\text{CO}_{jt} - \text{CI}_{jt}\right)\right)\right)$, on fund timing, peer chasing, and other observables. FUND_TIMING is the natural log of the number of years (1 plus years after the second) spent without a follow-on fund. PEER_CHASING is defined as difference between a fund's reported IRR-to-date for the $t-1$ quarter of fund's life and the median across all funds within 1 year from the fund's vintage year. Both these regressors are mean centered. Columns 1 and 3 present fund results including all deals, while columns 2 and 4 show fund results excluding investments made within 1 year before fundraising. The market beta of the fund assets is assumed to be 1.7 in models 3 and 4. All models include calendar year fixed effects (FE), as well as fund cash outflows (CO_{jt}) and cash inflows (CO_{jt}) over the current quarter scaled by the end of quarter fund NAVs. Standard errors are in parentheses, clustered at the fund level. *, **, and *** represent 2-tailed significance at the 10%, 5%, and 1% levels, respectively.

	β = 1.0		β = 1.7	
		w/o late i		w/o late i
	1	2	3	4
FUND_TIMING (FT)	0.146***	−0.004	0.124***	−0.002
	(0.040)	(0.005)	(0.032)	(0.005)
PEER_CHASING	0.021*	0.006	0.023**	0.014
	(0.012)	(0.011)	(0.010)	(0.010)
FUND_TIMING × PEER_CHASING	−0.119***	−0.003	−0.103***	−0.007
	(0.010)	(0.005)	(0.008)	(0.004)
Cash-flow controls	Yes	Yes	Yes	Yes
Calendar year FE	Yes	Yes	Yes	Yes
No. of obs.	3,818	3,764	3,818	3,764
Adj. R^2	0.304	0.358	0.328	0.378

F. NAVs of Club Deals

While results indicate that performance peaks on the fund level are driven by a cohort effect, I still test the inflation hypothesis from a different angle in this section. In particular, I compare valuations of the same deal across at least two GPs (club deals), where one is fundraising and the other one is not. Ultimately, I compare the valuations of 127 club deals at the time of a fundraising event in my sample. For these 127 club deals, I adjust the original NAV for distributions and contributions divided by the fundraising quarter NAV to examine differences in valuation. Thus, the adjusted NAV (aNAV) is defined as:

$$a\text{NAV}_i = \frac{\left(\text{NAV}_{i0} + \sum_{t=1}^{\text{FRE}} (\text{CI}_{it} - \text{CO}_{it})\right)}{\text{NAV}_{i\text{FRE}}}, \tag{5}$$

where $\text{CI}_{i\tau}$ and $\text{CO}_{i\tau}$ are the cash inflows and cash outflows, respectively, for deal i in quarter t and 0 is the investment quarter of deal i. If GPs inflate valuations at a fundraising event relative to other periods, then aNAVs would be systematically lower for fundraising club deals than for non-fundraising club deals. My sample includes 443 observations where club deals are unrealized at a fundraising event.

To set the stage, Figure 6 shows the percentage of cases where the fundraising GPs have the highest valuations at fundraising (that means the lowest aNAVs) (left bar), the same valuations as non-fundraising GPs (middle bar), and where non-fundraising GPs have the highest valuations in the fundraising quarter (right bar). GPs are only fundraising in about one-third of all investments with the highest valuation, while in 50% of all cases the highest valuations are linked to non-fundraising GPs. This difference between fundraising GPs and non-fundraising

FIGURE 6

Valuation of Club Deals

Figure 6 displays the percentage of club deals for which a fundraising GP has the highest/same/lowest adjusted deal NAV at fundraising time compared to non-fundraising GPs. Adjusted NAV (aNAV) is calculated using the original NAV adjusted for distributions and contributions divided by the fundraising event (FRE) quarter NAV. Thus, $a\text{NAV}_i = (\text{NAV}_{i0} + \sum_{t=1}^{\text{FRE}} (\text{CI}_{it} - \text{CO}_{it}))/\text{NAV}_{i\text{FRE}}$, where CI_{it} and CO_{it} are the cash inflows and cash outflows, respectively, for deal i in quarter t, and 0 is the investment quarter of deal i. If GPs inflate valuations at a fundraising event relative to other periods, then aNAVs would be systematically lower for fundraising club deals than for non-fundraising club deals. Graph A displays club deals for all sample funds raised by different GPs. Graph B displays club deals of high-reputation funds raised by different GPs, while Graph C shows results for the subsample of low-reputation funds raised by different GPs.

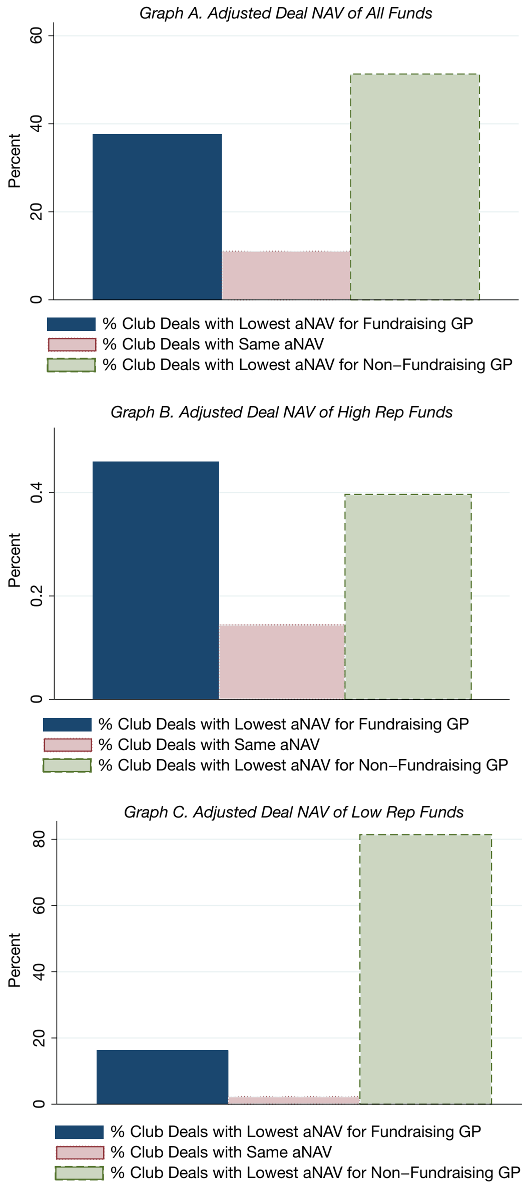


TABLE 11

Valuation Across Club Deals at Fundraising Time

Table 11 presents estimates of OLS regressions of adjusted NAVs of portfolio holdings at the fundraising event quarter (FRE), $a\text{NAV}_i$ ($a\text{NAV}_i = (\text{NAV}_{i0} + \sum_{t=1}^{\text{FRE}}(\text{CI}_{it} - \text{CO}_{it}))/\text{NAV}_{i\text{FRE}}$), where CI_{it} and CO_{it} are the cash inflows and cash outflows, respectively, for deal i in quarter t and 0 is the investment quarter of deal i. Only investments are considered that are held by funds of two or more GPs. If GPs inflate valuations at a fundraising event relative to other periods, then aNAVs would be systematically lower for fundraising club deals than for non-fundraising club deals. FR_INVESTMENT is a dummy variable that takes the value of 1 if a GP raises a new fund that quarter and 0 for the GPs without a fundraising event that quarter. Only GP deal pairs are considered if GPs fundraise in different quarters. Otherwise, by construction it cannot be tested if all GPs manipulate performance estimates due to the same fundraising event quarter. DEAL_SIZE denotes the size of the investment at the time of fundraising. Regression estimates are based on models of investment year, geographic region, and industry fixed effects (FE). Standard errors are in parentheses, clustered at the fund level. ** and *** represent 2-tailed significance at the 5% and 1% levels, respectively.

	All Funds		High Reputation		Low Reputation	
	1	2	3	4	5	6
FR_INVESTMENT	0.030	0.029	0.074	0.072	−0.082	−0.072
	(0.074)	(0.074)	(0.077)	(0.077)	(0.174)	(0.170)
DEAL_SIZE		0.012		0.016**		−0.939***
		(0.010)		(0.008)		(0.266)
Inv. year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	443	443	335	335	108	108
Adj. R^2	0.175	0.173	0.207	0.204	0.160	0.151

GPs is even larger for low-reputation funds (Graph C, Figure 6). The small percentage of investments with the same aNAVs indicates that GPs do not seem to coordinate their valuations of the same deal.

To formally test whether fundraising GPs increase the valuation of their deals compared to other invested GPs, I regress aNAVs in the same company on a fundraising investment dummy that is equal to 1 if the investment is made by a fundraising GP in the event quarter, and 0 otherwise.

Results presented in Table 11 show there is no statistically significant difference in valuation of individual investments between fundraising and non-fundraising GPs, controlling for investment year, geographic region, and industry. Results remain qualitatively unchanged by considering subsamples of high- and low-reputation funds (columns 3–6).[16] The results on club deals are consistent with previous findings of no evidence of strategically inflated valuations.

IV. Investment Behavior Before Fundraising

In this section, I explore the mechanism that is driving the result regarding bad deals undertaken before fundraising. In other words, why do GPs have fewer incentives to invest wisely at this point? First, I model the time to draw down dry powder as a function of observable fund and market characteristics, to test whether funds are more under pressure closer to the fundraising time. Then, I focus on deal pricing to assess whether there is direct evidence that funds under pressure pay more for deals relative to comparable M&A transactions.

[16]Results also remain qualitatively unchanged when testing whether there are differences in NAVs between club deals four quarters leading up to the fundraising quarter (see Supplementary Table A4).

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A. Buy Pressure

Table 12 displays estimates of accelerated time-to-failure models (duration models) that explain the log(time-to-failure) of drawing down 35% and 70% of the fund's committed capital. Typically, 70% is the contractual threshold to raise a new fund, which is also in line with the median and mean dry powder remaining in the fund around the closing time of a new fund as documented by Chakraborty and Ewens (2018). The advantages of duration models compared to OLS in my sample are that they can better deal with the problem of right-censoring (meaning observations not reaching the threshold can still be used in the estimation), accommodate time-varying covariates, and make more appropriate assumptions about the distribution of time. The data are well fit by a Weibull model, which I use for the distribution of the survival time. To account for potentially unobserved heterogeneity due to omitted variables, I estimate frailty models. I assume that the underlying distribution for the frailty (unobserved heterogeneity) is gamma-distributed. From an analytical point of view, the gamma distribution is convenient because it is easy to derive the closed-form expressions of the survival and density function, which is why this distribution is used in most applications (see, e.g., Hougaard (2000)).

TABLE 12

Drawdown Times for Low- Versus High-Reputation Funds

Table 12 reports coefficients of the time a fund takes to draw down 70% of committed capital (columns 1 and 2), which is the typical contractual threshold to raise a new fund, and 35% of committed capital (columns 3 and 4). Reported coefficients stem from accelerated time-to-failure models. The effect of a δ_j-unit change in covariate j is to multiply the failure time by $\exp(\delta_j \beta_j)$. I estimate frailty models. I assume that the underlying distribution for the frailty (unobserved heterogeneity) is gamma-distributed. The error is assumed to follow a Weibull distribution. LOW-REP is a dummy variable that takes the value of 1 for a low-reputation fund and 0 otherwise. OVERLAP is a dummy variable that takes the value of 1 for overlapping quarters with the investment period of the previous fund, and 0 otherwise. YIELD_SPREAD is calculated on corporate bonds (using Moody's BAA bond index, estimated quarterly in Mar., June, Sept., and Dec.) over the CRSP risk-free rate. 1999Q1_TO_2000Q1 dummy is a time-varying covariate: over the fund's life, and it equals 1 only in 1999Q1–2000Q2. Standard errors are in parentheses, clustered at the fund level. *, **, and *** represent 2-tailed significance at the 10%, 5%, and 1% levels, respectively.

	Time-Varying?	Drawdown			
		70%	70%	35%	35%
		1	2	3	4
LOW-REP	No	0.151 (0.139)	0.145 (0.138)	0.221*** (0.091)	0.182** (0.086)
OVERLAP	Yes	0.279* (0.149)	0.248* (0.133)	0.219*** (0.090)	0.187*** (0.079)
ln(FUND_SIZE)	No	0.006 (0.078)	0.021 (0.080)	−0.016 (0.037)	−0.015 (0.038)
YIELD_SPREAD	Yes	0.110 (0.072)	0.127* (0.068)	0.106*** (0.034)	0.139*** (0.029)
QRT_RET_ON_S&P500	Yes	−0.000 (0.001)	−0.000 (0.001)	0.000 (0.000)	0.000 (0.000)
S&P500_M/B_RATIO	Yes	−0.096 (0.116)		−0.125* (0.075)	
Q1_TO_2000Q1	Yes		−0.789* (0.440)		−1.011*** (0.217)
p		2.056	2.202	2.078	2.372
L ratio test: all coeff. = 0(χ^2)		31.5	38.2	56.7	84.5
p-value		0.000	0.000	0.000	0.000
No. of funds		121	121	121	121
No. of failures		90	90	113	113
No. of fund-quarters		1,697	1,697	940	940

Coefficients reported in Table 12 measure the effect of a covariate on the log of time (in quarters) between the beginning of a fund life and the fund having drawn down at least 35%/70% of committed capital. That means, the effect of a δ_j-unit change in covariate j is to multiply the failure time by $\exp\left(\delta_j\beta_j\right)$. For example, the coefficient in model 3 for the low rep fund dummy of 0.221 means that a low-reputation fund takes 25% $(\exp(0.221))$ longer to draw down 35% of committed capital than a high-reputation fund. This finding holds when controlling for different proxies of investment opportunities, such as the S&P 500 market/book ratio and a dummy equaling 1 during the heyday of the dot-com boom (1999Q1–2000Q2). Both proxies are highly negatively correlated with the log(time-to-failure). The opposite effect is found for an increase in the cost of capital. A 1 percentage point increase in the yield spread on corporate bonds (using Moody's BAA bond index, estimated quarterly in Mar., June, Sept., and Dec.) over the CRSP risk-free rate is associated with 11–15% more time to draw down 35% of capital (see columns 3 and 4). An economically important covariate is also a dummy variable equaling 1 for overlapping quarters with the investment period of the previous fund (retrieved from due diligence documents). This dummy is highly statistically significant, indicating that funds overlapping with the previous investment period take 20–25% longer to draw down the first 35% of capital. Since low-reputation funds tend to have less overlapping quarters, omitting the overlap dummy would impart a negative bias in the coefficient estimate for low-reputation funds. Most importantly, columns 1 and 2 show no evidence that low-reputation funds take longer to draw down 70% of committed capital. This means, low-reputation GPs make quicker investment decisions for the next 35% of committed capital and thus appear to be under buy pressure shortly before they start fundraising.

B. Valuation of Buyouts

The findings presented above call into question whether low-reputation funds pay a premium for buyouts that are made shortly before fundraising. To assess the valuation of their acquisitions, I construct excess purchase multiples as the dependent variable. I first merge my data set with valuation and sales data in S&P's Capital IQ. Second, I follow the procedure by Arcot et al. (2015) and compute for each buyout the ratio between enterprise value and latest available yearly sales for the target firm at the time of the buyout. Multiples are only constructed when deal value and yearly sales are nonmissing. To obtain the benchmarked variables, I subtract from each multiple the median purchase multiple by investment year, geography (North America, Europe, Asia/Pacific, Africa/Middle East, Latin America, and the Caribbean), industry (10 GICS industry groups), and public status (public or private), of all LBO deals from Capital IQ (with a value larger than $1 million) at the investment quarter of each buyout in my sample.

To account for the possibility that only specific types of deals can be matched with Capital IQ and may have more comprehensive valuation data, I perform Heckman (1979) regressions to correct for sample selection bias. That means I model the probability of a matched deal with Capital IQ of those that have available valuation and yearly sales information, generate Heckman's lambdas, and include them in the second stage to correct for sample selection.

I look at prior literature for guidance on identifying the first-stage equation (e.g., Strömberg (2008), Arcot et al. (2015)), and use variables that capture the determinants of a match. In particular, I include dummy variables indicating whether the acquisition was private-to-private, syndicated and located in the USA or Europe. For example, Strömberg (2008) reports that public-to-private transactions are underrepresented in Capital IQ compared with other studies using LBO data. While the first-stage variables are also used in the second-stage regression, I want to ensure that my Heckman models are well-specified, that is, beyond the identification via the nonlinear functional form (see Wooldridge (2010)). Consequently, I include as an exclusion restriction in my model dummy variables in the selection equation equaling 1 for deals between 2000 and 2004, 2005, and 2010, and 0 for the period 1996–1999. As pointed out by Strömberg (2008), the coverage in Capital IQ improves over time and is fairly complete after the late 1990s. In the first-stage regression, dummy variables for public-to-private, syndicated, and 2005–2010 period deals all are statistically significant and come in with the expected sign.

Table 13 reports the results. As shown in Panel B, the loading on Heckman's lambda is significant, which indicates that a selection bias is present.

The results in column 1in Panel B of Table 13 show no evidence that low-reputation funds are associated with higher excess purchase multiples. While the same holds true when I add a dummy variable for deals made within 1 year before fundraising (column 2), I find a positive relation of these later deals with excess purchase multiples. To tease out this effect more clearly, I introduce an interaction term between the low-reputation fund indicator and the dummy for whether the acquisition was made within 1 year prior to fundraising in column 3. The main effect of low-reputation funds conditional on acquisitions made more than a year before fundraising is statistically insignificant. That being said, the interaction term (≤YR_BEFORE_FRE × LOW-REP) indicates that deals undertaken by low-reputation funds are associated with a higher excess purchase multiple of approximately 35 percentage points in the year prior to fundraising. This result still holds when controlling for secondary deals which are positively correlated with excess multiples (in line with Arcot et al. (2015)). The premium for investments made shortly before fundraising seems to explain approximately the same drop of 35 percentage points in post-fundraising realized deal performance (see Table 5). The combined interpretation is that deals undertaken by low-reputation funds just before fundraising perform worse since they stem from buyers under pressure who pay more for their acquisitions.

The result holds also when controlling for unsuccessful prior exits (Supplementary Table A12) to alleviate concerns that GPs may be selecting less promising investments after previous successful deal exits (Barrot (2017)).

V. Conclusion

The common finding of previous studies is that fundraising for a new fund coincides with times of high current interim valuations, especially when costs of manipulation appear low. This finding is open to two different interpretations. One is that GPs may advertise strong current fund performance by manipulating true

TABLE 13

Valuation of Deals

Table 13 displays the results from a Heckman (1979) two-step estimation of excess purchase multiples in a deal acquisition on independent variables. The first step is a probit model for the selection of sample deals that can be matched with CapitalIQ and have information on enterprise value and LTM sales (Panel A). Excess purchase multiple is the difference between the target's purchase multiple (enterprise value/LTM sales) and a valuation benchmark constructed as follows. For every investment year, geography, industry, and public status (public or private), I compute the median purchase multiple for all merger transactions with value larger than \$1 million. US_DUMMY and EU_DUMMY are indicator variables that take the value of 1 for deals in Europe or the USA, and 0 otherwise. SECONDARY is equal to 1 if the seller in a transaction is a PE fund, and 0 otherwise. All other independent variables are as defined in Tables 14 and 10. Standard errors are in parentheses, clustered at the fund level. *, **, and *** represent 2-tailed significance at the 10%, 5%, and 1% levels, respectively.

Panel A. First Step Heckman Sample Selection Regression

$$Pr(\text{MATCH}_i = 1|\mathbf{x}_i) = \Phi(-1.631^{***} + 1.405^{***} \times \text{PRIVATE\_TO\_PRIVATE}_i + 0.642^{***} \times \text{SYNDICATED}_i$$
$$-0.131 \times \text{US}_i + 0.119 \times \text{EU}_i - 0.153 \times \text{2000\_2004}_i + 0.206^{**} \times \text{2005\_2010}_i)$$

Diagnostics LR test: all coefficients = 0, χ^2-stat.: 341.55*** Pseudo-R^2: 0.1864 N: 2484

Panel B. Second Step Heckman Regression

	1	2	3	4
LOW-REP	0.156	0.120	−0.281	−0.300
	(0.205)	(0.203)	(0.243)	(0.242)
OVERLAP	0.292	0.286	0.218	0.211
	(0.190)	(0.188)	(0.187)	(0.186)
ln(FUND_SIZE)	0.111*	0.105*	0.113*	0.105*
	(0.064)	(0.063)	(0.062)	(0.062)
PRIVATE_TO_PRIVATE	−0.174	−0.111	−0.035	0.039
	(0.417)	(0.413)	(0.408)	(0.407)
US_DUMMY	−0.207	−0.178	−0.233	−0.253
	(0.236)	(0.233)	(0.231)	(0.230)
EU_DUMMY	0.141	0.184	0.167	0.089
	(0.290)	(0.287)	(0.284)	(0.284)
≤YR_BEFORE_FRE		0.181***	0.195	0.149
		(0.064)	(0.210)	(0.210)
≤YR_BEFORE_FRE × LOW-REP			0.317***	0.354***
			(0.109)	(0.114)
SECONDARY				0.864**
				(0.402)
HECKMAN'S_LAMBDA	0.109*	0.048**	0.046**	0.060**
	(0.063)	(0.022)	(0.021)	(0.028)
No. of obs.	2,484	2,484	2,484	2,484
Selected obs.	338	338	338	338
Nonselected obs.	2,146	2,146	2,146	2,146
Wald χ^2-stat	36.344	45.200	54.827	60.195
Wald p-value	0.197	0.048	0.007	0.003

estimates of current asset values, as suggested by prior work. Alternatively, GPs may raise funds around true estimates of high current NAVs but undertake bad deals shortly before fundraising with increased pressure to deploy dry powder.

Instead of focusing on aggregated valuations on the fund level, this paper makes use of proprietary data on the portfolio company level of U.S. buyout funds from a major institutional investor to show that fund performance peaks are a result of a cohort effect. That is, current fund peaks stem from strong early deals increasing in value well ahead of fundraising and weak later investments declining in value primarily after fundraising. My overall results show no evidence of inflated valuations on the deal level.

Consistent with the forced-buyer hypothesis as opposed to strategic performance manipulation, GPs appear to take more time to deploy dry powder at the beginning of their fund life and are pressured to invest unspent capital before fundraising. I find that the drop in performance after fundraising of late investments is of similar magnitude as the paid premium compared to cohort buyouts.

The pattern found in my analysis mirrors results on the fund level found in previous studies. At first glance, these results support the hypothesis that GPs strategically manipulate performance. However, the analysis at the deal-level paints a different picture. This paper's contribution is to provide the first analysis of quarterly reported valuations at the deal level in buyout funds that provides evidence which speaks against the conclusion that GPs are systematically manipulating NAVs. Simply stated, the interim fund performance peak is more consistent with a cohort effect than manipulation of NAVs.

It is worth keeping in mind that my sample represents less than half the funds in the industry and is characterized by the most sophisticated LPs. There is certainly a possibility that NAV manipulation occurs outside of these funds. However, the fact that the sample exhibits the exact same peak performance pattern before fundraising as the prior literature lends significant support to the generalizability of the results.

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109022000990.

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