

Do Private Equity-Related Operational Improvements Persist After Exit? Evidence from Swedish IPOs

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Abstract. Prior evidence on the association between private equity (PE) sponsorship and portfolio company performance has predominantly focused on the holding period of the PE sponsor, and on market-based proxies of performance rather than fundamentals. Using a sample of 43 PE-backed and 105 non-PE-backed Swedish IPOs, we run two multivariate OLS regressions to test whether the operational (fundamental) performance levels of PE-backed firms persist after the sponsor exits via IPO, first relative to non-PE-backed IPO peers, and second relative to PE-backed firms' own holding-period performance levels. We find the clearest evidence of performance persistence in net sales, our proxy for the scale of customer-facing activity. PE-backed firms in our data outperform non-backed peers both before and after IPO, and sales *within* PE-backed firms are higher after exit (IPO) than during the holding period. Profitability results are more mixed, while efficiency and liquidity provide little support for persistence. Overall, our findings suggest that post-exit performance persistence among PE-backed firms is concentrated primarily in the scale of customer-facing activity rather than broad-based across fundamentals. Our study contributes to the PE literature by adding Swedish evidence on post-exit accounting performance, complementing prior work that has focused mainly on holding-period outcomes and post-exit market returns.

Keywords: *private equity; IPO exit; operational performance; post-exit persistence*

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1. Introduction

“You can’t have a trace of sentimentality. You have to be able to slash and burn.”

– Bryan Burrough (2024), co-author of *Barbarians at the Gate*, quoted in Blasdel (2024)

From its origins in the leveraged-buyout wave of the 1980s, private equity (PE) has grown into one of the central organising forces of modern capitalism. By the end of 2024, PE investors managed almost US\$8.6 trillion in assets, which is an almost tenfold increase from their level two decades earlier (Preqin, 2025). In the same year, investments by PE and venture capital funds in Europe amounted to 0.54% of European GDP, about five times the EU budget for Security and Defence (Invest Europe, 2025; European Commission, 2026); and in the United States, the largest market for PE in the world, the corresponding figure is approximately 2.56% (American Investment Council, 2025; World Bank, 2025). In short, over the past few decades, and across the global economy, PE has taken on a role of increasing economic significance.

The industry, however, has not been without controversy. There have been a number of concerns to the effect that PE investors are overly oriented towards the short-term. Oxford University’s Ludovic Phalippou (2025), for example, notes that the typical structure of a PE buyout – in particular, its short holding periods and high levels of debt – gives managers back-ended incentives to increase profits and exit value, at the potential expense of the firm’s longer-term interests. There also exists alongside such concerns, however, a substantial literature which documents the benefits that PE investors may bring. We shall return to this body of work later. For now, it is sufficient to note that whether the industry is ultimately *net*-beneficial for its portfolio companies (or other stakeholders) remains an open and contested question in the academic literature.

It is at any rate private equity – this expanding and contentious form of ownership and, nested within that topic, the consequential empirical setting of Sweden, the most PE-intensive market in Europe (SVCA, 2025) – that our paper focuses on. Specifically, given the scarcity of evidence on how PE-backed firms fare after the PE sponsor’s holding period ends, our paper asks whether the operational performance of PE-backed firms persists after the sponsor exits via IPO, which is the most common exit route in Sweden (OECD, 2025). We find that formerly PE-backed firms continue to outperform on net sales, relative both to their own holding-period levels and to non-PE-backed IPOs. We also find, interestingly, that

profitability is only superior to that of non-PE-backed peers *after* exit, while for efficiency and liquidity we find little support for either holding-period outperformance or post-exit persistence. Our findings contribute to the wider PE literature by bearing on the debate over whether PE creates lasting operational improvements at the portfolio-company level. They suggest that such persistence is concentrated in the scale of customer-facing activity, proxied by net sales, rather than extending uniformly across all operational fundamentals.

1.1 But what is private equity?

In broad terms, PE refers to specialised investment firms that acquire ownership stakes in companies that are not publicly traded. Our paper focuses specifically on one of two forms of PE – namely buyouts, as distinct from venture capital – since buyouts are more likely to involve the sponsor acquiring a controlling equity stake in the company (hitherto and henceforth referred to as “PE-backed firm” or “portfolio company”). This choice is intended to make it more plausible that changes in portfolio-company operational characteristics can be attributed to PE sponsorship itself, rather than to other factors (Michala, 2019).

The typical aim of a PE buyout is to improve the business and subsequently exit after a finite holding period at a higher valuation. Kaplan and Strömberg (2009) suggest that the primary levers through which PE sponsors can and have historically delivered such improvements are:

- I. through *leverage* and relatively large and illiquid *management ownership stakes*, which may sharpen managerial incentives and impose discipline;
- II. through *concentrated ownership* and *active board oversight* (common features of PE sponsorship; cf., Cornelli and Karakas, 2008), which may reduce agency problems;
- III. and through *hands-on operational involvement*, which may improve the strategy, organisation and ultimately the productivity and profitability of PE-backed firms.

Yet if this is the theory, the way these levers actually manifest themselves in firm-level improvements has largely remained opaque. As Biesinger et al. (2020, p. 4) put it, PE-related value creation has long been, as it were, a “black box” to outside observers. It has proved empirically challenging to link the documented holding-period outperformance of PE-backed firms to the specific mechanisms through which these gains are generated, and it also remains unclear whether such outperformance endures over the long run. On the second point, Kaplan and Strömberg (2009, p. 143) on balance hold an optimistic view. They go so far as to suggest

that PE has a “substantial permanent component”. As they also acknowledge, however, their suggestion is speculative. The question about the post-exit persistence of PE-driven operational improvements – as distinct from their existence during the holding period – is indeed still a point on which there does not appear to be a consensus in the literature. It is, as such, the point with which the present paper is chiefly concerned.

1.2 How should we think about private equity?

Based on what we have said so far, one of the main controversies in the debate about PE appears to concern whether PE sponsorship sustainably improves portfolio company performance (besides the broader question of whether any such improvements are socially desirable once their consequences for employees and other non-investor stakeholders are taken into account). Informed by Ljungquist’s (2024) review of the industry, one can, for present purposes, distinguish between two broad positions on the first of these two questions.

The first – call it the “persistence view” – adopts a relatively favourable position. Its adherents tend to emphasise that the academic literature has repeatedly found PE to generate strong returns for investors, as well as implement operational changes at portfolio companies which have tended to correlate with higher market valuations in other contexts. For reasons we will elaborate on in our literature review, the persistence view also expects operational changes of this kind to be sufficiently robust to persist long-term. In support of it, Biesinger et al. (2020) – summarising earlier PE literature – note that academic research has often found PE investors to earn returns after fees in excess of those available in public equity markets, and to have materially improved their portfolio companies’ operating performance prior to exit. They also find, in their own study, some evidence in support of longer-term persistence.

A second position, however – call it the “convergence view” – is less persuaded that improvements observed during the holding period should be read straightforwardly as evidence of durable value creation. This is a view which does not necessarily deny that PE can improve operational performance for portfolio companies or deliver attractive investor returns. Its worry is rather that such gains may prove temporary, may be realised within a relatively compressed (opportunistic/exit-oriented) horizon, or may come at the expense of longer-run and harder-to-measure forms of value. We shall discuss the theoretical and empirical foundations underlying both of these positions in our literature review.

Of course, reality is more complicated than this “persistence-convergence” dichotomy suggests. No doubt many observers occupy an intermediate position. They accept that PE can bring about operational improvements and strong returns for investors, but remain hesitant to infer from this that PE is unambiguously value-creating in the long run and/or in the welfare economics sense of the term (see, for example, the nuanced remarks that Phalippou, 2025, makes in this respect). The dichotomy is, in other words, a conceptual device introduced to facilitate the reading experience. We trust the reader knows that the world, and the human beings in it, are not so Manichean.

1.3 Research gap addressed

Having said this, it is important to be clear about the scope of our paper. We are not seeking to adjudicate the broader axiological question of whether PE is welfare-enhancing or value-creating. The theme that concerns us here is empirical and more narrowly situated within accounting. Specifically, we want to examine whether the operational performance levels associated with PE ownership, as captured by accounting fundamentals, persist after the sponsor exits its position in the firm.

1.3.1 First aspect: scarce evidence on post-exit performance

The reason this question interests us is twofold. First, while the literature remains divided on the broader welfare implications of PE, it has been extensively documented that PE-backed firms tend to outperform their non-PE-backed peers during the holding period. As alluded to above, numerous studies (e.g., Biesinger et al., 2020; Acharya et al., 2013; Guo et al., 2011; Boucly et al., 2011) document improvements in operational metrics such as return on assets and sales prior to exit. By contrast, *evidence on what happens after exit is considerably less clear*. In this sense, both the persistence and convergence views find some support in the data and in the lack thereof. The former is vindicated by the evidence of holding-period improvements in portfolio company fundamentals, and the latter by the uncertainty surrounding the persistence of such improvements. One natural way of helping to adjudicate between these positions, it therefore seems, is to examine post-exit performance.

1.3.2 Second aspect: performance proxied by market returns, not fundamentals

Second, *the existing literature on post-exit performance has, to a large extent, approached this question through the lens of market-based measures* (e.g., excess stock returns after IPO exit; see, for example, Levis, 2011; MacArthur & Lerner, 2020; Kalbe & Warin, 2020). As to

the observed outcomes of this approach, the evidence is somewhat mixed, though the balance of findings suggests that formerly PE-backed firms often continue to exhibit positive abnormal returns relative to comparable non-backed firms, at least over certain horizons (Kalbe & Warin, 2020). Yet this approach raises a concern. Market-based metrics are inherently sensitive to investor sentiment, expectations, and information frictions, and may therefore deviate from underlying fundamentals. In particular, the presence of a reputable PE sponsor may itself act as a signal of quality to the market, thereby inflating valuations independently of realised operational performance (cf., the seminal work of Michael Spence, 1973, to whom we owe the term *market signalling*). Such a “signalling effect” may persist beyond the exit event, and potentially obscure the extent to which observed excess returns reflect genuine and durable improvements in firm fundamentals as opposed to market perceptions thereof.

An approach based on accounting or operational metrics (profit margins, return on assets, sales, and so on) should therefore offer a more direct proxy for underlying performance. Despite this, there exists surprisingly little empirical evidence directly concerning the post-exit development of such measures. To the best of our knowledge, there are only two studies to date which explicitly examine whether the operational characteristics of PE-backed firms persist after exit: the first is the previously referenced paper by Biesinger et al. (2020), which focuses on Eastern Europe, and the second is a paper by Lavery & Wilson (2023), which adapts the econometric framework proposed by Biesinger et al. in a different empirical context, namely the UK.

Biesinger et al. (2020, p. 5) find that formerly PE-backed firms continue to exhibit higher sales and market shares in the five years post-exit, while also continuing to “employ significantly more people, pay higher wages, and operate more productively” than non-backed peers. Their findings suggest that PE sponsorship may indeed have long-lasting effects. Lavery & Wilson (2023) similarly provide evidence consistent with persistence in certain dimensions of portfolio company performance following PE exit (e.g., in sales and earnings), albeit with the nuance that portfolio company growth slows after exit, and that operating performance is not stronger post-exit than during the holding period.

Two studies alone, however, do not constitute a robust empirical literature. Indeed, the limited scope of existing evidence has itself been explicitly acknowledged, both by those who have contributed to it and those writing on parallel themes. Lavery & Wilson (2023, p. 33), for

example, describe their paper as “one of the first studies to offer empirical evidence on the performance of PE-backed firms post-exit”, and note that “further research is warranted”. Likewise, Kalbe and Warin (2020, p. 44), whose paper, like ours, looks at post-exit performance in the Swedish setting – but whose post-exit analysis, unlike ours, focuses on abnormal *market returns* rather than accounting performance – explicitly highlight as an open question “how the firm characteristics affected by [...] PE sponsorship develop after the owners’ exit”, recognising that their own research design does not address the post-exit development of operational performance. (Again apropos our discussion about taking, in this context, a market-based versus an accounting-based approach to proxying performance.)

Taken together, these considerations point to a clear research gap. While there exists a rich body of evidence on the performance of PE-backed firms during the holding period, and a growing literature on post-exit market performance, there is comparatively little evidence on whether any operational performance differentials associated with PE sponsorship will persist once that sponsorship ends. It is that gap which the present paper seeks to address.

1.4 Empirical scope & research question

Our ambition is to respond to these calls for further study by testing the post-exit persistence question, focusing on operational performance rather than market returns, in a new empirical setting. Since the small extant literature is confined to Eastern Europe and the UK, it remains unclear whether the documented persistence of operational performance reflects a more general feature of PE sponsorship, or whether it is contingent on the particular settings in which it has so far been observed.

These considerations help explain the relevance of the Swedish setting. Sweden not only differs in important respects from the UK and Eastern Europe (e.g., as it relates to corporate governance; cf., Henrekson & Jakobsson, 2012) – such that it is in effect an institutionally different context – but it is also an unusually consequential market for PE in its own right. According to SVCA’s *Investing in Sweden: Private Equity Activity 2024*, Sweden had the largest share of PE investments relative to GDP of any country in Europe in 2024. If the post-exit persistence documented in prior work is also observed in this setting, that would lend support to the view that such persistence is not context-bound. Beyond that, given the economic significance of PE in Sweden, the question seems an important one not only for researchers but also for firms, investors and policymakers in the Swedish context.

The purpose of this thesis is therefore to examine whether PE-backed firms continue to sustain their performance after their PE sponsor exits its position, specifically within a cohort of Swedish IPOs. The overarching research question guiding our paper is:

Is private equity sponsorship associated with superior levels of operational performance which persist after the sponsor exits?

1.5 What comes next

The remainder of the paper is structured as follows. Section 2 reviews the relevant literature in greater detail and develops the theoretical considerations that motivate our empirical approach. This is also where we present our hypotheses. Section 3 sets out the research design, including variable definitions, econometric specifications, information about sample construction and details on our data. Section 4 presents our results and diagnostics related to those results. Section 5 discusses their interpretation and limitations, as well as implications in light of the literature reviewed earlier. Finally, Section 6 sets out our conclusion.

2. Literature Review

Turning to the existing literature on PE and portfolio company performance, we begin by reviewing theories to help us develop *a priori* expectations about how PE sponsorship may affect operational performance, and whether any such effects persist after exit. We then review empirical work documenting what has actually been found against such expectations. The purpose is to provide a basis for hypothesis development grounded in both theory and evidence, and a framework through which to interpret our later results.

2.1 Theoretical literature

2.1.1 Should we expect PE sponsors to improve operational performance, and should we expect such improvements to persist?

A classic starting point to ground in theory what we have called the “persistence view” – the empirically supported view that PE is associated with improved holding period performance, and the more speculative view that performance-enhancing changes implemented by PE sponsors will persist into the long term – is Michael Jensen’s 1989 article *Eclipse of the Public Corporation*, which begins somewhat provocatively with the claim that, in many

sectors, “[the] public corporation has outlived its usefulness”. Jensen argues that the main weakness of the public corporation is the agency conflict between dispersed shareholders and managers, under which managers operating under weak oversight and with excess cash tend to retain free cash flow rather than distribute it to owners. The core claim is that new private ownership structures (“new” in late-20th century terms) – particularly LBOs and what he calls “LBO Associations”, which is to say PE firms – can mitigate this problem through leverage and concentrated ownership. In essence, the central advantage of PE, in this account, lies in its capacity to reduce the agency and free-cash-flow problems associated with the diffuse public corporation. Debt commits managers to deploy firm resources to productive uses rather than hoard or waste it, and concentrated ownership makes managerial replacement or strategic redirection easier; and this should ultimately lead to improved operational performance for PE-backed firms. Worth noting is that this argument does not directly pertain to persistence. It pertains mainly to the persistence view as it relates to *holding period* performance.

Kaplan and Strömberg (2009), whose work we referenced in the previous section, develop and elaborate Jensen’s argument systematically. These authors summarise the distinctive value proposition of PE as a combination of “financial, governance, and operational engineering”, and note that focus over time has shifted from the two former forms to the latter – i.e., that PE sponsors now engage in operational engineering to a greater extent than they have historically. Their article is also, throughout, a defence of the view that PE can generate genuine economic value, rather than merely redistribute it. To the extent that the focus of PE has indeed shifted towards operational engineering, one might reasonably expect improvements to emerge in measures tied to firm operations, at least while the sponsor is still actively involved in the firm. This expectation is further reinforced by Døskeland & Strömberg’s (2018, p. 17) point that the PE business model “is about engineering *significant* improvements in companies in a short period [emphasis added]”. Lastly, perhaps the strongest support for this expectation is that even Phalippou (2025), who we noted was more critical of PE, acknowledges that there are strong reasons to expect operational (especially profit-related) improvements in portfolio companies during the holding period. His reasoning is incentive-based and echoes Kaplan and Strömberg’s proposition that PE can sharpen managerial incentives through large, illiquid ownership stakes and the discipline they impose.

Taken together, there are good *a priori* reasons to expect stronger operating performance among PE-backed firms than among non-PE-backed peers during the holding period, and this is a point on which the literature appears to be in broad agreement.

The more difficult theoretical question is what should happen after exit. As noted earlier, one suggestion that Kaplan and Strömberg (2009) put forth is that a large part of PE-driven improvements ought to persist into the long run. This “persistence view” is reinforced by their observation that PE sponsorship is not always as short-horizon as critics suppose, in the sense that the holding periods of PE sponsors have increased over time (which seems to run counter to the contention that sponsors may prefer to flip their investment for a quick return rather than make durable changes to operations). It is also reinforced by the high likelihood that, when the PE sponsor decides to exit, prospective investors are not indifferent to the durability of portfolio company performance. They have little reason to pay for improvements that appear likely to disappear once the sponsor has left. Lastly, if PE sponsorship is capable of installing, as the authors suggest, improved cost discipline and more effective governance routines – on top of reorienting strategy in ways that become embedded in the PE-backed firm – then one would expect at least some post-exit persistence in accounting performance.

As noted earlier, however, a competing view points instead toward *convergence with peers* after exit, which is to say a deterioration in performance relative to holding period levels and relative to non-PE-backed peers. One can usefully frame this alternative “convergence view” through Christensen et al.’s (2008) reformulation of the classic principal-agent problem as an “agent-agent problem”, as we shall proceed to do in what follows.

Drawing on the agency framework conventionally associated with Jensen and Meckling (1976), Christensen et al. argue that the traditional principal-agent lens is increasingly ill-suited to a financial system in which the supposed principals are themselves often intermediated actors (i.e., agents – e.g., institutional investors managing the assets of their clients; hence “agent-agent problem”). The relevant concern is that the performance of such actors is assessed over relatively short time horizons, so that the benchmark for success ceases to be the long-run health of the enterprise, and becomes instead the improvement of short-term financial metrics by which both “principal” and agent are evaluated. Applied to our context: it may well be that PE sponsors mitigate the classic agency- and free-cash-flow problems emphasised by Jensen (1989). But that does not mean that they mitigate the more systemic short-term incentive problem that Christensen et al. have in mind. Indeed, if the principals to whom PE sponsors answer are themselves *de facto* agents who act on behalf of other principals, and are evaluated over relatively short time horizons, then both sides of the relationship may be oriented primarily towards the short term rather than the portfolio company’s long-run development. And if so, the operational gains observed during the

holding period may reflect short-term performance management rather than durable operational improvement, and hence be less robust than the persistence view assumes.

This way of framing the convergence view, however, is admittedly quite broad. It is broad in the sense of structural, in that its focus lies with the wider financial system. A broad focus of this kind situates PE within what is potentially a larger paradigm shift in the governance of firms; but it does risk becoming overly abstract if left there. Let us therefore narrow the aperture and consider what, at the level of the portfolio company itself, might cause operational gains realised during PE sponsorship to weaken once the sponsor exits.

One such firm-level reason is that, as Lavery & Wilson (2023, p. 2) put it, PE sponsors typically “buy to sell”. This largely echoes the argument above, albeit on a narrower level. The purpose of PE sponsorship, essentially, is never indefinite stewardship of the firms that they acquire, but is instead the improvement of those firm characteristics most likely to facilitate a profitable exit. Of main relevance to the argument, such improvements may not be those that are most conducive to long-term performance. For example, a sponsor preparing for exit may boost short-term margins by reducing discretionary expenditures such as R&D, employee training, maintenance or marketing. These cuts may improve reported performance before sale, but at the cost of weakening the portfolio company’s longer-run capacity to innovate, retain capabilities or sustain growth. And if that is so, operational improvements observed during the holding period may ultimately prove fragile after exit, since they may partly reflect an intertemporal trade-off more than they reflect an improvement in the company’s underlying operating capacity.

Lastly, a final reason to expect convergence follows from the very mechanisms invoked by the *pro*-persistence camp. If superior performance under PE is driven by concentrated ownership, active board oversight, and sponsor-imposed discipline, then the dissipation of those features after exit may itself imply partial reversion. In other words, if the source of outperformance is sponsor-specific monitoring rather than capabilities internalised by the portfolio company, convergence after exit would be theoretically unsurprising.

A tentative verdict on which of the two views seems most borne out by the literature will be provided at the end of Section 2.2.2.

2.1.2 Clarifying concepts: what do we mean by operational performance?

The two existing studies which have looked at post-exit operational performance (Lavery & Wilson, 2023; Biesinger et al., 2020) use a wide array of measures to gauge firm operations. Only Biesinger et al. explicitly separate these measures into groups, which we find useful for the sake of conceptual clarity. To this end, we specify three buckets of operational performance, namely: (i) *scale of demand*, (ii) *efficiency & liquidity* and (iii) *profitability*.

Our *scale of demand*-bucket consists of **net sales**, the realised volume of a firm's revenue-generating activity with customers. As Wagenhofer (2014) notes, in the case of some companies (e.g., those with a history of losses), revenue may be viewed by outside users of accounting information as a more important indicator of firm value than profit-related measures. We take this to suggest that it can be used as a leading indicator of operational performance more broadly. The metric is also useful to consider alongside profitability measures since revenue is generally more difficult to manage than earnings, which can be affected through a wider range of accounting choices and expense-related adjustments (Chandra and Ro, 2008). For our purposes, however, net sales will primarily serve as a proxy for scale of demand – hence the name of the bucket it belongs to. We shall use it to assess whether PE-backed firms exhibit a higher level of customer-facing operating activity than their peers, and whether any such difference persists after exit.

As to the *efficiency & liquidity* category, we will look at **asset turnover** and the **current ratio**. These are standard accounting-based measures of operating efficiency and liquidity, commonly used in financial statement analysis (Robinson et al., 2020). Note that the former relates to efficiency, while the latter relates to liquidity. Asset turnover, defined as net sales divided by total assets, captures how effectively a firm uses its asset base to generate revenue. The current ratio, defined as current assets divided by current liabilities, captures short-term liquidity. As Robinson et al. (2020) note, a higher current ratio can be interpreted as reflecting a greater ability to meet short-term obligations. Taken together, we shall use these two measures to assess whether PE-backed firms differ from non-PE-backed peers in terms of operational efficiency and short-term financial flexibility.

Finally, with regard to the last category, it is important that we distinguish between profit and *profitability*. Johansson and Runsten (2023, p. 11) point out that absolute profit “is meaningless as a measure of profitability”, since in order to serve as a valid point of peer comparison it must be put in relation to something – be it sales (as in the case of profit

margin) or some capital base (e.g., return on assets). We shall in our own study use **return on assets** (ROA) and the **operating margin** as profitability indicators. ROA measures operating income generated per unit of assets (how effectively a firm uses its resources to produce profits). It is more reflective of operational performance than e.g., return of equity in virtue of being less affected by capital structure. The measure is a common proxy for firm performance (Johansson & Runsten, 2023; Bhagat & Bolton, 2008), and Phalippou (2025), specifically discussing PE portfolio company performance, identifies it as among of the more meaningful metrics for assessing operational performance in that it takes capital into account. Our second profitability measure is operating margin, defined as operating income (EBT less interest expense, where the latter is a negative item in our data), divided by net sales. We use operating income rather than net income in the numerator because our concern is with operating performance rather than financing structure, tax effects or other below-the-line items. Operating margin therefore captures how much operating profit a firm generates per unit of sales, and complements ROA by relating profitability to revenue rather than to the asset base. All of the metrics outlined here will serve as dependent variables in our analysis. A summary list can be found in the Appendix, Exhibit A.

Having now outlined some theoretical views on what we might expect regarding the presence of a PE sponsor and its relation to operational performance, and having also outlined our working definition of operational performance, let us now move on to ascertain what empirical support the persistence view and the convergence view may respectively appeal to.

2.2 Empirical literature

2.2.1 Holding period evidence

It is more or less an economic truism that *past performance does not predict future returns*. Analogously, for our purposes, it may be that *what happens during the holding period stays in the holding period*; that the documented evidence on holding period improvements is not predictive of the long term. So why consider holding-period evidence at all, given that our paper concerns post-exit persistence? We argue that this evidence may prove useful for informing our expectations about what, if anything, there may later be to persist.

One contribution in this regard is Acharya et al. (2013). Using a sample of 395 deals by 48 European PE funds between 1991 and 2008, they show that portfolio companies grow sales

and profitability faster during the holding period than the median listed firm in the same sector. These findings are also broadly echoed in Lavery & Wilson (2023), who note that much past work has shown positive effects of PE sponsorship on various dimensions of portfolio company outcomes and performance in the post-buyout (but, *nota bene*, pre-exit) period. In particular, they cite evidence that is broadly suggestive of improved operating performance under PE sponsorship (i.e., the most relevant dimension of firm operations for us; e.g., Kaplan, 1989; Boucly et al., 2011; Fracassi et al., 2022); and they also reference studies which document gains in other dimensions, including productivity, innovation, employment, exporting, resilience during recessions and certain aspects of workplace conditions – several of which may be seen as leading indicators of improved performance (Hartmann et al., 2020). Furthermore, beyond the papers cited by Lavery & Wilson, there are also more recent studies that examine the holding-period performance of PE-backed firms by comparing post-buyout outcomes with those of non-PE-backed peers. And like those we have already mentioned, these too point towards outperformance in terms of accounting-based proxies for performance (e.g., Cohn et al., 2021; Lavery et al., 2021; Fracassi et al., 2022).

As such: based on the balance of evidence on holding period performance, we should expect PE-backed companies to outperform their industry peers while the sponsor is still present.

2.2.2 Post-exit evidence

When a PE sponsor decides that it is time to exit its position in a portfolio company, there are, as Michala (2019) writes, primarily three forms of exit for which one might count the divestment successful. These are (i) secondary sales, where the acquirer is a financial buyer, e.g., another PE firm; (ii) trade sales, where the target company is sold to a strategic buyer; and (iii) IPO exits, where the target is sold on a public exchange. Our paper, as we have noted, focuses only on the last of these. We will address the potential bias that this introduces under Section 5.2 (“Limitations”). For now, suffice to say that it remains unclear whether there exist any systematic differences between different exit routes which would be important to take into account (Cao & Lerner, 2009; Michala, 2019).

Turning at any rate to post-exit operational performance, there is currently not much evidence on which to base confident hypotheses about persistence. Most of the research that does exist looks, as we have said, at post-exit performance through the lens of market returns. With that caveat in mind, though, the available market-based evidence broadly suggests, at least up to

five years post-exit, that formerly PE-backed firms continue to outperform their non-PE-backed peers after the PE sponsor has exited.

Kalbe and Warin (2020), for example, studying Swedish PE-backed IPO exits between 1996 and 2019, find that PE-backed IPOs are able to generate superior excess returns following the IPO compared to their non-PE-backed peers. It is worth noting that their time horizon extends only two years beyond the IPO, which is arguably too short if the aim is to say much about the long run. Even so, their findings are useful as a starting point, not least because they position their study in relation to a wider body of work that considers somewhat longer post-exit time horizons.

One such longer-horizon study is a paper by Cao and Lerner (2009), which examines the post-IPO stock performance of 526 PE-backed US firms listed between 1981 and 2003. Looking three to five years beyond the IPO, the authors find that PE-backed firms tend, on average, to outperform other non-PE-backed IPOs. Levis (2011) reports evidence in a similar direction. Studying firms listed on the London Stock Exchange, he compares PE-backed IPOs with both venture-capital-backed and non-backed firms, and finds that the PE-backed group outperforms both over the one- and three-year period after listing.

This is not to say that the literature speaks with one voice, however. As Kalbe & Warin (2020) note, there does not appear to be a determinate consensus on post-exit market performance. Some evidence points in the opposite direction. They cite, for example, MacArthur & Lerner (2020), whose findings suggest that PE-backed IPOs may in general underperform their industry peers. Kalbe & Warin's overall reading, however, is that the balance of the market-based evidence points towards outperformance in the post-exit period – a reading that seems broadly borne out by Ljungqvist's (2024) wider survey of the literature.

Finally, we reiterate that there are two studies of which we know that *do* in fact consider operational rather than market-based performance, as is also our own intention. These, again, are Lavery & Wilson (2023) and Biesinger et al. (2020), both of whom find some evidence in support of persistence in their respective empirical settings. The reader may revisit Section 1.3.2 for a slightly more discursive account of their findings.

Taken together, given that the theoretical discussion in Section 2.1.1 did not produce any clear winner between the persistence and convergence views, we place greater weight here on the balance of empirical findings. That is: our hypothesis development will be more inductive

than deductive, in the sense that our expectations will primarily be informed by what seems plausible to extrapolate from relevant evidence. As such, we find it reasonable to expect that operational performance will, in directional terms, behave similarly to market-based performance after exit and thus show persistence rather than full convergence (cf., e.g., Fama, 1970, on prices reflecting available information; and Ohlson, 1995, on the link between equity value and accounting fundamentals). We also find this expectation supported by the two studies most closely related to our own research question (Lavery & Wilson, 2023; Biesinger et al., 2020), both of which find evidence consistent with post-exit persistence in operational performance.

2.3 Hypothesis development

Having presented our research question, and having reviewed the theory and evidence relevant to it, we now turn to the hypotheses that will guide our overarching goal of addressing it. There are three such hypotheses, as outlined in what follows.

2.3.1 Holding period validation (H_1)

Our first hypothesis is validity in nature. Although our paper is about whether performance differences between PE- and non-PE-backed firms persist *after* the holding period, this question presupposes that such differences were present prior to exit. The hypothesis that comes after this one (H_2) is about the persistence of outperformance, and thus becomes a non-sequitur if we do not first establish that PE-backed firms outperform their non-PE-backed peers while the sponsor is still actively involved in the firm. As such, we first formulate a holding-period hypothesis, and treat rejecting the null hypothesis for H_2 as contingent on rejecting the null for H_1 .

As to what we expect: based both on the theoretical mechanisms discussed in Section 2.1.1 (primarily those of Jensen, 1989, and Kaplan & Strömberg, 2009), and on the empirical evidence reviewed in Section 2.2.1, we expect PE sponsorship to be associated with stronger portfolio company outcomes during the holding period. The literature, as noted, gives us good reasons to expect PE-backed firms to exhibit stronger operational performance than comparable non-PE-backed firms while the sponsor remains in place. Accordingly:

H_1 : *PE sponsorship is positively and significantly associated with higher levels of operational performance prior to the sponsor's exit.*

2.3.2 Persistence in the cross-section (H_2)

Next, we turn to the two hypotheses, H_2 and H_3 , that bear most directly on our research question. Both concern the post-IPO (post-exit) period, but they approach the idea of operational performance persistence in slightly different ways.

The first way we define persistence is in terms of *continued outperformance in operational metrics relative to non-PE-backed peers*. More specifically, in the cross-sectional setting which H_2 pertains to, what may or may not persist – what the “persistence” concept qualifies – is the performance differential that PE-backed firms exhibited over non-PE-backed firms during the holding period. (Something which again assumes there was a positive differential to persist in the first place, hence H_1 .)

As to our hypothesis, we expect that formerly PE-backed firms will continue to outperform their non-PE-backed peers on operational measures after IPO. This expectation is informed partly by the market-based literature, which, as discussed in Section 2.2.2, tends to find continued post-exit outperformance among PE-backed firms. We have already warned the reader that market returns do not always track firm fundamentals; but as we have also noted, they often reflect them closely enough to be informative, at least for the present task of grounding an expectation. More importantly, the limited literature that examines post-exit operational performance directly points towards persistence. As such:

H_2 : *PE sponsorship continues to be positively and significantly associated with higher levels of operational performance after the sponsor exits.*

2.3.3 Persistence within PE-backed firms (H_3)

Lastly, we introduce a second and somewhat stricter notion of performance persistence. Whereas H_2 defines the term as continued outperformance relative to non-PE-backed peers, H_3 defines it *within* the group of PE-backed firms. Specifically, the question that this hypothesis addresses is whether operational performance among PE-backed firms deteriorates after IPO relative to their level during the holding period. This complements H_2 since formerly PE-backed firms may continue to outperform non-PE-backed peers after exit even if their own performance weakens once the sponsor leaves.

Our expectation is that operational performance levels will deteriorate within the PE-backed sample as a function of the sponsor exiting its position. This is informed primarily by Lavery

& Wilson’s (2023) finding, discussed in Section 1.3.2, that growth within PE-backed firms slows after exit and that operating performance is not stronger post-exit than during the holding period, which points against expecting continued *within-firm* improvement for formerly PE-backed firms. It is also consistent with the convergence view developed in Section 2.1.1, according to which holding period performance enhancements may partly depend on sponsor-specific monitoring and governance. Accordingly:

H₃: *Among PE-backed firms, operational performance deteriorates in the post-IPO period relative to their level during the holding period.*

In summary, we have presented one hypothesis about the holding period, in order to establish whether there is any operational outperformance among PE-backed firms to persist in the first place, and two hypotheses that capture different dimensions of persistence. In the next section, we will describe how these hypotheses are tested, and the data we collected to do so.

3. Data & Methodology

Beginning with methodology, we start by decomposing our overarching research question into two parts, in order to clearly separate our two definitions of persistence. As such, each question respectively addresses the second and third hypothesis from above. We ask:

- I. whether having had a PE sponsor is associated with continued operational outperformance in the post-exit period; and
- II. whether, *given* that a firm had a PE sponsor, the event corresponding to the sponsor’s exit is associated with higher, lower, or unchanged performance.

The two questions are each addressed through one of two complementary model specifications (“Specification 1” and “Specification 2”, respectively). These are set out below.

3.1 Research design

Our research design is observational and relies on multivariate panel regressions. Within this broader scope we take two slightly different empirical approaches. Since the first constituent of our overarching research question is about comparing PE-backed firms to non-PE-backed peers, the first of our two specifications is cross-sectional. Meanwhile, since the second constituent only pertains to the group of PE-backed firms, the specification relevant to *it*

(“Specification 2”) adopts a within-firm event-style design, in which the event is the exit of the PE sponsor. This distinction will hopefully become clearer as we go along.

3.1.1 Specification 1: cross-sectional model

We begin by estimating whether PE sponsorship is associated with higher levels of operational performance relative to non-PE-backed peers during both (i) the PE sponsor’s holding period; and (ii) the post-exit period of PE-backed firms. As noted above, this specification addresses the outperformance dimension of persistence, i.e., whether any performance gap associated with PE sponsorship is visible during the holding period and whether a similar gap remains observable after exit. Formally, we estimate:

$$y_{it} = \alpha_j + \alpha_t + \gamma X_{it} + \beta_1 (PE_i \times Holding_{it}) + \beta_2 (PE_i \times PostIPO_{it}) + \varepsilon_{it}$$

...where the dependent variable y_{it} denotes the operational performance measure of firm i in year t (which, depending on the regression, may refer to any one of the operational performance metrics outlined in Exhibit A of the Appendix).

The term α_j denotes industry fixed effects, which captures the time-invariant characteristics of industry j , while α_t denotes year fixed effects, which captures shocks common to all firms in year t . Across all specifications, we follow prior work (e.g., Lavery & Wilson, 2023) in controlling for firm size. We choose the logarithm of employees as our firm size proxy on the basis that PE- and non-PE-backed firms in our sample show notable group-level differences in mean employment levels (cf., Section 4.1); and we show that our main results are not undermined by other size controls, e.g., $\log(Assets_{it})$, in Section 4.4. We also include a post-IPO indicator, $PostIPO_{it}$, which equals one for firm-years after IPO. This is conceptually analogous to Lavery & Wilson’s (2023) use of a post-exit indicator, and is intended to account for systematic differences between the pre- and post-IPO period that are not specific to PE sponsorship. X_{it} is thus a control vector comprising $PostIPO_{it}$ and $\log(Employees_{it})$.

As for independent variables, the indicator PE_i equals one if firm i belongs to the PE-backed group, and zero otherwise. $Holding_{it}$ equals one if, for a PE-backed firm, year t falls within the holding period of firm i ’s sponsor; or if, for a non-PE-backed firm, year t belongs to the corresponding set of calendar years over which holding-period observations of PE-backed firms are observed. $PostIPO_{it}$ is again defined analogously for the period after IPO.

The coefficient β_1 captures the difference in operational performance between PE-backed firms and non-PE-backed peers during the holding-period window, holding constant industry, year and firm size. In other words, it is our parameter of interest for the holding-period association between PE sponsorship and firm outcomes. A positive estimate, $\widehat{\beta}_1 > 0$, would suggest that PE-backed firms on average outperform non-PE-backed peers while the PE sponsor is still in place, conditional on included controls. The coefficient β_2 captures the corresponding difference in the post-IPO period, and is thus the parameter of interest for assessing persistence in this specification. A positive estimate $\widehat{\beta}_2 > 0$ would suggest that formerly PE-backed firms outperform non-PE-backed firms after IPO. Conditional on $\widehat{\beta}_1$ also being positive and significant, such a result would be consistent with persistence in our first sense of the concept.

3.1.2 Specification 2: within-PE-firm model

Our second persistence test (“Specification 2”) is restricted to formerly PE-backed firms only, and tests whether there is a structural break in performance around the IPO (*viz.*, exit) event for PE-backed firms. In particular, this specification asks whether performance changes once the PE sponsor exits, given that a firm was at one point PE-backed, holding constant all time-invariant firm characteristics. Formally, we estimate:

$$y_{it} = \alpha_i + \gamma X_{it} + \beta_3(PostIPO_{it}) + \varepsilon_{it}$$

...where $PostIPO_{it}$ is once again an indicator equal to one in the years following IPO and zero in the years prior, and where the logarithm of employees remains present as a control for firm size. $PostIPO_{it}$ is no longer included in the control vector X_{it} , since it is here the independent variable of interest. α_i denotes firm fixed effects, and y_{it} is defined as before.

Since the sample used for Specification 2 is restricted to PE-backed firms only, the coefficient β_3 measures the average change in operational performance once the sponsor has exited, relative to the holding period. β_3 thus captures the within-firm changes associated with exit. A positive and significant estimate would suggest that operational performance continues to improve after exit; a negative estimate would suggest that performance weakens; and an estimate close to zero would suggest relative continuity across the two periods.

3.2 Links & constraints in relation to prior research

The approach outlined above is loosely informed by Biesinger et al. (2020) and Lavery & Wilson (2023). These sets of authors both distinguish, as we do, between holding-period effects and post-exit persistence. Unlike us, however, they do so using a highly parameterised differences-in-differences (DiD) model (DiD being a quasi-experimental design that infers treatment effects from differential changes over time between groups; cf., Baker, 2025), and also use propensity-score matching to improve comparability between PE-backed and non-PE-backed firms on observed pre-buyout characteristics.

We initially tried to replicate Biesinger et al.'s (2020) matching procedure. This involved estimating, in a probit specification, each firm's *ex ante* likelihood of receiving PE sponsorship on the basis of pre-buyout firm characteristics and fixed effects for industry and buyout year, and then assigning up to five companies from the same industry-year cell to each PE-backed firm. However, when doing so, we ended up having to discard approximately 75% of our already-small sample of PE-backed firms, since there was not a sufficient number of industry-year pairs for these discarded firms to be assigned a match. Loosening industry definitions – e.g., clustering, on a discretionary basis, adjacent industry classifications such as “Health Care Equipment” and “Health Care Facilities” – did not substantially improve the outcome; nor did relaxing the year-related constraint, so that non-PE-backed firms could be assigned to PE-backed firms whose $t = 0$ was within up to ± 2 years of the non-PE-backed firm; and nor did adjusting the number of non-PE-backed firms to which a PE-backed firm could be assigned as a matched control.

We therefore opted for a simpler approach towards alleviating, though not ruling out, the concern about reverse causality which matching is intended to address (cf., Sections 4.4 and 5.2 for more extensive discussions about this concern). In particular, we tried to ensure that PE-backed and non-PE-backed firms were sufficiently similar in terms of baseline characteristics by specifying that the ROA of non-PE-backed firms had to lie within two percentage points of at least five PE-backed firms' ROA at $t = 0$. This caliper ($c = 0.02 =$ two percentage points) is stricter than what has generally been used in prior work – cf., e.g., Boucly et al. (2011), who require that PE-backed firms and assigned controls must have an ROA within the 50% bracket pre-buyout – in order to partially redress our inability to assign controls based on industry-year pairs. There is indeed a tradeoff, as Boucly et al., note in relation to their own relatively unconservative 50% ROA bracket, between sample size

retention on the one hand, and the rigorousness of the approach one takes to ensure comparability on the other. Our approach to comparability allows us to retain a considerably more complete sample than was possible with propensity-score matching, and mean differences between groups at baseline are still similar to what has been obtained in prior work (e.g., Kalbe & Warin, 2020; cf., Sections 3.3 and 4.1 for more information about our final sample). We have also followed Boucly et al. in testing the sensitivity of our results to different ROA calipers, of which a discussion can be found in Section 4.4 – where we also elaborate on how and why reverse causality is a concern in the context of PE sponsorship in the first place, and why even rigorous matching does not rule it out.

3.3 Data collection

Moving on, the following section describes how we collected and cleaned our data, and the rationale behind some of the discretionary decisions we had to make in this process. The sources we used during this process were Capital IQ (for identifying IPOs), SVCA (for identifying PE-backed IPOs from 2013 onwards), Business Retriever (for firm identifiers and annual reports) and Serrano (for accounting data).

Identifying Swedish IPOs. As the first step, we used Capital IQ’s (CIQ) screening function. We filtered for Swedish IPOs – which we defined as IPOs of firms both headquartered in Sweden and subsequently listed on a Swedish exchange – that were closed, and not subsequently withdrawn, during the time frame of interest (1996-2021). We chose 1996 as the lower bound since it is the earliest year for which Serrano provides the accounting data we needed for our analysis, and 2021 as the upper bound to allow for a meaningful post-IPO period across the sample. This left us with 656 firms.

Identifying which IPOs were PE-backed. For this step, we mainly relied on a report by SVCA (“Study of Initial Public Offerings, Performance of Private Equity-Backed Swedish IPOs 2013-2022”), which outlines a list of 59 unique PE-backed IPOs. Out of this list we ruled out three firms (see Appendix, Exhibit E1) from the financial services industry, given that “banks” tend to have materially different balance sheets from other firm types, which makes them unsuitable for peer analysis based on operating performance. We also eliminated EQT from the list, since EQT is itself a PE firm. This brought our PE-backed sample down to 55 firms. We further ruled out another 12 firms which we identified, through desktop research, as having been venture capital-backed (see Appendix, Exhibit E2; and cf., Section

1.1 for the rationale behind excluding venture capital). This left 43 post-2013 firms of relevance to our study out of the full list of IPOs identified through CIQ.

Next, to find PE-backed IPOs that occurred *before* 2013, we flagged those transactions in the full CIQ set of IPOs which were initiated by a PE sponsor. To this end, we required that a financial sponsor classified by CIQ under “Private Equity / Buyouts” appeared as a seller in the IPO transaction; and following Kalbe & Warin (2020), we specified that the buyer had to invest in one of the following segments: “Turnaround”, “Middle Market”, “Mature”, “Later Stage”. These filters were imposed to decrease the likelihood of accidentally identifying venture capital-backed firms. This left us with 11 PE-backed IPOs (see Appendix, Exhibit E3) in addition to those identified based on the SVCA list.

(NB: our CIQ screen yielded 28 pre-2013 IPOs as PE-backed. However, after reviewing these through desktop research, we found that 16 of them were in fact venture capital-backed, despite aforementioned filters. Moreover, on a separate but still CIQ-related note, two of the IPOs that CIQ identified as PE-backed actually occurred *after* 2013, despite not having shown up in the SVCA list. After cleaning our CIQ list and merging it with the list of PE-backed firms already identified, we at any rate found ourselves with 54 such firms.)

Retrieving firm identifiers. Before we could extract the accounting data we needed from Serrano for each firm in our full IPO set, we first – based on Serrano’s input requirements – had to find each firm’s organisation number (i.e., the unique identifier assigned to Swedish legal entities – cf., Bolagsverket, n.d.). To do this, we fed the name of each firm as input to the search function in Business Retriever. In many cases, the firms of interest were part of complex group structures, and in most cases, we therefore used Business Retriever’s “Group Tree” tab as a starting point, and the associated annual reports to identify the entity where the group accounts were consolidated. We then used the organisation number associated with this entity to extract the relevant accounting data from Serrano.

Worth noting is that we often had to identify more than one organisation number for each firm. One reason was that PE sponsors had often acquired companies using acquisition vehicles or holding companies (such that the consolidated accounting data for the year prior to the buyout sometimes occurred in a different entity, which we managed to identify through a combination of examining the “Group Tree” tab in Business Retriever and reviewing annual reports). Another reason was that, in some cases, a new holding company was created in preparation for the IPO.

Retrieving accounting data. For the firms that we were able to, we collected accounting data starting one year before the year of the last PE buyout through five years after IPO. We managed to obtain pre-buyout data for all firms except 12. For these firms, the data was either unavailable (some private firms were headquartered outside Sweden, some were established by the PE fund in the process of consolidating smaller firms, and for some, we were unable to identify the earlier reporting entity); or we found ourselves unable to obtain five years of post-IPO data (e.g., due to some IPOs having occurred less than five years ago, some firms having been later acquired by strategic buyers such that their accounting data was absorbed into the acquiring entity, and some having been bought out again by another PE fund).

Final exclusions from the PE-backed sample. After examining the full ownership history of each firm in our PE-backed sample, we excluded four firms, bringing our PE-backed sample down to 50 firms (see Appendix, Exhibit E4). During the process of obtaining accounting data we were unable to obtain the necessary accounting data for 7 of the PE-backed firms (for details, cf., Appendix, Exhibit E5). As such, we ultimately ended up with a final set of 43 PE-backed firms.

Constructing the non-PE-backed comparison group. With the PE-backed sample in place, the next step was to clean the remaining IPOs in our full CIQ sample which were not PE-backed, after again excluding firms operating in financial services or firms for which the required accounting data could not be obtained from Serrano.

A concern we wanted to account for in this step was that PE sponsors do not select portfolio companies at random. In particular, prior literature suggests that PE investors are likely to select firms that are already comparatively strong performers before acquisition (cf., e.g., Ljungkvist, 2024, and our earlier discussion in Section 3.2). This concern was visible in our own raw sample. Before imposing the baseline-performance restriction we outlined in the previous section (and shall briefly reiterate below), median ROA among non-PE-backed firms was approximately -11%, compared with 7% among PE-backed firms. After the restriction, the corresponding figure for non-PE-backed firms increased to approximately 5%, bringing the two groups considerably closer in terms of baseline profitability.

As such, non-PE-backed firms were retained only if their $t = 0$ ROA lay within two percentage points of at least five PE-backed firms' $t = 0$ ROA, so that the non-PE-backed comparison group would not be mechanically dominated by firms with substantially weaker

baseline operating performance. This left us with a final sample of 105 non-PE-backed firms (versus 413 such firms in the raw sample after cleaning it). Our final sample therefore consists of 43 PE-backed + 105 non-PE-backed = 148 firms in total.

It is important to note that our baseline-ROA-restriction approach does not eliminate concerns about reverse causality – something we will, as mentioned, discuss further in Section 5.2. At the same time, we emphasise that the aim of our paper is not to make inferences about what PE investors did (or did not do) to their portfolio companies. This would have required a more rigorous quasi-experimental setup. Our concern lies instead with the subsequent performance of firms that were in fact PE-backed. This potentiality and caveat, then, is important for how our results should be interpreted, but not fatal to the associative question we ask.

Missing data, outliers, and implications for our sample. We next assessed the extent to which missing data could affect the composition of the final sample. Missingness is potentially problematic if it is systematic rather than random, e.g., if it is more prevalent among smaller firms, since this would bias the final sample towards larger firms. For some variables initially considered for inclusion, such as patent licenses as an innovation proxy, missingness appeared sufficiently severe – and, indeed, dominated by small firms – that we excluded them from the analysis. For the main dependent variables used in our regressions, however, missingness was not a significant problem, and the number of usable observations were broadly similar across metrics.

Finally, we addressed extreme observations. Most of our accounting variables were highly skewed, and for many variables the maximum value was several times larger than the 99th percentile, or the minimum several times smaller than the 1st percentile. We therefore log-transformed variables where economically and mathematically appropriate, e.g., net sales and asset turnover (after verifying that there were no anomalous negative entries). We kept our profitability ratios in levels, since these can take negative values, though we winsorised those variables – and ultimately *all* dependent variables – at the 1st and 99th percentiles. This was in order to prevent our regression results from being disproportionately driven by extreme firm-year observations. We ensure that our main regression results are robust both to imposing more aggressive winsorisation thresholds (up to 10th and 90th percentiles) and to removing winsorisation altogether. This is shown in Section 4.4.

4. Findings & Analysis

4.1 Descriptive statistics

Table I reports baseline descriptive statistics for the final sample. The mean holding period for PE-backed firms is 5.2 years, which is broadly consistent with previous evidence on holding periods among Swedish and European PE funds (SVCA, 2022; Invest Europe, 2025), suggesting that our sample is not atypical in this respect. There are, however, some scale- and baseline performance differences between the PE- and non-PE-backed groups. For example, PE-backed firms are meaningfully larger in terms of both mean number of employees and sales, while non-backed firms have higher mean tangible fixed assets and operating margins.

It is worth noting that baseline imbalances of this kind are not unusual in the prior literature. For instance, in Lavery & Wilson's (2023) propensity-score-matched sample, the mean annual sales growth rate of the PE-backed group is still approximately twice as high as that of the control group in the buyout year. Nonetheless, we recognise that there clearly are some between-group differences at baseline, primarily in scale (e.g., employees); hence the import of controlling for firm size even after our ROA-based baseline restriction described earlier.

Table I. Baseline comparison between PE- and non-PE-backed firms

Firm characteristic	PE-backed firms	ROA-similar non-backed firms
<i>No. of firms</i> ‡	43	105
<i>Tangible fixed assets</i>	MSEK 570.3	MSEK 884.5
<i>No. of employees</i>	2,368	888
<i>Average salary</i>	SEK 364,745	SEK 324,458
<i>Net sales</i>	MSEK 3,074.9	MSEK 1,865.3
<i>Operating margin</i>	10.0%	18.0%
<i>Holding period</i>	5.2 years	-

This table reports the first-observed-year ($t = 0$) group means of firm characteristics for our final sample of PE- and non-PE-backed firms. The non-PE-backed group is restricted to firms whose $t = 0$ ROA lies within the caliper $c = 0.02$ of at least five PE-backed firms' $t = 0$ ROA. ‡ marks rows where the reported figures are counts rather than means.

The mean post-IPO period observed in our total sample is 5.54 years (median 5 years). However, for 37 of these firms (30 non-PE-backed, 7 PE-backed), we only have up to three years of post-IPO data. We verify that our main results are not sensitive to capping the post-IPO period at the three- or five-year thresholds. However, it is worth keeping in mind, when interpreting our results, that the post-IPO period corresponds to a relatively short time horizon for some of the firms in our dataset.

4.2 Hypothesis testing

We shall now move on to consider the formal hypothesis-testing framework that will guide our interpretation of the regression results, which we present in the following section. This framework is based on the qualitative formulations developed in Section 2.3.

Beginning with H_1 , which we test in Specification 1, the reader may recall our expectation that PE-backed firms will exhibit stronger operational performance than their non-backed peers while the sponsor is in place. Formally, this is a one-tailed test where the null hypothesis states that PE sponsorship is *not* associated with superior operational performance during the holding period ($H_0: \beta_1 \leq 0$), and which we reject in favour of the alternative ($H_1: \beta_1 > 0$) upon finding a positive and significant coefficient estimate of β_1 .

Specification 1 also tests our second hypothesis, which we said captures our expectation that formerly PE-backed firms continue to outperform their peers on operational measures after the sponsor has exited. Formally, this is therefore again a one-tailed test, where $H_0: \beta_2 \leq 0$, and where $H_2: \beta_2 > 0$ *conditional on finding support for H_1* . Recall that we treat rejection of the null for H_1 as a precondition for this test to be semantically meaningful, since persistence of a performance differential assumes that such a differential existed in the first place.

Lastly, our third and final hypothesis is tested using Specification 2, and captures our within-firm notion of persistence. Here, we expect that the IPO event marks a negative break in operational performance among PE-backed firms, i.e., that post-IPO performance is lower than – and therefore does not persist at – the level observed while the sponsor was still in place. Formally, the null hypothesis accordingly states that performance does *not* deteriorate following the sponsor's exit ($H_0: \beta_3 \geq 0$), with the alternative therefore being $H_3: \beta_3 < 0$. The reader is advised to visit the Appendix, Exhibit B, for an overview of our hypotheses.

4.3 Main results

We here present our main regression results, beginning with the outcomes of Specification 1 on the cross section of PE-backed and non-PE-backed firms in the pre- and post-IPO periods (Table II), and then moving on to Specification 2 on within-PE-firm changes (Table III).

Table II. PE Sponsorship and Operational Outcomes in the Cross-Section

Each row reports a separate regression. All variables are in logarithmic form unless otherwise indicated by †.

Dependent variable	$\widehat{\beta}_1$	Std. error	$\widehat{\beta}_2$	Std. error	Observations	R ²	Industry FE	Year FE	Clust. SE
Net Sales	0.4075***	(0.1080)	0.3147***	(0.1133)	1,869	0.795	Yes	Yes	Yes
Operating Margin †	0.0212	(0.0147)	0.0468***	(0.0181)	1,858	0.646	Yes	Yes	Yes
ROA †	-0.0138	(0.0201)	0.0389**	(0.0195)	1,724	0.271	Yes	Yes	Yes
Asset Turnover	-0.3671***	(0.1340)	-0.1701	(0.1373)	1,724	0.808	Yes	Yes	Yes
Current Ratio	0.0247	(0.0846)	0.0737	(0.1100)	1,868	0.497	Yes	Yes	Yes

Notes. The controls specified in Section 3.1.1 as belonging to X_{it} are included in all regressions. Standard errors clustered at the firm level are reported in parentheses. For logged dependent variables, coefficients on indicator variables are interpreted as approximate percentage differences. When discussed in text, we use $100(e^{\beta x} - 1)$ for the corresponding percentage effect; cf., Giles (1982). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Scale of demand. The coefficient estimates $\widehat{\beta}_1$ and $\widehat{\beta}_2$ in our net sales regression are both positive and statistically significant at the 1% level. In particular, having a PE sponsor is associated with $\widehat{\beta}_1 = 0.4075$ higher log net sales ($\approx 50.3\%$ higher net sales levels) during the holding period, holding constant included controls and fixed effects; and with $\widehat{\beta}_2 = 0.3147$ higher log net sales ($\approx 37.0\%$ higher net sales levels) in the post-IPO period, compared to not having one. PE-backed firms appear, in other words, to outperform their non-PE-backed peers in terms of net sales both while under PE sponsorship and subsequent to exit, and the effect of sponsorship is strongly significant. We therefore find support for both H_1 and H_2 , and interpret this as consistent with persistence in our first sense of the concept.

Profitability. Meanwhile, in the operating margin regression, it is only our second coefficient estimate that is rendered significant (i.e., on $PE_i \times PostIPO_{it}$). Specifically, the point estimate $\widehat{\beta}_2 = 0.0468$ indicates that PE-backed firms on average have approximately 4.68 percentage points higher operating margins than do non-PE-backed peers in the post-IPO period; and given that $p < 0.01$, this effect is strongly significant. Meanwhile, the holding-period estimate $\widehat{\beta}_1$ has the expected sign, but is not significant at conventional levels ($p > 0.10$). We see the same pattern in the case of our ROA regression. The holding-period estimate $\widehat{\beta}_1$ is statistically insignificant, while the post-IPO estimate $\widehat{\beta}_2$ is positive and significant at the 5% level. The latter estimate suggests that PE-backed firms on average outperform non-PE-backed peers in terms of ROA by about 3.89 percentage points after IPO,

holding constant controls. (Note that these two ratios are raw, not logged, hence differences in interpretation vis-à-vis net sales – cf., notes under Table I.)

Since the coefficient estimate $\widehat{\beta}_1$ is insignificant in both profitability regressions, we do not find support for H_1 as it relates to profitability; and since our decision rule for rejecting the null hypothesis for H_2 was to have first rejected the null for H_1 , we fail to find support for either H_1 and H_2 . As such, our data do not support that PE sponsorship *continues* to be positively associated with higher levels of profitability after the sponsor exits. Note, however, that this conclusion only follows from the semantics of the word *continues*, i.e., our notion of “persistence”. We will discuss the potential economic implications of this and other points, as we also will for the two other categories, in Section 5.

Efficiency & liquidity. In our asset turnover regression, the holding period coefficient is negative and strongly significant, while the post-IPO coefficient remains negative but is statistically insignificant. This means that we fail to reject the null hypotheses corresponding to both H_1 and H_2 – the former because we expected the opposite sign; the latter due both to having failed to find support for H_1 and on account of $\widehat{\beta}_2$ being insignificant.

As to the current ratio, the coefficient estimates are positive, as expected, but statistically insignificant in both periods. We therefore do not find evidence that PE-backed firms differ systematically from their non-backed peers in terms of liquidity. Moving on, Specification 2:

Table III. Post-Exit Changes in Operational Performance: Formerly PE-Backed Firms

Dependent variable	$\widehat{\beta}_3$	Std. error	Observations	Firms	R ²	Firm FE	Clust. SE
Net Sales	0.5503***	(0.0811)	629	43	0.890	Yes	Yes
Operating Margin †	0.0144**	(0.0068)	628	43	0.594	Yes	Yes
ROA †	0.0136	(0.0120)	615	43	0.574	Yes	Yes
Asset Turnover	-0.0280	(0.0545)	615	43	0.810	Yes	Yes
Current Ratio	-0.0498	(0.0597)	628	43	0.707	Yes	Yes

Notes. The logarithm of employees as a control for size is used across all regressions. We again use $100(e^{\beta x} - 1)$ for the corresponding percentage effect of logged variables, following Giles (1982). *** p<0.01, ** p<0.05, * p<0.10

Scale of demand. Beginning with our net sales regression, the coefficient estimate $\widehat{\beta}_3$ is positive and significant at the 1% level, and the magnitude (= 0.5503 log points) is large in

percentage terms. This runs directly counter to our expectation of post-exit deterioration in performance. We accordingly fail to find support for H_3 that the exit event is associated with a deterioration in net sales for formerly PE-backed firms.

Profitability. Our operating margin regression shows a similar pattern. The coefficient estimate on $PostIPO_{it}$ is also positive and significant, albeit at the 5%, not 1%, level. Formerly PE-backed IPOs on average exhibit approximately 1.44 percentage points higher operating margins in the period after the sponsor's exit in our sample, holding constant included controls. Following the rationale above, then, we do not find support for H_3 in the case of operating margins.

As for the remaining metrics (ROA and the metrics belonging to *Efficiency & liquidity*), the coefficient estimates are statistically insignificant, and as such, we ultimately fail to find support for H_3 across the full range of dependent variables considered. We therefore do not find evidence that the sponsor's exit is associated with a significant deterioration in the performance of PE-backed IPOs.

As previously indicated, we shall discuss how the results of Specifications 1 and 2 relate to one another, as well as their economic interpretation, in Section 5.

4.4 Diagnostics & validation

Before seeking to make sense of our results, however, it is important to consider the assumptions underlying our regressions. It is also worth considering whether our findings are sensitive to, for example, our choice of firm-size proxy, or to the ROA caliper used to construct the non-PE-backed comparison group. This is what we set out to do in this section.

Linearity and normality. One of the assumptions underlying our methodology (and linear regressions more generally) is that the dependent variable can be expressed as a linear function of the explanatory variables plus an error term (Newbold et al., 2023). Early on in the process, plots of observed against predicted values suggested that this assumption was unlikely to hold cleanly in our raw data. After some further exploratory data analysis, we also found that virtually all continuous variables collected from Serrano were highly skewed (most positively so). As explained in Section 3.3, we therefore winsorised all dependent variables at the 1st and 99th percentiles, and log-transformed those variables for which doing so was appropriate, in order to reduce the influence of outliers. For particularly denominator-sensitive

variables (e.g., operating margin), we also tested more aggressive winsorisation thresholds – e.g., the 5th and 95th percentiles – outside our main regressions, to see whether further reducing the influence of extreme observations would impact our results; and we also re-ran our regressions without winsorisation to assess the sensitivity of our results to these pre-processing choices. The coefficient estimates driving our main findings remained stable in both direction and statistical significance.

Homoskedastic errors. Linear regressions further rely on the assumption that the variance of residuals is constant across observations (Newbold et al., 2023). If, for instance, residuals are systematically larger for large firms than for small firms, this assumption is violated. We assessed whether this was a concern using the Breusch-Pagan test, where a significant result indicates heteroskedasticity (Breusch & Pagan, 1979). At first, this saw us rejecting the null hypothesis of homoskedasticity across all our main specifications. This is unsurprising given the heterogeneity of firm characteristics in our data, but in light of this outcome, our main results use firm-clustered standard errors. This allows errors to be both heteroskedastic and correlated within clusters (Cameron & Miller, 2015).

Absence of autocorrelation. Autocorrelation arises when residuals are correlated over time within the same firm. If present, this can lead to underestimated standard errors and inflated t-statistics, which may in turn make coefficients appear statistically significant when they in fact should not be (Newbold et al., 2023). This is a relevant concern in our setting since performance often persists within firms over time. (A firm with e.g., high sales levels in one year is likely to continue having high sales levels in the next.)

To diagnose whether this appeared to be a problem, we used the Durbin-Watson test. Values close to 2 are usually interpreted as indicating little autocorrelation, while values below 2 suggest positive autocorrelation (Newbold et al., 2023). Since our Durbin-Watson statistics generally fell below 2, we tested the robustness of our main results by re-estimating the models while controlling for the lagged version of each of our dependent variables. When doing so, the resultant Durbin-Watson statistic of each regression fell within ± 0.1 of the threshold of 2 (most +), suggesting that autocorrelation was meaningfully reduced.

Importantly, the significance and directionality of our main coefficients remained unchanged relative to the baseline results. We therefore conclude that although autocorrelation is present in the baseline residuals, our main findings do not appear to be driven by this issue.

Exogeneity. A further assumption underlying the results we saw in the previous section is that the explanatory variables are not systematically related to omitted factors contained in the error term. In our case, the most relevant concern which might undermine this assumption – besides certain aspects of omitted variable bias (e.g., unobserved year- or industry-specific conditions that affect firm performance, and are partly addressed through our included fixed effects) – is endogeneity due to selection on expected performance. Specifically, PE sponsors may select firms that were already likely to perform well, in which case superior post-exit outcomes may in fact reflect the “winner-picking” phenomenon that e.g., Ljungkvist (2024) highlights, rather than PE *sponsorship* as such. In the presence of such a phenomenon, the direction of influence would run not only from PE sponsorship to performance, but also from firms’ performance prospects to the likelihood of receiving sponsorship.

As mentioned earlier, we try to make PE- and non-PE-backed firms more comparable at baseline by specifying that the ROA at $t = 0$ of non-PE-backed firms must lie within two percentage points of at least five PE-backed firms’ ROA at $t = 0$. We noted elsewhere that this yielded a substantially more similar baseline than the unrestricted sample, although a sizable difference remained in e.g., the number of employees between PE- and non-PE-backed firms. We explained that we used the employees as our control for firm size in light of this.

Nevertheless, despite these improvements in baseline comparability, we recognise that our more rudimentary approach to ensuring comparability – relative to, for example, Biesinger et al. (2020) – does less to exclude the possibility of endogeneity arising from selection. To further assess whether selection on observables seemed to be a material concern in our sample, we therefore compared the subset of PE-backed firms *for which pre-buyout data were available* with non-backed industry peers observed in the same calendar-year environment. As reported in the Appendix, Exhibit C, we found no significant differences in the characteristics considered when doing so. In other words, we did not find explicit indications of observable differences between the PE-backed and non-backed groups prior to PE entry.

While this result cannot rule out selection on *unobservables* (managerial quality, for example), we at least find no positive evidence that observable pre-treatment dimensions are the sole driver of our results. Moreover, in relation to the above, it is worth noting that a more rigorous matching approach would not have resolved the concern about selection on unobservable characteristics either, since it involves balancing firms on characteristics that the econometrician can actually measure (*viz.*, observe; cf., Biesinger et al., 2023). The test just

outlined therefore addresses the principal margin along which (for example) propensity-score matching would have strengthened the design, and the absence of significant pre-buyout differences on observables alleviates the concern that our results are materially undermined by the simpler design we adopted due to sample constraints.

Lastly, we wanted to test the related concern that PE sponsors may time their entry into particular industries ahead of expected positive shocks by including industry-by-year fixed effects. The main coefficients became statistically insignificant in this specification, suggesting sensitivity to industry-specific time shocks. However, given our relatively small sample, such a highly parameterised specification is also likely to be underpowered.

Sensitivity to ROA-restriction choices. Since our non-PE-backed comparison group depends on two discretionary restriction choices (the ROA caliper c and the minimum number of PE-backed firms n to which each retained non-PE-backed firm had to be ROA-similar), we assessed whether our results were sensitive to alternative choices of these values.

We first tested ROA calipers ranging from $c = 0.01$ to $c = 0.05$, corresponding to one to five percentage points. Changing the caliper did not materially alter baseline comparability in terms of ROA: depending on the caliper imposed, median ROA differed between the PE- and non-PE-backed groups by around ± 2 percentage points, and with the most restrictive caliper ($c = 0.01$) there was no difference in median ROA between the groups. However, stricter calipers reduced the number of retained non-PE-backed firms considerably. For example, at $c = 0.01$ and $n = 5$, only 37 non-PE-backed firms were retained, compared with 105 under our final restriction at $c = 0.02$ and $n = 5$.

We then varied the minimum number of PE-backed firms to which each retained non-PE-backed firm had to be ROA-similar. Baseline characteristics were somewhat more sensitive to this choice. In particular, once we required ROA-similarity to six or more PE-backed firms, the number of retained non-PE-backed firms began to fall sharply, without a corresponding improvement in between-group similarity at $t = 0$. For this reason, we limited the tested values of n to the range 1 to 5, and examined combinations of $c \in \{0.01 : 0.05\}$ and $n \in \{1 : 5\}$. It was on the basis of this sensitivity analysis that we set the ROA caliper at $c = 0.02$ and required similarity to at least $n = 5$ PE-backed firms. This choice allowed us to retain approximately one quarter of the original non-PE-backed sample, while keeping

baseline ROA reasonably similar across groups (median approximately 7% for PE-backed firms and 5% for non-PE-backed firms).

We also re-estimated our main regressions under alternative combinations of c and n . The results for net sales, operating margin and ROA were stable in direction and statistical significance. The efficiency and liquidity results were more sensitive to the restriction choice, but since these outcomes were generally insignificant, or moved against our hypotheses, this does not alter our main conclusions. We therefore treat the results for scale of demand and profitability as robust to our restriction choices, but interpret the efficiency and liquidity results more cautiously.

Sensitivity of results to firm-size proxy. The magnitudes of our coefficients were sensitive to the firm size control used – especially in the case of our net sales regression, where they decreased notably when using the logarithm of assets as a control instead of employees ($\widehat{\beta}_1 = 0.1757^*$ and $\widehat{\beta}_2 = 0.2891^{***}$, compared to our main results where $\widehat{\beta}_1 = 0.4075^{***}$ and $\widehat{\beta}_2 = 0.3147^{***}$). However, coefficient directionality and statistical significance remained the same in the case of all other regressions. Moreover, while the significance of PE sponsorship on sales during the holding period went from strong to only marginal when using $\log(Assets_{it})$ as the firm-size control, the estimates are still sign-positive, and both estimates remain significant, despite the diminution of significance in the case of $\widehat{\beta}_1$. Thus, we still reject the nulls for both H_1 and H_2 in the case of net sales when using alternative firm size controls.

Sensitivity of results to exit event indicator. For some PE-backed firms in our sample, the IPO event corresponds to a gradual decline in ownership on the part of the sponsor rather than full divestment of shares. As a robustness check, we therefore re-estimated our specifications using the sponsor's final exit year (instead of the IPO year) when constructing the indicator $PostIPO_{it}$ for PE-backed firms specifically. The significance of our main coefficient estimates did not change after doing so, which suggests that our results are not driven by treating the IPO itself as the exit event. Nevertheless, we retain the IPO-based exit definition in our main specification for two reasons. First, using the final exit date shortens the observed post-exit window for many firms, which reduces our ability to assess persistence over a meaningful horizon. Second, IPO plausibly marks an economically relevant change in the ownership setting even if the sponsor does not fully divest immediately, since the extent of a sponsor's

scope for e.g., active governance typically begins to decline after IPO. This interpretation is also supported by Specification 2 and Exhibit D of the Appendix, where the post-IPO changes in performance are broadly consistent with what one might expect following a reduction in active oversight. We return to this and other points in the discussion section below.

5. Discussion

5.1 Interpretation of results

Scale of demand. The net sales results point in the same direction across both specifications in our study, and provide the clearest evidence of post-exit persistence in operational outperformance among PE-backed firms. First, PE-backed firms exhibit significantly higher sales levels than non-backed peers prior to IPO, and this difference remains significant after IPO. Second, within the PE-backed sample itself, net sales levels are significantly higher after IPO than during the holding period. In light of the fact that our study – as the reader may recall from Section 2.1.2 – uses net sales as a proxy for customer-facing operating activity, our results accordingly suggest that the scale of demand for PE-backed firms in Sweden not only *persists*, but *continues to expand*, after the PE sponsor exits its position in such firms.

This finding is broadly consistent with the persistence view developed in Section 2.1.1. In particular, it aligns with Kaplan & Strömberg’s (2009, p. 143) suggestion that PE-related improvements may contain a durable – “permanent” – component, and with Biesinger et al.’s (2020) finding that formerly PE-backed firms continue to exhibit higher sales after exit. It also runs counter to the convergence view. If PE-related improvements were primarily the result of short-term performance management before exit, one would reasonably expect sales-outperformance to weaken after IPO. Our results suggest, contrary to such a contention, that whatever occurred under PE sponsorship was compatible with continued revenue generation after exit.

At the same time, the result cannot be interpreted as causal evidence that PE sponsorship *created* the effect we see. As noted, our design is observational, and as discussed in e.g., Section 4.1, the PE-backed firms in our sample are larger in net sales-terms at baseline; and it may also be (apropos endogeneity) that PE investors selected firms with, for example, stronger growth prospects. What our results do suggest, however, is that the higher sales levels observed among PE-backed firms are not merely a temporary holding-period phenomenon.

Profitability. The profitability results are more ambiguous. In Specification 1, the estimated coefficients on $PE_i \times PostIPO_{it}$ are positive and significant in both the operating margin and ROA regressions, while the estimated coefficients on $PE_i \times Holding_{it}$ are statistically insignificant. We noted previously that this means we fail to find support for H_1 and, by extension, H_2 as it is formulated in Section 2.3.2.

However, the post-IPO significance is still interesting from an economic point of view. What it shows is that the PE-backed firms in our sample are, on average, more profitable than non-backed peers *after* IPO, but that such a difference is not observable intra-holding period. One possible interpretation is that profitability improvements associated with PE sponsorship take time to materialise, and are therefore reflected more clearly after exit than before. Indeed, this is reminiscent of what Guo et al. (2011, p. 497) write in relation to their own limited evidence of profitability improvements during the holding period: that “[even] if profitability has not significantly improved by the time of exit, firms may still increase in value (producing high returns to capital) if they have increased the productivity of their assets”.

At any rate, the absence of significant holding-period profitability effects prevents us from describing our post-IPO result as “persistence” in the first (outperformance-)sense which we have used in this paper. Nevertheless, a strong-form version of the convergence view seems unwarranted in light of our results. If PE sponsors intervened in their portfolio companies only in ways designed to facilitate a lucrative exit, it would be less plausible to expect the profitability of those companies to become *more* favourable relative to non-PE-backed peers in the post-IPO period.

Efficiency & liquidity. The efficiency and liquidity results are the least supportive of persistence. Asset turnover is in fact *negative* and significant during the holding period, and insignificant after IPO – which runs counter to the expectation that PE sponsorship is associated with more efficient asset use (*viz.*, the relevant sense of “operational performance” in this context). Meanwhile, the current-ratio estimates are insignificant in both periods, suggesting no clear difference in short-term liquidity between PE-backed and non-backed firms. Overall, the results do not indicate that PE-backed firms outperform their peers in efficiency or liquidity. However, they show no strong support for the convergence view either, since there is no evidence of a favourable holding-period effect that subsequently disappears.

5.2 Some limitations of our study

IPO exits are only one form of exit. Our analysis is restricted to IPO exits. It therefore excludes other PE exit channels, i.e., sponsor-to-sponsor sales and strategic acquisitions. Sponsor-to-sponsor exits fall outside our research question by design, since our interest lies with examining firm performance once sponsor involvement *ends*. Strategic exits are excluded mainly for practical reasons. Acquired firms are often absorbed into the acquirer's group, after which their stand-alone accounting data is no longer observable. We did not systematically verify the prevalence of this issue, but several individual cases we encountered during the process of collecting data seemed indicative of such a concern being warranted. For example, accounting data for *Capio AB* and *Com Hem Holding AB* (both acquired by strategic buyers in 2018 after their IPOs) were not available in the years after acquisition for those firms. As such – and given that IPOs are, as we mentioned at the outset of this paper, the most common PE exit route in Sweden – our assessment was ultimately that the costs of extending the sample to strategic exits outweighed the benefits, especially since including strategic exits would have made it less comparable to our non-backed group (of IPOs).

Another thing worth noting in relation to this point is that including other forms of exit would still have entailed a form of survivorship bias. By construction, any sample based on realised exits includes only PE-backed firms for which the PE sponsor ultimately found it possible, or desirable, to exit its position; and this would still have been the case if one were to have complemented IPO exits with other exit routes. In other words, any potential buyout deal that is not eventually exited – e.g., because of bankruptcy intra-holding period – will systematically be absent from the PE-backed sample. This is likely not a material problem in our setting since, as Kalbe & Warin (2020) point out, PE-related bankruptcies in Sweden have been few. Nevertheless, it is a design limitation that the reader should be aware of. It means that our results should be interpreted as applying to PE-backed firms that survive to exit, rather than to the full population of PE-backed firms.

Some post-IPO periods may not be so representative of the long run. Another limitation concerns the length of the post-IPO window for some firms in our dataset. As noted earlier, for seven of our PE-backed firms, we only had available data for three years after IPO. Since high levels of e.g., sales and margins may take several years to mean-revert, and since some PE sponsors in our sample sold down their stakes gradually rather than fully divesting at IPO, our results should be interpreted as evidence on post-exit performance over the observed

window (i.e., approximately five years on average, but in some cases only three years). It would therefore be premature to treat our results as vindicating, for example, Kaplan & Strömberg's (2009, p. 143) conjecture that there is a “substantial permanent component” to PE sponsorship.

The driver of group differences may be the consequence of PE investors selecting firms that were already predestined to perform well. As we have written elsewhere, our study is observational. We have not sought to link specific changes implemented by PE sponsors to subsequent firm outcomes, and we therefore cannot draw any direct causal inferences about whether the results we see are due to sponsor *intervention*, or sponsor *selection* of firms that were already strategically well-positioned to perform well in the future prior to PE entry. As such, it may be that some of our results reflect PE sponsors simply being good at “picking winners”. This concern, as Ljungqvist (2024) points out, is difficult to rule out empirically, since one cannot observe the relevant counterfactual where a given PE-backed company – i.e., one that *was in fact bought out* by a PE sponsor – is *not* bought out. The reader may recall, however, that we attempt to address the issue in a few ways. First, we constrain our sample so that PE- and non-PE-backed groups are baseline-similar in terms of ROA. Second, we test for differences in pre-buyout characteristics between groups for the subset of our sample where our data allow us to do so (Appendix, Exhibit C). Third, we re-estimate our main regressions while interacting industry- and year-fixed effects, apropos the concern that PE sponsors may enter industries ahead of tailwinds that are simultaneously industry- *and* year-specific; though we noted that our coefficients turned insignificant under these re-estimations, likely on account of being highly parameterised relative to our rather small sample.

In spite of these attempts to address the issue, however, we acknowledge that the results we see may still partly reflect PE sponsors being good at selecting firms which would have performed well without sponsorship. Therefore, keeping especially in mind students and researchers with a taste for epistemology, and perhaps better placed than us to think of creative approaches to the problem, we end this section with a parting pointer to Ljungqvist (2024, p. 22) – and to his observation that the explanatory role of winner-picking, in the context of PE, still “remains an open question”.

With that unresolved question in the background, let us now draw the discussion to a close.

6. Conclusion

This paper has examined whether PE-backed firms in Sweden show persistence in operational performance after the sponsor has exited its position via IPO, where “persistence” is understood both as continued outperformance relative to other newly listed, non-PE-backed firms and as the absence of deteriorating performance relative to within-firm levels during the holding period. The purpose has been to address a gap in the existing PE literature, arising from the fact that prior work has primarily focused either on holding-period performance or on post-exit market returns, while leaving the question of post-exit development in the *fundamentals* of PE-backed firms comparatively underexplored.

Our results suggest that persistence, in the two senses described above, depends on the dimension of operational performance considered. The clearest evidence we find relates to firm scale of demand, which we proxy for using net sales. We find that formerly PE-backed firms exhibit significantly higher sales levels than non-PE-backed peers both before and after IPO, and that sales are also higher within PE-backed firms *after* exit than during the holding period. This suggests that PE-backed firms in our Swedish setting sustain, and indeed continue to expand, their customer-facing scale of activity after their sponsor exits.

The results for profitability, efficiency and liquidity are more mixed. We find that PE-backed firms outperform non-backed peers in terms of operating margin and ROA only *after* IPO (not during the holding period), which prevents us from interpreting the result as persistence in the strict first sense used in this paper. Nevertheless, the finding is difficult to reconcile with a strong-form version of the convergence view, since convergence would lead us to expect weaker relative performance after exit, not stronger profitability relative to non-backed peers.

Meanwhile, we find no support for persistence, or even for a favourable *holding-period* effect, in efficiency or liquidity. Asset turnover is negative before IPO – contrary to expectations – and insignificant after IPO, while the current ratio is insignificant in both periods.

Overall, our findings do not point unequivocally towards either persistence or convergence. Sales performance persists most clearly after exit, profitability becomes more favourable only *after* exit, and our efficiency and liquidity findings provide little support for either view. This makes a strong short-termist interpretation difficult to sustain, but it also falls short of showing broad-based, durable operational outperformance of the kind that would definitively vindicate the persistence view.

These conclusions should be read with the limitations of our design in mind. Our study is observational, and we therefore cannot rule out the possibility that some of the results reflect selection rather than sponsor intervention. PE sponsors may, in other words, have selected firms that were already well-positioned to perform strongly. We address this concern by restricting the non-PE-backed comparison group on baseline ROA, testing observable pre-buyout differences where data availability permits, and re-estimating specifications with industry-by-year fixed effects. These steps are reassuring in some respects, but they do not eliminate the problem. Our results also apply only to PE-backed firms that exit via IPO and survive to exit, and the observed post-IPO window is in some cases too short to make strong claims about very long-run persistence.

Future research could build on this study in two main ways. First, the post-exit persistence question could be revisited in the Swedish setting using the propensity-score-matched DiD framework employed by Biesinger et al. (2020) and Lavery & Wilson (2023). Our own attempt to replicate such a design was constrained by the thinness of our IPO sample, but drawing the matching pool from the full Serrano universe of Swedish firms should make the approach more feasible. It may also allow researchers to include strategic exits where stand-alone post-acquisition reporting continues. Second, our profitability results generate a hypothesis that may be worth testing in a larger setting. A pan-Nordic or European sample could test whether the delayed-profitability pattern we observe replicates into other contexts.

Ultimately, our clearest finding is that Swedish PE-backed IPOs continue to outperform on net sales after sponsor exit. Whether this reflects durable value creation, selection, or some combination of the two remains unresolved; but the unevenness of our results suggests, in our view, that the question deserves more direct empirical attention than it has so far received.

7. References

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8. Appendix

Exhibit A. List of Dependent Variables

Each row in the leftmost column (except the heading) corresponds to one of the possible metrics that y_{it} can represent in both Specifications 1 and 2.

Dependent variable (y_{it})	Description	Performance bucket
<i>Net sales</i>	$= \text{Net Sales}_t$	Scale of demand
<i>Asset turnover</i>	$= \frac{\text{Net Sales}_t}{\text{Total Assets}_{t-1}}$	Efficiency & liquidity
<i>Current ratio</i>	$= \frac{\text{Current Assets}_t}{\text{Current Liabilities}_t}$	Efficiency & liquidity
<i>Return on assets (ROA)</i>	$= \frac{\text{Operating Income}_t}{\text{Total Assets}_{t-1}}$	Profitability
<i>Operating margin</i>	$= \frac{\text{Operating Income}_t}{\text{Sales}_t}$	Profitability

Exhibit B. Summary of Hypotheses

Overview of the hypotheses developed in Section 2.3 and formalised in Section 4.2.

Specification	Research expectation	H_{null}	$H_{\text{alternative}}$
H_1 and Specification 1	Positive holding-period PE performance differential	$\beta_1 \leq 0$	$\beta_1 > 0$
<u>Persistence (a):</u> H_2 and Specification 1	Positive post-IPO PE performance differential	$\beta_2 \leq 0$	$\beta_2 > 0$
<u>Persistence (b):</u> H_3 and Specification 2	Deterioration in performance for PE firms after IPO	$\beta_3 \geq 0$	$\beta_3 < 0$

Exhibit C. Pre-Buyout Observable Differences Between PE- and Non-PE-Backed Firms

Comparison of PE-backed firms in the year immediately preceding buyout with non-PE industry peers. Each row reports a separate regression of one of our dependent variables.

Specification used: $y_{it} = \alpha_i + \alpha_j + \theta(\text{PrePE}_{it}) + \gamma X_{it} + \varepsilon_{it}$

Dependent variable	$\hat{\theta}$	Std. Error	Significant at $\alpha = 10\%$?	PE-backed mean	Non-PE-backed mean
Net Sales	0.4255	(0.2789)	No	21.0159	20.0478
Asset Turnover	-0.2326	(1.1024)	No	0.3898	0.3738
Operating Margin †	0.0614	(0.0541)	No	0.0955	0.0582
ROA †	0.0436	(0.0790)	No	0.1293	0.0914
Current Ratio	0.0736	(0.1987)	No	0.1889	0.2307

Notes. The independent variable PrePE_{it} , with associated coefficient θ , equals one for PE-backed firms observed in the year immediately preceding buyout and zero for non-PE firms in the same year, while fixed effect indicators are defined as in Specification 1. Standard errors are clustered at the firm level, and our ordinary size control is included across specifications. Note that differences between Exhibit C and Table I are expected since Exhibit C uses a narrower pre-buyout subsample of PE-backed firms, and (for example) applies logged transformations to positive level variables and controls for firm size.

In Exhibit C above, we compare the subset of PE-backed firms for which pre-buyout data are available with the non-PE-backed firms from our final sample in the same year, while controlling for industry, year and firm size. Specifically, PE-backed firms are observed in the year immediately preceding buyout, before the sponsor could have affected their operational performance. The intended purpose of this test is to assess whether firms that later received PE sponsorship were already observably different from non-PE-backed peers prior to PE entry. The coefficient θ captures the pre-buyout difference between (soon-to-be) PE-backed firms and non-PE-backed firms, conditional on industry, year and firm size; cf., *appurtenant Notes*. As the table shows, none of the estimates is statistically significant, and repeating the test with additional firm characteristics not included among our dependent variables (e.g., salaries, total assets) yields the same result.

However, this should not be read as evidence of there being no winner-picking effect present in our sample. Indeed, following Popper's (1963) distinction between verification and falsification, the test is better understood as an attempted *refutation* of the claim that no observable pre-buyout differences existed between the groups. If we *had* found significant differences, this would have given us stronger grounds to interpret later performance differences as partly reflecting pre-existing firm characteristics. We do not find such differences, but failure to reject a null hypothesis is not proof that the null is true. Furthermore, since the test is limited to the PE-backed firms for which pre-buyout data are available (69.8% of the total PE-backed sample), and since it can neither rule out selection on unobservables nor selection among firms for which pre-buyout data are missing, the result can at best be interpreted as *reassuring*.

Exhibit D. Leverage and labour-related variables

Re-estimations of our main regressions on dependent variables other than those of primary interest to us. † indicates not a logarithm. Note that we use the logarithm of assets, rather than employees, as our size control for the employee metrics to avoid controlling for a variable that is mechanically related to the dependent variable.

Specification 1

Dependent variable	$\widehat{\beta}_1$	$\widehat{\beta}_2$	Obs.
Debt-to-Equity †	1.8880*	0.0247	1,700
Employee Growth †	-0.1483***	-0.0222	1,705
Employee Efficiency	-0.1312	-0.1312	1,869

Specification 2

Dependent variable	$\widehat{\beta}_3$	Obs.
Debt-to-Equity †	-2.5335**	458
Employee Growth †	-0.0190	611
Employee Efficiency	0.0859	629

Exhibit D reports additional regressions on leverage and labour-related outcomes. We do not include these among main dependent variables since they are not direct measures of

operational performance in the same sense as e.g., profitability- or efficiency-related financial metrics. While leverage is central to the PE ownership model and may affect managerial incentives (Jensen, 1989; Kaplan & Strömberg, 2009), it is primarily a financing choice rather than an operating outcome. Similarly, employee growth *could* reflect operational expansion or contraction, but it could also reflect, for example, outsourcing, automation, restructuring, or similar. These variables are therefore better interpreted as supplementary rather than primary proxies for operational performance.

In Specification 1, at any rate, PE sponsorship is associated with significantly higher debt-to-equity levels (leverage) during the holding period, while the corresponding post-IPO coefficient is small and statistically insignificant. This is broadly consistent with Boucly et al.'s (2011) finding that leverage increases more strongly among PE-backed firms than among comparable non-backed firms during the buyout period. In Specification 2, leverage is significantly lower after IPO within the PE-backed sample, which is consistent with economic intuition in that the burden of debt associated with (especially leveraged) buyouts would be expected to decline after the sponsor leaves, and the LBO model is no longer actively in place.

The labour-related results point in a similar direction. Employee growth is significantly lower for PE-backed firms during the holding period (consistent with the findings of e.g., Kalbe & Warin, 2020), which may reflect either layoffs or slower hiring relative to non-backed firms. The effect is no longer significant after IPO, suggesting that this difference does not continue once sponsor involvement has declined. This is consistent with the possibility that active sponsor oversight places greater pressure on labour-related adjustment during the holding period than after exit.

Exhibit E. Notes Regarding Sample Construction

E1	Financial services firms excluded	<i>Hoist Finance AB, Nordax Group AB, Resurs Holding AB</i>
E2	Venture-capital-backed firms excluded from the <i>SVCA</i> list	<i>Tobii AB, CLX Communications AB, Wilson Therapeutics AB, Wilson Bioscience AB, Edgewise AB, Oncopeptides AB, MIPS AB, Bone Support Holding, Calliditas Therapeutics AB, Ascelia Pharma AB, Ready International AB, Truecaller AB</i>
E3	Additional PE-backed IPOs identified through CIQ	<i>Alfa Laval, Bulten AB, Bygghem AB, Duni AB, Hemtex AktieBolag, Nederman Holding AB, Nordic Paper Holding AB, OX2 AB, SOBI, Gant Company AB, BE Group AB</i>
E4	Firm exclusions after examining ownership history	<i>Pierce Group AB</i> was removed because another PE fund (<i>Verdane</i>) acquired a stake approximately one year after the IPO,

		such that the PE “treatment” effect persisted into the post-IPO period. Similarly, <i>Cary Group Holding AB</i> was excluded on account of being taken private again just one year after listing. <i>OX2</i> was removed since its PE owner, <i>Altor Equity Partners</i> , had only acquired its stake one year prior to the IPO and divested just one year before <i>OX2</i> was subsequently taken private by <i>EQT</i> . Finally, <i>Nobina</i> was excluded because its pre-IPO “PE ownership” arose from a distressed-debt-to-equity conversion led by a consortium of hedge and credit funds, not conventional buyout sponsors.
E5	Some explanations regarding firms we were unable to obtain data for	(i) <i>Handicare Group AB</i> was bought out in 2010 by <i>Nordic Capital</i> ; however, group consolidation occurred in Norway up until 2014, so we were not able to obtain the data from Serrano for the prior years. (ii) We were unable to find a firm identifier for either <i>Sanitec Oyj</i> and <i>Norva 24 Group AB</i> . (iii) <i>Byggfakta Group</i> , <i>Bygghemma</i> , <i>Gant Company AB</i> & <i>BE Group AB</i> : We were not able to obtain accounting data for the relevant years.

Use of Large Language Models (LLMs)

We have used two LLMs during the writing process. The first is ChatGPT-5.4 (developed by *OpenAI*, accessible at chat.openai.com), and the second is Claude Opus 4.6 (developed by *Anthropic*, accessible at anthropic.com).

ChatGPT-5.4 was used for language-related queries. It was especially useful as a tool to find context-relevant synonyms wherever we felt our wording was overly evaluative, and whenever consulting online thesauruses did not list any words we felt were satisfactory. We decided against using AI to brush up on sentence cadence, however, since we felt the suggestions it provided tended to be overly formulaic (lists of threes; em-dashes; etc.), and often failed to preserve discursive continuity between ideas. The tradeoff is that the reader may find some of our sentences a bit long-winded.

Claude Opus 4.6 was the tool we used for coding. The model helped us produce much of the Python script that we used to conduct exploratory data analysis and to run our regressions. The reason we used Python rather than R in this process was mainly because LLMs tend to handle Python particularly well, given the abundance of Python documentation available online (on the advice of Joel Smedberg of the *Centre for Data Analytics* at the Stockholm School of Economics). We emphasise that we have been careful to review all suggestions to ensure they align with our aims. It quickly became apparent, for example, that the pseudocode we supplied to Claude had to be extremely precise, lest it misinterpret our intentions. We recognise the shortcomings of AI, particularly the possibility of hallucinations, and have not used these tools to collect or generate empirical or theoretical information.