Do Private Equity Funds Game Returns?☆

Gregory W. Brown*, Oleg R. Gredil*, Steven N. Kaplan**

Abstract

By their nature, private equity funds hold assets that are hard to value. This uncertainty in asset valuation gives rise to the potential for fund managers to manipulate reported net asset values (NAVs). Managers may have an incentive to game valuations in the short-run if these are used by investors to make decisions about commitments to subsequent funds managed by the same firm. Using a large dataset of buyout and venture funds, we test for the presence of reported NAV manipulation. We find evidence that some managers boost reported NAVs during times that fundraising activity is likely to occur. However, those managers are unlikely to raise a next fund, suggesting that investors see through the manipulation. In contrast, we find that top-performing funds under-report returns. This conservatism is consistent with these firms insuring against future bad luck that could make them appear as though they are NAV manipulators. Our results are robust to a variety of specifications and alternative explanations.

Keywords: Private Equity, Venture Capital, Mutual Funds, Institutional Investors
JEL Classification: G23, G24, G30

☆First version: February 18, 2013; This version: February 28, 2015

*University of North Carolina at Chapel Hill, Kenan-Flagler Business School, gregwbrown@unc.edu, (919) 962-9250 and oleg_gredil@unc.edu

**University of Chicago, Booth School of Business. skaplan@uchicago.edu, (773) 702-4513 and NBER

Helpful comments and suggestions provided by James Bachman, Diego Garcia, Tim Jenkinson, Bob Harris, James Kocis, Anna Kovner, Ludovic Phalippou, Kelvin Law, Minjie Zhang and seminar participants at the 2012 Global Private Investing Conference, the 2013 Meeting of the European Finance Association, Berkeley-Haas Fraud and Misconduct Conference, the 2014 Meeting of the American Finance Association, the 2014 Meeting of Financial Intermediation Research Society, and the 2014 Vanderbilt Private Equity and Venture Capital Conference. We thank Burgiss and especially Brandon Renkart and Wendy Hu for providing research support. This research has benefited from the support of the Private Equity Research Consortium (PERC) and the UAI Foundation. Kaplan has consulted to buyout and venture fund general partners and limited partners.

Do Private Equity Funds Game Returns?

Abstract

By their nature, private equity funds hold assets that are hard to value. This uncertainty in asset valuation gives rise to the potential for fund managers to manipulate reported net asset values (NAVs). Managers may have an incentive to game valuations in the short-run if these are used by investors to make decisions about commitments to subsequent funds managed by the same firm. Using a large dataset of buyout and venture funds, we test for the presence of reported NAV manipulation. We find evidence that some managers boost reported NAVs during times that fundraising activity is likely to occur. However, those managers are unlikely to raise a next fund, suggesting that investors see through the manipulation. In contrast, we find that top-performing funds under-report returns. This conservatism is consistent with these firms insuring against future bad luck that could make them appear as though they are NAV manipulators. Our results are robust to a variety of specifications and alternative explanations.

I. Introduction

Recently it has come to light that the SEC is investigating the reported returns of private equity funds.[1] The SEC inquiries center on the potential for private equity partners to overstate portfolio net asset values (NAVs) in an attempt to attract investors to future funds. Because there is no liquid market for most assets held by private equity funds, investors must rely on estimates of NAVs that are provided by general partners (GPs). Increasingly, NAVs are determined by outside valuation consultants and auditors, but the process is nonetheless subjective and potentially manipulated by data produced by the portfolio companies themselves (that are directly owned by the funds). Of course, the odds of obtaining benefits from manipulating NAVs depend on a variety of assumptions including the reliance of investors on past NAVs to make investment decisions as well as the inability of investors to detect or punish manipulators.

In this paper we examine the empirical evidence on NAV manipulation using a large dataset of private equity funds – both buyout and venture capital funds – obtained from Burgiss, a data service provider catering to private capital investors. Our findings suggest that little manipulation of NAVs goes unnoticed by institutional investors. Some GPs of poorly performing funds appear to game returns in an effort to raise a follow-on fund. However, these attempts are unsuccessful in so far as those firms are unable to raise

[1] For example, see "Private Equity Industry Attracts S.E.C. Scrutiny" by Peter Lattman, New York Times, February 12, 2012.

a follow-on fund on average. At the same time, we find evidence of conservatism among the GPs of the best performing funds. This is consistent with a concern on the part of GPs about being wrongly labeled a manipulator. This also suggests that the equilibrium behavior of GPs in reporting NAVs is influenced by the potential for gaming reported values.

In our analysis, we focus primarily on measures of risk-adjusted abnormal returns derived from the public market equivalent (PME) method of Kaplan and Schoar (2005). It is important to take risk-adjustments seriously in our analysis because public market returns are positively correlated with subsequent private equity fund formation. Insufficient risk-adjustment could falsely identify firms as NAV manipulators by making return estimates early in a fund's life look abnormally good (during a bull market) and returns later in a funds life look abnormally low (during a bear market). We also address risk-related concerns by conducting simulations where we create placebo samples of funds using portfolios of public equities with similar properties. Experiments with these placebo private equity portfolios make us confident that our analyses are unlikely to be tainted by unmodeled risk factors that affect public and private company values.

We examine both buyout and venture funds and find generally consistent results across both types of funds. Both the average buyout and average venture fund in our sample experience a positive lifetime abnormal return. Abnormal returns are somewhat greater during the early years of the average fund's life but remain positive in later years. The slowing in performance late in life is more pronounced for venture funds than buyout funds.

To see if fundraising for a subsequent fund is related to abnormal returns, we align returns around the first capital call for a firm's next fund. If there is no next fund, we assume an event date occurs near the end of the fund's life (i.e., this is when a firm would have to be making a final push to raise a new fund). We observe a decline in performance around these events for both the average buyout and venture fund so we examine the source of the change in more detail by separating funds into three groups that raise a next fund early, late, or not at all. For buyout funds, the decline in performance is entirely due to funds that are unable to raise a follow-on fund. For venture funds, those with no next fund also exhibit a reversal in returns late in life. Venture funds that raise a next fund late experience a leveling off of returns after fundraising. However, their returns before fundraising are better than for the average fund, so lifetime returns match those of funds that raise a next fund early in their life. We further condition our analysis on public market returns as a proxy for the fundraising environment. We find that these effects are typically more pronounced when the fundraising environment is difficult (i.e., market returns are low). Overall, this evidence is consistent with NAV manipulation primarily by poor performing funds that are making a final effort to raise a fund. Such funds make up roughly 15 percent of all funds in our sample.

We also examine how NAV changes depend on the performance of peer funds (i.e., those of similar vintage and strategy). We find strong evidence of "peer-chasing" where top performing funds report lower returns and bottom performing funds report higher returns. The former is consistent with conservatism among top funds that is widely discussed among practitioners and documented by Harris et al. (2013). Our results are robust to a variety of methods and alternative tests.

The paper proceeds as follows: Section II provides a more detailed discussion of the game managers and investors might play. Section III describes the data used in the analysis. Section IV provides our main result. Sections V and VI report additional tests and more carefully address the endogeneity of fundraising and performance. Section VII concludes.

II. Reporting NAVs with incomplete and asymmetric information

In this section we describe the relationship between general partners (GPs) in a private investment firm and the outside investors (LPs) that make investments in the funds operated by the GPs.

LPs seeking to make investments in private funds face the problem of deciding which GPs to invest with. Very little, if anything, is known about what specific investments will be undertaken by the GPs after capital has been committed to a fund. Consequently, investors are forced to rely largely on reported values of previous funds and soft information about the value-relevant qualities of GPs (e.g., access to deal flow, reputation in the industry, etc.) when selecting private equity managers.

Given that valuations from existing and past funds represent most of the hard information available to investors, it is not surprising that anecdotal evidence suggests that LPs make decisions under a prior belief of persistent performance of general partners (GPs). Academic evidence suggests that this is likely to be a valid part of the selection process. For example, Kaplan and Schoar (2005) document that absolute and relative performance of early funds predicts that of subsequent funds managed by the same private equity firm.[2] In a recent study, Harris, Jenkinson, Kaplan and Stucke (2013) find additional evidence of persistence for venture funds established after 2000 (about when sample coverage of many prior empirical studies stops). This study documents a decline in (but not disappearance of) persistence for buyout funds with inception dates after 2000. Harris et al. (2013) also find that positive performance predictability decays in fund sequence – that is, the second previous fund is less informative about the current fund performance. Because a typical firm seeks to raise a new fund every few years, these findings suggest that investors using current fund performance to evaluate managers must rely largely on net asset values (NAVs) reported by the fundraising GPs. This is particularly relevant for buyout funds since the performance of already resolved funds (e.g., third and fourth back in a sequence) appears to contain little predictive power on average.

The GPs make investments in companies that are (or will be) privately held, and thus market prices are not observable for most of the fund's assets. GPs have a potentially difficult problem of obtaining a valuation for each portfolio company at the end of each reporting period (normally quarterly). The GPs observe contemporaneous and lagged company characteristics for their portfolio companies (e.g., sales, profits, etc.) as well as public market characteristics for other, sometimes quite similar, companies, industries, and markets. Public market information is important because it is used as a basis for comparable company valuation analyses, the value for exiting existing investments (e.g., through IPOs and sales), as well an indication of the value of future investments.

In addition, GPs may observe, but only with a lag, the performance of their competition – that is, the performance of other funds of similar vintage and investment strategy.[3] Given this information set, GPs have an incentive to assign valuations in a way that maximizes the value of the fund management firm. This problem includes maximizing not only the return from the current fund, but also possible future funds. Metric and Yasuda (2010) and Chung, Sensoy, Stern, and Weisbach (2011) show that the expected income from subsequent funds comprises a larger fraction of a GP's lifetime income than the income from the GP's current fund. Chung et al. propose and find empirical support for a rational learning model where follow-on funds represent an indirect pay for performance to GPs. This channel is stronger when management

[2] The persistence appears notably stronger for venture capital funds. Sorensen (2007), Hochberg, Ljungqvist, and Lu (2010), Cai, Sevilir and Tian (2012), Ewens and Rhodes-Kropf (2013) suggest a micro-foundation for such persistence at the firm-level based on portfolio entrepreneur rational self-selection, institutional network effects as well as individual skill.

[3] Discussions in Phalippou (2009) and Harris, Jenkinson and Stucke (2012) as well as the proliferation of private equity benchmarking data vendors and consultants suggest that most GPs are indeed very well aware where they stand versus the peers.

skills are (perceived) to be more scalable, so that future commitment size, and expected compensation, can increase faster.[4] Taken together, the extant evidence suggests that GPs have an incentive to overstate recent fund performance in an attempt to assist in fundraising.[5] We call this *Fund Timing*.

Fund timing, however, could be a two-edged sword. A practice of aggressive portfolio marks by GPs could become an equilibrium outcome and not provide a valuable signal to potential investors. Given the informational asymmetry, it might also be difficult for GPs to credibly convey that their portfolio marks are more conservative than those of their peers. A further complication derives from the fact that many LPs are sophisticated institutional investors and often other investors view commitments to a new fund by prominent LPs as a quality certification. If sophisticated LPs can determine that GPs manipulate reported returns this could jeopardize future fundraisings. Thus, GPs likely face a set of trade-offs in deciding whether or not to game reported NAVs.

Given the environment the LPs and GPs operate in, it is unclear to what extent modest NAV timing in the past may tarnish a GP's reputation enough to outweigh a marginal increase in the odds (and size) of a commitment for an upcoming fund. For example, for a GP with a very poorly performing fund, there is not much value in reputation if the firm ceases to exist after an unsuccessful attempt at raising a new fund. Second, there are idiosyncratic shocks affecting the path of a given fund's asset values. This can make it difficult to identify with confidence a "nefarious" change in reported performance due to manipulation even with additional information such as details about fund holdings. Thus, a GP would need to weigh the extent of NAV biases against the probability of being discovered. Third, there exists substantial heterogeneity in LPs performance (Lerner, Schoar and Wongsunwai, 2007) and it is unclear what equilibrium the most sophisticated LPs would prefer given the apparent difficult access to some top funds. Specifically, LPs can punish *ex post* for what looks like bias by cutting back on participation in a GP's subsequent funds. At the same time, there is a "bad luck" possibility that cannot always be credibly conveyed to LPs. This suggests that top performing funds would have an incentive to be conservative with their portfolio valuation in order to reduce the odds of being mistakenly classified as manipulators. Such long-term reputational stains would

[4] Arguably, this is more relevant for buyout firms that are more commonly obligated to deliver a certain net-of-fee return to LPs before being entitled to performance-based compensation. See Metric and Yasuda (2010).

[5] There are other ways that funds might try to fool investors such as with hidden fees, by cherry-picking a peer comparison set, etc. See Phalippou (2009) for a related discussion. We focus our analysis on potential NAV manipulation. We argue that if some investors can spot asset misvaluations, finding such "other traps" should not be harder.

be less of a concern for GPs who are less likely to raise a new fund because of weaker performance of their current fund.

Given this motivation, we propose the following testable hypotheses:

H1 : Optimistic portfolio valuations reduce the probability of raising a successor fund.

H2 : GPs of poorly performing funds are more likely to manipulate NAVs upward.

H3 : Top performing funds may understate NAVs to build in some insurance against bad idiosyncratic shocks that could be misconstrued by investors as manipulations (i.e., overstatements) of NAVs.

Our work relates to other existing work. Conceptually, our paper is related to the large literature on information asymmetries in financial intermediation. Perhaps the setting most analogous to ours is described in Chemmanur and Fulghieri (1994) where investment banks are hired to underwrite issues by firms of unknown quality and must rely on their reputations to place issues. On the empirical front, Cumming and Walz (2010) find systematic biases in managers' reporting of fund performance and that the effects are related to country legal and accounting standards. Jenkinson, et al. (2013) examine a similar issue and find evidence of wide-spread NAV manipulation in a set of CALPERS funds. Barber and Yasuda (2014) also examine reported fund performance around fundraising and find that fund performance relative to vintage year cohort funds affects the likelihood of successfully raising a follow-on fund. They also find evidence of NAV markdowns following the fundraising window which is consistent with our results for firms that face elevated survival risk. Meanwhile, Chakraborty and Ewens (2014) find that, in a short-term, even highly reputable GPs tend to overstate NAVs during fundraising periods since the write-off rate increases after the successor fund launches.

More broadly, similar tensions for under-performing and over-performing managers have been detected in the context of mutual funds competing for investors' assets. Starting with Brown, Harlow, and Starks (1996), researchers have found that mutual funds with relatively bad performance tend to increase portfolio risk relative to other funds towards year-end. Private equity funds have very limited ability to change the riskiness of their portfolios through asset re-allocation.[6] Instead, a GP's discretion is related to the reporting

[6] Unlike mutual funds however, private equity GPs have discretion over company operational and investment policies subject to constraints from portfolio company creditors and equity co-investors.

of values of assets and the corresponding reputational risk. Yet, the underlying incentives induced by the compensation scheme are analogous: the adverse long-term implications are important only so long as that "long-term" is likely to exist.

III. Data

Private equity fund cash flows and NAVs for this study come from Burgiss. The dataset is sourced exclusively from LPs and includes the complete transactional and valuation history for their primary fund investments. Burgiss rescales flows to be representative of the full fund. The Burgiss data include all funds and cash flows from the LPs that provide data. Data are provided by 220 investment programs and represent over $1 trillion in committed capital. The Burgiss LP base consists of approximately 60% pension funds (a mix of public and corporate) and 20% endowments or foundations with the remainder an assortment of other institutional investors such as funds-of-funds and sovereign wealth funds. Once aggregated, the data are supplemented with classifications and scaled to be representative of the full fund. The resulting dataset maintains confidentiality by removing all names and identifications.

The Burgiss dataset has been utilized in other academic studies. Harris, Jenkinson, and Kaplan (2013) compare several private equity datasets and conclude that the Burgiss dataset is representative of the buyout and venture funds investable universe. A major advantage of the Burgiss dataset is a high degree of accuracy resulting from cross-checks among investors in the same funds and a direct recording of the fund accounting information disseminated to LPs. This feature is very important for our research question since it likely insures against breaks in voluntary reporting by GPs or selection biases in reporting resulting from Freedom of Information (FOIA) requests to certain LPs. We limit our sample to U.S.-dollar denominated buyout (venture) funds with more than 25(10) million in capital commitments. Our total sample includes 997 buyout funds and 1,074 venture fund. 641 (910) focus on North America. 488 (323) remain active (i.e., are unresolved) as of March 2012. For each fund we have: (1) industry sector according to Global Industry Classification Standard; (2) amount of capital committed; (3) strategy description; (4) affiliation within firm; (5) dated amounts of cash in-flows and out-flows as well as reported Net-Asset Values (NAVs).

Table 1 reports summary statistics for the funds in our sample by fund type (i.e., buyout or venture). Results in Panel A indicate the well-known heterogeneity and positive skew in performance among both already-resolved and still-active funds as well as the generally larger commitment amounts for buyout funds. We define a fund as no longer active or "resolved" once it has an NAV less than 2% of the fund's initial commitment amount. The median buyout (venture) fund makes a distribution or capital call in 32% (25%) of active quarters. Fewer than half of funds are resolved within 12 years (as often stipulated per fund terms) with a quarter of funds remaining active after 14 years.[7]

The dataset allows us to track each fund's affiliation with an investment firm so that we are able to generate fund sequences.[8] Panel A of Table 1 also shows that for firms with at least two funds the (inter-quartile) time between a particular fund's inception and a follow-on fund generally varies from two to five years. Panel B of Table 1 presents further detail on successive fundraising patterns by breaking out each fund type into groups based on the number of years between a fund's inception and the next fund offering by the same firm as measured by the date of first capital call. In addition, we tabulate fund counts (i) by firm experience as measured by the number of previous funds raised and (ii) fundraising conditions as measured by public equity market performance through the third year of fund operations. If public market total returns in the three years around a fund's inception were in the bottom (top) tercile in comparison to other funds of the same type, we classify the fund as starting operations in a low (high) market environment. Overall, Panel B of Table 1 suggests that bigger, better performing funds by more experienced firms are more likely to have a follow-on fund, yet the relation is not strictly monotonic.

IV. Primary Results

We start our analysis by examining the patterns in fund returns since inception and around fundraising events. Most generally, we try to detect the presence of a reporting bias in NAVs. Before 2009, GPs had a large amount of discretion in valuing their portfolios. Many chose to value their portfolio assets at cost until there was an explicit valuation change. Since 2009, topic 820 of the Financial Accounting Standards Board (FASB) requires private equity firms to value their assets at fair value every quarter, rather than permitting them to value the assets at cost until an explicit valuation change. This has likely had the practical effect of

[7] See also Metrick and Yasuda (2010).

[8] It is possible, however, that we do not have information for all the funds for a given firm although from Burgiss we know if there are gaps in the sequence of funds of a given PE firm in our data. We will drop funds for which the date of the follow-on launch is unknown and will treat those potentially missing funds as nonexistent in our analysis which is likely to generate a bias against finding significant relationships.

making estimated unrealized values closer to true market values than in the past. We directly examine this question in a section VI.B at the end of the paper.

For Level 3 assets (i.e., those whose fair value cannot be determined using observable measures) the push toward fair-value accounting does not necessarily constrain valuations because a GP may still influence the valuation process. From conversations with Burgiss, we know that about 80% of fund holdings are reported as Level 3 assets.[9]

A bias could enter NAVs in several ways. First, valuing companies using comparable firms requires judgment as to which set of firms constitutes the appropriate group of comparables and which metrics are the most suitable for determining value. Alternatively, valuing companies using cash flow models requires a set of subjective modeling assumptions about growth rates, discount rates, etc. Finally, a bias in NAVs can derive from the timing of revaluation versus historical cost (or write-downs of failed investments), particularly for venture funds. Specifically, fund managers typically have some flexibility on when to switch valuation methods.

While many funds use external valuation advisors and have audited procedures, the valuation process remains subjective and therefore allows for the possibility of negotiation around the valuation of portfolio companies. This type of back-and-forth is also common in other areas of corporate and security evaluation such as bond ratings. In the case of private equity funds, not only are external advisors paid by GPs, but their assessments could be prone to selective disclosure of value-relevant information by GPs.

A. Return timing

If private equity firms inflate existing fund NAVs to boost to-date performance during new fundraisings, existing fund performance should subsequently deteriorate. The unwinding of such biases need not be immediate, but would necessarily occur with the realization of investments. We start our analysis by averaging quarterly performance as a function of time since inception across many funds. This should help reveal return patterns by averaging out idiosyncratic returns. In essence, we want to examine the variation in mean returns through time.

Because internal rate of return (IRR) is a time-and-money-weighted mean, quarterly changes in the

[9] Of those not categorized as Level 3, the majority are categorized as Level 1 and represent portfolio companies that have already been sold to the public partially or that have never been delisted.

IRR are noisy measures of the performance in that period.[10] IRRs depend on previous period returns and the schedule of fund cash flows. The commonly-used money-multiple (MM or TVPI) is the ratio of all fund distributions and remaining NAV (i.e. the "Total Value") to total capital calls (the "Paid-in Capital"). Quarterly changes in MMs do not depend on the previous period returns (as do IRRs). However, as funds return capital to investors, MMs reflect a diminishing weight of NAV and the current period distributions that carry information about the current period performance. If firms make substantial distributions from previous funds before raising a new fund (as they often do), then changes in a previous fund's MM may get smaller in magnitude subsequently even if the relative performance has increased. The same critique would apply to changes in a benchmark-adjusted multiple, such as the Public Market Equivalent (PME) index of Kaplan and Schoar (2005). Thus, analyzing returns from reported NAVs is challenging for a number of reasons.

In Appendix A, we show that a change in MM is a special case of a change in the to-date PME, when gross benchmark returns, $R_{m,t}$, are set equal to 1 for all periods. Consequently, most of our analysis relies on a change in PME measure defined as

$$\Delta PME_t = (R_t^{NAV} - R_t^{mkt}) \frac{NAV_{t-1}}{fv_t(Calls)} \quad , \tag{1}$$

where R^{mkt} and R^{NAV} are, respectively, the public equity index gross return and the fund gross return.[11] The latter is computed using NAV changes adjusted for cash flows. We define $fv_t\,(Calls) = \sum_{i=0}^{t} Calls_i \prod_{\tau=i}^{t} R_{\tau+1}^{mkt}$ as the time t value of cumulative capital calls adjusted by cumulative market returns. $(R_t^{NAV} - R_t^{mkt})$ is the excess return of a fund's invested assets over period t. Equation (1) captures the importance of NAV changes in the performance numbers that investors get to observe. To compute abnormal performance based on NAVs

[10] In Appendix A we provide a simple example where the interpretation of IRR-to-date provides an incorrect assessment of actual fund to-date performance.

[11] We use the CRSP Value-Weighted return as a proxy for public market returns. However, the choice of benchmark is not an obvious one (see Phalippou, 2013) so we have also examined alternatives and our results are robust to the choice of benchmark. In addition, our subsequent analysis with placebo fund returns uses style- and size-matched public equity portfolio returns based on Fama-French research portfolios. Note that Kaplan-Schoar PME, as an estimator of the expectation of a product of cash flows and the discount factor, is inherently robust to risk-exposure misspecification. See Korteweg and Nagel (2013), Sørensen and Jagannathan (2013) for a theoretical exposition as well as Robinson and Sensoy (2011) for an empirical assessment of the sensitivity of cross-sectional mean *PME* to different beta/benchmark assumptions.

over a time interval (a,b) for a cross-section S of funds, we define the Weighted-PME (WPME) as

$$WPME_a^b = 1 + \sum_{t=a}^{t=b} \left[\sum_{i \in S} \Delta PME_{i,t} \Big/ \sum_{i \in S} \frac{NAV_{it-1}}{fv_{it}(Calls)} \right] \quad . \tag{2}$$

In a Monte-Carlo experiment described in detail in Appendix A, we find that (1) excess returns and, correspondingly, WPME changes yield sharper estimates of time trends in mean excess returns than do raw returns and money-multiples, and (2) misspecification of fund-level systematic risk is unlikely to confound inference about the path of cross-sectional mean returns.

Figure 1 presents the WPMEs across all buyout and venture funds in our sample since fund inception (Panel A) and +/-12 quarters around firms next fundraising events. We define the date of the next fundraising event as the quarter of the first capital call by the next fund by the same firm given at least 11 quarters since the current fund inception. In case there is no such follow-on fund in our dataset, the event quarter is the 13^{th} quarter preceding the fund resolution or its 10^{th} year anniversary if still unresolved.

Panel A of Figure 1 shows that average abnormal performance since inception for both buyout and venture funds increases fairly steadily for the first few years of fund life. Around quarters 15-20 average fund returns start to grow more slowly, though excess returns remain mostly positive. The slowing in return growth is slightly more pronounced for venture funds. Interestingly, the change in slope appears to occur near the typical time for follow-on fund launches.

Since funds launch a follow-on fund at different times in the existing fund's life, we next examine returns around the subsequent fundraising event. In particular, the 3-year window before a fund's first capital call is the time that a firm is most likely to be active in trying to secure commitments to a new fund. Panel B of Figure 1 plots cumulative abnormal performance starting 3 years before the next fundraising event. The plots show the same pattern suggested by Panel A. The cumulative average excess return for both buyout and venture funds in the 3 years following the fundraising event is less than in 3 years preceding the event. However, it is important to note that after fundraising (t>0 in Panel B) excess returns remain positive.

Jenkinson et al.(2013) and Barber and Yasuda (2013) conclude that such a flattening in the reported returns over the fund life-time may constitute NAV overstatements ahead of the new fund launches. Both studies find that different NAV-based return proxies correlate negatively with post-fundraising period indicators in panel regressions with fund fixed effects. These results suggest that post-fundraising excess returns are lower than pre-fundraising (consistent with our Figure 1) but do not necessarily indicate that they are negative.

We propose that the assumption of constant excess returns over a fund life is not an appropriate null hypothesis when examining the possibility of NAV manipulation. In fact, there are alternative explanations that are consistent with the flattening of excess returns post-fundraising which do not involve a bias (deliberate or not) in reported valuations. For example, if a part of the value-added by a GP involves finding underpriced assets then excess returns will decline as investments are made and then properly valued. GPs may then need time to facilitate an exit from the investment or simply add additional value, but at a lower rate, through "nurturing" portfolio companies.

In addition, the elevated write-off rates after fundraising that Barber and Yasuda (2013) and Chakraborty and Ewens (2014) document may also occur as a result of GPs learning about the ultimate prospects of specific investments. Intuitively, managers will be more likely to throw in the towel for any given investment later in a funds life.[12] A fund's performance may also deteriorate after a next fund is raised if GPs dedicate most of their efforts (and possibly better deals) to the new fund. While this potentially represents an agency cost born by LPs in the old fund, we do not consider this NAV manipulation.

Figure 1 is also consistent with anecdotal evidence about how LPs may evaluate performance of GPs. If LPs require a certain level of successful divestments from the current fund before committing new capital, GPs may have to exit some of their best investments early to credibly convey their ability. Such actions can be viewed as a cost of asymmetric information and uncertainty about NAVs that LPs are nonetheless willing to endure to better learn about the GPs skill. However, that sort of behavior is also distinct from reporting biased valuations. In addition, some investors could simply overreact to particularly strong (yet truthfully reported) returns over the last few quarters. Thus, a reversion to lower levels (that would occur irrespective of the new fund launch) may induce the aforementioned pattern. Finally, it is possible that broad market conditions relevant to buyout and venture fund returns (e.g., access to exits or new capital) determine the timing of fundraising. Much of our subsequent analysis seeks to differentiate among these explanations.

[12] Barber and Yasuda (2013) control for time since fund exception. Chakraborty and Ewens (2014) control for the company age and development stage fixed effects. These might not be sufficiently informative about the duration (and the depth) of the GPs involvement with a particular company (or the event that led to the distress). For example, Gredil (2014) documents that the cross-sectional interquartile range for the fraction of called capital exceeds 0.33 between the 2nd and 3rd year since funds' inception in both subsamples, buyout and venture.

A.1. Successful fundraisers versus unsuccessful

As a next step, we investigate WPMEs for subsets of buyout and venture funds. First, we categorize funds into groups, based on the time it takes to raise a next fund. We create three groups: The Early (Late) Next Fund group is defined as those funds that take less (more) than the median time to raise a new fund. The No Next Fund group is defined as those funds for which we do not observe a follow-on fund. Recall that we define a hypothetical fundraising event for the No Next Fund group as the thirteenth quarter before the funds resolution or its 10^{th} year anniversary if still unresolved.[13] We also sort funds into two groups, based on median 5-year rolling public markets returns as of the 13^{th} quarter of the funds life and call these High and Low Market funds.

Panel A of Figure 2 show the cumulative changes in excess returns for buyout and venture funds conditional on the time it takes to raise a follow-on fund. Unsurprisingly, funds with no next fund have much weaker performance than funds which are successful at fundraising. For both buyout and venture funds that are successful, the moderation is only apparent for those with a late next fund. Excess returns in early years are typically as good or better for those funds that take longer than average to raise a next fund though we show subsequently that this is partly related to market conditions.

Panel B of Figure 2 reveals the most interesting results. For both buyout and venture funds, the excess returns of funds that are unsuccessful at raising a next fund show clear patterns consistent with funds gaming returns. In both cases, excess returns increase in the few quarters during which a firm is likely to be making a final effort to raise a next fund only to reverse returns in the final years of the fund (as cash flows are realized).[14] We note that these represent not just lower excess returns, but in fact, negative excess returns. Thus, this evidence is suggestive of attempts at manipulation that are not successful since investors are not willing to commit to a next fund. In other words, the market for buyout and venture funds appears to look through attempts at gaming NAVs and determine the actual quality of a fund.[15]

In Figure 3, we further refine the analysis by considering the performance of different fund groups

[13] In the robustness section we examine an alternative definition of the event time for the No Next Fund group.

[14] Overstatements towards the end of the fund term do not rule out NAV manipulations earlier. However, conditional on failure to have raised a successor, the temptation to overstate increases as the time elapses and the disciplining effect of long-term reputation diminishes (as discussed in Section II).

[15] An alternative explanation would be that these GPs had been abstaining from fundraising (even though the current fund investment period expired) and then got very unlucky. However, such delays appear irrational given that GPs normally get five years to deploy new capital and how significant the continuation value is for GPs (e.g. see Chung et al., 2011).

during strong and weak market periods. Both buyout and venture funds have higher excess returns prior to fundraising when market returns are low (regardless of when the fund was raised) suggesting a higher bar for raising funds during a weak market. The evidence for No Next Funds shows that the degree of potential gaming also appears to be more pronounced during weak markets. Nonetheless, there is no evidence that the mean excess returns become negative after successful fundraising in either of the subsamples.[16]

A.2. Heterogeneity among successful fundraisers

To more closely examine the issue of performance reporting around fundraising, we consider future performance conditional on past performance using tercile transition probabilities (similar to Kaplan and Schoar (2005) and Phalippou (2010) but over a given fund life rather than across funds). Table 2 reports transition probabilities between performance terciles based on IRR-to-date within each fund peer group. Panel A shows results for buyout funds and Panel B shows results for venture funds. In both cases we examine only funds that have a follow-on fund. For example, the first row of each panel reports the probability of being in each final performance tercile conditional on being in the bottom tercile at the conclusion of fundraising. The last row of each panel reports the unconditional distribution of funds across final performance terciles, and the last column reports how many funds successfully raised a next fund in each tercile (at the conclusion of fundraising).

First, the last columns in each Panel show that a firm is about twice as likely to raise a follow-on fund when the current fund performance is in the top tercile. This result confirms the findings of Barber and Yasuda (2013) who show that performance-rank peaks around the follow-on fund launches. This finding is consistent with a gaming explanation as well as with investors requiring credible evidence of investment success through exits before committing new capital. However, for both buyout and venture funds, top and bottom performers during the fundraising period are most likely to remain in the same performance tercile. Thus, the interim performance rank is typically informative about the final performance rank for the current fund. In the appendix, we demonstrate that this result holds if PME is used in place of IRR (Table B.1) and appears to be even stronger with quartiles (Table B.2). Nonetheless, there are some funds that transition

[16] Admittedly, past market returns are not completely exogenous with regards to the choice when to fundraise. However, we compare Late Low fundraiser with Late High and Early Low with Early High (rather than Early versus Late) to mitigate this concern. It should be even less of an issue with the NoFund case where event time is about 5 year ahead of the sorting date.

between top and bottom terciles. For example, about 10% of top buyout and venture funds at life end were in the bottom tercile during fundraising. This suggests that investors put weight on other indicators of GP quality besides interim returns. For both buyout and venture funds, there were slightly more transitions from top to bottom terciles than from bottom to top. Likewise, for both buyout and venture funds more middle-tercile funds at fundraising transition to the bottom final tercile than to the top final tercile. These results suggest heterogeneity in NAV reporting biases among successful fundraisers. Nonetheless, it is not clear if it pays to overstate NAVs. In the next sections, we investigate if overstated NAVs actually increase the odds of a successful fundraising.

A.3. A signaling model of fundraising

While the evidence presented previously is quite suggestive, we have yet to provide any statistical tests of our hypotheses. As a first pass, we estimate a linear probability model where the dependent variable equals one if we observe a follow-on fund and zero otherwise. For now, we limit our sample to the funds that were resolved or operated for at least 10 years. As before, the event time is defined by the quarter in which the successful fundraising took place or the 13^{th} quarter before the resolution (or the 10^{th}anniversary if unresolved by then). We consider the following explanatory variables, all defined as categorical variables to simplify the interpretations:

- *PME drop (after)* – equals 1 if the funds PME at resolution is lower than at the event time and zero otherwise;
- *PME run-up (before)* – equals 1 if the funds PME 1 year before the event time is lower than at the event time and zero otherwise;
- *Large Distribution (before)* – equals 1 if the sum of distributions over the year preceding the event time exceeds 20% of NAV and zero otherwise;
- *Top tercile-to-date* – equals 1 if the fund is in the top (best) IRR-tercile across vintage and strategy peers at the event time and zero otherwise, and
- *Bottom tercile-to-date* – equals 1 if the fund is in the bottom (worst) IRR-tercile at the event time and zero otherwise.

Table 3 reports the results of this estimation, separately for buyout (Panel A) and venture (Panel B) subsamples. All specifications include the interaction of the fund vintage year and industry fixed effects to absorb the variation in investor demand for certain types of funds over time. The standard errors are clustered by the event year to account for possibly correlated shocks affecting the fund returns. In specifications (3) and (4), we also include the level of PME at the event date as well a dummy indicating whether the market return was positive in the year prior to the event.

From specifications (1) through (3), we see that negative post-event abnormal returns as well as lower return just before the event correspond to a lower probability of successful fundraising. The magnitudes of the effects exceed those of not being in the top performance tercile. As discussed above, *PME drop (after)* is likely to be an unambiguous indication of overly optimistic NAVs as of the event time. It therefore appears that investors scrutinize fund portfolios and consider aggressive valuations in the current fund a bad signal about a GPs ability. The negative coefficient on *PME run-up (before)* suggests that investors also appear to react negatively to above market return reports over the few quarters before the event time which is not consistent with results in Jenkinson et al. (2013) regarding the run-up in (raw) NAVs during the quarters immediately before successful closings of new funds. One possible explanation for this difference is that most of new fund closings occur following periods of positive market returns and Jenkinson et al. may not adjust for fund risk sufficiently. The authors effectively use a beta of about 0.3 to adjust for the growth in NAVs. Specification (4) examines another possible explanation. The significant positive coefficients on *Large Distribution (before)* interacted with *PME run-up (before)* suggests that investors appear to appreciate positive excess returns when accompanied by large distributions from the fund. As we discuss in the next section, this may drive much of the performance rank affect around the fundraising quarters documented by Barber and Yasuda (2013). Overall, these results are strongly consistent with hypothesis *H1* that NAV-overstatement per se reduces the odds for fundraising success.

In light of our results so far, it seems unlikely that overstating interim returns has been a winning strategy for GPs on average.[17] Although the current fund performance clearly has bearing on the odds of future fundraising, overoptimistic NAVs (nefarious or not) are generally associated with lower probability of raising a follow-on fund. Therefore, GPs seem to have an incentive to be truthful or even conservative with

[17] We arrive at qualitatively similar results as in Table 3 with alternative definitions for the event times (e.g. as in Figure B.3).

their unrealized investment valuations. These results are interesting (and distinct from the findings of related papers) because they show that while top performing funds are more likely to raise a follow-on fund, inflating NAVs seems to reduce the odds of success. Next, we try to more carefully describe the sources of cross-sectional variation in NAV biases.

B. Peer-Chasing

There is little that a private equity firm can do about overall flows of capital to the asset class and systematic risk exposures of investments. Consequently, the extent and nature of any strategic behavior regarding NAV reporting is likely to also depend on to-date performance of a fund as compared to its peer funds. This is reflected in the standard industry practice of comparing performance across funds of similar vintage year. Some large LPs even apply tougher evaluation and compliance procedures for prospective fund commitments when the firm's previous fund performance falls short of the comparable vintage year peer group.[18] It is therefore natural to assume that GPs track their funds performance as compared to that of the peers. Likewise, GPs likely have an incentive to incorporate this knowledge in the valuation process to some extent. Discussions in Phalippou (2009), Stucke (2011) and Harris and Stucke (2012) suggest that GPs might be "managing" the peer set and benchmark selection also supports this conjecture.

If the resulting behavior results in mimicking peer fund performance, or what we call peer-chasing, this could cause NAV manipulation to spread across firms as a strategic response to the informational asymmetry between GPs and LPs. For example, underperforming funds have an incentive to report upward-biased NAVs and may have limited tools to credibly convey that their NAVs are more conservative than those of their peers. At the same time, top performing funds may want to insure against bad luck that could tarnish the reputation in the long-term.

Empirically, peer-chasing would appear as mean-reversion in reported performance. As a first pass at identifying peer-chasing, we compare next period reported returns conditional on cumulative to-date performance. Specifically, for each fund-quarter we compute the 4-quarter ahead change in PME-to-date. We then rank these changes by funds of similar vintage year (+/- one year) and plot the distribution of the ranks by cumulative performance tercile (as measured by IRR-to-date) for different fund-life periods.

[18] For example, CalPERS has such a policy.

Specifically, we look at 8-17 quarters since inception (denoted as ˜3yrs), 18-27 quarters since inception (˜5yrs), and 28 or more quarters (>7yrs).

Given the probable relation between fund returns on public market returns, we need to be careful about the null hypothesis for peer-chasing tests. It could be that mean-reversion is indeed present in the unobservable true return-generating process if one weights returns by fund-quarter population. To address this concern, we also construct placebo return series for each fund in our dataset as a sum of style-matched public equity portfolio returns and a random innovation. [19] Thus, we assume that, although unknown, the return generating process is the same for private and public equities, up to a constant and an unpredictable error. Hence, if private equity funds were to simply mark their holdings to some fixed set of public equities, fund transitions across performance percentiles on short horizons must be similar to those of the public equities being referenced.

Results for these tests are reported in Figure 4 for buyout funds and Figure 5 for venture funds. In each figure, actual returns are reported in Panel A while placebo returns are in Panel B. In each panel, top to-date performers are shown in the top graph and bottom to-date performers are shown in the bottom graph. The results suggest strong peer-chasing patterns for both buyout and venture funds that are not present in public placebo portfolios. For example, in Panel A of Figure 4, a buyout fund that is in the top to-date tercile after 3 years is much more likely to report relatively low returns over the next year (as the darkest bar is much higher than the other two). This effect persists but is notably weaker for the 5^{th} through 6^{th} years since inception. By the 7^{th}-year since inception the mean-reversion gives place to persistence as the top-to-date funds are more likely to report relatively high changes in PMEs over the next 12 months (the darkest bar is the lowest). In contrast, before the 5^{th} year since inception, buyout and venture funds in the bottom tercile to-date are notably less likely to report relatively weak excess returns over the next 12 month. While after the 6^{th}-year since inception, when performance numbers become increasingly driven by cash flows (rather than

[19] The style-matched public portfolio for each fund is a weighted subset of Fama-French research portfolios that represent U.S. equity sorts into deciles based on mid-year book-to-market ratios and market capitalization. To better match the placebo series to the underlying fund assets, we use only the below-median size portfolios. For buyout funds we use the 25 highest book-to-market portfolios and lever their returns by a factor of two. For venture funds we take actual returns of the 25 lowest Book-to-Market portfolios. Once the weights are selected, they remain fixed over the fund life-time while the placebo returns correspond to the actual fund operation periods. Essentially, this placebo comparison can be thought of as deriving from a simulation where we draw factor-returns from a sample of actual paths rather than taking a stand on the funds' return-generating process explicitly. An advantage of this approach is that it retains the cross-sectional heterogeneity in the actual time-series of public equity returns (including any anomalies).

NAVs), their 12-month excess returns become notably worse than those of top-do-date tercile peers. The placebo returns generated from public portfolios (shown in Panel B of each figure) indicate that comparable public market returns exhibit no meaningful return-reversal patterns.

The evidence in this section reveals interesting, and economically significant, patterns in reported NAVs. These patterns appear related to some poor performing funds inflating returns around fundraising as well as potentially more widespread evidence of peer-chasing. However, the findings are not consistent with the fundraising success being positively related to NAV overstatements. On the contrary, we show evidence that investors punish overoptimistic NAVs by not providing capital to new funds. In addition, LPs appear to prefer positive interim performance signals in the form of cash distributions following successful divestments by funds. The data are also consistent with the realized performance bar being higher (and/or attempts to manipulate NAVs upward being stronger) in a tough fundraising environment as measured by low market returns. Luck experienced by funds in their early years appears to have less of a material effect on fundraising. If luck were important, we would observe lower excess returns after early fundraising events (than after late ones). Instead the post-fundraising excess returns for funds that are early to raise a next fund are on average positive and no different from those after late fundraisings. However, the analysis thus far is incomplete in so far as we have not attempted to examine how fund timing and peer-chasing interact nor have we provided a full assessment of the statistical significance of these results.

V. Fund timing and peer-chasing together

In the reminder of the paper we seek to characterize the variation in the informativeness of performance reporting that is robust to measurement errors and certain alternative explanations. Given the variety of factors that may affect NAV, focusing on just cross-sectional mean changes for excess returns is limiting in many regards. Ideally, we would like to understand how excess returns covary with explanatory variables in a multivariate setting. However, the unobservable nature of an NAV bias makes this a potentially tricky problem. In this section we take a careful look at how fund timing and peer-chasing may jointly determine NAVs. We define the NAV-bias and make clear what may obscure inference about it using fund-level reported returns rather than those of individual holdings within the fund portfolio.

We define the NAV bias as a ratio ($\equiv \Gamma_t$) of reported NAV to an unbiased assessment of a market price of the asset in an arms-length transaction.[20] By construction, this ratio will have a value greater than zero and equal to one when the bias is zero (e.g. when NAVs turn into cash). Therefore, it is natural to model this bias as a continuously compounded change from the level in the previous period. Starting from a valuation identity, such a change in bias over a period can be written as:[21]

$$\Delta bias_t = \log(NAV_t) - \log(NAV_{t-1} \times R_t^m - K_{t-1} \times CF_t) - \log(R_t^{\varepsilon}), \tag{3}$$

where R_t^{ε} and R_t^m are, respectively, idiosyncratic and priced risk-factor gross returns; CF_t represents net distributions to fund investors over period t; K_{t-1} is a ratio of the valuation bias multiple (Γ) at time $t-1$ to time t idiosyncratic gross return.

The intuition behind $\Delta bias_t$ is fairly straightforward. It is a change in log(NAVs) that cannot be explained by the asset returns or fund cash flows. Conditioning on the previous level of the bias (through multiplication by K_{t-1}) in periods with cash flows is necessary because the cash flows implicitly change the level of aggregate bias. For example, if $\Gamma_{t-1} = 1.1$ while the asset returned zero this period (i.e. $R_t^{\varepsilon} R_t^m = 1$) and a third of the assets were distributed as cash (i.e. $CF_t = 1/3 * NAV_{t-1}/\Gamma_{t-1}$), Γ_t has to increase to 1.15 for $NAV_t + CF_t$ to equal NAV_{t-1}.[22] So K_{t-1} would pick-up the true innovations in the bias (rather than the interaction of the past levels with the cash flows). However, neither asset returns nor past levels if the valuation bias are observable (e.g., R_t^m is also unknown since the fund factor loadings are not directly observable) so we must replace them with proxies. Next, we discuss the rationale behind our choices of proxies and the constraints on statistical inference they entail.

A. Dependent variables

For our main dependent variable, we utilize equation (3) assuming K_{it} and R_{it}^{ε} are equal to 1 while R_t^m is the value-weighted CRSP index return (or CRSP index returns levered by market beta estimates from the literature). So for each fund-quarter it, $\widetilde{\Delta bias}_{it}$ is defined as:

[20] An unbiased assessment satisfies the GAAP fair value definition as the value "at which that asset could be bought or sold in a current transaction between willing parties, other than in a liquidation." We do not distinguish between cases when GPs (i) pretend that reported NAVs are fair values in the GAAP sense and (ii) report NAVs that are conditional on a successful realization of the business plan (which is a very uncommon practice according to our conversations with LPs). Under the null hypothesis of "no gaming",' in neither case should changes in reported valuations depend on, for example, changes in the fund's past performance rank.

[21] See Appendix A for derivation.

[22] For example, this would be [1-0.333/1.1]/[(1-0.333)/1.1] with NAV_{t-1} normalized to 1.

$$\widetilde{\Delta bias}_{it} = \log\left(NAV_{it}\right) - \log\left(NAV_{i,t-1} \times R_t^{CRSP} - CF_{it}\right). \quad (4)$$

Thus, $\widetilde{\Delta bias}_{it}$ is just the market and cash flow adjusted NAV growth between $t-1$ and t. As outlined above, the measurement error on this feasible proxy for $\Delta bias_t$ will be a function of three items:[23]

- fund i idiosyncratic returns for period t;
- the market return for period t (when true exposure to the market deviates from assumed);
- the fund i cash flow for period t (to the extent the ratio of the previous period valuation bias to the current period idiosyncratic return deviates from one).

Thus, any multivariate analysis with this dependent variable would be prone to spurious coefficients whenever the regressors (X) correlate with either of the above mentioned factors. To make sure that our results are not driven by such spurious relations, we also construct a placebo dependent variable that is a function of misspecified systematic risk and the actual fund cash-flow pattern. Substituting $(NAV_t + CF_t)/R_t^{placebo}$ for NAV_{t-1} in equation (3) while keeping K_{t-1}=1, yields the following placebo counterpart:

$$\widetilde{\Delta bias}_t^{placebo} = \log(R_t^{placebo}) - \log\left(R_t^m + (R_t^m - R_t^{placebo})\frac{CF_t}{NAV_t}\right). \quad (5)$$

If controlling for cash flows nonetheless results in spurious correlations of residuals of $\widetilde{\Delta bias}_t$ and X then we should observe similar spurious correlations with $\widetilde{\Delta bias}_t^{placebo}$ and X. Similarly, if R^{CRSP} or idiosyncratic returns are correlated with X, this will also be the case for $\widetilde{\Delta bias}_t^{placebo}$. In other words, regressions using $\widetilde{\Delta bias}_t^{placebo}$ will indicate the direction and magnitude of the econometric bias in the estimates arising from the measurement errors' dependence on X.

Note that our approach is somewhat different from that in Jenkinson et al. (2013) and Barber and Yasuda (2013) who (in our context) attempt to infer Γ using the relations between fund NAVs and cash distributions. [24] These authors implicitly assume that the innovations to the valuation bias on the remaining assets do not depend on whether the fund has recently sold any holdings or made new investments. When this assumption fails, the measurement error on their dependent variable becomes a function of all past and future cash

[23] See Appendix A for details.

[24] Jenkinson et al. (2013) consider a change in NAVs from the past period as their main dependent variable while Barber and Yasuda (2013) define a "markdown" variable as $min\{NAV_t - (NAV_{t-1} - CF_t), 0\}$.

flows.[25] Simply put, with just the fund-level NAV data, it is hard to pin-down the timing of the valuation bias onset and unwind.

B. Explanatory variables

Our two primary explanatory variables of interest are $FundTiming$ and $PeerChasing$. $FundTiming$ is defined as the natural log of the number of years (after the first year) spent without a follow-on fund. It is a proxy for a growing incentive to boost NAV as the GP goes longer without raising a follow-on fund. By construction, the change in $FundTiming$ will be smaller for each subsequent quarter without a fund.[26]

PeerChasing is the difference between a funds reported IRR-to-date and the median across the fund's peers. We construct fund peer groups as we did for Figures 4 and 5). Specifically, peer groups consist of other funds of the same strategy and adjacent vintage years (including already resolved funds) as of the previous quarter. For placebo tests, we also construct a *PeerChasing* series from placebo returns. Under the null of unbiased (independently distributed) NAV changes, risk-adjusted returns should not correlate with their own lags. Additional details on the construction of both variables are presented in Appendix A.

An alternative explanation for a relation between NAV growth and $FundTiming$ or *PeerChasing* is that some funds have stale NAVs. That is, some GPs simply lag behind their peers in updating their portfolio valuations. For example, GPs may wait to revalue until a next funding round or follow a convention of holding assets at cost. Such firms may nonetheless have to bring stale NAVs more up to-date when it is time to start marketing a new fund. Thus, managerial style may result in mean-reversion of returns that is stronger when it has been awhile since the previous fund's inception. We address this concern via our cross-sectional tests in this section as well as in separate tests in the robustness section.

Because we want to focus on NAV reports that can be plausibly manipulated and also affect the fund

[25] For example, consider a fund that decides to value the remaining holdings more conservatively having made some distributions recently. The inference using their method would be that NAVs were overvalued prior to when those distributions took place even if in reality they were undervalued (i.e. $\Gamma_t < \Gamma_{t-1}$ is considered evidence of $\Gamma_{t-1} > 1$). One can also think of Γ_t as meaning a money- and time-weighted average of all vintages of capital v the fund has called to date, $\Gamma^t_{v,\tau}$. Hence, the estimates that these authors arrive at reflect the histories of fund capital calls besides the growth rates in each $\Gamma_{v,\tau}$.

[26] We note that it is possible that reverse causality drives the relationship between upward-biased NAVs and follow-on fund launches and that using $FundTiming$ should help mitigate concerns about us identifying this as nefarious manipulation. Suppose that, innocuously, GPs become overly optimistic about the investment opportunity set or their skill. These are precisely the times when they would seek to start another fund for a good reason. In other words, GPs may make honest mistakes that induce correlation between reported returns and new fund launches. Unlike dummy variables indicating lead/lags from the fundraising quarter, the variation in $FundTiming$ can be considered relatively exogenous with respect to such "honest optimism" waves in so far as the optimism monotonically increasing in the time spent without a fund is not a reasonable null hypothesis.

performance assessment by investors, we only consider reports between the 6^{th} and 28^{th} quarter of fund life for this analysis. To reduce the impact of outliers and remain realistic about the extent to which a common slope may hold across funds with dramatically different performance, we include only fund-quarter observations where IRR-to-date is within 30 percentage points from the peer-group median. This corresponds to censoring observations with regards to *PeerChasing* at approximately 5% from each side.

C. Main effects

Table 4 reports estimates for the following two models separately for both buyout and venture funds over the sample period covering 1984 through 2011:

(i) $\widetilde{\Delta bias_{it}} = [FundTiming_{it} \quad PeerChasing_{it}]\beta + Controls_{it} + v_{it}$

(ii) $\widetilde{\Delta bias_{it}} = [FundTiming_{it} \quad PeerChasing_{it} \quad FundTiming_{it} \times PeerChasing_{it}]\gamma + Controls_{it} + u_{it}$,

Results from model (i) are reported in specifications (1)-(3) and (6) while results from model (ii) are reported in specifications (4), (5), and (7). $Controls_{it}$ in specification (1) include fund fixed effects, year fixed effects, and quarter fixed effects. All other specifications include fund distributions and capital calls over the current quarter scaled by the end-of-quarter NAVs. The time controls are replaced with year-quarter fixed effect in specifications (3) and (5) through (7), so that the adjusted NAV growth is de-meaned across all funds in each calendar quarter. Specifications (1) through (5) have adjusted NAV growth computed assuming a beta of one relative to the value-weighted CRSP stock index. In specifications (6) and (7), we use beta estimates from Driessen, Lin and Phalippou (2012) who obtain the highest estimates of the market risk loading for both buyout funds (1.7) and venture funds (2.4) among the papers we reviewed.[27]

For buyout funds (Panel A), estimation results for model (i) indicate a positive and significant coefficient on *FundTiming* and a negative and significant coefficient on *PeerChasing* across all specifications. The corresponding results for the venture sample (Panel B of Table 4) show similar relations with somewhat smaller magnitudes for the *FundTiming*. These coefficients constitute a prediction of next period fund reported returns up to a fund-specific trend. The results are somewhat stronger when all of the calendar time-related variation is absorbed via year-quarter fixed effects and are virtually insensitive to the beta

[27]Brav and Gompers (1997), Cao and Lerner (2007), Kortoweg and Sorensen (2009), Jagadeesh, Kraussel adn Pollet (2010), Franzoni, Nowak and Phalippou (2012), Driessen, Lin and Phalippou (2012), Ewens, Jones and Rhodes-Kropf (2013).

assumption or the cash-flow controls.[28]

To gauge the economic significance, we can calculate that for a buyout fund, the fourth year spent without a follow-on fund elevates the reported excess returns an average of about 6.5% next quarter (0.06*log(3)). The coefficient on *PeerChasing* indicates how much the average fund excess return increases next quarter if it is above the peer group median IRR-to-date by 1.0%. For example, the estimate in the first column of Table 4 Panel A of -0.198 for buyout funds suggests a reversion of about 20 basis points.

In model (ii), we examine the interaction between fund timing and peer chasing. For both buyout and venture funds, the inclusion of the interaction term results in the coefficients on $FundTiming$ being smaller and less significant. However, in this case, the marginal effects of $FundTiming$ are for when *PeerChasing* equals zero whereas in model (i) the effects are implicitly evaluated at a mean level of other variables. The coefficient on *PeerChasing* switches sign (and even becomes significantly positive in some specifications), suggesting that when the $FundTiming$ variable is zero there is no reversion in returns (but rather a persistence).[29]

The negative and significant coefficient on the interaction term reinforces the conclusion that peer-chasing is stronger when the incentive to do so is high (as measured by $FundTiming$). In other words, the longer it takes to raise a next fund, the more strongly the funds reported returns revert to those of its peers. Because the effect is stronger when incentives are large, this finding is consistent with NAV manipulation rather than alternative explanations discussed in the previous section. To the extent longer time spent without a fund is associated with lower performance, these results are consistent with hypothesis *H2* that underperforming funds tend to overstate NAVs.[30]

D. Cross-sectional differences

We now investigate how cross-sectional differences affect fund timing and peer-chasing. We extend model (i) by including the interactions of $FundTiming$ and *PeerChasing* with the following variables:

[28] This suggests that the measurement error discussed in the previous section is not driving the coefficients on the variables of interest. The root-mean squared error and R-squared improve when cash-flow controls are included, particularly in the buyout subsample, suggesting the inclusion of cash flows may have a positive impact on the estimation efficiency.

[29] This is consistent with Figures 4 and 5.

[30] Year-quarter fixed effects, very close point estimates across different beta assumptions leave hardly any room for a risk-based explanation of our results, even if the risk varies over the life of the fund as documented in Barrot (2012) and Crain (2014).

- $Rookie_i$, equals 1 if the firm has had two or less previous funds in the sample and zero otherwise;
- $TopTercile_{it}$, equals 1 if fund i to-date-IRR at time t is in the top tercile of peer funds in the same strategy in adjacent vintage-years and zero otherwise;
- $BtmTercile_{it}$, equals 1 if fund i to-date-IRR at time t is in the bottom tercile of peer funds in the same strategy in adjacent vintage-years and zero otherwise.

Table 5 reports four specifications separately for buyout and venture funds. All are estimated with fund fixed effects, year-quarter fixed effects, and fund distributions and capital calls over the current quarter scaled by the end-of-quarter NAVs. Since $TopTercile_{it}$ and $BtmTercile_{it}$ are time varying characteristics over a fund's life, we can identify the effect on reporting bias in the quarters right after transitions to and from the respective tercile. $Rookie_i$ is a time-invariant characteristic for a given fund so only its interaction terms are present in the model.

Specification (1) examines the *Rookie* effect, (2) examines the $TopTercile$ effect, (3) includes all effects (thus, the base case is middle tercile funds with two or more previous funds from the same firm), and (4) investigates whether our inference is sensitive to the level of market beta we assume. It is important to note that the control group for top tercile funds in specification (2) is both middle and bottom tercile funds, whereas the control group in specifications (3) and (4) is only middle tercile funds.

We first consider the results from specification (1) for buyout and venture funds which examines the Rookie-effect. The coefficient on the interaction with $FundTiming_{it}$ is negative but insignificant and small.[31] We note that rookie venture funds do not exhibit significantly different fund timing or peer-chasing behavior. However, peer-chasing is more pronounced among rookie buyout funds.

In specification (2), we consider how the effects differ for top-performing funds. The positive and significant coefficient on $TopTercile$ indicates that top performing funds to-date continue to report abnormally high NAV-growth in both subsamples, buyout and venture. This is consistent with these funds carrying conservative valuations or having superior ability. The coefficient on the interaction with $FundTiming$ is negative and significant suggesting that top-tercile buyout and venture funds time less than their underperforming peers. However, the insignificant coefficient on the interaction between $TopTercile$ and *PeerChasing* indicates that

[31] An untabulated F-test rejects the null of the sum of coefficients equal to zero at 1% level for both buyout and venture.

the top performing funds do not appear to peer-chase as much as other funds. When the peer-chasing effect is accounted for, our estimates suggest that many top-performing funds actually report below average returns during the fundraising period. These results are consistent with hypothesis *H3* that top-performing funds tend to report conservative NAVs (to built a cushion against idiosyncratic shock realizations in the future).

In specification (3), we examine all effects simultaneously and find similar results for fund-timing overall. However, we find strong evidence that a large amount of fund-timing in both buyout and venture funds takes place in the bottom tercile (as indicated by the large positive coefficient on the *BtmTercile* interactions with $FundTiming$. For peer-chasing, the result appears limited to bottom tercile buyout funds, but is characteristic of the typical venture fund. The fact that $TopTercile$ main effect diminishes when *BtmTercile* is added (and comes out strongly negative) indicates that many of the middle-tercile funds report as good returns post fund-raising as their top-tercile peers. Meanwhile, specification (4) suggests that our inference about the cross-sectional effects is unlikely to be affected by heterogeneity in the risk exposure across funds. The estimates with high betas are virtually identical to those with unit betas.

The evidence presented in Table 5 is not consistent with a hypothesis that less experience leads to more aggressive NAV marks (as opposed to the results in Cumming and Walz (2010) and Barber and Yasuda (2013)). Actually, the rookies appear somewhat more conservative regardless of their performance to-date which is consistent with incentives to build a long-term reputation as per Chung et al. (2011). It instead appears that it is the current fund performance that largely determines the direction of the bias in NAV reports. Therefore, these results strongly support hypothesis *H2* that underperforming funds tend to overstate.

We note that the cross-sectional results are inconsistent with stale NAVs for some funds driving the main results. If stale NAVs were a significant driver in Table 4, we would expect that funds with the highest true returns had the largest gap to cover which predicts positive coefficients on $Top \times FundTiming$ and a positive total peer-chasing effect for that group (in contrast to what we find in specification (2)).

E. Placebo tests

As noted above, to better calibrate the null hypothesis for our tests, we examine a set of specifications similar to those in Table 4 and 5 but use placebo equivalents to determine if our estimation method is capturing something inherent in market conditions. Essentially, we estimate how style-matched public equity returns, conditional on actual fund cash flows, associate with lagged public equity returns since the respective fund inception (via *PeerChasing*). Also, we can identify actual calendar time patterns in subsequent funds starts (via *FundTiming*). The interactions with *Rookie*, *TopTercile* and *BtmTercile* dummies allow us to check whether these relations (1) are different in time periods when funds with less than two predecessors were operating, and (2) vary across performance ranks.[32] The results are tabulated in the appendix (Table B.3) but simply reveal no consistent results for either fund-timing or peer-chasing. Although a few coefficients are statistically different from zero, they tend to have opposite signs from what we find in Tables 4 and 5. This makes us confident that the effects we document in Table 5 are not spurious by construction.

VI. Robustness and other tests

A. Alternative estimators

In this section we scrutinize the assumptions about the fund return-generating process in the panel regressions of Section V. Namely, (1) the strict exogeneity of fund fixed effects with regards to other regressors included in the model, (2) the constant trend in fund excess returns between the 6^{th} and 28^{th} quarters of fund life, and (3) the stale NAV explanation for the fund-timing and peer-chasing effects in Table 4.

Assumption (1) is a concern since both key explanatory variables, *FundTiming* and *PeerChasing* values depend on past idiosyncratic returns of the fund which are also components of the measurement error on the dependent variable, $\widetilde{\Delta bias}$. In other words, the underlying model has strong features of a dynamic panel (i.e. $y_{i,t} = \gamma y_{i,t-1} + \alpha_i + \varepsilon_{i,t}$) where fixed effects estimators may yield biased estimates of γ because $E[y_{i,t-1}(\varepsilon_{i,t} - \bar{\varepsilon}_i)] \neq 0$.[33]

Assumption (2) appears vulnerable in light of the discussion in section IV. Absent any valuation biases, the abnormal performance trend may nonetheless deteriorate after a follow-on fund launches because of changes in asset composition, lack of manager attention, etc. Fixed effects models will disregard such changes during a fund's life and may falsely relate them to the variation in the explanatory variables. A possible fix for these econometric difficulties is to use a first-difference (FD) estimator to remove fund-level unobserved heterogeneity. Further, if we make an assumption that real changes to a fund's return

[32] In matching placebo portfolios, we further condition on placebo to-date returns being in the same tercile as the actual fund IRR as of 28^{th} quarter since inception or the last quarter in the sample for younger funds.

[33] The bias of the fixed-effect estimates would be finite and decreasing in panel length, but still can be sizeable in the case of highly persistent regressors. See, for example, Wooldridge (2002).

generating process (i.e. due to incentives) do not happen in a short interval (e.g., over few quarters) whereas manipulated changes to NAV do, FD estimator should yield more power against the "gaming" alternative.

Not demeaning the dependent and explanatory variables at the fund level also allows for including explanatory variables that are functions of future idiosyncratic returns. This would greatly help with controlling for the possible effects of stale NAVs. Note that stale NAVs can be formulated as self-correcting valuation errors which should be greater the further the reported performance level is from the final value (i.e. by the time all holdings are converted to cash flows). So if some GPs are simply slow to update values, the difference between the final PME from its level in the next period should absorb all of the suspicious variation in $\widetilde{\Delta bias}$.

With first-differencing, there is still a concern regarding endogenous variables so long as parts of $FundTiming$ and *PeerChasing* (henceforth, X_{it}) depend on returns at $t-1$. Therefore, we instrument ΔX_{it} with two lagged levels, X_{it-1} and X_{it-2}. Provided that the process for X is persistent and carries information about unobserved heterogeneity among funds, lagged levels are valid instruments for the difference (see Wooldridge, 2002).

Table 6 reports estimates of models (i) and (ii) (see C) in first-differences over fund-quarters via a two-step GMM with an optimal weighting matrix, robust to heteroscedasticity and autocorrelation. Results are reported separately for buyout (Panel A) and venture (Panel B) subsamples. All specifications except (3) use the instruments discussed above, namely: $(X_{it-1}\, X_{it-2}\, Controls_{it})$. We seek to further clarify the explanation for the effects we document in specification (3) by using *ExcessFundTiming* and *ResidualPeerChasing* as instruments for X. The tabulated partial F-statistics for the first stages suggest that we do not have a weak-instruments problem.

We define *ExcessFundTiming* as a ratio of the time spent without a follow-on by a given fund over the median time it took to raise a follow-on by the vintage and strategy peers. Thus, we adjust the temptation to fund-time by the average peer-pressure so that the "higher performance bar"-alternative (see section IV.A) is unlikely to interfere with the biased NAVs explanation. We define *ResidualPeerChasing* as the residuals from a regression on four lags of median-IRR by peer group, allowing for fund-varying slopes. Hence, this instrument should disregard the variation due to lack of timely updating by some funds.

Specifications (1) and (2) of Table 6 are very consistent with those in 4 although the effects are larger in magnitude and stronger statistically (particularly, for venture fund-timing). A comparison of (3) with (1) suggests that some of the peer-chasing effect might be indeed explained with a distributed lag of peers' returns but the residual effect is highly significant still. It also looks like that some of the fund-timing by venture might be explained by the variation in the pre-fundraising realized performance required by investors but not much.

We only consider funds that are nearly resolved in specification (4) so that the final PME value is known. Although the sample of fund-quarters drops by half, the coefficient estimates on *FundTiming* and *PeerChasing* are close to those in specification (1) suggesting high structural stability of the model. Meanwhile, the coefficient on the proxy of the self-correcting valuation error, the distance between the final PME, is insignificant (and wrong sign in the buyout subsample).

B. NAV reporting and SFAS 157

In September of 2006, the U.S. Financial Accounting Standards Board adopted Statement of Financial Accounting Standards 157 (SFAS 157) which effectively changed the NAV reporting standard for PE funds. A part of SFAS 157 referred to as ASC 820 requires fair-value reporting of balance sheet assets. Thus, the implementation of FAS 157 occurred during our sample period. The earliest adopters began complying in the fourth quarter of 2006 with all U.S. funds complying by the end of 2008. As a consequence, our sample may allow us to determine if FAS 157 had a notable effect on reported NAVs. Unfortunately, the timing of the adoption coincides with the financial crisis of 2007-2008 which complicates the analysis.

We undertake two simple tests in an attempt to identify effects that might be attributable to accounting changes related to SFAS 157. First, Figure 6 plots median fund performance during this period based on changes in PME indexed to 2003:Q4 value of 1.0. The figure shows that in 2008 PMEs for both buyout and venture funds increase significantly, regardless of the performance and fundraising success. This is consistent with funds marking-to-market undervalued investments *en masse*. However, if this were the case, we would expect PMEs to stay at this new level after being marked up. Instead PMEs drop substantially in 2009 so that the combined net change in PME is close to zero over the period from 2007-2009. Panel A also shows that the net effect is similar for both funds that are, and are not, successful at raising a next fund though it is more pronounced for those that are not. A likely explanation for the pattern in PMEs is that funds did not mark their portfolios down as far as the public market returns in 2008 nor up as much in 2009. Consequently, PMEs give the appearance of outperforming in 2008 and underperforming in 2009. Panel B shows similar plots based on performance tercile as of 2006:Q1. Panel C shows similar plots based on performance tercile as of the end of a funds life. In all cases fund relative returns as measured by PME appear to jump in 2008 and then drop in 2009 and it is difficult to attribute this return pattern to SFAS 157.

Our second test compares estimates of return autocorrelation before and after the adoption of SFAS 157. Specifically, we estimate the following AR(1) model:

$$NAVret_{it} = \mu + fas157_t + \rho_1 \cdot NAVret_{it-1} + \rho_2 \cdot NAVret_{it-1} \cdot fas157_t + \nu_{i,t}$$

and compare the estimates of ρ_1 and ρ_2. Given our previous analysis (as well as many others in context of marking illiquid assets), we expect to find positive values of ρ_1 consistent with positive return autocorrelation. A material impact from SFAS 157 (in the direction of timely and unbiased marking) would be consistent with a negative ρ_2 and the sum of ρ_1 and ρ_2 being insignificantly different from zero.

Table 7 reports results from the AR(1) model above for both buyout and venture funds for the full sample of funds and a variety of subsamples. We also examine both raw returns and de-meaned returns (i.e., returns accounting for fund fixed effects). Panel A reveals the expected significant positive values for ρ_1 in most samples. Values for ρ_2 are sometimes negative, but only weakly significant in two cases for de-meaned returns (i.e., specifications 3 and 6 which are for funds with weaker performance). Panel B reports results for venture funds. We again find generally positive and significant coefficients for ρ_1. However, for venture funds values for ρ_2 are often negative and significant. These results suggest that adoption of SFAS 157 may have had an important impact on NAV reporting for venture funds but not for buyout funds. The results for venture funds are consistent with Cumming and Walz (2009) finding of accounting standards effect on the private equity reporting.

C. Other tests

We also conduct tests in which we relax the parametric assumptions imposed on the relationship between lagged to-date performance and next-period reported returns. We estimate local polynomial regressions of $\widetilde{\Delta bias}_{it}$ and $\widetilde{\Delta bias}_{it}^{placebo}$ on $PeerChasing_{it-1}$ and $PeerChasing_{it-2}$ constructed from actual and placebo returns. Both variables are residuals from regressions on cash flows and a constant (estimated separately for each fund). The results are plotted in Appendix B (Figure B.4). We find a negative association between returns and peer-chasing when returns are close to zero. However, far from zero, the relationship is unstable and not different from zero. This suggests that peer-chasing is only important within a certain range where it might be credible (or relevant). The results are quite different from the largely flat relations for the placebo return series (plotted in Panel B).

For some funds there are quarterly return estimates that are quite large in magnitude and we also include some funds with investments outside the U.S. To make sure these large returns are not driving our results we drop all funds that exhibit reversion in reported returns of more than 50% over the course of six consecutive quarters and also drop funds with global investments to ensure that our U.S. benchmark is appropriate. Results using this much more restrictive sample are qualitatively similar to those presented in the main text.

The main results revealed a sharp and quick reversal in returns for the No Next fund group. To gain additional confidence in our results, we compare WPME and TVPI paths of the No Next Fund groups with those of the successful fundraisers stacked around the thirteenth quarter before the respective fund's resolution (or 10^{th} anniversary).[34] Figure B.3 reports the results. The top charts of Figure B.3 Panel A, show that the hump-shaped pattern of WPME in buyout and venture subsamples remains largely unchanged for the No Next Fund group when the outlier quarters are excluded while not being evident among the successful fundraisers. In Panel B we define the event time as 3 years after the median peer fund raised a successor fund. The hump-shape remains quite pronounced for the No Next Fund group in the venture subsample and less so in the buyout subsample. However, the subsequent underperformance relative to the successful fundraisers remains stark in both subsamples. Meanwhile, the bottom charts in both panels of Figure B.3 demonstrate the advantage of using our preferred metric based on Kaplan-Schoar PME and that the deterioration in No Next Fund performance is unlikely to be driven by differences in the market conditions. The aggregate TVPI path remains positive for the No Next Fund group as the variation in the market trends confounds the deterioration that the unsuccessful fundraisers experience while unwinding the current fund holdings. Nonetheless, the increase in the performance gap from the successful fundraisers is evident with TVPI as well.

[34] In this analysis, we also exclude Q2:2008-Q2:2009 return-quarters to mitigate the effects in PME-to-date shifts due to the apparent appraisal smoothing over the 2008 financial crises. See Section VI.B.

VII. Conclusion

We investigate whether there is evidence that private equity firms manipulate their NAV reports to investors. We find that some reported returns are abnormally high during periods when firms are likely to be marketing their new funds to prospective investors. We show that it is unlikely to be driven by reverse causality, such as time-varying optimism about the investment opportunity set. However, this fund timing pattern appears limited to the subset of underperforming funds. Moreover, it does not go unnoticed by the investors as firms reporting run-ups and reversals typically fail to raise a follow-on fund.

We also find that during periods when NAVs comprise a large fraction of reported to-date performance, most private equity fund managers exhibit abnormal reversion to the median to-date returns of their peer funds. Such peer-chasing for top-performing firms is consistent with an "underpricing equilibrium" that emerges as a response to asymmetric information problem present between fund managers (GPs) and their investors (LPs). LPs appear to punish GPs for what looks like aggressive interim reporting in the midst of the GPs' next fundraising by not providing capital to subsequent funds. Correspondingly, top performing GPs may try to safeguard their long-term reputation from bad luck by reporting conservative NAVs. They are more likely to do this when it does not jeopardize their high relative performance rank. For underperforming GPs, these long-term reputational concerns appear to be dominated by a short-term survival concern related to raising a next fund and, perhaps, a lack of credible ways to signal that their valuation marks are more conservative than those of similarly underperforming peer funds. Therefore, they are incentivized to boost to-date results to the extent the gap is not too large.

There are limitations to our analysis. Because our data are sourced from institutional investors, it is possible that NAV manipulation is different for funds in which institutional investors choose not to invest. Also, there may be other methods by which GPs could try to mislead investors with hidden fees, peer group selection, etc. Arguably, if (on average) investors can spot asset misvaluations, they should be able to learn and recognize other methods as well. Although we show that NAV overstatements appear to associate with a lower probability of fundraising success, the same might not be true for other methods. Regardless, an assessment of the welfare effects of a performance-gaming equilibrium hinges on the degree to which relatively unskilled LPs misallocate capital. In light of our results, sophisticated LPs are unlikely to misallocate capital and may therefore prefer the current stance to one with more regulation and (possibly) less gaming.

References

Ang, Andrew, and Morten Sorensen, 2012, Risks, returns, and optimal holdings of private equity: A survey of existing approaches, *Quarterly Journal of Finance Vol. 2, No. 3*.

Barber, Brad M, and Ayako Yasuda, 2014, Interim fund performance and fundraising in private equity, *University of California – Davis Working Paper*.

Barrot, Jean-Noel, 2012, Investor horizon and the life cycle of innovative firms: Evidence from venture capital, *Massachusetts Institute of Technology Working Paper*.

Brown, Keith C., W. V. Harlow, and Laura T. Starks, 1996, Of tournaments and temptations: An analysis of managerial incentives in the mutual fund industry, *The Journal of Finance* 51, 85–110.

Cai, Ye, Merih Sevilir, and Xuan Tian, 2012, Do entrepreneurs make good VCs?, *Indiana University Working Paper*.

Chakraborty, Indraneel, and Michael Ewens, 2014, When reputation is not enough: Portfolio manipulation and fund-raising in venture capital, *Southern Methodist University Working Paper*.

Chemmanur, Thomas J., and Paolo Fulghieri, 1994, Investment bank reputation, information production, and financial intermediation, *The Journal of Finance* 49, 57–79.

Chung, Ji-Woong, Berk Sensoy, Lea Stern, and Michael Weisbach, 2011, Pay for performance from future fund flows: The case of private equity, *Review of Financial Studies 25 (11), 3259-3304*.

Crain, Nicholas, 2014, Venture capital and career concerns, *Vanderbilt University Working Paper*.

Cumming, Douglas, and Uwe Walz, 2009, Private equity returns and disclosure around the world, *Journal of International Business Studies* 41, 727–754.

Gompers, Paul, Anna Kovner, Josh Lerner, and David Scharfstein, 2010, Performance persistence in entrepreneurship, *Journal of Financial Economics* 96, 18–32.

Gredil, Oleg, 2014, Market-timing and agency costs: Evidence from private equity, *University of North Carolina Working Paper*.

Harris, Robert, Tim Jenkinson, and Rüdiger Stucke, 2012, Are too many private equity funds top quartile?, *Journal of Applied Corporate Finance* 24, 77–89.

Harris, Robert S., Tim Jenkinson, and Steven N. Kaplan, 2013, Private equity performance: What do we know?, *forthcoming in the Journal of Finance*.

——— , and Rüdiger Stucke, 2012, Has persistence persisted in private equity? evidence from buyout and venture capital funds, *University of Virginia Working Paper*.

Harris, Robert S., Tim Jenkinson, and Rüdiger Stucke, 2010, A white paper on private equity data and research, UAI foundation working paper.

Hochberg, Yael, Alexander Ljungqvist, and Annette Vissing-Jorgensen, 2013, Informational hold-up and performance persistence in venture capital, in *forthcoming in Review of Financial Studies*.

Hochberg, Yael V., Alexander Ljungqvist, and Yang Lu, 2010, Networking as a barrier to entry and the competitive supply of venture capital, *The Journal of Finance* 65, 829–859.

Jegadeesh, Narasimhan, Roman Kraeussl, and Joshua Pollet, 2009, Risk and expected returns of private equity investments: Evidence based on market prices, *Emory University Working Paper*.

Jenkinson, Tim, Miguel Sousa, and Rüdiger Stucke, 2013, How fair are the valuations of private equity funds?, *Oxford University Working Paper*.

Kaplan, Steven N., and Josh Lerner, 2010, It ain't broke: The past, present, and future of venture capital, *Journal of Applied Corporate Finance* 22, 36–47.

Kaplan, Steven N., and Antoinette Schoar, 2005, Private equity performance: Returns, persistence, and capital flows, *The Journal of Finance* 60, 1791–1823.

Kaplan, Steven N., and Per Strömberg, 2008, Leveraged buyouts and private equity, *Journal of Economic Perspectives 22(4)*.

Kocis, James M., James C. Bachman, Austin M. Long, and Craig J. Nickels, 2009, *Inside Private Equity* (John Wiley & Sons, Inc.: Hoboken, N.J., USA).

Korteweg, Arthur, and Morten Sorensen, 2010, Risk and return characteristics of venture capital-backed entrepreneurial companies, *Review of Financial Studies* 23, 3738–3772.

Lerner, Josh, Antoinette Schoar, and Jialan Wang, 2008, Secrets of the academy: The drivers of university endowment success, *Journal of Economic Perspectives 22(3)*.

Lerner, Josh, Antoinette Schoar, and Wan Wongsunwai, 2007, Smart institutions, foolish choices: The limited partner performance puzzle, *The Journal of Finance* 62, 731–764.

Ljungqvist, Alexander, and Matthew Richardson, 2003a, The cash flow, return and risk characteristics of private equity, *NBER Working Paper*.

——— , 2003b, The investment behavior of private equity fund managers, *NYU Working Paper*.

Metrick, Andrew, and Ayako Yasuda, 2010, The economics of private equity funds, *Review of Financial Studies* 23, 2303–2341.

Phalippou, Ludovic, 2009, Beware of venturing into private equity, *Journal of Economic Perspectives* 23, 147–166.

———, 2010, The performance of private equity funds, *Journal of Banking and Finance* 34, 568–577.

———, 2013, Performance of buyout funds revisited, *forthcoming in Review of Finance*.

———, and Oliver Gottschalg, 2009, The performance of private equity funds, *Review of Financial Studies* 22, 1747–1776.

Roberts, Michael R., and Toni M. Whited, 2012, Endogeneity in empirical corporate finance, *University of Pennsylvania Working Paper*.

Robinson, David, and Berk Sensoy, 2011a, Cyclicality, performance measurement, and cash flow liquidity in private equity, *NBER Working Paper*.

———, 2011b, Do private equity fund managers earn their fees? compensation, ownership, and cash flow performance, *Duke University Working Paper*.

Sorensen, Morten, 2007, How smart is smart money? a two-sided matching model of venture capital, *The Journal of Finance* 62, 2725–2762.

Stucke, Rüdiger, 2011, Updating history, *Oxford University Working Paper*.

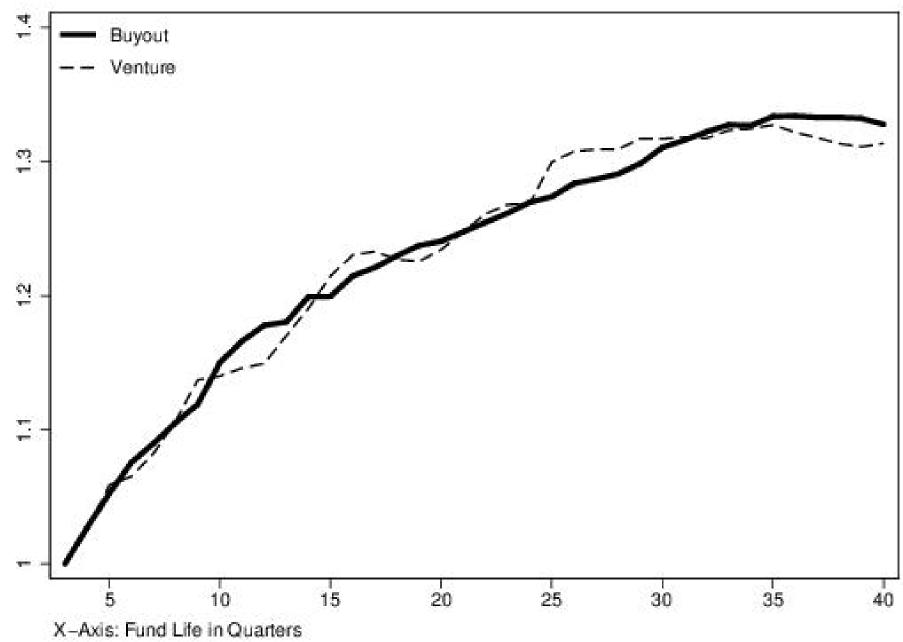
Thomsen, Steen, 2009, Should private equity be regulated?, *European Business Organization Law Review* 10, 97–114.

Wooldridge, Jeffrey M., 2002, *Econometric Analysis of Cross Section and Panel Data* (MIT Press: Cambridge, M.A., USA).

Figure 1: Average Fund Performance

This figure reports cumulative NAV-weighted excess returns of private equity funds over the public market index. Panel A plots values since inception and Panel B plots values since twelve quarters preceding the follow-on funds first capital call (x=-12). As described in equations (1) and (2), the change in a given quarter is a mean PME-to-date change from the previous period across a subset of funds multiplied by the average ratio of NAV to market-adjusted paid-in capital (to date). Appendix A shows that this is equal to a weighted-average excess return and that the inference about the path is robust to funds risk misspecification.

Panel A: Since Inception



Panel B: Around Fundraising

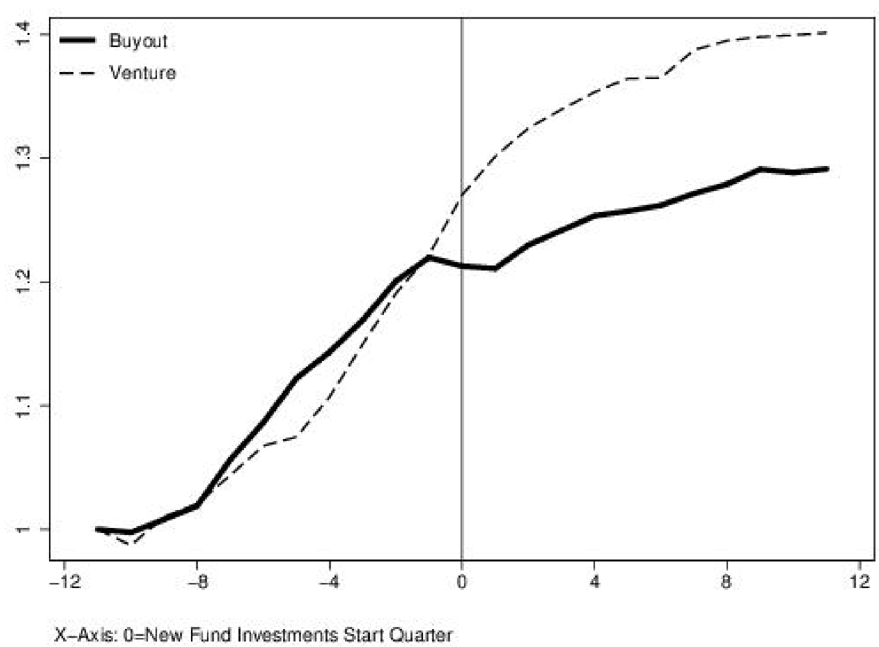
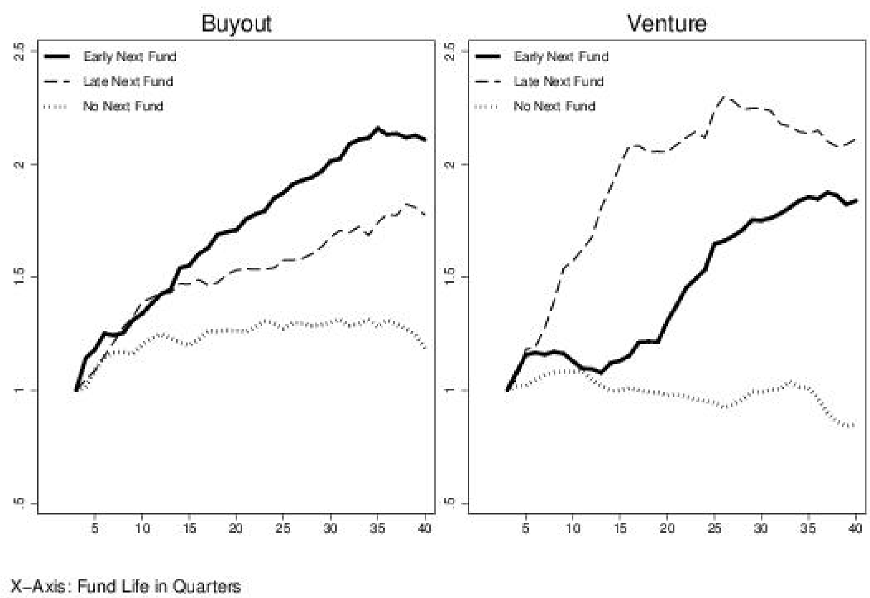


Figure 2: Average Performance Paths by Time Until Next Fund

This figure reports cumulative NAV-weighted excess returns of private equity funds over the public market index. Panel A plots values since inception and Panel B plots values since twelve quarters preceding the follow-on funds first capital call (x=-12). As described in equations (1) and (2), the change in a given quarter is a mean PME-to-date change from the previous period across a subset of funds multiplied by the average ratio of NAV to market-adjusted paid-in capital (to date). Appendix A shows that this is equal to a weighted-average excess return and that the inference about the path is robust to funds risk misspecification. We define subsets of funds in the legends of respective subfigures. In case there were no follow-on funds for this firm-fund in our sample (*No Next*), the Event quarter is the 13^{th} quarter preceding the funds resolution or its 10^{th} anniversary of the fund if still unresolved. *Late(Early)* denotes whether the follow-on fund was later(earlier) than the sample median across all buyout and venture funds respectively.

Panel A: Since Inception



Panel B: Around Fundraising

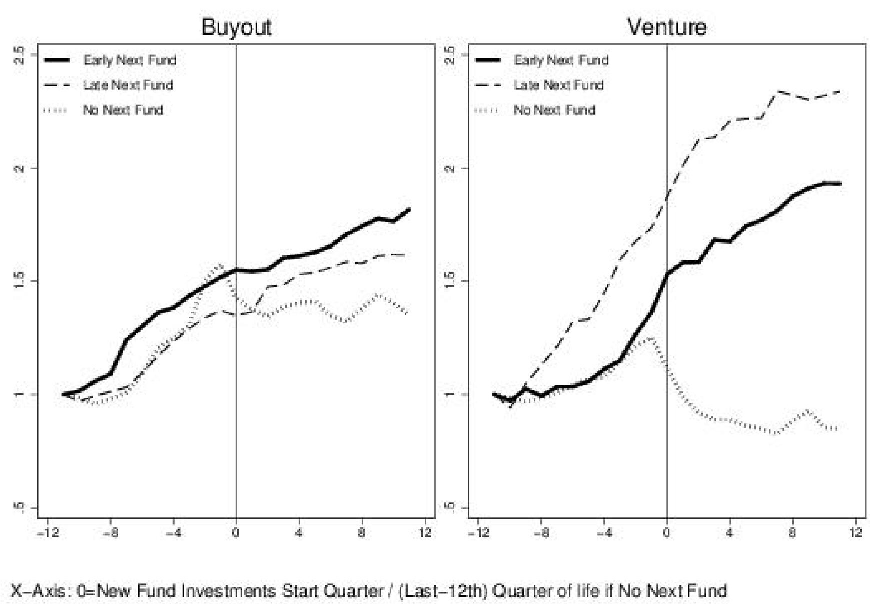
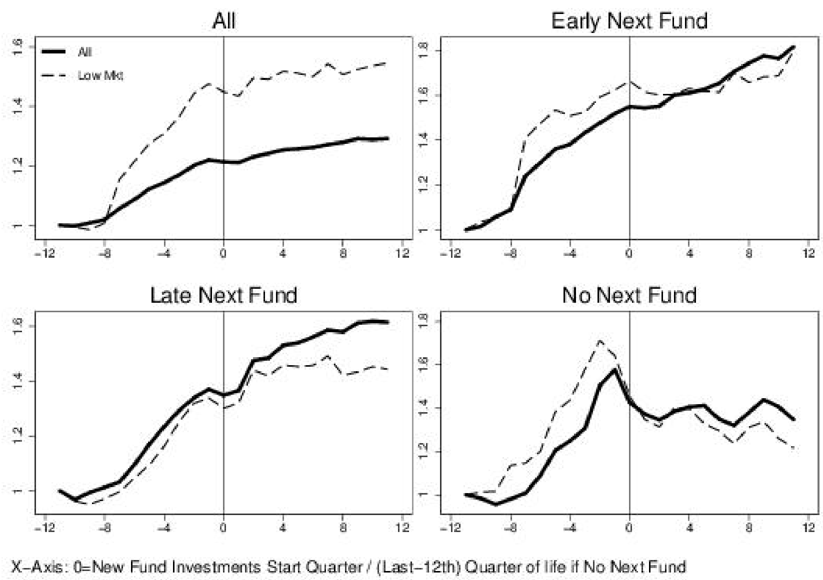


Figure 3: Average Fund Performance Path Around Fundraising

This figure reports cumulative NAV-weighted excess returns of private equity funds over the public market index. Panel A plots values since inception and Panel B plots values since twelve quarters preceding the follow-on funds first capital call (x=-12). As described in equations (1) and (2), the change in a given quarter is a mean PME-to-date change from the previous period across a subset of funds multiplied by the average ratio of NAV to market-adjusted paid-in capital (to date). Appendix A shows that this is equal to a weighted-average excess return and that the inference about the path is robust to funds risk misspecification. We define subsets of funds in the legends of respective subfigures. In case there were no follow-on funds for this firm-fund in our sample (*No Next*), the Event quarter is the 13^{th} quarter preceding the fund's resolution or its 10^{th} anniversary of the fund if still unresolved. *Late(Early)* denotes whether the follow-on fund was later(earlier) than the sample median across all buyout and venture funds respectively. *Low Mkt* denotes whether public market's 5-year rolling return as of the 13^{th} quarter of the fund life was below the sample median.

Panel A: Buyout



Panel B: Venture

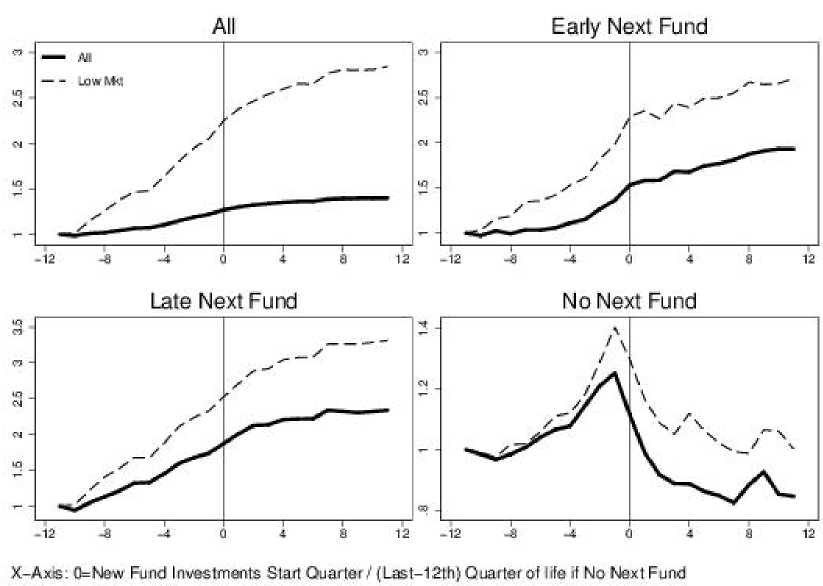
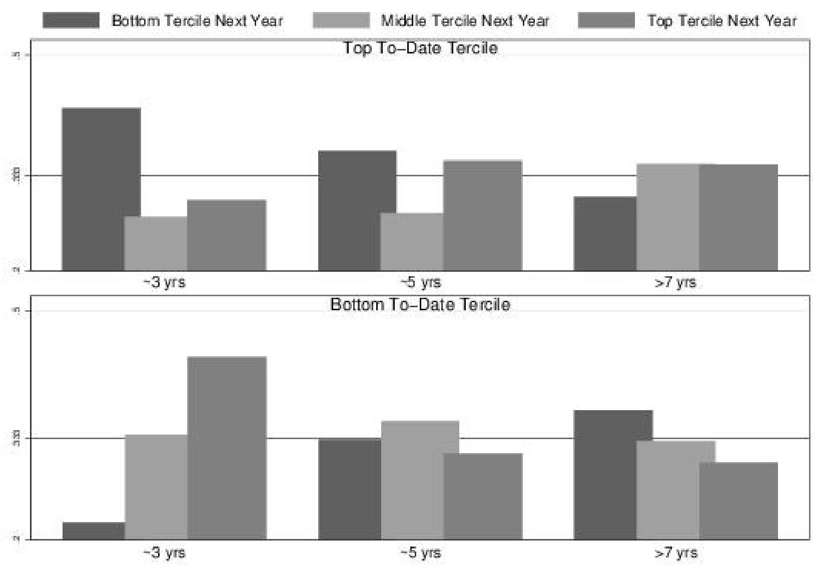


Figure 4: Next Year PME Growth Conditional on To-Date Performance: **Buyout**

This figure reports the probabilities of a buyout fund's excess returns over the next 4 quarters being in the top(bottom) tercile conditional on the fund's to-date performance tercile and time elapsed since its inception. We define the fund peer group for to-date and next year terciles as all funds of the same strategy incepted within one year from the fund vintage year. The top chart of each panel reports results for top to-date tercile funds as of 8 to 17 quarters since inception (3yrs), 18 to 27 quarters since inception, and more than 27 quarters. The bottom chart of each panel reports values for the bottom tercile to-date funds. Panel A uses actual fund returns and IRRs-to-date while Panel B is a placebo experiment with public equity portfolios returns assigned to the same set of funds and to-date performance computed as the average return of that portfolio since the fund inception. Placebo returns are constructed using subsets of Fama-French 100 U.S. Equity research portfolios as described in Appendix A.

Panel A: Actual - Reported Returns



Panel B: Placebo - Public Portfolios

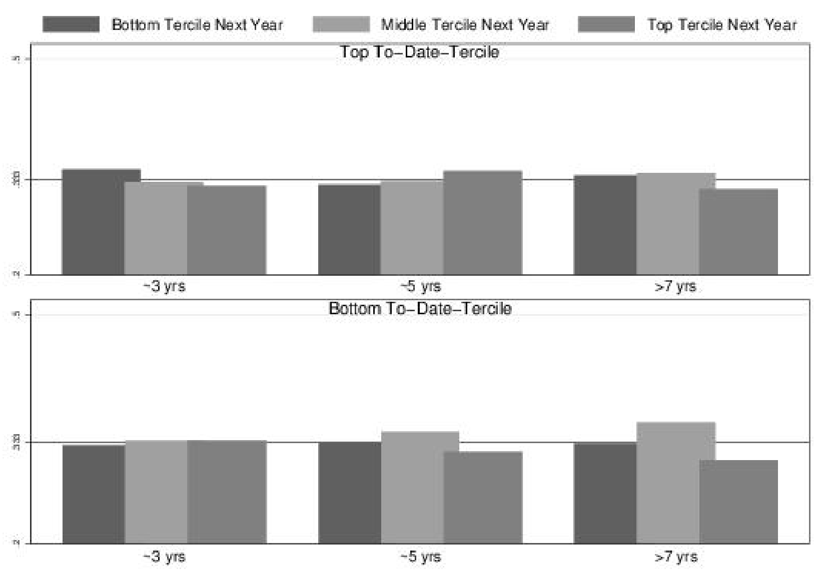
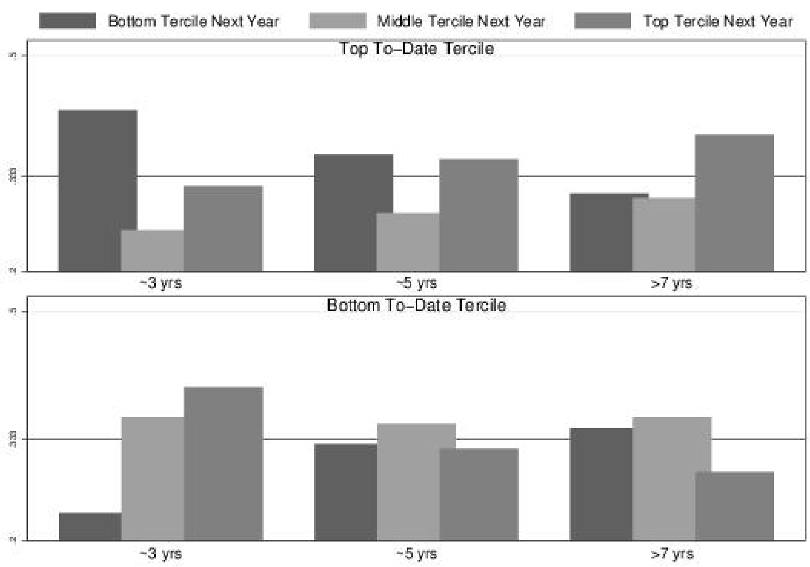


Figure 5: Next Year PME Growth Conditional on To-Date Performance: **Venture**

This figure reports the probabilities of a venture fund's excess returns over the next 4 quarters being in the top(bottom) tercile conditional on the fund's to-date performance tercile and time elapsed since its inception. We define the fund peer group for to-date and next year terciles as all funds of the same strategy incepted within one year from the fund vintage year. The top chart of each panel reports results for top to-date tercile funds as of 8 to 17 quarters since inception (3yrs), 18 to 27 quarters since inception, and more than 27 quarters. The bottom chart of each panel reports values for the bottom tercile to-date funds. Panel A uses actual fund returns and IRRs-to-date while Panel B is a placebo experiment with public equity portfolios returns assigned to the same set of funds and to-date performance computed as the average return of that portfolio since the fund inception. Placebo returns are constructed using subsets of Fama-French 100 U.S. Equity research portfolios as described in Appendix A.

Panel A: Actual - Reported Returns



Panel B: Placebo - Public Portfolios

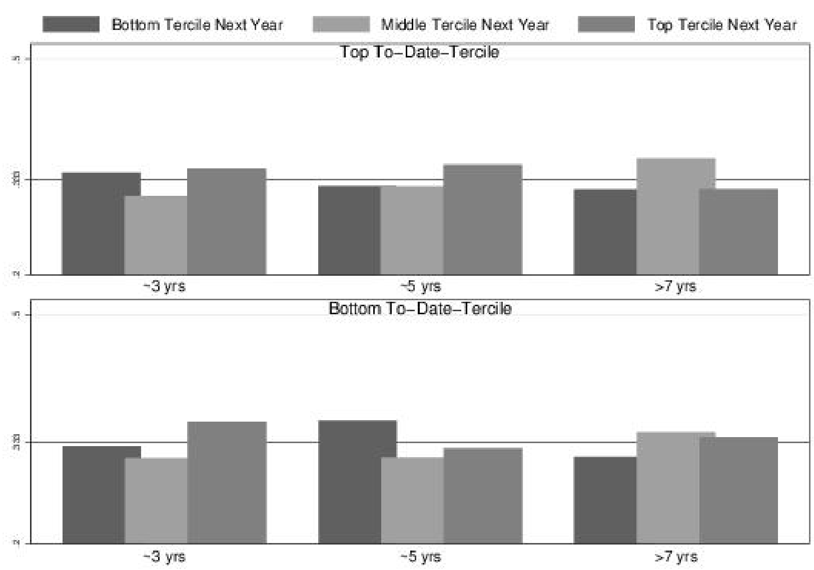
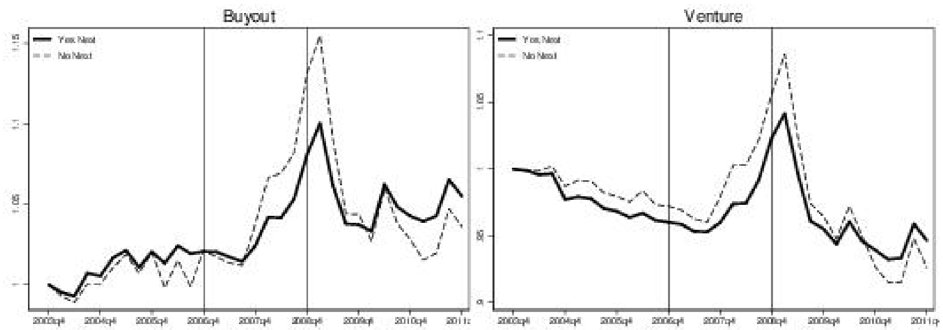


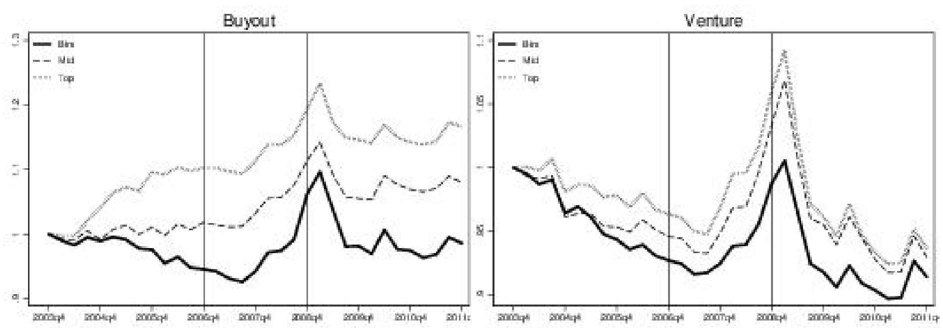
Figure 6: Median Fund Performance Over SFAS157 Adoption Period

This figure reports cumulative excess returns over a public equity index as measured by PME around SFAS157 adoption period, separately for buyout and venture funds. Panel A additionally breaks down the funds into groups based on whether or not a follow-on fund was raised. Panel B(C) breaks the funds into groups based on performance rank as of the end of 2006 (upon resolution). A change in a given quarter is a median PME-to-date change from the previous period across the respective subset of funds.

Panel A: By Fundraising Success



Panel B: By Performance Tercile as of 4Q'06



Panel C: By Performance Tercile End of Life

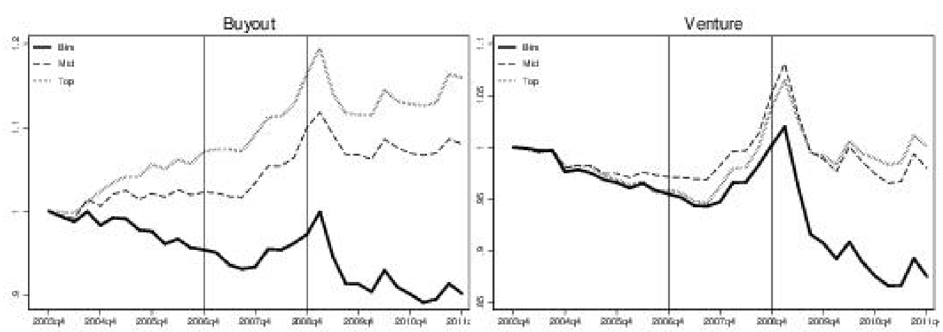


Table 1: Summary Statistics

This table reports summary statistics for the 997 buyout and 1,074 venture funds in our sample. Panel A provides basic statistics for buyout and venture funds separately. We also report common performance statistics conditional on whether or not the fund is resolved (or older than 8 years). Panel B provides detailed statistics on the timing of subsequent funds for buyout and venture funds separately. We provide statistics for subgroups based on number of prior funds and market return terciles (low, mid, high) in the 3 years prior to the fundraising period.

Panel A: Basic Statistics

		Buyout							Venture						
		Mean	StDev	5	25	50	75	95	Mean	StDev	5	25	50	75	95
All	Funds Per Firm	2.0	2.0	1.0	1.0	2.0	3.0	5.0	3.0	2.0	1.0	1.0	2.0	4.0	8.0
	Fund Size ($ mln)	1,324	5,755	80	220	450	1,070	4,210	390	2,288	26	74	170	330	740
	Vintage Year	2002	6	1989	1998	2004	2006	2008	1999	7	1984	1995	2000	2005	2008
if >1 Fund	Funds Per Firm	3.0	2.0	2.0	2.0	3.0	4.0	6.0	4.0	2.0	2.0	2.0	3.0	5.0	8.0
	Median interval per Firm	3.5	3.1	1.5	2.8	3.8	4.8	5.8	3.5	2.5	1.0	2.3	2.8	4.3	10.3
If Already Resolved	Life (years)	12.1	3.0	7.8	10.0	12.0	13.8	17.3	13.1	3.2	8.3	11.3	12.5	14.8	19.3
	IRR (%)	13.63	18.62	−10.48	4.22	11.29	22.27	38.83	14.48	48.53	−18.76	−6.12	3.46	16.00	86.31
	TVPI	1.72	1.00	0.60	1.18	1.56	2.03	3.38	2.00	3.25	0.31	0.72	1.19	1.98	5.87
	PME	1.27	0.58	0.48	0.89	1.22	1.53	2.16	1.26	1.95	0.21	0.52	0.80	1.22	3.61
If Still Alive	Life (years)	5.3	1.1	3.5	4.3	5.3	6.3	7.3	5.3	1.1	3.5	4.5	5.3	6.3	7.3
	IRR (%)	6.37	12.17	−12.93	0.03	6.87	12.69	23.69	4.44	14.63	−14.83	−4.72	3.82	11.77	30.57
	TVPI	1.21	0.40	0.72	1.00	1.18	1.35	1.78	1.19	0.46	0.67	0.88	1.11	1.35	2.01
	PME	1.04	0.35	0.62	0.85	1.00	1.17	1.59	0.98	0.39	0.55	0.72	0.93	1.13	1.67
If Resolved or Older than 8yrs	Number of Distributions	38	30	9	19	30	47	92	23	16	5	11	19	31	52
	Number of Capital Calls	38	31	7	20	32	48	86	20	17	3	9	16	24	48
	% of Quarters w/ Flows	32.4	9.1	18.0	26.0	32.0	38.0	48.0	25.5	8.0	13.0	20.0	25.0	31.0	38.0

Panel B: Follow-on Fundraising by Current Fund Age (if Resolved or Older than 8 years)

		Years Before Next Fund Raised										During	After	None
		1	2	3	4	5	6	7	8	9	10+	Life	Finish	So Far
		Buyout												
Fund Count	All Buyout	6	55	86	71	76	41	17	3	4	8	369	2	94
	No Previous Funds	1	15	23	32	28	17	6	3	3	1	129	0	40
	One Previous Fund	2	8	21	16	16	9	5	0	0	3	80	0	22
	Two or More	3	32	42	23	32	15	6	0	1	4	160	2	32
	Low Market	1	25	45	20	12	4	2	0	0	3	112	0	27
	Med Market	3	23	26	32	24	14	7	2	4	5	141	1	35
	High Market	2	7	15	19	40	23	8	1	0	0	116	1	32
Fund Means	Vintage Year	1999	1997	1998	1998	1998	1998	1998	1995	1996	1992	1998	1999	1998
	Size ($ mln)	1036.7	919.3	969.5	654.5	1008.8	895.1	969.9	159.3	206.8	923.9	884.5	665.0	497.0
	Final PME	1.6	1.3	1.4	1.3	1.4	1.1	1.5	1.7	1.6	1.1	1.3	0.9	1.0
	Final IRR	30.0	10.0	20.0	20.0	20.0	10.0	20.0	20.0	20.0	10.0	20.0	3.0	5.0
		Venture												
Fund Count	All Venture	37	116	123	88	71	27	18	6	2	10	527	29	193
	No Previous Funds	4	26	24	22	15	5	4	3	1	7	115	4	54
	One Previous Fund	9	14	16	17	17	7	5	2	1	1	95	6	51
	Two or More	24	76	83	49	39	15	9	1	0	2	317	19	88
	Low Market	1	48	66	25	19	6	6	0	0	5	180	4	38
	Med Market	19	40	37	33	18	5	5	5	1	4	182	15	75
	High Market	17	28	20	30	34	16	7	1	1	1	165	10	80
Fund Means	Vintage Year	1998	1996	1995	1995	1995	1996	1994	1994	1993	1990	1995	1996	1996
	Size ($ mln)	305.4	217.1	182.9	233.1	283.1	358.4	186.8	121.2	43.5	199.4	231.8	236.0	279.1
	Final PME	1.2	2.3	1.6	0.9	1.0	1.0	1.0	1.0	1.1	1.4	1.4	0.9	0.7
	Final IRR	2.0	40.0	30.0	8.0	8.0	20.0	9.0	1.0	10.0	20.0	20.0	10.0	−0.3

Table 2: Performance Tercile Transition Probabilities

This table reports transition probabilities between IRR-to-date terciles within each fund peer group estimated separately for buyout and venture funds in Panel A and Panel B respectively. Only the funds that have raised a follow-on fund within ten years since inception are included. The first row of each panel reports the probability of being in the respective to-date tercile at the end of a fund's life (*At Life End*) conditional on being in the bottom to-date tercile in the quarter preceding the follow-on fund's first capital calls (*At Fundraising*). The second(third) row reports *At Life End* tercile conditional on being in the middle(top) *At Fundraising* tercile. The last row of each panel reports the unconditional distribution of funds across *At Life End* terciles, while the last column reports how many funds were in each fundraising tercile and the respective fraction in the total fund count. The peer group is all funds of the same strategy incepted within one year from the fund vintage year. Since peer groups overlap, and follow-on fundraising at different points of life of the current funds, and funds duration varies, neither *At Fundraising* nor *At Life End* terciles need to have an equal number of funds.

Panel A: Buyout

		At Life End				
		Btm	Mid	Top	Fund Count	
At Fundraising	Btm	61.2%	26.9%	11.9%	67	(18.8%)
	Mid	36.9%	42.3%	20.8%	130	(36.5%)
	Top	13.2%	25.2%	61.6%	159	(44.7%)
	All	30.9%	31.7%	37.4%	356	(100%)

Panel B: Venture

		At Life End				
		Btm	Mid	Top	Fund Count	
At Fundraising	Btm	55.6%	36.7%	7.7%	117	(22.9%)
	Mid	31.8%	41.3%	26.8%	179	(35.1%)
	Top	14.5%	25.2%	60.3%	214	(42.0%)
	All	30.0%	33.5%	36.5%	510	(100%)

Table 3: Do LPs Vote With Their Feet?

This table linear probability model of a follow-on fund being raised separately for buyout (Panel A) and venture (Panel B) funds that were resolved or operated for at least 10 years. The event time is defined by the quarter in which the successful fundraising took or the 13^{th} quarter before the resolution (or the 10^{th}anniversary if unresolved by then). The explanatory variables of interest are the following indicator variables: *PME drop (after)* – equals 1 if the value of fund Kaplan-Schoar PME at resolution is lower that at the event time; *PME run-up (before)* – equals 1 if the value of fund Kaplan-Schoar PME 1 year before the event time is lower that at the event time; *Large Distribution (before)* – equals 1 if the sum of distributions over the year preceding the event time exceeds 20% of NAVs; *Top tercile-to-date* – equals 1 if fund is in the top (highest) IRR-tercile across vintage and strategy peers at the event time and zero otherwise, and *Bottom tercile-to-date* – equals 1 if is in the bottom (lowest) IRR-tercile at the event time and zero otherwise. All specifications include the interaction of the fund vintage year and industry fixed effects. In specifications (3) and (4) we include the event-time level of PME as well a dummy indicating where market return was positive in the pre-event year as additional controls. *t*-statistics reported in parentheses are robust to error clustering at the event year, */**/*** denotes significance at 10/5/1% confidence level.

	Panel A Buyout				**Panel B** Venture			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
PME drop (after)	-0.192^{**}	-0.173^{**}	-0.184^{**}	-0.156^{**}	-0.154^{**}	-0.124^{*}	-0.128^{*}	-0.125^{**}
	(-2.52)	(-2.38)	(-2.61)	(-2.67)	(-2.14)	(-1.96)	(-2.03)	(-2.05)
PME run-up (before)	-0.171^{**}	-0.161^{**}	-0.143^{**}	-0.188^{***}	-0.281^{***}	-0.288^{***}	-0.204^{***}	-0.265^{***}
	(-2.12)	(-2.06)	(-2.39)	(-4.34)	(-3.90)	(-4.17)	(-4.43)	(-5.69)
Large Distr. (before)				0.079				0.041
				(1.21)				(0.51)
PME run-up × Large Distribution				0.172^{***}				0.282^{***}
				(3.11)				(2.82)
Top tercile-to-date		0.115^{***}	0.071^{**}	0.064^{**}		0.123^{***}	0.107^{***}	0.088^{***}
		(4.26)	(2.41)	(2.21)		(4.51)	(4.12)	(3.91)
Bottom tercile-to-date		-0.222^{**}	-0.188^{**}	-0.160^{*}		-0.107^{***}	-0.105^{***}	-0.099^{***}
		(-2.43)	(-2.05)	(-2.00)		(-2.90)	(-2.92)	(-2.91)
Industry × Vintage FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PME_E, $I(Mkt_{(E-1y,E)} > 0)$	No	No	Yes	Yes	No	No	Yes	Yes
Observations	541	541	541	541	765	765	765	765
R-squared	0.309	0.370	0.379	0.403	0.330	0.365	0.391	0.399

Table 4: Fund Timing and Peer-Chasing

This table reports the parameter estimates a linear regression model estimated separately for buyout (Panel A) and venture (Panel B) funds. The dependant variable measures risk- and cash flow-adjusted changes in NAV for quarter t that is constructed to be unpredictable under the null of reported NAVs being unbiased estimators of true asset values. The market beta of the fund assets is assumed to be 1.7 [2.4] in specifications (6) and (7) for buyout [venture] subsample and 1 everywhere else. Explanatory variables of interest include *FundTiming* - the natural log of one plus time spent to-date without a follow-on fund in excess of two years, *PeerChasing* - the difference between fund i reported Internal Rate of Return to-date for the calendar quarter corresponding to $t-1$ quarter of fund i life and its peers as measured by the median IRR-to-date across all funds of the same strategy incepted within one year from fund i *Vintage* year. Specifications (4), (5) and (7) also include the interaction of *FundTiming* and *PeerChasing* variables. All specifications include fund fixed effects, all except (1) include fund distributions and capital calls over the current quarter scaled by the end of quarter NAVs. Specifications (3) and (5) through (7) include year-quarter fixed effects, others have year and quarter fixed effects instead. t-statistics reported in parentheses are robust to heteroskedasticity and autocorrelation, */**/*** denotes significance at 10/5/1% confidence level.

	$\beta=1$					$\beta=1.70B/2.4V$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Buyout							
FundTiming	0.060***	0.059***	0.080***	0.038**	0.057***	0.076***	0.053**
	(2.78)	(3.13)	(4.22)	(2.02)	(3.00)	(3.63)	(2.57)
PeerChasing	−0.198***	−0.202***	−0.205***	0.123**	0.131**	−0.202***	0.117**
	(−5.95)	(−6.31)	(−6.51)	(2.31)	(2.55)	(−5.46)	(2.09)
FundTiming × PeerChasing				−0.295***	−0.304***		−0.289***
				(−6.22)	(−6.61)		(−5.62)
Observations	12,150	12,150	12,150	12,150	12,150	12,150	12,150
R-squared	0.046	0.094	0.237	0.098	0.242	0.420	0.423
RMSE	0.184	0.172	0.158	0.172	0.158	0.180	0.180
Panel B: Venture							
FundTiming	0.029**	0.026*	0.051***	0.018	0.043***	0.054***	0.046***
	(2.08)	(1.89)	(3.62)	(1.34)	(3.08)	(3.78)	(3.26)
PeerChasing	−0.151***	−0.168***	−0.175***	0.068*	0.045	−0.180***	0.037
	(−7.91)	(−8.53)	(−9.18)	(1.79)	(1.21)	(−9.21)	(0.99)
FundTiming × PeerChasing				−0.217***	−0.202***		−0.200***
				(−6.88)	(−6.52)		(−6.29)
Observations	15,124	15,124	15,124	15,124	15,124	15,124	15,124
R-squared	0.110	0.118	0.305	0.121	0.309	0.607	0.608
RMSE	0.136	0.135	0.120	0.135	0.120	0.124	0.124
Controls in Both Panels:							
Fund FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cash Flows	No	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	No	No	No
Quarter FE	Yes	Yes	No	Yes	No	No	No
Year-Qtr FE	No	No	Yes	No	Yes	Yes	Yes

Table 5: Cross-Section of To-Date Performance

This table reports the parameter estimates a linear regression model estimated separately for buyout (Panel A) and venture (Panel B) funds. The dependant variable measures risk- and cash flow-adjusted changes in NAV for quarter t that is constructed to be unpredictable under the null of reported NAVs being unbiased estimators of true asset values. The market beta of the fund assets is assumed to be 1.7 [2.4] in specification (4) for buyout [venture] subsample and 1 everywhere else. Explanatory variables of interest include *FundTiming* - the natural log of one plus time spent to-date without a follow-on fund in excess of two years, *PeerChasing* - the difference between fund i reported Internal Rate of Return to-date for the calendar quarter corresponding to $t-1$ quarter of fund i life and its peers as measured by the median IRR-to-date across all funds of the same strategy incepted within one year from fund i *Vintage* year. *Rookie*, *Top* and *Btm* are dummies denoting if the PE firm had less than two funds before i, if i was in Top(Bottom) tercile as measured by IRR-to-date as of quarter $t-1$ across the peers. Control variables in all specifications include funds fixed effects, year-quarter fixed effects as well as fund distributions and capital calls over the current quarter scaled by the end of quarter NAVs. t-statistics reported in parentheses are robust to heteroskedasticity and autocorrelation, */**/*** denotes significance at 10/5/1% confidence level.

	Panel A Buyout				**Panel B** Venture			
	$\beta = 1.0$			$\beta = 1.70$	$\beta = 1.0$			$\beta = 2.40$
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
FundTiming	0.088***	0.082***	0.066***	0.063***	0.053***	0.053***	0.040***	0.043***
	(4.67)	(4.31)	(3.47)	(3.07)	(3.74)	(3.81)	(2.86)	(3.03)
PeerChasing	−0.138***	−0.191***	−0.038	−0.025	−0.167***	−0.182***	−0.127***	−0.126***
	(−3.03)	(−4.99)	(−0.64)	(−0.38)	(−7.55)	(−6.81)	(−3.23)	(−3.10)
Rookie × FundTiming	−0.015		−0.017*	−0.019*	−0.007		−0.003	−0.002
	(−1.43)		(−1.70)	(−1.71)	(−0.96)		(−0.54)	(−0.55)
Rookie × PeerChasing	−0.134**		−0.114*	−0.123	−0.022		−0.025	−0.031
	(−2.15)		(−1.77)	(−1.62)	(−0.54)		(−0.46)	(−0.60)
TopTercile-to-date		0.046**	0.023	0.021		0.024**	0.004	0.003
		(1.99)	(0.96)	(0.74)		(2.13)	(0.33)	(0.24)
Top × FundTiming		−0.041***	−0.021	−0.017		−0.025***	−0.019	−0.018
		(−2.92)	(−1.38)	(−0.96)		(−3.49)	(−1.43)	(−1.26)
Top × PeerChasing		−0.026	−0.111	−0.123		0.045	0.005	0.010
		(−0.37)	(−1.39)	(−1.35)		(0.96)	(0.10)	(0.19)
BtmTercile-to-date			−0.073***	−0.070***			−0.053***	−0.050***
			(−3.94)	(−3.50)			(−4.21)	(−3.90)
Btm × FundTiming			0.051***	0.050***			0.041***	0.039***
			(5.03)	(4.74)			(5.23)	(4.90)
Btm × PeerChasing			−0.206**	−0.207*			−0.103	−0.107
			(−2.11)	(−1.89)			(−1.59)	(−1.63)
Controls	Fund FE, Year-Qtr FE, Cash-Flows							
Observations	12,150	12,150	12,150	12,150	15,124	15,124	15,124	15,124
R-squared	0.238	0.238	0.241	0.423	0.305	0.306	0.323	0.608
Pr(F-stat>F[*FundTiming by Top*])		0.301				0.234		

Table 6: Fund Timing and Peer-Chasing: Dynamic Panel Specifications

This table reports the parameter estimates a linear regression model estimated separately for buyout (Panel A) and venture (Panel B) funds. The dependant variable measures risk- and cash flow-adjusted changes in NAV for quarter t that is constructed to be unpredictable under the null of reported NAVs being unbiased estimators of true asset values. Explanatory variables of interest (X) include *FundTiming* - the natural log of one plus time spent to-date without a follow-on fund in excess of two years, *PeerChasing* - the difference between fund i reported Internal Rate of Return to-date for the calendar quarter corresponding to $t-1$ quarter of fund i life and its peers as measured by the median IRR-to-date across all funds of the same strategy incepted within one year from fund i *Vintage* year. All specifications are estimated in first differences by fund-quarters via two-step GMM with the optimal weighting matrix. Everywhere expect in specifications (3), we use lagged two levels of X to instrument for the difference whereas in (3) we use the lagged two levels of *Excess FundTiming* and *Residual PeerChasing* as the instruments (both defined in Section VI.A). In all specifications, *control* variables include year year and quarter fixed effects as well as fund distributions and capital calls over the current quarter scaled by the end of quarter NAVs. Specifications (4) also includes $(PME_T\text{-}PME_{t+1})$, a difference between the next period PME-to-date and the final PME for the funds that were fully resolved by the end of March 2012. t-statistics reported in parentheses are robust to heteroskedasticity and autocorrelation, */**/*** denotes significance at 10/5/1% confidence level.

	Panel A Buyout				**Panel B** Venture			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
FundTiming	0.165***	0.144**	0.143**	0.159**	0.235***	0.213***	0.157***	0.284***
	(2.96)	(2.57)	(2.45)	(2.13)	(5.01)	(4.70)	(3.21)	(4.87)
PeerChasing	−0.315***	0.100	−0.196**	−0.310***	−0.366***	0.286***	−0.117**	−0.403***
	(−5.72)	(0.72)	(−2.50)	(−3.67)	(−7.80)	(2.80)	(−1.97)	(−6.25)
FundTiming × PeerChasing		−0.735***				−1.253***		
		(−3.07)				(−6.60)		
$(PME_T\text{-}PME_{t+1})$				−0.036				0.023
				(−1.27)				(1.00)
Fund Effects	First-Differences							
Controls	Year and Quarter Fixed Effects, Δ.Cash-Flows							
Observations	12,003	12,003	12,003	5,875	14,979	14,979	14,979	7,119
R-squared	0.099	0.146	0.08	0.108	0.106	0.211	0.064	0.112
F-stat[*1st stage*]	35.3	33.8	33.2	9.2	16.8	18.5	14.5	7.4

Table 7: Autocorrelation of Reported Returns Before and After FAS157

This table reports the parameter estimates for a following linear regression model estimated separately for buyout and venture funds in Panel A and B respectively:

$$NAV\,ret_{it} = \mu + fas157_t + \rho_1 \cdot NAV\,ret_{it-1} + \rho_2 \cdot NAV\,ret_{it-1} \cdot fas157_t + \nu_{i,t}$$

for two specifications, *Fund FE* and *Pooled*, and four subsamples. $NAV\,ret_{it}$ is fund i reported return for quarter t as measured by NAV change adjusted for net distributions during that quarter in *Pooled* while de-meaned over each fund's lifetime in *Fund FE*. $fas157_t$ is a dummy taking value of one for quarters after 2Q09 and zero otherwise. *All* includes all Funds in our sample, so that the control group includes funds already resolved by end of 2006 as well as earlier reports by fund that remained active after 2Q09. *Btm, Mid, Top* are subsamples of funds that remain active end of 2006 and were in the respective performance tercile according to reported IRR-to-date. We drop reports for 10 quarters between 1Q07 and 2Q09 for all funds in each subsample to insure that our inference is not confounded by developments during the adoption period, the onset of the 2008 crisis and the subsequent rebound in liquid market prices. Also, we drop all reports by funds younger than 8 quarters since inception. t-statistics reported in parentheses are robust to heteroskedasticity and autocorrelation, */**/*** denotes significance at 10/5/1% confidence level.

	Fund FE				Pooled			
	All	Btm'06	Mid'06	Top'06	All	Btm'06	Mid'06	Top'06
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Buyout								
ρ_1	0.137***	0.204*	0.023	0.130***	0.116**	0.207*	0.001	0.0943**
	(3.30)	(2.02)	(0.44)	(3.18)	(2.20)	(1.98)	(0.02)	(2.24)
ρ_2	−0.069	−0.278**	−0.149	0.041	−0.030	−0.236	−0.026	0.228
	(−0.80)	(−2.44)	(−1.29)	(0.39)	(−0.50)	(−1.76)	(−0.19)	(1.65)
Pr(F-stat> $[\rho_1+\rho_2]$)	0.423	0.356	0.203	0.237	0.145	0.766	0.820	0.078
Observations	9,181	1,675	2,047	1,867	9,181	1,675	2,047	1,867
Panel B: Venture								
ρ_1	0.063	0.172***	0.108*	0.182***	0.0781*	0.190***	0.112*	0.178***
	(1.62)	(6.18)	(1.93)	(5.15)	(1.93)	(7.59)	(1.95)	(4.83)
ρ_2	−0.216***	−0.344***	−0.279**	−0.292***	−0.125	−0.322***	−0.241	−0.180***
	(−3.75)	(−3.93)	(−2.43)	(−5.83)	(−1.45)	(−3.69)	(−1.76)	(−3.19)
Pr(F-stat> $[\rho_1+\rho_2]$)	0.000	0.090	0.131	0.034	0.490	0.230	0.316	0.965
Observations	15,230	2,624	2,873	3,430	15,23	2,624	2,873	3,430

Appendix A.

In this Appendix, we provide a more detailed discussion of the primary variables we utilize in our analysis. We start by explaining why simple measures such as IRR-to-date and PME-to-date can provide misleading metrics (where 'to-date' measures use the NAV at a particular date as though it were the final cash flow from a fund). Figure A.1 illustrates the inconsistency of IRR-to-date for the purpose of NAV bias assessment by studying two hypothetical cash-flow and abnormal return patterns (i.e., funds). Case 1 considers a hypothetical fund in existence from June 1993 through June 2003 and Case 2 considers a different hypothetical fund in existence from June 1998 through June 2008. The value process in both cases is defined as, $FundValue_t = FundValue_{t-1}(1 + r_{S\&P500,t} + \alpha_t) + Calls_t - Distrib_t$. That is, the fund's return over a period equals the return to the S&P 500 plus an abnormal return (α_t). In Case 1, the alpha is fixed at 4% across all periods. Whereas in Case 2, the alpha is initially 5% per period but than decays to zero over the life of the fund. Panel A of Figure A.1 plots the alpha and the cash-flow patterns for both cases. Panel B plots the total return to the S&P 500 index over each hypothetical fund's life. Panel C plots the resulting PMEs-to-date and IRRs-to-date. These two cases show that IRR-to-date may provide completely misleading indications of when 'gaming' of fund NAVs could be taking place. Specifically, the fund with constant alpha (Case 1) exhibits an apparent decline in IRR-to-date after the fund's fifth year. In contrast, the fund with declining alpha (Case 2) shows an increasing IRR-to-date after the fund's fifth year. The PME-to-date analysis, on the other hand, exhibits nearly identical patterns for both cases and therefore may not be informative either. Consequently, we next develop our method for identifying abnormal returns that in essence unwinds the flattening effect that intermediate distributions have on the PME-to-date.

I. Key Variable Definitions

We start by considering the Kaplan and Schoar (2005) Public Market Equivalent index

$$PME = \frac{\sum_{t=0}^{T} D_t \prod_{\tau=t}^{T} R_{\tau+1}}{\sum_{t=0}^{T} C_t \prod_{\tau=t}^{T} R_{\tau+1}}, \tag{A.1}$$

where D_t and C_t are, respectively, the fund distributions and capital in time t while R_τ is public market gross return over period τ. While PME is typically calculated using all cash flows associated with a fund (i.e., the full life of a fund), our analysis requires the use of an interim measure of performance. Consequently, we define a measure of performance from fund inception through an interim date that is analogous to PME. Intuitively, we think of it as a measure of PME-to-date for any time $t*$, $0 < t* < T$. To construct the measure we simply consider the stated net asset value (NAV) at date $t*$ as a terminal distribution and ignore all subsequent cash flows. Thus, we can define PME-to-date at time $t*$ as

$$\begin{aligned} PME_{t*} &= \frac{\sum_{t=0}^{t*} (D_t + NAV_{t*}) \prod_{\tau=t}^{t*} R_{\tau+1}}{\sum_{t=0}^{t*} C_t \prod_{\tau=t}^{t*} R_{\tau+1}} \\ &= \frac{\sum_{t=0}^{t*} D_t \prod_{\tau=t}^{t*} R_{\tau+1}}{\sum_{t=0}^{t*} C_t \prod_{\tau=t}^{t*} R_{\tau+1}} + \frac{NAV_{t*}}{\sum_{t=0}^{t*-1} C_t \prod_{\tau=t}^{t*-1} R_{\tau+1} + C_{t*}} \end{aligned} \tag{A.2}$$

To simplify notations, rewrite A.2 as:

$$PME_t = PME_t^{exNav} + \frac{NAV_t}{fv_t(C)}, \tag{A.3}$$

so that $fv_t(C)$ represents the time t future value of all capital calls calculated using the public market returns from the respective date of each capital call while PME_t^{exNav} is the PME-to-date value as of time t if NAV is assumed to be 0.

The change in PME-to-date from the previous period can be thought of as a product of the abnormal fund return over the period t and the ratio of NAV_t to the future value of cumulative capital calls to date. To see this note that, absent capital calls at t, from A.1 and A.2 it follows that:[35]

$$\begin{aligned} PME_t^{exNav} &= PME_{t-1}^{exNav} \cdot \frac{R_t}{R_t} \cdot \frac{R_{t+1}}{R_{t+1}} + \frac{D_t}{fv_t(C)} \cdot \frac{R_{t+1}}{R_{t+1}} \\ &= PME_{t-1}^{exNav} + \frac{D_t}{fv_t(C)} \end{aligned} \tag{A.4}$$

where we are adding the ratio of period t distributions to the period t value of cumulative capital calls to-date to PME_{t-1}^{exNav}. Since we can express reported return, R_t^{nav}, as a solution to

$$NAV_t = NAV_{t-1}R_t^{nav} - D_t + C_t, \tag{A.5}$$

the change in PME from $t-1$ to t can written as

$$\begin{aligned} \Delta PME_t &= PME_t^{exNav} - PME_{t-1}^{exNav} + \frac{NAV_t}{fv_t(C)} - \frac{NAV_{t-1}}{fv_{t-1}(C)} \\ &= \frac{D_t}{fv_t(C)} + \frac{NAV_t}{fv_t(C)} - \frac{NAV_{t-1}}{fv_{t-1}(C)} \cdot \frac{R_t}{R_t} = \frac{D_t}{fv_t(C)} + \frac{NAV_t}{fv_t(C)} - \frac{NAV_{t-1}R_t}{fv_t(C)} \\ &= \frac{NAV_t + D_t - NAV_{t-1}R_t}{fv_t(C)}. \end{aligned} \tag{A.6}$$

Therefore (substituting NAV_t from A.5 into A.6) a change in PME can be witten:

$$\Delta PME_t = (R_t^{nav} - R_t)\frac{NAV_{t-1}}{fv_t(C)}. \tag{A.7}$$

[35] The assumption that $C_t = 0$ applies through equation A.7 only and does not affect the intuition. If we drop this assumption, expression (A.7) will have three additional terms: $\frac{C_t}{fv_t(C)} + (k_t - 1)PME_{t-1}^{exNav} + (R_t^{nav} - R_t)\frac{NAV_{t-1}}{fv_t(C)}$,
where $k_t = \frac{fv_{t-1}(C)R_t}{fv_t(C)} \in (0,1)$ (e.g. for $t=3$, $k_t = [(C_1R_2 + C_2)R_3] / [C_1R_2R_3 + C_2R_3 + C_3]$.
Therefore, the first term is positive and tends to be large when $k_t \ll 1$, the second term has a negative sign and cancels out with the first term when $PME_{t-1}^{exNav} = 1$. The sign on the third term is negative while the magnitude increases in the first term too. We study the implications of this measurement error via a simulation.

The intuition behind this expression is that the excess return of the fund (as a difference between fund return as implied by *NAV*-change and the public market return) gets scaled down by the prior-period *NAV* as a percent of paid-in-capital adjusted for the market returns. Thus, keeping the mean and variance of excess return unchanged, one would observe a leveling-out in abnormal performance (as measured by PME-to-date) once a fund starts distributions, as the ratio of $NAV_{t-1}/fv_t(C)$ will typically drift downwards. That is, ΔPME_t will keep the sign but trend toward 0 over time, all else the same.[36] The same leveling-out will occur to the money-multiple ($TVPI$) which can be thought of as a special case of PME-to-date where R_τ is assumed to equal 1 for all τ.

When analyzing a cross-section of funds, the ΔPME_t is a useful metric since it effectively represents a weighting scheme for fund returns. The weight is proportional to the sensitivity of the performance-to-date to NAV. Multiplying the cross-sectional mean ΔPME_t by mean $NAV_{t-1}/fv_t(C)$ removes the downward bias due to the scale effect and obtains the average fund returns weighted by the fraction of unrealized NAVs in the market-return-adjusted sum of capital calls-to-date. The same re-weighting can be applied to mean money-multiple changes. Similarly, weighted-ΔPME_t nests mean fund *NAV*-returns and excess returns ($R_t^{nav} - R_t$) as special cases with $NAV_{t-1}/fv_t(C)$ being equal across funds in both cases (and market returns being zero in the former).

We design a Monte-Carlo experiment to study the time-series properties of weighted PME-to-date. We draw a fund's β from two normal distributions, $N(1, 0.125)$ and $N(2, 0.166)$ whereas α's come from a common distribution, $N(0.05, 0.05)$. Here α and β are in the context of the standard market model. The same Poisson process drives all cash flows independently of market and idiosyncratic shocks to returns. Figure A.2 suggests that a misspecification of fund-level β does not confound inference about the question of interest, i.e., the trajectory of cross-sectional mean abnormal returns. Also, it follows that if more successful funds (higher α) tend to not distribute capital as fast as their less successful peers, the null hypothesis for constant life-time excess returns should be a convex trajectory for WPME. This is because funds with higher excess returns tend to have relatively higher ratios of residual NAV-to-capital as fund life progresses. Introducing heteroscedasticity and reasonable correlations in the data generating process does not change these conclusions.[37]

I.1. Monte Carlo Experiment

Because our weighted *PME* change measure of returns has not been utilized in previous studies, we conduct a series of Monte-Carlo experiments and examine how this measure of excess returns compares to simpler measures based on raw returns and money-multiples that we show to be its special cases. For our

[36] Again, with net-negative cash flows in period t the expression get less clear but the intuition remains the same: ΔPME_t tends to be positive so long as $R^{nav} - R$ is positive. In simulation (Section I.1), we verify that the additional terms (when C_t are positive) do no affect the inference about the path of the PME to-date pooled over a cross-section of funds.

[37] For brevity, we do not report these results.

Monte Carlo experiments we assume that fund i asset value at time t ($V_{i,t}$) evolves as:

$$V_{i,t} = V_{i,t-1} exp\left\{\alpha_i + \beta_i r_{m,t} + e_{i,t}\right\},$$

where $\alpha_i = \bar{\alpha} + e_\alpha$ is the abnormal return for fund i; $\beta_i = \overline{\beta_{H(L)}} + e_{H(L)}$ is the level of systematic (factor) risk for fund i; $r_{m,t} = \mu + e_{m,t}$ is the net return on the market index; $e_{(\cdot)}$ are all independently drawn from a normal distribution $N(0, \sigma^2_{(\cdot)})$. For our experiments we let $\mu = 0.04\, per\, annum$ and $\bar{\alpha} = 0.05\, per\, annum$. The specification for β_i allows us to have funds with low risk ($\overline{\beta_L} = 1.0$) or high risk $\overline{\beta_H} = 2.0$). We set the standard deviations of $e_{(\cdot)}$ al follows: $\sigma_i = \sigma_m = 0.300\, per\, annum$; $\sigma_L = 0.125$; $\sigma_H = 0.167$; $\sigma_\alpha = 0.05$.

At time t fund i distributions, D_{it}, and contributions, C_t, are independent Poisson processes. The parameters of the cash flow process are calibrated so they closely match the cross-sectional moments of actual funds cash flows in our sample. Specifically, we set

$$D_s = V_s \varphi \eta_{ds}\ if\ s > \lfloor f_d \cdot T \rfloor$$
$$C_s = \varphi \eta_{cs}\ if\ s < \lfloor f_c \cdot T \rfloor,$$

where we set $T = 300$ as a fund maximum life in bi-weekly intervals, $\eta_{(\cdot)}$ are independent Poisson distributions $Pois(\lambda_{(\cdot)})$ with $\lambda_d = 0.1$ and $\lambda_c = 0.07$. We let $f_c = 0.5$, $f_d = 0.3$, and $\varphi = 0.2$.

For our experiment we draw 30 paths of market returns, $r_{m,t}$, at a daily frequency. For each market path we draw 40 α_i and β_i, half with a mean of $\overline{\beta_L}$ and half with $\overline{\beta_H}$. Given the set of α_i and β_i, we draw 40 paths of idiosyncratic returns at a daily frequency, and 40 paths of distributions and contributions at a bi-weekly frequency. We then construct the series of quarterly NAVs and cash flows for each market path. Finally, we compute PMEs-to-date for the simulated funds and average ΔPME_q and $NAV_{q-1}/fv_q(C)$ across all (30×40) market paths and funds. Results are presented in Figure A.2 and discussed in Appendix AI.

II. A proxy for NAV bias change

Central to our analysis is the idea that reported *NAV* can be a biased estimate of the true value. We next formulate our specific measure of the *NAV* bias that we examine in our empirical tests in section V. We start by defining V_t as the true (unbiased) asset value at the end of period t and Γ_t as a gross valuation bias such that reported $NAV_t \equiv V_t \cdot \Gamma_t$. We next define the gross abnormal return in period t as $R_t^{\varepsilon} = exp\{\delta \cdot \varepsilon_t\}$ where δ is a constant (for a given fund) and ε_t is a mean-zero random error arbitrary distributed. If we further define $R_{\beta,t}$ as gross return due to risk factor (market) exposure β then,

$$V_t + D_t = V_{t-1} R_t^{\varepsilon} R_{\beta,t} + C_t. \tag{A.8}$$

Recalling that D_t and C_t are, respectively, the fund distributions and capital calls at t, we define the evolution of the gross valuation bias as $\Gamma_t = \Gamma_{t-1} e^{g(\cdot)}$. Substituting this definition into A.8 yields the following NAV identity:

$$NAV_t = NAV_{t-1} R_t^{\varepsilon} R_{\beta,t} e^{g(\cdot)} + \Gamma_{t-1} e^{g(\cdot)} (C_t - D_t). \tag{A.9}$$

We assume that returns $R_{\beta,t+1}$ and ε_{t+1} are unpredictable. We would like to estimate per period change in bias, $g_i(\cdot)$, for each fund (henceforth we add subscript i to each variable) from the following model:

$$log\left[\frac{NAV_{i,t}}{NAV_{i,t-1} R_{\beta_i,t} - \frac{\Gamma_{i,t-1}}{R^{\varepsilon} i,t}\left(D_{it} - C_{it}\right)}\right] = g(\cdot)_{i,t} + \delta_i + \varepsilon_{i,t}. \tag{A.10}$$

Since we have relatively few observations per fund and do not know β_i and $\Gamma_{i,t-1}/R_{i,t}^{\varepsilon}$, a feasible alternative to estimating A.10 is an average effects linear panel model:

$$\widetilde{\Delta bias_{it}} \equiv log\left[\frac{NAV_{i,t}}{NAV_{i,t-1} R_{\beta=1,t} - D_{it} + C_{it}}\right] = \gamma' X_{i,t} + \delta_i + \eta_i + \varepsilon_{i,t} + \zeta_{i,t}, \tag{A.11}$$

where η_i and $\zeta_{i,t}$ are (additional to δ_i and $\varepsilon_{i,t}$) fund fixed effects and disturbance shocks that arise due to the mismeasurement of the left-hand side and the misspecification of the right-hand side of A.11 relative to A.10. We note that the measurement error also constrains the set of covariates $X_{i,t}$ to not be contemporaneously correlated with market returns and fund cash flows, D_{it} and C_{it}.

Unlike in A.10, the expression in the logarithm in A.11 is not guaranteed to be positive. Therefore, in our implementation we Winsorize the values at the 2% level which results in all arguments for the log being greater than zero in our sample. In addition, we drop fund-quarters where ending Net Asset Values represent less than 2% of capital committed, and fund-quarters where the previous available report was more than one quarter ago.

To verify that A.11 is a sensible estimator of γ, the average bias loading on the covariates of interest, we also use a placebo dependent variable constructed as follows:

$$\widetilde{\Delta bias}_{it}^{placebo} \equiv log\left[\frac{NAV_{it}R_{\{FF100\},t}}{NAV_{it}R_{\beta=1,t}-(R_{\{FF100\},t}-R_{\beta=1,t})(D_{ti}-C_{it})}\right] \tag{A.12}$$

where $R_{\{FF100\},t}$, referred to $R_t^{placebo}$ in the main text, is the return in period t of a public equity portfolio constructed from Fama-French 100 U.S. Equity Research Portfolios (henceforth, FF100). We randomly select a subset of the FF100 portfolios and take average returns for these to generate a placebo return series for a specific fund. Once assigned, the portfolio remains the same across all periods for the given fund. For buyout funds we limit our selection to the subset of FF100 that includes only the 25 highest Book-to-Market portfolios out of the 50 lowest market value portfolios and scale (lever) each return series by a factor of 2 (by taking gross returns squared).

For venture funds we select returns from the 25 lowest Book-to-Market portfolios out of the 50 smallest market value portfolios. In the random placebo portfolio matching, we only condition on placebo to-date returns for a given fund being in the same tercile among its peers as the actual fund IRR as of the 28^{th} quarter since inception.[38] Peers are funds incepted in the same or adjacent vintage years and having the same strategy (Buyout, Early Stage Venture, Biotech Venture, Other Venture).

We arrive at the expression for $\widetilde{\Delta bias}_{it}^{placebo}$ by substituting $NAV_{it}/R_{\{FF100\},t}$ for NAV_{it-1} in A.11 in order to obtain the growth in NAVs from the previous period that would have occurred if $R_{\{FF100\},t}$ had been the return generating process. In addition, A.12 allows us to test whether the cash flow dependency of the disturbance term in A.11 is sufficiently attenuated by controlling for concurrent cash flows. Just as for $\widetilde{\Delta bias}_{it}$, we Winsorize the right-hand side of the expression at the 2% level before taking the log.

[38] or the last quarter in the sample for funds younger than 28 quarters as of the sample end date, December 2011

Figure A.1: Why not simply plot IRRs since inception? A Simple Case Study

This figure illustrates the inconsistency of IRR-to-date for the purpose of NAV bias assessment by studying two hypothetical cash-flow and abnormal return patterns (i.e., funds) described in Appendix AI. Panel A plots the alpha and the cash-flow patterns for both cases. Panel B plots the total return to the S&P 500 index over each hypothetical fund's life (rescaled to 1.0 at inception). Panel C plots the resulting PMEs-to-date and IRRs-to-date.

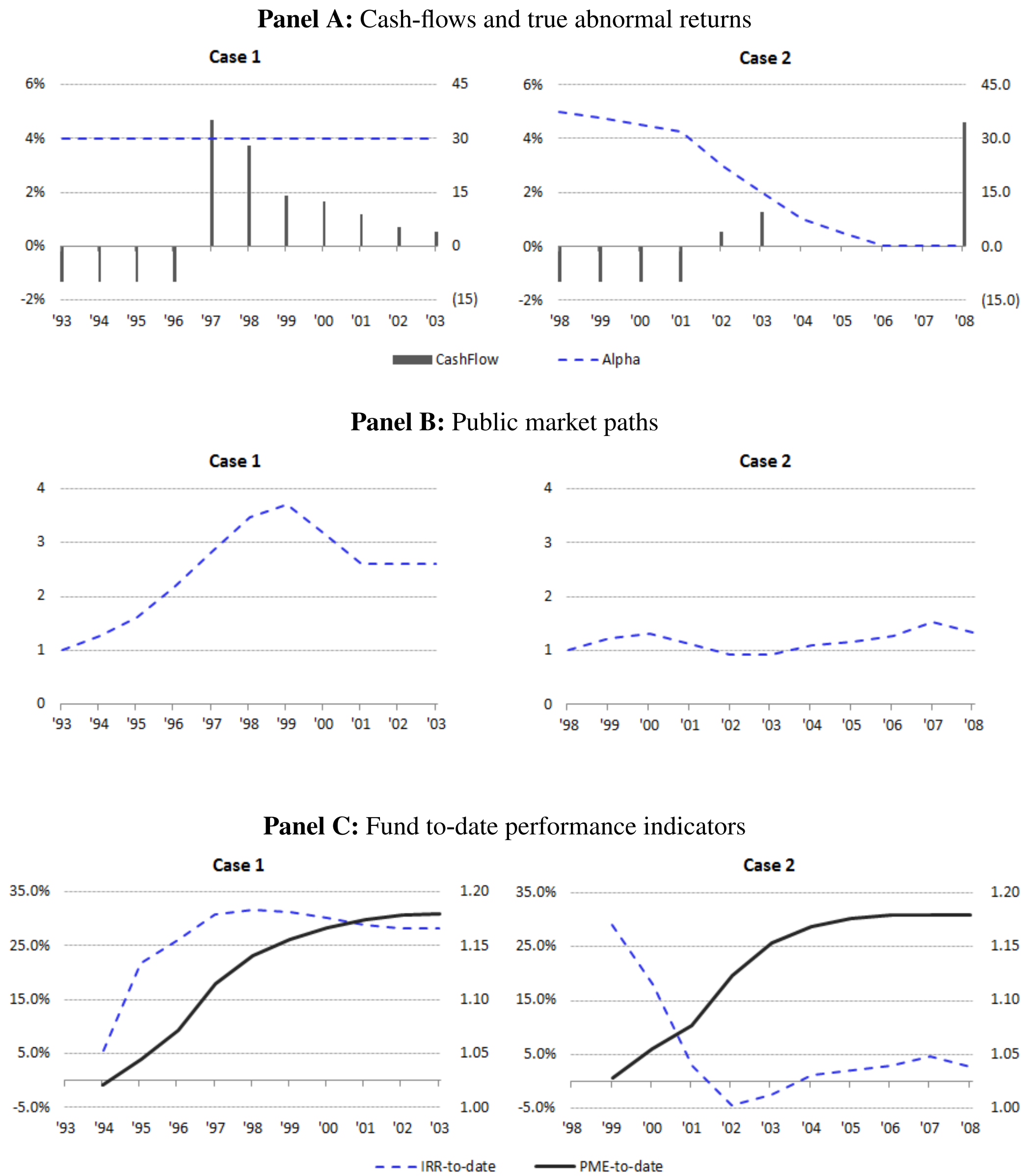
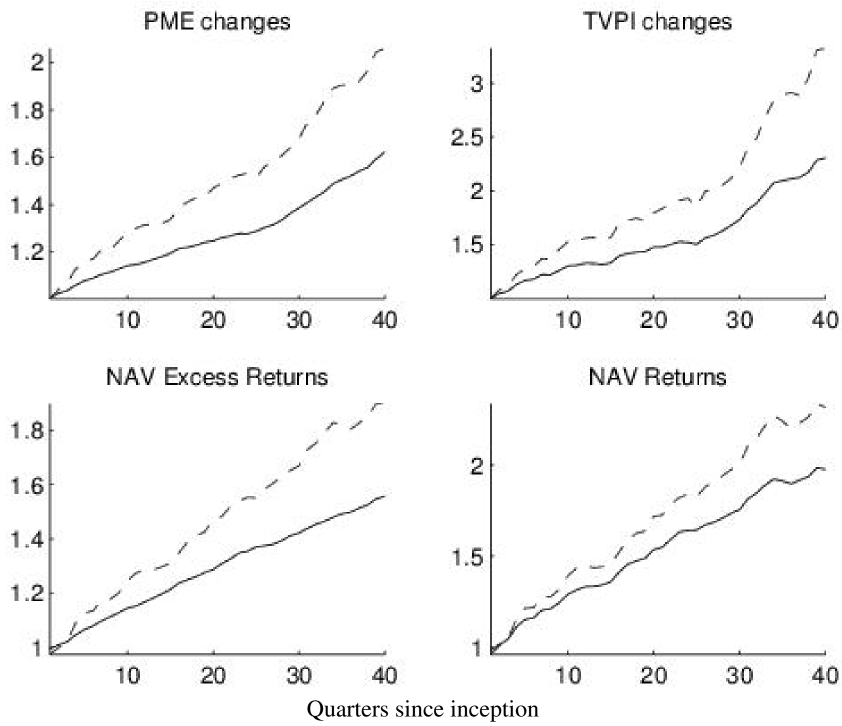


Figure A.2: Average Fund Performance Paths: Simulated Data

This figure reports results of the Monte Carlo Experiment described in Appendix AI.1 to suggest a null hypothesis appropriate for average fund to-date performance as measured by the proposed metric: weighted-PME cumulative changes. A change in a given quarter is a weighted average of PME-to-date changes from the previous period across the simulated funds for a given quarter since inception. The weights are ratios of NAV to cumulative capital calls since inception adjusted for market returns. The simulated funds differ by their market betas and abnormal returns. Fund cohorts have different market return paths as well. The solid line represents the mean over 600 funds drawn from a distribution with a high mean β. The dashed line stands for the mean over 600 funds drawn from a distribution with a low mean β. The top-right panel reports weighted money-multiple cumulative changes while bottom-left(right) panel reports mean NAV excess(raw) returns. All are shown to be a special case of the NAV-weighted PME change in Appendix AI.



Appendix B. Supplementary Figures and Tables

Table B.1: Performance Tercile Transition Probabilities: *PME*

This table reports transition probabilities between PME-to-Date terciles within each fund peer group estimated separately for buyout and venture funds in Panel A and Panel B respectively. The first row of each panel reports the probability of being in the respective to-date tercile at the end of a funds life (*At Life End*) conditional on being in the bottom to-date tercile in the quarter preceding the follow-on fund started its capital calls (*At Fundraising*). The second(third) row reports *Life End* conditional on being in the middle(top) *At Fundraising* tercile. The last row of each panel reports the unconditional distribution of funds across *At Life End* terciles, while the last column reports how many funds were in each fundraising tercile and the respective fraction in total fund count. The peer group is all funds of the same strategy incepted within one year from the fund vintage year. Since peer groups overlap, and follow-on fundraising at different points of life of the current funds, and fund duration varies, neither *At Fundraising* nor *At Life End* terciles need to have an equal number of funds.

Panel A: Buyout

		At Life End				
		Btm	Mid	Top	Fund Count	
At Fundraising	Btm	61.2%	26.9%	11.9%	67	(18.8%)
	Mid	36.9%	42.3%	20.8%	130	(36.5%)
	Top	13.2%	25.2%	61.6%	159	(44.7%)
	All	30.9%	31.7%	37.4%	356	(100%)

Panel B: Venture

		At Life End				
		Btm	Mid	Top	Fund Count	
At Fundraising	Btm	56.7%	31.3%	11.9%	67	(21.8%)
	Mid	32.0%	43.2%	24.8%	125	(35.8%)
	Top	10.4%	21.6%	68.1%	214	(42.4%)
	All	26.8%	31.0%	42.2%	355	(100%)

Table B.2: Performance Quartile Transition Probabilities: *IRR*

This table reports transition probabilities between IRR-to-Date quartiles within each fund peer group estimated separately for buyout and venture funds in Panel A and Panel B respectively. Only the funds that have raised a follow-on fund within ten years since inception are included. The first row of each panel reports the probability of being in the respective to-date quartile at the end of a funds life (*At Life End*) conditional on being in the bottom to-date quartile in the quarter preceding the follow-on fund started its capital calls (*At Fundraising*). The forth row reports *At Life End* conditional on being in the top (*At Fundraising* quartile. The second(third) row does so for the middle quartile that is closest to the bottom(top). The last row of each panel reports the unconditional distribution of funds across *At Life End* quartiles, while the last column reports how many funds were in each fundraising quartile and the respective fraction in total fund count. The peer group is all funds of the same strategy incepted within one year from the fund vintage year. Since peer groups overlap, and follow-on fundraising at different points of life of the current funds, and fund duration varies, neither *At Fundraising* nor *At Life End* quartiles need to have an equal number of funds.

Panel A: Buyout

		At Life End					
		Btm	3rd	2nd	Top	Fund Count	
At Fundraising	Btm	55.0%	20.0%	17.5%	7.5%	40	(11.2%)
	3rd	40.7%	34.1%	16.7%	6.6%	91	(25.6%)
	2nd	16.4%	22.7%	40.0%	20.9%	110	(30.9%)
	Top	12.2%	9.6%	22.6%	55.6%	115	(32.3%)
	All	25.6%	21.1%	26.4%	27.0%	356	(100%)

Panel B: Venture

		At Life End					
		Btm	3rd	2nd	Top	Fund Count	
At Fundraising	Btm	48.1%	27.3%	20.8%	3.9%	77	(15.1%)
	3rd	33.0%	36.5%	20.9%	9.6%	115	(22.5%)
	2nd	18.5%	23.8%	29.1%	28.5%	151	(29.6%)
	Top	7.2%	11.4%	23.9%	57.5%	167	(32.8%)
	All	22.5%	23.1%	24.3%	30.0%	510	(100%)

Table B.3: Cross-Section of To-Date Performance: *Placebo*

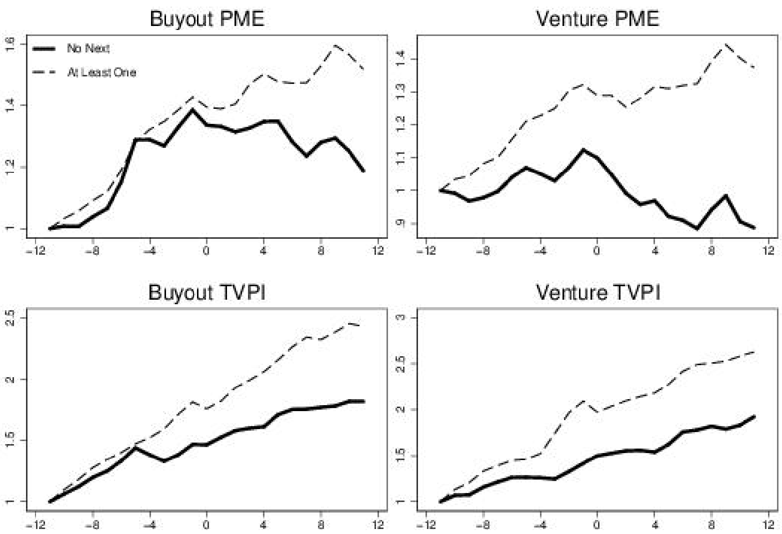
This table reports the parameter estimates a linear regression model estimated separately for buyout (Panel A) and venture (Panel B) funds. The dependant variable measures a fund adjusted return for quarter t if its NAVs were tracking a same style public equity portfolio based Fama-French 100 U.S. equity portfolios (Appendix A provides details). *FundTiming* is the natural log of one plus, essentially, time spent to-date without a follow-on fund in excess of two years. Specifications (1) through (4) have *PeerChasing* is a difference between fund i to-date average public portfolio cumulative return-to-date for the calendar quarter corresponding to $t-1$ quarter of fund i life and that of its peers. *Rookie* is a dummy for whether the PE firm had less than two funds before i. *Top* and *Btm* are dummies denoting if to-date return of the assigned public equity portfolio was in Top(Bottom) tercile by return-to-date as of quarter $t-1$ among those assigned to the fund peers. Control variables in all specifications include funds fixed effects, year-quarter fixed effects as well as fund distributions and capital calls over the current quarter scaled by the end of quarter NAVs. t-statistics reported in parentheses are robust to heteroskedasticity and autocorrelation, */**/*** denotes significance at 10/5/1% confidence level.

	Panel A Buyout				**Panel B** Venture			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
FundTiming	−0.002	0.013	0.007	0.018	0.017	−0.013	−0.005	−0.013
	(−0.08)	(0.51)	(0.36)	(0.61)	(0.73)	(−0.76)	(−0.35)	(−0.61)
PeerChasing	0.039	0.014	0.031**	0.015	−0.011	0.007	−0.009	0.001
	(1.26)	(0.72)	(2.00)	(0.67)	(−0.38)	(0.48)	(−0.56)	(0.06)
Rookie × FundTiming		0.011		0.005		0.007		0.008
		(0.32)		(0.15)		(0.28)		(0.32)
Rookie × PeerChasing		0.009		0.008		−0.001		0.006
		(0.34)		(0.32)		(−0.06)		(0.24)
TopTercile-to-date			−0.030*	−0.029*			−0.008	−0.008
			(−1.86)	(−1.79)			(−0.64)	(−0.67)
Top × FundTiming			0.049	0.036			−0.018	−0.015
			(0.99)	(0.71)			(−0.52)	(−0.41)
Top × PeerChasing			−0.011	−0.000			0.046**	0.035
			(−0.32)	(−0.01)			(2.03)	(1.42)
BtmTercile-to-date				0.003				0.002
				(0.19)				(0.15)
Btm × FundTiming				−0.033				0.011
				(−0.91)				(0.40)
Btm × PeerChasing				0.078				−0.044
				(1.10)				(−1.21)
Controls	Fund FE, Year-Qtr FE, Cash-Flows							
Observations	12,150	12,150	12,150	12,150	15,131	15,131	15,131	15,131
R-squared	0.467	0.477	0.436	0.436	0.191	0.194	0.169	0.169

Figure B.3: The "No Next"-fund Puzzle

This figure reports cumulative NAV-weighted returns of private equity funds over the public market index. Panel A plots values since since the 25th quarter preceding the minimum over the fund resolution or the 10th anniversary (x=-12). Panel B plots values since the quarter that corresponds to inception plus the median time it took to raise a follow-on fund by the vintage-year peers. As described in equations (1) and (2), the change in a given quarter is a mean PME(TVPI)-to-date change from the previous period across a subset of funds multiplied by the average ratio of NAV to market-adjusted paid-in capital (to date). Appendix A shows that PME-based variable equals to a weighted-average excess return and that the inference about the path is robust to funds risk misspecification while the TVPI-based path is a special case where the market return is assumed to be zero. *No Next* (*At Least One*) denotes the subset of funds without (with) at least one follow-on fund for this firm-fund in our sample. For robustness, we exclude 2008Q2-2009Q2 return quarters.

Panel A: Event = 12 Quarters Before the Resolution



Panel B: Event = 12 Quarters After the Median Fundraising

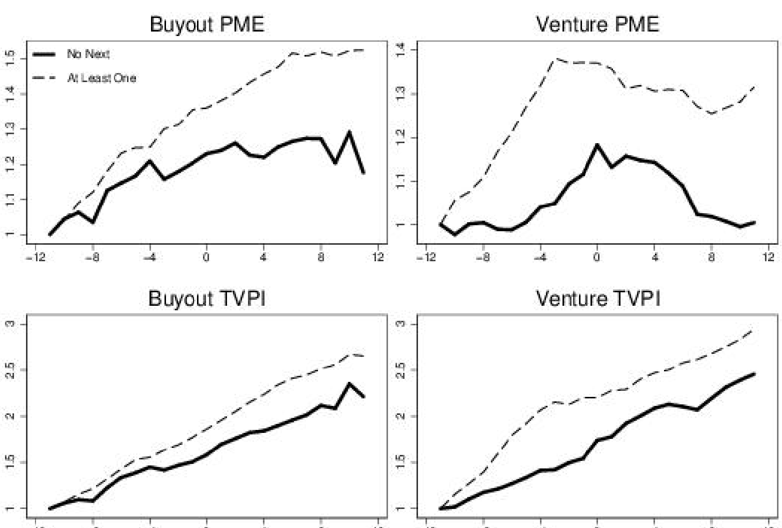
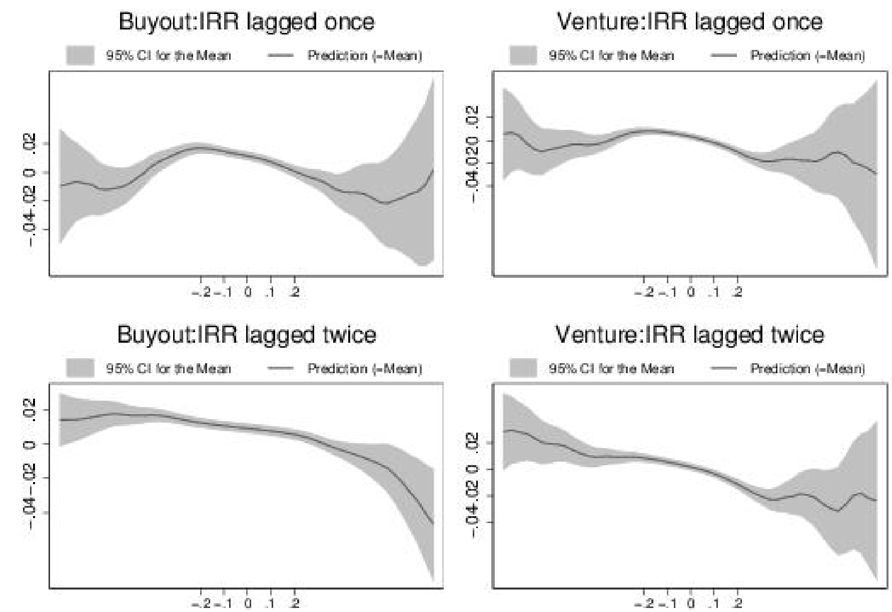


Figure B.4: Peer-Chasing: Non-Parametric Evidence

This figure reports local polynomial regression fits of fund excess returns on lagged to-date IRR relatively to that of peer median separately for buyout and venture funds. Reported returns orthogonalized with respect to fund cash flows are in Panel A with one and two period lagged IRR being in top and bottom row respectively. Panel B reports similar exercise based on placebo returns. Placebo returns are constructed using subsets of Fama-French 100 U.S. Equity research portfolios as described in Appendix A.

Panel A: Actual Data



Panel B: Placebo - Public Equity Portfolios