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VALUATION EFFECTS OF LBO ANNOUNCEMENTS ON TARGET RIVALS

An Event Study of European Leveraged Buyouts between 2010 and 2016

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ABSTRACT

- Title:** Valuation Effects of LBO Announcements on Target Rivals:
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- Keywords:** LBO, leveraged buyout, private equity, competitors, rivals, announcement effects, event study, abnormal returns
- Purpose:** The primary goal of this thesis is to determine whether leveraged buyouts perceptibly affect the stock prices of core competitors of the LBO target. Through the use of an original, meticulously constructed data set, the study aims to augment the scarce and discordant extant literature on the subject. A follow-up cross-sectional test is conducted to provide further insights into the dynamics that may influence the studied valuation effects.
- Methodology:** An event study for individual rival firms and, in order to put to rest any concerns of cross-sectional correlations, for equally-weighted portfolios of rival firms is conducted. The (cumulative) abnormal returns generated therefrom are subsequently used as the dependent variable in a regression analysis.
- Theoretical perspective:** This study is rooted in two major strands of the LBO/M&A literature that are pertinent to intra-industry valuation effects. First, the notion that a private equity investment alters the competitive profile of the target company, which in turn affects industry dynamics. Secondly, signaling theories which posit that an LBO announcement entails valuable information effects about the industry in question.
- Conclusions:** We report zero-mean, insignificant abnormal returns across all relevant event windows, indicating that, on average, rival firms display negligible net valuation effects as a result of leveraged buyouts occurring in their industries. While these findings may seem unspectacular, they do constitute a novel verdict in the literature which simultaneously both challenges and supports previous empirical findings and neatly fits into the existing theoretical framework.

*There once were two students in Sweden
Who pulled data till their eyeballs were bleedin'
To add to their depression
They then ran a regression
But with every p-value exceedin'
5% – goodbye, we're concedin'*

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TABLE OF CONTENTS

1	Introduction	6
1.1	Background	6
1.2	Problem Discussion	7
1.3	Research Questions	9
1.4	Scope and Limitations	10
1.5	Thesis Structure.....	10
2	Literature Review	11
2.1	Leveraged Buyouts and Private Equity	11
2.2	LBO Consequences on Product Market Competition	15
2.2.1	Capital Structure Effects.....	15
2.2.2	Corporate Governance Effects.....	18
2.2.3	Operational Effects.....	20
2.3	A Signaling Perspective on LBOs.....	22
2.4	Theoretical Conclusions & Hypothesis Development	23
3	Methodology.....	26
3.1	Event Study Methodology – General	26
3.2	Event Study Methodology – Specifics	28
4	Event Study Results	41
4.1	Sample	41
4.2	Results – Descriptive Statistics	43
4.3	Results – Inferential Statistics.....	45
4.4	Discussion.....	48
5	Regression Analysis.....	49
5.1	Regression Specification.....	49
5.2	Diagnostic Tests & Regression Results.....	53
6	Conclusion.....	56
	List of References.....	59
	Appendix I – Detailed Results (Announcement Dates).....	68
	Appendix II – Detailed Results (Rumor Dates).....	70

1 INTRODUCTION

1.1 Background

Leveraged buyouts (LBOs) are among the more extensively studied phenomena in the (corporate) finance literature. Their prominence in academia is likely a reflection of their popularity in practice. Following an initial boom in the mid-1980s, and a second superlative LBO wave in the years leading up to the financial crisis of 2007/08 (Kaplan and Strömberg, 2009), they have long become a seemingly indispensable component of modern economies. As overall M&A activity has recently rebounded from a period characterized by downturns and turmoil, so too have LBOs – a revival that has been particularly pronounced in European markets (Grupp et al., 2015).

Definable as financially motivated and primarily debt-backed acquisitions, LBOs are widely used by private equity firms for friendly and not-so-friendly takeovers of targets across all types and sizes – from niche private firms, to individual company divisions, to publicly listed corporate behemoths, and everything in between (Strömberg, 2007). The common denominator of LBO targets is that they usually sit on unrealized value viable to be unlocked by changing how the companies are structured or managed. For example, firms that lack operational discipline or are too hesitant in utilizing their debt capacity may find themselves in the crosshairs of private equity funds as buyout targets (Ogden et al. 2003). As a powerful tool in the arsenal of any hands-on investment firm, LBOs have thus fundamentally shaped the market for corporate control throughout the past three decades. Indeed, they have even been prophesied – albeit somewhat prematurely – to replace public companies as the dominant form of corporate organization (Jensen, 1989).

The focal units of analysis in LBO research have typically been, rather unsurprisingly, the two main parties affected by a buyout – the private equity fund (the buyer) on the one hand and the firm that is being bought out (the target) on the other. A cornucopia of studies have over the years illuminated every conceivable aspect surrounding these transactions: antecedents of LBOs, change mechanisms implemented by the private equity sponsor, short- and long-term effects on operating performance, implications for innovation on a micro- and macroeconomic scale, the financial performance of private equity firms, etc.¹ Much less common, but no less interesting, are questions driven by a broader focus; a focus that goes beyond merely the

¹ For an outstanding review of the LBO research field that discusses these and other issues, see Kaplan and Strömberg (2009).

entities directly involved in the LBO and instead investigates the ripple effects permeating their environments. Specifically, in the case of the target firm, this may include its competitors which could arguably be affected by a leveraged buyout occurring in their proximity².

The idea of examining rival firms in order to better understand transactions for corporate control is not new. In an M&A context slightly different from LBOs, namely strategically motivated horizontal acquisitions, such an approach has been used to provide more objective measures for the need of antitrust action (Eckbo, 1983; Clougherty and Duso, 2011). The underlying logic is this: the observation that, say, upon the announcement of a horizontal acquisition, the target's value increases significantly has limited informational value. It may certainly be the case that this increase is the result of market power consolidation. This is, however, conjecture at best and utterly insufficient as a decision basis for policymakers worried about anticompetitive effects. There are a number of other reasons that may explain the boost in the target's stock price without warranting antitrust concerns, such as the acquisition premium or expected operational synergies (Keller, 2010). However, by also taking into account competitors' reactions to the deal, much more meaningful conclusions with regards to the effect on market power may be drawn.

The appeal of applying a similar methodology to leveraged buyouts is evident. Broadening or shifting the focal area across which the impact of an LBO is evaluated so as to include target rivals allows for a more comprehensive and balanced understanding of the motivations, dynamics, and consequences of these deals. Besides the direct insights into the behavior and reactions among competitors, one may also uncover insights that address "old" questions from new perspectives. For example, do LBO targets generally compete more aggressively post-buyout? Or, how are intra-industry competitive forces shaped by highly leveraged M&A activity? These and similar issues are at the center of the research project at hand.

1.2 Problem Discussion

The intricacy of financial markets tends to impede, if not outright defy, theories and models that prove too simplistic or unidimensional to describe them – in essence, an issue of requisite complexity that makes theory building tricky. LBO dynamics and their effects on target rivals are no exception in this regard. One of the first obstacles one faces when studying this matter is the realization that the problem is not a shortage of explanations or hypotheses, but rather

² Proximity referring to distance in terms of relative positioning within a product market setting.

the fact that they often stand in conflict with each other; a fact which will become evident throughout the second chapter.

Examples for two such contrasting theories are the growth probability hypothesis on the one hand and a competition-centric argument on the other. The former, arguing from an information asymmetry point of view, posits that the acquisition of a firm signals growth opportunities in the respective industry. This is good news for other firms in that sector and should therefore lead to an increase in the value of competing firms as well (Gaur et al., 2013). The latter, in contrast, based on product market competition theory, hypothesizes that after an LBO, firms tend to be better managed and thus operate more efficiently (Bloom et al., 2009). This puts competitive pressure onto rivals, which should then experience a decrease in value.

Turning to empirical findings in the hope of finding unequivocal answers, in this particular field of research, is an exercise in futility. For one, because there are simply not many studies in this direction out there; and secondly, to keep things interesting, the few empirical findings that have been reported are fairly conflictive and thus inconclusive. Whereas, for example, Slovin et al. (1991) and Chevalier (1995a) describe positive effects of LBO announcements on rivals' stock prices, Hsu et al. (2011) find the exact opposite. Of course, just because two theoretical perspectives (or empirical findings) entail opposing outcomes does not necessarily mean that one of them must be in some way erroneous. Not only might we expect to see one effect in some LBOs and the other in other LBOs, but it is perfectly possible for both to affect one and the same deal, cancelling each other out to some extent. This is what the opening lines of this section referred to – complex realities make it hard to disentangle outcomes in search for theory substantiation. And so it is precisely the pursuit of such disentanglement that emphasizes the need for additional empirical investigation.

It is thus the purpose of this thesis to shed more light onto this area of research. Making use of an original, high-quality dataset, we investigate how the stock prices of LBO targets' competitors react upon announcement of the transaction. A subsequent regression analysis is then conducted in an attempt to add further explanatory power to our findings. The few previous studies that have tackled the issue primarily investigated transactions up until the end of the second LBO wave in 2007 and were heavily US-centric. With the goal of adding fresh and topical insights to the existing body of research, our focus therefore lies on buyouts of European firms over the time period following the financial crisis of 2007/08. This, to the best

of our knowledge, constitutes a novel research avenue in a fairly specific, but all the more intriguing, niche of the vast M&A literature.

As a result, this study contributes to the literature in several ways. First, it takes upon itself the critical task of attempting to replicate the general research approach of previous studies. Hsu et al. (2011) find significant negative abnormal stock returns for LBO target rivals in their large U.S. sample. Examining whether the same holds true in a European setting would be a worthwhile goal in and of itself. Secondly, emphasizing methodological rigor and precision, the thesis enhances our understanding of the topic at hand by employing a meticulously constructed, original dataset, which may very well lead to more insightful findings than previously seen. Finally, we go one step further and make use of our event study findings in a follow-up regression analysis designed to extract more valuable information from the collected data.

Given this ambitious agenda, the study is well-positioned to help further reconcile the somewhat disconnected theoretical and empirical conclusions reported in the literature so far. In doing so, this thesis answers the recent call for further research from Bernstein et al. (2014), who stress how “important [it is] to understand the mechanisms by which the presence of private equity-backed firms affects their peers ... [as] much more remains to be explored here” (p.27).

1.3 Research Questions

There are two major questions of interest for the thesis at hand. While the first one is aimed at uncovering any potential abnormal returns, the second one pursues an objective more explanatory in nature.

Do core competitors of an LBO target experience abnormal returns around the announcement of the transaction?

To what extent do competitive and signaling effects determine or influence these abnormal returns?

In a similar study, Hsu et al. (2011) phrase their research question based on the assumption of a one-sided effect on stock returns. They explicitly look for negative abnormal returns for competitors. Having studied not only previous empirical findings but, crucially, the great

number of theoretical arguments that have been developed over time, we deem it more appropriate to keep the research question slightly more open. Investigating a potentially bidirectional effect seems to be more in line with the literature. Section 1.2 has already provided some insights into how we arrived at this conclusion. For a detailed discussion, we refer to Chapter 2.

1.4 Scope and Limitations

No empirical study is without limitations, and this one is no exception. The data set employed in this thesis is comprised exclusively of European leveraged buyouts which were announced between 2010 and 2016. In total, 124 transactions and 393 rival firms have been included in the sample. The focal time period is a particularly critical factor in LBO-related studies since LBO waves tend to be fundamentally different from each other, making extra-periodic inferences precarious (Bernstein et al., 2014). It may also very well be that the geographical focus introduces certain biases. European LBOs have made a particularly strong comeback after the financial crisis of 2007/08 compared to other regions (Grupp et al., 2015). Accordingly, the findings reported herein may not necessarily be applicable elsewhere.

While we went to great lengths to ensure a high-quality sample, our detail-oriented, semi-manual data collection method inevitably introduces a certain degree of subjectivity – however, given the benefits of the approach, we deem this a price worth paying.

Finally, due to our fundamental requirement for stock price data, only publicly listed competitors could be included. If private companies were to react systematically different from public ones to LBO announcements, this is something our study would not pick up on.

1.5 Thesis Structure

The rest of the thesis is organized as follows. In Chapter 2, we review the existing literature, based on which we develop research hypotheses. Chapter 3 outlines our research process and contains a comprehensive discussion of the methodologies employed. In Chapter 4, we present and interpret the results from the event study. Chapter 5 covers the subsequent cross-sectional regression analysis. Chapter 6 concludes the thesis. Additional tables which provide further detailed insights into the results of the study are included in the appendix.

2 LITERATURE REVIEW

The purpose of this second chapter is to provide the theoretical foundation necessary to ensure that the study conducted in this thesis is firmly grounded in the established pertinent literature. To this end, various dimensions of LBOs and the consequences they entail will be analyzed.

The first sub-section of the chapter is comprised of the general characteristics of LBOs. It provides a glimpse into the history of the relatively young phenomenon and also highlights its somewhat controversial nature.

Afterwards, we will review the literature directly or implicitly dealing with target rivals and the effects they may expect from an LBO in their industry. Different perspectives will be brought to the fore, each giving rise to different – and potentially conflicting – sets of arguments. At first, the focus will be on external effects of LBOs from a product market competition perspective. In this regard, it is imperative to first carefully study the impact of LBOs on the target firms, as these dynamics to a large extent ultimately determine the competitive forces rivals will find themselves subjected to. In particular, three dimensions will be emphasized: capital structure, corporate governance, and operations.

Subsequently, we will also highlight factors that go beyond purely competitive considerations. Specifically, information and signaling theories that may be applicable to leveraged buyouts will be examined.

Having thoroughly analyzed the findings from previous research, we will then close out the chapter by reconciling the different perspectives and formulating the hypotheses to be tested in the subsequent empirical section of the thesis.

2.1 Leveraged Buyouts and Private Equity

Leveraged buyouts (LBOs) are acquisitions where the buyer uses, as the name suggests, large amounts of debt to finance the transaction. Typically, these types of deals are associated with private equity firms taking a majority position in the target (Acharya et al., 2012). Indeed, Kaplan and Strömberg (2009) regard the terms leveraged buyout and private equity as synonymous. Consequently, LBOs are, from the acquirer's point of view, to be considered non-strategic deals, i.e. their primary motivation is financial.

Just how much debt has to be employed for a transaction to qualify as an LBO is up for debate – though typical numbers reported in empirical research suggest a range of 60-80% of debt

(De Maeseneire and Brinkhuis, 2012). Another important dimension of classification is the type of the target company (public vs. private). As Chung (2009) observes, the pertinent literature often leads one to believe that LBOs are usually going-private transactions, i.e. it is implied that they target public companies. In reality, private equity firms may very well acquire private companies. In fact, Strömberg (2007) finds that the majority of LBO transactions involve privately held targets or individual divisions of public companies. Besides the simple reality of data availability, one reason for why the literature seems to favor public-to-private LBOs may be that it is in these deals where academics have found the most prominent and widely accepted explanations for why LBOs exist in the first place: as will be discussed in detail in later sections, corporate governance is a key instrument of private equity – and one that is most effective when targets make the radical change from diffused (public) to concentrated (private) ownership (Chung, 2009).

It is interesting to note that leveraged buyouts are a relatively young phenomenon. While deals of this nature have certainly been sporadically around earlier, it was not until the 1980s that they experienced their first massive surge in popularity. The cause of this initial rise of highly leveraged transactions can best be described as a sort of awakening – an awakening among investors as the great value to be unlocked through a more active form of corporate governance began to dawn on them. Before 1980, shareholders had to content themselves with little in terms of control and influence. Weak boards, unsophisticated incentive structures, and rarely-used external governance mechanisms were a far cry from the obsession with shareholder value common today (Holmstrom and Kaplan, 2001). From this perspective, capital markets were clearly ripe for disruption. And disrupted they were, as investors conjured up novel instruments to wield their newfound power. It is no surprise, then, that the burgeoning sense of proactiveness in the market for corporate control that paved the way to the LBO simultaneously heralded the era of investor activism, hostile takeovers, and high-yield debt (Hurduzeu and Popescu, 2015).

These developments gave rise to much excitement. In a seminal article titled “Eclipse of the Public Corporation”, Michael C. Jensen (1989), one of finance’s most notable scholars, was lavish in his praise for LBOs as a superior form of governance, predicting their dominance going forward as “the publicly held corporation ... has outlived its usefulness” (p.61). At the heart of his fondness for the new organizational form was its purported power to resolve the key issue of separation of ownership and management – the great strength and inherent flaw of public corporations. Private equity, by taking a hands-on approach to governance and incentivizing managers through equity stakes, bridges the chasm between principal and agent.

Jensen also emphasizes the disciplining nature of leverage as an essential benefit. By trading in the “pillow of equity” for the “sword of debt” (Stewart and Glassman, 1988), post-LBO firms experience a potentially severe sense of pressure and urgency. This should, in theory, ensure that managers make value-adding decisions (or at least stop making outright wasteful ones) and, crucially, should force them to proactively take tough but necessary actions to secure the financial viability of their company. In other – intentionally counter-intuitive – words: “high debt can make bankruptcy less likely” (Jensen, 1989, p.72).

It is also interesting to keep in mind that Jensen (1989) framed his article not just in support of the LBO concept, but more specifically in its defense. For LBOs have attracted then, as they do now, ardent critics who emphasize the potential forces of value destruction behind the organizational form. One prominent and straightforward piece of criticism is that private equity firms simply pile too much debt onto their targets, unnecessarily putting their financial health in jeopardy – an argument advanced not least by regulators (Financial Services Authority, 2006). Another concern is the effect LBOs have on a firm’s long-term performance. According to Zahra and Fescina (1991), LBOs are “widely viewed as ‘get rich quick’ schemes [that] may not be expected to create or add value” (p.7). In this regard, the focus has often been on supposed reductions in R&D expenses after a buyout. More generally, the entrepreneurial prowess of bought-out companies, their ability to innovate, has been called into question (Zahra, 1995). Empirically, these claims have found limited support. Indeed, many studies suggest the opposite may be the case – that LBOs relieve managers of short-term pressures and thus enhance long-term performance and innovation (Palepu, 1990; Cumming et al., 2007; Lerner et al., 2011; Amess et al., 2015). Nevertheless, leveraged buyouts remain a controversial phenomenon.

Ultimately, Jensen’s (1989) questionably utopian private-equity-dominated economy did not come to pass as the dominance of the LBO, at least initially, was short-lived. For much like more general M&A activity comes and goes in waves (Gaughan, 2015), so too has the LBO sphere been characterized by temporally clustered ups and downs. Following the collapse of the high-yield bond market and a number of high-profile LBO failures in the form of post-buyout bankruptcies, the early 1990s saw a massive decline in the market for leveraged transactions – especially the ones involving big, public targets. With the benefit of hindsight, the extent of Jensen’s bullish stance has thus often been described as “premature” (Strömberg, 2007; Cheffins and Armour, 2008). However, as Kaplan and Strömberg (2009) point out, this does not mean that LBOs disappeared completely. Throughout the 1990s and early 2000s,

private firms and company divisions were still targeted; these transactions simply occurred comparatively more under the radar than the prominent deals of the preceding boom.

And yet, it was not too long before the LBO would make its triumphant return as private equity markets surged once again in the post-dot-com-bubble era. Global M&A buyouts backed by financial sponsors reached new record levels each year in 2005, 2006, and 2007, with cumulative deal values reaching almost USD 2 trillion across these three years³ (Guo et al., 2011). Accordingly, this period is considered the second LBO wave (Kaplan and Strömberg, 2009); and with it returned the doomsayers of the public corporation: “the publicly quoted company could be largely marginalized in the not too distant future”, according to Cheffins and Armour (2008, p.3), who thus revived Jensen’s sentiment from 20 years earlier. The second wave also brought with it some novel developments. For example, Strömberg (2007) observes that the number of secondary buyouts has gone up substantially. These types of deals, which have been less significant in the first wave, see private equity investors acquire target companies from other private equity firms. Interestingly, empirical findings suggest that having gone through an LBO process already does not necessarily lower the value creation potential in another subsequent LBO (Achleitner and Figge, 2014). It seems, then, that the first private equity owner exploits, but does not exhaust, improvements in the target firm.

When and how this second LBO wave abruptly ground to a halt should not come as a surprise. With the onset of the global financial crisis following the years 2007/08, the market for LBOs and, again, especially for mega-buyouts, dried up. As the economy recovered in the most recent years between 2010 and 2016 (the focal period of this thesis), so did LBOs, at least to some extent. Compared to strategic M&A activity, which has surged at breakneck speed and recently came close to pre-crisis boom levels, the share of the LBO subset in the overall M&A sphere may be more modest than it used to be (Fitch Ratings, 2015). Speaking of a third wave already would be unwarranted. Overall, however, Grupp et al. (2015) report that the market for LBOs is healthy once again – especially in Europe, where they “have grown increasingly strong after the end of the financial crisis”.

A final noteworthy general observation is that just as the LBO phenomenon has come and gone in waves, so too has the literature covering it. As is noticeable throughout this chapter, many of the seminal articles of the field stem from the late 1980s / early 1990s. Similarly, the boom years of the mid-2000s have effected renewed academic interest in LBOs. A variety of

³ And this despite the abrupt decline in deals in the second half of 2007 as the global financial crisis loomed.

illuminating research has been published in recent years as a result. This may allow for interesting comparisons between the cycles to better understand if and how antecedents, processes, and outcomes of LBOs have changed over time. On the other hand, in this regard one has to keep in mind that findings from the private equity literature may be highly sensitive to timing. As Bernstein et al. (2014) point out, the LBO boom of the second wave was so intense and the financial crisis so devastating that any empirical conclusions drawn from this era may have been rendered highly idiosyncratic. So it is all the more important to keep investigating this diverse and fascinating research area going forward.

2.2 LBO Consequences on Product Market Competition

The discussion in the previous section has emphasized a key characteristic of LBOs that is well worth reiterating: leveraged buyouts are much more than just deals. As Kester and Luehrman (1995) put it, “under the right conditions ... they represent an alternative model of corporate ownership and control” (p.120). In other words, the modifications to firm structure and governance implemented as part of the LBO process are typically so comprehensive and severe that the target company is changed on a fundamental level. The notion that these changes alter its competitive behavior, which in turn affects rival firms, is the unifying idea behind the strand of literature that investigates external effects of LBOs. Within this perspective, the subjects of corporate finance and industrial organization converge (Grupp et al., 2015).

Kaplan and Strömberg (2009) identify three sets of changes that private equity firms typically impose on their targets. They refer to these dimensions as financial, governance, and operational engineering⁴. For the following discussion we will adopt this perspective. For each one, first its impact on the LBO target will be assessed; this, in turn, provides the necessary basis for the discussion about how rivals may be affected.

2.2.1 Capital Structure Effects

The shift in capital structure via a substantial increase in debt is the most immediate and prominent outcome of an LBO – as it should be; leverage is, after all, the titular factor of the phenomenon. It is also a vast and multi-faceted topic, as capital structure is probably the

⁴ Their term “engineering” is particularly well-picked in this context, as it already implies the deliberateness and hands-on approach that private equity firms bring to the table.

single most intensely studied and controversially discussed issue in all of corporate finance. From Modigliani and Miller's (1958) stance of indifference in a frictionless world, to a dynamic optimum through capital structure fluidity (Myers and Majluf, 1984), to Jensen's (1986) borderline misanthropic view on firm managers – the literature has produced a myriad of arguments and perspectives to take when analyzing this subject. A comprehensive discussion is therefore beyond the scope of this thesis. If there is, however, one general key takeaway from capital structure theory, it is this one: similar to how LBOs are more than just deals, leverage is about more than just sourcing capital. The type of financing mix employed by a firm has tremendous impact on how it is run – a notion that proves particularly relevant in an LBO context. It is telling that Kaplan and Strömberg (2009) discuss leverage primarily as an instrument for controlling management, thus taking the same line as Kester and Luehrman (1995), who posit that “debt and equity are not merely different types of financial claims [but] alternative approaches to governance” (p.128). From this perspective, the reduced financial flexibility, i.e. the constraining effects of interest payments, in highly levered firms is supposed to discipline management into making value-enhancing decisions – see the “sword of debt” argument mentioned previously in this chapter⁵. This highlights a characteristic of LBOs that will be prevalent throughout this section: the interconnectedness between capital structure and corporate governance and, indeed, other dimensions as well.

So how does capital structure affect firm behavior in the product market? Does being financially cornered make LBO targets easier prey for their rivals or do they emerge a more aggressive and formidable challenger? Unfortunately, far from a clear-cut answer, the findings are very much ambiguous. In their review of the capital structure and product markets literature, Istitieh and Rodríguez-Fernández (2006) draw the seemingly vacuous conclusion that...

Either a high leverage level induces firms to act more aggressively, leading to a tougher product market competition; or a high debt level leads firms to behave less aggressively, making product market competition softer. This shows how capital structure affects the firm's strategic behavior in product markets. (p.72)

Put differently, it is widely accepted that leverage does influence the competitive structure and behavior within an industry “because financing arrangements can alter a firm's incentives (or ability) to compete” (Campello, 2003, p.374). However, it is much less straightforward how it does so, as both theoretical models and empirical observations have shown bidirectional effects to be plausible.

⁵ Axelson et al. (2009) point out that this disciplining mechanism of leverage applies not only to the agents (managers) but also to the principals (the private equity funds) themselves, who need to convince third party co-investors to put up the debt portion of a deal's capital structure.

The literature explicitly linking capital structure and product markets is a relatively recent amalgamation that appeared in the late 1980s (Harris and Raviv, 1991). It is certainly no coincidence that it coincided with the emergence of LBOs as a widespread phenomenon and it was also this initial LBO wave that provided the material for the first empirical investigations in this area. In their pioneering article on the issue, Brander and Lewis (1986) introduce the argument that higher leverage leads to more aggressive competitive behavior due to a “limited liability effect”. This refers to the fact that shareholders are protected on the downside while benefitting from unlimited upside potential. They therefore have an incentive to pursue riskier strategies in an effort to expropriate wealth from bondholders – a phenomenon also known as asset substitution or risk shifting (Ogden et al., 2003). A contrary theoretical perspective is the long purse (or deep pockets) hypothesis as discussed by, for instance, McGee (1958) or Telser (1966). It postulates that in an environment of cut-throat competition, firms with more financial leeway, i.e. less leverage, will outlast their more constrained peers⁶. In other words, rivals with relatively greater access to capital can prey on their (over-)levered competitors – a tactic fittingly discussed in the literature under the term “predation” (Tirole, 1988).

And indeed, predation and long purses in action is precisely what Chevalier (1995b) finds in one of the first empirical investigations into the effects of LBO-based financial engineering on the business models of target rivals. Investigating the supermarket industry, she reports overall softer post-buyout competition and observes predatory behavior from competitors attempting to enter local markets that have seen increased LBO activity. Similarly, in a study investigating firms’ promotional activities, Grullon et al. (2006) find that a lower debt ratio is associated with more aggressive advertising and vice versa, thus indicating that “financial leverage has a ‘dampening’ effect on the intensity with which a firm chooses to compete in the product market” (p.1).

It is important to keep in mind, though, that considerations about potential competitive effects of capital structure, especially within theoretical models, typically emanate from a *ceteris paribus* assumption. Since leveraged buyouts entail more comprehensive changes than just recapitalization, it would thus be ill-advised to apply findings from this specific literature one-to-one to LBOs without further reflection.

A final issue that needs to be part of any practically relevant capital structure discussion is taxation. The tax relief provided by higher interest payments can be a significant driver behind recapitalization efforts such as LBOs. While difficult to precisely quantify, a rough estimate

⁶ While a high debt load may imply restricted access to capital, this is not to say that all LBO targets are inevitably financially constrained following the buyout.

places the value of such a tax shield in the region of 10 to 20 percent of firm value (Kaplan and Strömberg, 2009). Unrealized tax savings are thus a significant source of potential value waiting to be unlocked through a restructuring. However, the gains from tax relief are naturally limited. As classic capital structure trade-off theory suggests, the benefits from tax savings based on a high debt load will be continuously offset and at some point eclipsed by costs of financial distress (Ogden et al., 2003). Furthermore, there has been some evidence that it is in fact not the private equity fund, but the targets' shareholders that would capture most of the tax shield gains – via the acquisition premium (Jenkinson and Stucke, 2011). It is therefore unlikely that tax considerations are the primary, let alone the sole, driver of an LBO. In summary, it appears that while, overall, direct competition effects of leverage are ambiguous, its positive influence on firm governance is well-documented.

2.2.2 Corporate Governance Effects

The introductory discussion in section 2.1 already provided a glimpse into the beneficial properties of private equity with regards to governance issues. Indeed, according to Cheffins and Armour (2008), the ability to improve how target firms are run is among the most celebrated characteristics of LBOs:

As early as 1990, a clear consensus was forming among academics who studied leveraged buyouts from an economic perspective that the carrying out of such transactions involved a distinctive set of business arrangements ‘with the potential to correct long-standing problems in corporate governance’.⁷ (p.30)

One such governance-enhancing “business arrangement” is, as described above, capital structure. However, it is not only the disciplining mechanism of debt that plays a role in this regard. Arguably even more powerful is the strategic deployment of equity as a central part of the overall incentive structure within a firm. Managers in private-equity-owned companies receive compensation contracts that are substantially geared towards incentivization, including significant equity positions and variable, performance-based pay (Leslie and Oyer, 2013). Partial ownership through a considerable personal stake exposes management to both upside and downside risks, which helps align their interests with those of the firm. What is more, as Kaplan and Strömberg (2009) point out, equity in a private business is rather illiquid. There are also no continuously moving share prices to monitor. As a result, managers are free of the pressures and allures of short-term performance manipulation – an ever-present concern in listed companies. Naturally, then, this effect is most pronounced in the case of public-to-

⁷ The direct quote within the reference is from Baker and Smith (1998, p.40)

private buyouts. An additional benefit of going-private transactions is the overall lower cost of corporate governance, as private firms do not face the considerable compliance costs of being publicly traded (DeAngelo et al., 1984).

The other major instrument of corporate governance in bought-out businesses, besides debt servicing and managerial incentive structures, is the active involvement of the private equity firm itself (Cumming et al., 2007). The notion that the buyout sponsor would take a hands-on approach to running the target is intuitive. Essentially, the *raison d'être* of private equity firms and LBOs is to buy, fix, and sell companies (Leslie and Oyer, 2013). That middle part – fixing – naturally requires active engagement on the part of the new owner. As the central platform wherefrom oversight, control, and influence are exercised, the boards of LBO targets therefore play a critical role and have, accordingly, been studied extensively. Compared to public companies, the boards of target companies following a buyout are generally structured for smaller size⁸, convene more frequently, are typically in close informal contact, and take a more active role overall (Kaplan and Strömberg, 2009; Cornelli and Karakas, 2012). They are also unhesitant to swiftly replace firm management if deemed necessary (Acharya et al., 2009). However, all this is not to say that the private equity sponsors are in charge of the day-to-day business of their portfolio companies. In normal times, i.e. barring any crises or major occurrences, they will be content with taking on an advisory role and providing a supporting function to firm management (Cheffins and Armour, 2008).

Having established how LBOs affect firms from a corporate governance perspective, we now turn to the impact this may have on target rivals. The basic argument is straightforward: as laid out in the preceding paragraph, through various mechanisms LBOs enhance firm governance. Better-run firms are more formidable competitors and thus bad news for the competition. Managers, who through high-powered incentives are enticed to make value-adding decisions, tend to do so. Thus, LBO targets may become more entrepreneurial and competitive (Zahra, 1995), to the detriment of their industry peers.

Oxman and Yildirim (2008) raise yet another interesting and highly pertinent point: their findings suggest that rival firms, after LBOs occurring in their industry, tend to emulate changes in internal corporate governance from their bought-out peer. For example, the use of equity-based compensation increases, CEO turnover occurs more frequently, and board composition changes (they become smaller). This can be interpreted as a reaction to the revelation that there may be industry-wide agency problems – in other words, a signaling

⁸ In empirical studies, smaller boards have been found to be more efficient (Yermack, 1996).

issue; a sort of collective wake-up call (Slovin et al., 1991). To substantiate this argument, Harford et al. (2014) investigate whether or not it is actually the private equity activity that triggers these modifications in governance within an industry, as opposed to them selecting into industries that undergo such changes organically. Their results support the causality hypothesis.

2.2.3 Operational Effects

Moving the discussion into this third dimension of LBO engineering, a pattern emerges. First, we argued that capital structure choices are, in some way, actually corporate governance measures. Now, we will see how corporate governance choices, such as incentives and a hands-on mentality, may result in improved firm operations. It seems clear, then, that the power of a leveraged buyout lies in its individual elements mutually reinforcing each other so as to augment the overall intended effect. Recall the quote from Kester and Luehrman (1995) at the beginning of this section: “Under the right conditions, LBOs are not merely deals. They represent an alternative model of corporate ownership and control.” (p.120) It is the congruity and reciprocal amplification between the different change mechanisms – be it financial, governance, or operational engineering – that constitute such “right conditions”.

Overall, the literature overwhelmingly supports the general notion of improved operating performance and higher productivity of companies following an LBO (Lichtenberg and Siegel, 1990; Palepu, 1990; Murray et al., 2006; Cumming et al., 2007). The sources of these improvements are manifold. One major contribution is private equity firms’ ostensible ability to turn their portfolio companies into highly efficient organizations. The paradigms of lean structures, a relatively light asset base, and regular performance assessment are typically key ingredients to LBO success (Bloom et al., 2009). In a complementary fashion, Murray et al. (2006) suggest that it is not margins where private-equity-led firms trump their competition, but rather asset productivity. In sum, the *kaizen* management practice of continuous improvement and waste reduction seems to resonate well with private equity houses.

Arguing from a negotiating power perspective, Brown et al. (2009) also find operational benefits for LBO firms. They report that, following a firm’s buyout, its suppliers experience negative abnormal returns and deteriorating bargaining power. Interestingly, their data also suggests that it is not simply the higher financial leverage of the target that explains these outcomes; non-LBO recapitalizations without the typical governance changes associated with a buyout did not produce the same effects. As a result, LBO targets manage to keep their

operating costs relatively lower, which should negatively influence their competition (Grupp et al., 2015). Brown et al.'s (2009) findings can be interpreted as congruent with Palepu's (1990) postulation about firm risk following an LBO. He argues that there are two opposing effects. While financial risk increases due to higher leverage, changes to firm strategy and structure result in lower business risk. "The net result is that LBO investors bear significantly lower risk than comparably levered investments in public corporations." (p.261)

Yet another point of view discusses M&A activity of the LBO target itself as a driver of operational performance. As part of waste reduction, targets often divest peripheral or underperforming business segments following a buyout (Murray et al., 2006). However, they may also be active acquirers in an effort to strengthen their product offering and competitive position (Guo et al., 2011). In this regard, Nikoskelainen and Wright (2007) speak of a "buy-and-build strategy" commonly observed in private equity investments, and note that such acquisition activities partially explain the returns ultimately generated through the LBO. Similarly, Grupp et al. (2015) highlight the cost and competition effects of a target's post-buyout M&A activities. They even go so far as to position this issue as a fourth key dimension of LBO change mechanisms, next to capital structure, governance, and operations.

Executing successful mergers and acquisitions is tricky, though. How come they are such a popular and seemingly well-functioning avenue for value creation in an LBO context? A potential explanation leads us to one of the most important reasons for targets' operational gains in general: industry expertise of the private equity sponsor. Instead of making investments in a broad range of sectors, many private equity firms seek to specialize in certain industries, thus developing extensive experience and knowledge within their specific domains (Fenn et al., 1997). Focus trumps diversity, and LBO targets stand to benefit from it, as industry specialization has been found to contribute significantly to the above-average operating profitability of private-equity-backed firms (Cressy et al., 2007). This raises an important point: we have argued that LBOs, in contrast to horizontal or vertical M&A, are financially motivated deals and not strategic ones. However, as becomes apparent now, this is not to say that private equity houses cannot bring substantial strategic expertise to the table. In terms of what this means for the competition, empirical findings support intuition. Hsu et al. (2011) report that industry specialization of the LBO sponsor is negatively related to rivals' stock price reactions upon deal announcement. This illustrates how firms expect their peer, backed by specialist investors, to gain an improved competitive position.

2.3 A Signaling Perspective on LBOs

Besides the arguments based on changes in competitive behavior, another major strand of the literature is concerned with information effects of LBOs. These dynamics may occur on two levels of a transaction.

The first one is firm-specific asymmetrical information concerning the buyout target. Based on this view, private equity firms manage to extract and exploit valuable private information from incumbent firm management, which stands to benefit from an LBO via powerful equity-based incentive structures (Kaplan and Strömberg, 2009). This private information can be regarded as a source of inspiration for operational engineering, leading to better firm performance and thus affecting competition. It is essentially an agency problem, where managers prefer to be bought out by private equity for personal gain – a win-win for the private equity fund and target management, to the detriment of the target's pre-buyout shareholders. However, there are observations that contradict these arguments. Most importantly, post-LBO retention rates of incumbent management are too low to support such a line of reasoning. As Acharya et al. (2009) report, roughly one third of their investigated buyouts sees top executives being replaced within the first 100 days. This number doubles over the entire investment period. Overall, Kaplan and Strömberg (2009) conclude that “the evidence does not support an important role for superior firm-specific information on the part of private equity investors and incumbent management” (p.136).

The second domain where the information asymmetry arguments apply is even more compelling for the subject at hand, as it takes on an industry-wide perspective that explicitly includes effects on rival firms. The central idea, as laid out by Slovin et al. (1991), is that LBO-like transactions “reveal valuable information ... about the target and that elements of this information apply to firms involved in similar economic activities” (p.1539). One such piece of valuable information revealed by a buyout is the acquirer's ostensibly bullish outlook on the industry in question. The fact that a firm is being bought out, the argument goes, signals positive expected future developments for the entire sector – a phenomenon Gaur et al. (2013) call the growth probability hypothesis. This argument carries even more weight considering that, often, the buyers are specialized funds, as previously discussed in section 2.2.3. They possess a high degree of expertise in their targeted sectors, which adds credibility to their opinions and makes for a higher-quality and thus more impactful signal. Empirical findings suggest the industry-wide post-buyout optimism in accordance with the growth probability hypothesis may be warranted. Bernstein et al. (2014) find that industries that have

experienced private equity investments in the recent past subsequently grow faster relative to other sectors.

However, higher expected industry growth is not the only signal an LBO may send to firms competing with the buyout target. Another (related) information effect has to do with speculation on future M&A activity in an industry. This view dates back to the early empirical studies investigating horizontal mergers and anti-trust concerns, such as Eckbo (1983; 1985), Eckbo and Wier (1985), and Stillman (1983). They find positive abnormal returns for target rivals following merger announcements. The explanation they propose is equally applicable to non-horizontal, financial deals: a buyout occurring in any given industry sends a signal that more buyouts may take place in the future. Thus, following an LBO, rival firms are more likely to become takeover targets themselves – a lucrative prospect for their shareholders (Kim and Singal, 1995; Clougherty and Duso, 2009). This line of reasoning is congruent with the empirical fact that LBOs (and M&As in general) tend to come in waves – not just for the aggregate economy, but for individual industries (Öberg and Holtström, 2006). It has also found tentative support in a study specifically investigating this acquisition probability hypothesis empirically (Song and Walkling, 2000).

On a highly related note that supports all the signaling theories discussed so far, studies have shown that one of the major contributors to LBO investment returns are valuation changes in the invested industries (Renneboog et al., 2007; Guo et al., 2011). In other words, private equity funds succeed in identifying and exploiting systematically undervalued industries; they buy low and sell high. This perspective is further sustained by the observation that private equity firms pay significantly lower premia than public companies do in cash acquisitions (Bargeron et al., 2008). From this point of view, then, an LBO signals to the market that other firms in the industry may be undervalued and therefore constitute attractive investments as well. This drives further M&A activity, reinforcing the signaling arguments presented above.

2.4 Theoretical Conclusions & Hypothesis Development

To recapitulate the findings from the extant literature, we can see that there are essentially two major effects which have been postulated to determine the impact an LBO has on target competitors. First, there is the argument that, due to various improvement mechanisms, target firms perform better after an LBO, which affects rival firms via product market competition. This has also been empirically substantiated, for example recently by Hsu et al. (2011) and Grupp et al. (2015), who find that long-term operating performance measures of rivals suffer

following leveraged buyouts in their industries. This point of view clearly points to the hypothesis that rival firms should experience negative abnormal returns, since capital markets would be expected to immediately take into account this deterioration of future performance. Indeed, this is precisely what Hsu et al. (2011) find: highly significant, unequivocally negative abnormal returns across for all reported event windows.

This stands in stark contrast to previous studies which have predominantly found the exact opposite, namely positive abnormal returns for target rivals. These include Slovin et al. (1991), Chevalier (1995a), Song and Walkling (2000)⁹, and Oxman and Yildirim (2011). As we have seen, the literature also has explanations for these findings at the ready. Within the product market competition domain, the ambiguous effects of a firm's leverage on competitive behavior may provide some arguments in this regard. Even more applicable, for less equivocal, are the signaling theories which provide a clear basis for explaining positive abnormal returns for the competition of LBO targets.

Based on all these observations, we turn to the development of hypotheses. Hsu et al. (2011) frame their research as a quest to find not just any abnormal returns, but explicitly negative ones, in effect only considering competition-based arguments. Given the theoretical basis we have established in this chapter, however, we refrain from following their example. It is our impression that there is still substantial uncertainty with regards to rival effects of LBOs. Therefore, it seems more prudent to hypothesize, more cautiously, potentially bidirectional effects. We formulate our first hypothesis accordingly.

H₁: Rival firms of LBO targets experience abnormal returns upon announcement of the transaction.

It is important to keep in mind that, given the theoretical foundation we depart from, we may actually expect zero-mean abnormal returns; not because there are none, but because the directionally opposing effects may cancel each other out. From this point of view, *H₁* can also be regarded as a test for which effect appears to dominate. To explicitly investigate this proposition, we formulate a secondary set of hypotheses.

⁹ Song and Walkling (2000) do not study LBOs specifically; they report, however, that “on average, rival firms earn positive abnormal returns regardless of the form and outcome of acquisition” (p.143)

H_{2a}: Positive abnormal returns among rivals are associated with information effects that signal positive industry development and increased likelihood of future M&A activity in the industry.

H_{2b}: Negative abnormal returns among rivals are associated with an expected improved competitive positioning of the LBO target.

3 METHODOLOGY

3.1 Event Study Methodology – General

The event study methodology is a veritable cornerstone of (corporate) finance research. It was first widely introduced to the discipline half a century ago¹⁰, when Fama et al. (1969) set out to “examine the process by which common stock prices adjust to [new] information” (p.1). In doing so, they described a research approach that became not only a mainstay going forward, but, impressively, remains largely unchanged to this day (MacKinlay, 1997; Kothari and Warner, 2007).

Retrospectively, the advent of the event study has been hailed as a “methodological revolution” (Binder, 1998, p.111). In the decades that followed the Fama et al. (1969) article, the approach they had outlined yielded an abundance of empirical insights. Regulatory changes, macroeconomic developments, modifications to accounting practices, as well as earnings announcements and other industry/firm-specific occurrences (not least M&A activities and their effects) were among the more popular subjects of event studies within finance and economics (Binder, 1998). However, at the same time their reach extended into a variety of other disciplines as well, including marketing, management, law, history, and political science (Corrado, 2011).

The underlying logic behind an event study in finance with stock prices (or returns) as the variable of interest is intuitive. A precondition for every study of this type is acceptance of the efficient market hypothesis in its semi-strong form, which concerns the market’s capacity and inclination to immediately react to new public information (Fama, 1970). While there has been considerable debate about whether – or at least to what extent – this hypothesis holds (Malkiel, 2003), its validity is simply a *condicio sine qua non* for event studies. Unless markets are expected to instantaneously absorb and reflect new information, there is little value in attempting to capture abnormal stock returns. A belief in efficient markets thus implicitly underlies every event study of this kind.

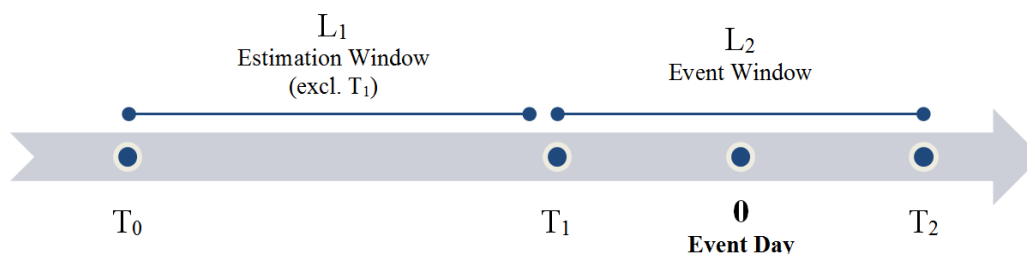
The basic methodological framework of event studies is straightforward. In his definitive article on the topic, MacKinlay (1997) provides a review-cum-manual that has become a standard work of reference in its domain. In it, he outlines a procedure that can be broken down into four distinct steps which constitute a complete event study.

¹⁰ Event studies had been in use already before, but it was not until Ball and Brown (1968) and Fama et al. (1969) that they gained significant traction. For a selected list of event studies dating back as far as 1933, see Corrado (2011).

Figure 1

EVENT STUDY TIMELINE

(adapted from MacKinlay, 1997)



First, the event of interest needs to be identified. It could also be, as in the study at hand, multiple events that are included. Furthermore, the event window's size has to be defined. This is the time period over which abnormal returns (and thus the hypothesized effect of the event) are measured. It is common to compare results from different event windows to get a more comprehensive picture. They could include just the event day $[0]$; a symmetrical distribution around it, say, $[-1;1]$ or $[-5;5]$; or an asymmetrical range, such as $[-1;3]$. These sets of numbers refer to T_1 and T_2 , respectively, as illustrated in Figure 1. The reason for including pre-event days is the possibility of rumors or information leaks affecting returns prematurely. This is particularly important in M&A-related studies, since news about these transactions may very well break before the official announcement date. By also looking at the days following an event, non-instantaneous market reaction is captured.

Secondly, the securities (companies) included in the study are to be defined. Typically in M&A event studies, this would be the takeover targets. In our case, however, it is their rivals that are of interest. As we will see, this distinction makes the firm selection process significantly more intricate.

In a third step, normal (expected) returns are calculated against which the sought-after abnormal returns may subsequently be contrasted. Again, the estimation window – the duration over which actual returns are averaged in some way – requires that a period length be defined. For daily observations, somewhere around 120-250 days prior to the event is a common choice (Kwoka and Gu, 2015). It is also important to note that the estimation and event windows, as depicted in Figure 1, should not overlap (MacKinlay, 1997).

Finally, abnormal returns are measured and tested for significance. They are defined as any (positive or negative) returns in excess of the normal return model's predicted values and may be calculated for any given day or cumulatively over a specific period. The analysis

mechanisms for abnormal returns can be as simple or complicated as required, ranging from classic t-tests to massively augmented parametric as well as non-parametric tests. They quantify to what extent the identified abnormal returns are statistically significant. Advancement in the sophistication of significance tests is one area where event studies have evolved the most throughout the decades (Kothari and Warner, 2007). As will be discussed below, the particular nature of this thesis' event study will require careful consideration with regards to significance testing.

3.2 Event Study Methodology – Specifics

One shortcoming of some previous studies in this specific field of research is their brevity and subsequent lack of transparency with regards to their methodological choices. Especially the critical process of rival firm identification is woefully underdeveloped in some papers (e.g. Hsu et al., 2011), which unfortunately makes their interesting findings somewhat less meaningful. In order to ensure transparency and reproducibility to the greatest extent possible, we devote significant space to our methodological discussion. What follows is an attempt to produce a clear and detailed step-by-step guide that describes the employed research process, the choices made and, most importantly, the justifications behind them. We start with a brief presentation of Capital IQ, Datastream, and Zephyr – the key data sources for the study – and then systematically traverse the event study process as outlined above.

Data Sources

The decision to utilize S&P's Capital IQ database was an easy one. It comprises a vast range of diverse data that proves particularly useful in LBO research as private equity transactions (especially from 1999 onward) are meticulously covered. Its capacity to serve as a one-stop-shop for LBO identification, rival identification, companies' stock prices, plus any other firm-level variables that may be of interest made it a most convenient tool for this thesis. The fact that S&P has its own strong industry classification system (GICS – more on that below) well integrated in the database was another benefit. It should therefore come as no surprise that Capital IQ has also been the data source of choice in previous studies with a similar focus (e.g. Hsu et al., 2011).

One drawback of the Capital IQ data was, however, that most securities were missing stock price records for certain dates. While unproblematic on a case by case basis, this meant that

combining all the data would have resulted in a great number of missing observations in aggregate. By using Datastream's padded stock price data, we were able to circumvent this issue. Datastream was subsequently also used to pull firm data for the regression analysis following the event study.

Finally, Zephyr, a prominent M&A-centric database, served as a source for additional, detailed information about individual transactions. In particular, it was used to verify announcement dates and also obtain rumor dates with which to perform alternative calculations. It also proved useful when classifying the individual deals as primary vs. secondary LBOs.

Step 1 – Event Definition & Event Window

With its focus on large, private-equity-backed, European LBOs between 2010 and 2016, the thesis has a firm grasp on precisely what type of events is of interest. Accordingly, the employed identification measures are largely self-explanatory. Overall, the filtering criteria applied to Capital IQ's M&A database comprise six dimensions:

(1) Merger/Acquisition Features: Leveraged Buyout (LBO)

This first step in filtering all M&A transactions is straightforward. Capital IQ lists all deals which have been categorized as leveraged buyouts – in total, 59.534 hits.

(2) Geographic Locations (Target/Issuer): Europe

The second criterion is equally clear-cut. We are interested in European LBOs only – overall, 28.713 deals remain. It is important to note that this includes all transactions with European targets. The private equity sponsors may well be non-European.

(3) M&A Announcement Date (Including Bids and Letters of Intent): 1/1/2010-4/1/2016

As discussed in the previous chapter, the second LBO wave ended with the onset of the financial crisis in 2007/08. We are interested exclusively in buyouts that took place following this period, i.e. post-crisis. We therefore pick, somewhat arbitrarily but confidently, the beginning of 2010 as the start of our focal period. Of course, this is not to say that the crisis had ended at this point. For our purposes, “post-crisis” merely means that the initial shock had

been overcome and widespread frenzy started to give way to recovery. The fact that 2010 was the first year after the crisis to see an uptick in global M&A activity (Thomson Reuters, 2015) lends support to our decision. Since we do not investigate long-term post-buyout effects, we are able to include even the most recent transactions. The cutoff date is April 1st, 2016. Within this timeframe 11.303 European LBOs are recorded in the Capital IQ database.

(4) Industry Classifications (Buyers/Investors): **Diversified Financials**

This criterion was put in place as a backup measure to ensure that all transactions identified as LBOs in step (1) are truly buyouts of interest to this thesis – i.e. private-equity-backed, financially motivated (non-strategic) deals. Using S&P’s own industry classification system (GICS), we configure all buyers to fall into the “Diversified Financials” category. This segment includes precisely the types of investment firms typically associated with LBOs while excluding other financial entities that may be out of place in this context, such as conventional banks, insurance companies, and real estate investment trusts (REITs). This step cuts the number of applicable deals roughly in half – to 5.527. We believe the majority of deals “lost” in this round of filtering are buyouts that may be partially or largely based on strategic intent and therefore fall outside of this thesis’ definition of an LBO.

Industry Classifications (Target/Issuer): **Non-Financial**

Financial companies are fundamentally different from the rest of the corporate sector. They are also only a small fraction of the overall LBO target market. Thus, for the sake of improving comparability without having to sacrifice too much in sample size, we choose to remove financial targets from the sample. The exclusion of buyouts of financial institutions is in line with established convention for this type of study (Grupp et al., 2015). As a result, 4.642 deals remain.

(5) Company Type (Target/Issuer): **Companies with Public Financials**

This thesis is concerned primarily with movements in stock prices of rival firms. Having publicly listed target companies is therefore not essential. However, for purposes related to peer identification we included a filter for targets with public financials. The insights gained from disclosed financial data, such as revenues or total assets, are subsequently useful in

identifying core competitors. This reduces the number of applicable LBOs in Capital IQ to 287.

(6) Total Transaction Value (GBP, million): **Greater than 25**

Finally, we define a minimum limit for transaction value at GBP 25 million. It can be expected that, the smaller the LBO target, the more difficult it is to find public competitors. Furthermore, bigger deals tend to attract more attention – and consequently yield better information. For example, news reports are often essential in checking whether or not the official announcement date reflects the true timing of when news about a deal got out. Due to the nature of event studies, rumors and information leaks have tremendous power to distort results. Event date definition thus plays a vital role in determining research quality and outcome. Similarly, the relatively better information to be found on bigger target companies also helps in the rival identification process. This ultimate filter criterion results in a final LBO sample size of 169 transactions.

There is one additional criterion based on which deals could have been filtered, but ultimately were not: our sample deliberately includes secondary buyouts, i.e. LBOs where a private equity firm buys the target from another private equity firm. In other words, it is a buyout of a firm which has already undergone an LBO. As will become evident in the regression analysis, this variable can be useful in disentangling the effects that may produce abnormal returns for rivals following an LBO in their industry.

With regards to the event window, we follow the convention of calculating (cumulative) abnormal returns over various periods. The event day [0] is in most cases simply the official announcement day of the transaction as recorded in Capital IQ. However, in order to validate this key factor, the dates were cross-checked with the ones reported in Zephyr. There, we also checked for any additional information that might have been germane to the identification of the actual event day of interest, such as release dates of letters of intent preceding the announcement, or any indications that information may have leaked prematurely. The use of multi-day event windows further alleviated these concerns. As a robustness check for our results, we decided to run the event study calculations not only for the official announcement dates but also for rumor dates.

Step 2 – Firm Selection

The identification of rival firms of the buyout target is arguably the single most important step in the research process of this thesis. Output quality can only ever be as good as input quality, so ensuring that the competitor selection reflects market realities is imperative. Accordingly, much consideration and substantial time investments and have gone into this part.

In general, researchers face a trade-off when it comes to rival identification. On the one hand, they could take a hands-on approach, for example by manually sifting through annual reports and other company disclosures, industry reports, business news, etc. This method should ultimately yield high quality data since the rivals thus identified would be expected to truly be the target's core competitors. Unfortunately, such precision comes at a price, as it is extremely time-consuming and, more importantly, introduces subjectivity into the process.

On the other hand, researchers may simply match competing firms by relying on industry codes. These are numerical values that have been assigned to different sectors, industries, and sub-industries so as to categorize any company based on its activities. Numerous systems have been developed and are in use, each with varying degrees of granularity and exhaustiveness. The most common classification systems include SIC (Standard Industrial Classifications), which come in both a US and UK version; NAICS (North American Industry Classification System) which serves as a successor to the nonetheless still in use US SIC codes; as well as various systems developed by supranational bodies or financial services companies, such as GICS (S&P/MSCI) or TRBC (Thomson Reuters). Companies with the same codes should be active in the same business areas and are thus assumed to be competing with each other to some extent. This approach is objective, reproducible, and time-efficient, as it can be more or less fully automated using modern databases and filtering mechanisms. The disadvantage is that industry codes, while conceptually elegant, may leave a lot to be desired in practice. As firms can have multiple codes, some of which are hopelessly generic, the data quickly becomes messy, if not outright irrelevant.

In practice, objectivity and efficiency seem to beat meticulousness. The latter of the two outlined approaches has been widely used in studies that rely on rival identification, both in the early days of LBO research and in more recent investigations (e.g. Slovin et al., 1991; Hsu et al., 2011; Grupp et al., 2015). Since the literature thus clearly lends legitimacy to this approach, we initially intended to follow it for this thesis, too. However, a preliminary data collection and quality assessment procedure yielded alarming results. Upon manually inspecting the business descriptions of automatically identified rivals, it was clear that the

method was positively unreliable. For example, seeing that a fashion retailer had Italian automotive company Ferrari listed as a core rival left us skeptical, to say the least. When we then came across a British water supplier whose supposed competitor was RSC Energia – none other than the Russian lead developer and operator of the International Space Station – any remaining belief in the approach was shattered.

The basic problem seems to be that business realities are often simply too multifaceted for the relatively crude industry classification systems. Especially in a research context that demands high levels of specificity, exclusive reliance on these systems therefore seems ill-advised. After having seen the (severe) limitations of the pure, automated industry code approach in action, we concluded that modifications to this method were required. Ultimately, we employed what may best be described as a hybrid tactic that combines the raw power of automation with the quality of manual screening. We used industry codes as a starting point and then introduced additional criteria to filter out unsuitable companies. While this necessarily involved personal judgment to some degree, we designed the overall identification process so as to ensure the highest objectivity possible.

Our hybrid method, described in detail in the following section, is not completely unprecedented. Oxman and Yildirim (2011) also complement the industry code approach with additional matching steps to achieve greater similarity between targets and their respective rivals.

Overall, the painstaking manual data collection method employed in this thesis stands in stark contrast to cruder, broad-brush approaches to rival selection seen in previous studies of this research area.

Selection Process in Detail

As mentioned above, industry codes were used as starting points for the rival identification process. The classification system used was GICS (Global Industry Classification Standard), jointly developed by S&P and MSCI. The logic behind choosing this particular standard over others is twofold. First, in a comparison study between four common classification systems, Bhjoraj et al. (2003) show that GICS is superior to other standards when it comes to analyzing capital market variables such as stock returns in an industry setting¹¹. It is for this reason that GICS has also been a preferred choice of academics investigating rivals effects of LBOs (Hsu

¹¹ GICS' superior performance is most pronounced among larger firms, making it particularly suited for this study.

et al., 2011; Grupp et al., 2015). Secondly, the system from S&P is the natural choice since data collection for this study was conducted within S&P's Capital IQ database. Naturally, the GICS classification is seamlessly and comprehensively integrated with all other company data available on the platform. On the top level, GICS is based on ten broad sectors (e.g. Industrials, Financials, Consumer Discretionary, etc.) which are then broken down into further segments. On the lowest level there are 156 sub-industries (MSCI, 2014). The number of sub-industries a company belongs to is thus a useful indicator for how diversified it is.

To counteract the potential imprecision embedded in this first step, we first read the target company's business description (available within Capital IQ) and checked if the provided industry codes seemed appropriate. We also manually removed industry codes that are too generic to have any informational value for our specific purposes. For example, one all too common sub-industry is "Diversified Support Services" (GICS code 20201070), a catch-all category that may include, among others, cleaning services, catering services, storage and warehousing, uniform rental services, repair and maintenance services, or simply "other business support services". If this category is the only overlap between two companies, odds are their industries are entirely unrelated. Removing this code (and similarly unspecific, high-level ones) allowed us to circumvent this issue. Firms which overlapped with the target in multiple GICS codes were prioritized, as this likely indicates a higher degree of similarity.

Once the target company's business activities were defined and codified via GICS, a geographic filter was applied. In the vast majority of cases this was simply set to "Europe" to account for the European focus of the study. For some companies, however, it was clear that the entire continent would be too broad to capture their effective competition. This was the case for some clearly national firms such as certain energy or telecommunication companies, but also for other companies whose business models simply keeps them regionally confined. For example, PizzaExpress' business description clarified that within Europe they are only active in the UK and Ireland¹². This information allowed us to consequently focus our search for competitors on this area. As is to be expected, this issue affected primarily industries typically associated with the public sector as well as some service providers. In contrast, competitive forces acting on (export-oriented) product companies naturally know no borders.

At this point of the process, companies within the same business activities and the same geographical focus have been selected. For competitive industries, however, this may still leave scores of potential rivals. Getting from general competition to core competition requires

¹² This example serves as yet another reminder of the benefits of (semi-)manual rival identification.

additional criteria. In line with Oxman and Yildirim (2011) we make use of financial statement data to further ensure similarity between target and rivals. Specifically, for every LBO we look at the target's revenue and, based on this value, define a two-sided range around it, usually around +/- 50%. While not an ideal proxy for overall similarity, it does provide some idea of the general nature of a business (especially firm size and market share). Revenues are also a preferable choice over, say, earnings measures, since the latter are more volatile and generally harder to interpret. Another benefit is data availability, as Capital IQ records revenue data even for many privately held firms. This step ensures that, for example, a small or medium sized niche ERP (enterprise resource planning) software developer is not immediately matched with industry behemoth SAP, but instead with other business application providers of similar dimensions.

In the penultimate filtering step, for obvious reasons (the study's need for stock prices) only publicly traded rivals were included.

In most cases, all of these criteria combined resulted in somewhere between one and 50 competitors. If there were no or too few hits, the revenue range was extended. Similarly, if there were too many results (sometimes 100+ hits) we would redefine the selection criteria more narrowly.

Finally, in the most laborious but crucial last step, we would then read through every potential rival's business profile and, in particularly equivocal cases, consult further sources such as their web presence, investor relations documents, or industry reports. Over the course of our data collection we thus manually analyzed well over 1.000 firms to find suitable matches. The reward for this time-consuming process was the confidence that, for each of the 124 LBOs, our semi-hand-picked rivals should only be firms which are indeed in direct competition with the targets. Overall, 3.2 competitors per LBO were included on average.

Selection Process Limitations

One potential limitation of the outlined approach is that rival identification is conducted as of today (April 2016) instead of the date of the respective LBO. Over time, firms and industries change, so this could lead to a sub-optimal sampling of competitors. And, indeed, if the study had a longer time horizon (like Hsu et al., 2015, who look at the 29-year period between 1980 and 2008) this would certainly be of concern. However, we remain confident in our method for two reasons. First, due to the limited time frame (2010-2016), all LBOs occurred within the past six and most LBOs within the past three to four years. It is reasonable to assume that

for the vast majority of cases, the competitive landscape has not been extensively transformed over this duration. Secondly, companies' business descriptions would make mention of recent major organizational reorientations that would affect the competitive assessment. By perusing them for every firm involved, matching errors due to this matter should be minimized.

Step 3 – Normal Returns Model

In order to be able to discover abnormal returns one first needs to define a normal return to base deviation measurements on. There are two fairly simplistic but powerful models that have been widely used (MacKinlay, 1997). The *constant mean model* takes the average return for a security over the estimation period and assumes this to be the constant normal return one may expect. The *market model* adds one layer of complexity by making normal returns variable. It is based on a regression of the past returns of each security observed in an event study against the returns of a market index:

$$R_{it} = \alpha_i + \beta_i R_{mt} (+\varepsilon_{it})$$

R_{it} and R_{mt} are the returns in period t of stock i and the market index, respectively. For each security, the intercept (α) represents the constant part of the normal return, which is complemented by a variable part resulting from volatility in the market (R_m) and the stock's sensitivity to it (β). ε_{it} denotes the error term. By definition, for observations falling inside the event window this error term is the abnormal return. The benefit of this model is intuitive: broader market movements that may affect stock returns are now removed. In a constant model this variation, unrelated to the event being investigated, would be classified as abnormal and could thus distort results.

Naturally, there are other, more sophisticated normal return models, such as multi-factor models that augment the regression above with additional independent variables (e.g. industry indices returns). However, as MacKinlay (1997) is quick to point out, any gains from these advanced models are marginal at best. This is in line with earlier findings, where Brown and Warner (1980) report that "beyond a simple, one-factor market model, there is no evidence that more complicated methodologies convey any benefit" (p.249). It should therefore come as no surprise that the market model has been the most commonly used normal returns model (Armitage, 1995).

For these reasons, we also rely on a market model to estimate normal returns¹³. Doing so requires an index that serves as a proxy for the market. By far the most common indices used in event studies are CRSP ones (Center for Research in Security Prices) or the S&P500 (MacKinlay, 1997). However, their US-centric nature disqualifies them for the purposes of this thesis.

Instead, we opt for various national market indices reflecting where firms are traded. In addition, again as a robustness check, we recalculate everything using the MSCI Europe, a broader index that aims to comprehensively cover stocks from the continent. The MSCI Europe is weighted by market capitalization and is therefore a value-weighted type of index. There has been some discussion in the literature about the use of equally-weighted vs. value-weighted indices. While some arguments have been made about the superiority of equal weights under very specific circumstances (Brown and Warner, 1980), practically, the choice makes little difference. Indeed, subsequent investigations have shown that the two are equally suitable. As Krueger and Johnson (1991) summarize eloquently and succinctly, “anomaly research findings are generally robust to market surrogate selection” (p.579). Finally, our choice of also employing the MSCI Europe for a European event study is furthermore legitimized due to precedent in the literature (for a recent example, see Sahin and de Haan, 2015).

When it comes to the estimation window, i.e. the pre-event period throughout which normal returns are calculated, there is yet another trade-off that has to be balanced. On the one hand, a longer window should provide more robust parameter estimates for α and β , as they are then less susceptible to unrepresentative short-term fluctuations. On the other hand, the longer the estimation period, the higher the possibility of calculating outdated estimates that might be less representative going forward. In practice, around 100-300 daily observations prior to an event are commonly used (Armitage, 1995). We also follow this convention, and observe returns over a 252-day period ending on $T_1 - 1$, the day before the event window. This period reflects one trading year and therefore cancels out any potential distortions due to dividend effects that may otherwise occur.

¹³ In order to check the robustness of our results, we additionally calculate abnormal returns based on a constant mean model.

Step 4 – Abnormal Returns & Significance Tests

Following steps 1-3, abnormal returns may be calculated by subtracting the estimated normal returns from observed actual returns for each company. These abnormal returns are then aggregated and tested for overall significance, for example via a standard t-test:

$$t_statistic = \frac{CAAR}{\frac{s}{\sqrt{N}}}$$

... where *CAAR* is the cumulative average abnormal return across all firms, *s* is the sample standard deviation of all cumulative abnormal returns, and *N* is the number of observations (firms).

In most cases, this is all there is to significance testing. The event study setup in our particular case, however, demands some additional statistical considerations due to an issue known as clustering. This refers to the problem of having abnormal returns that exhibit cross-sectional correlation. For detailed discussions and potential solutions, see Bernard (1987) and Kolari and Pynnönen (2010). In simple terms, clustering may most illustratively be explained by means of visualization.

Figure 2

TYPICAL EVENT STUDY I

Multiple events dispersed over time; one observation each

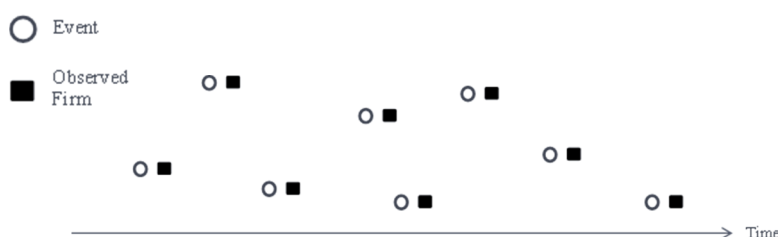


Figure 3

TYPICAL EVENT STUDY II

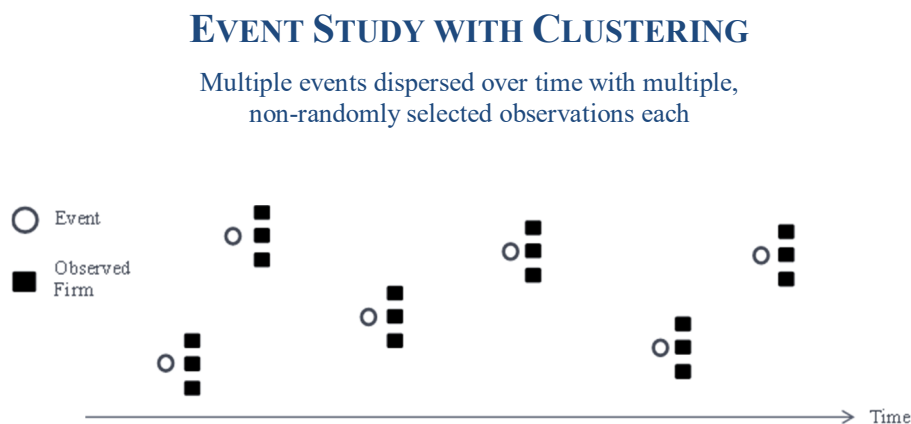
One event with multiple, randomly selected observations



Figures 2 and 3, respectively, display two common types of event studies. The former shows multiple events dispersed throughout time with one focal firm each. The latter illustrates a single event with multiple, randomly selected focal firms. Due to the dispersion through time (Figure 2) and randomness among targets (Figure 3), there is no reason to expect cross-sectional dependencies. If this thesis only investigated LBO targets instead of rivals, it would

look like Figure 2. However, with the focus on competitors, each event (LBO) includes multiple firms from the same industry. As a result, their return values are cross-sectionally dependent. Figure 4 illustrates this scenario.

Figure 4



Note that cross-correlation is only an issue within each block, but is zero between blocks (Kolari and Pynnönen, 2010). As a result of this correlation, without adjustments in the calculations, the variance term of aggregate abnormal returns will be underestimated and the null hypothesis of zero abnormal returns will be rejected too often, i.e. the results will be biased towards significant non-zero abnormal returns (Bernard, 1987).

There are various ways of dealing with this issue. For example, Kolari and Pynnönen (2010) propose an enhanced t-test statistic that incorporates a term which captures the cross-correlation between estimation period regression residuals of cross-dependent firms. Another, more intuitive solution is to simply aggregate firms within each block into a single, equally-weighted portfolio of securities (MacKinlay, 1997). This way, dependencies between returns are captured and no longer distort any subsequent significance tests. The price for this is a loss of information, as previously individual observations are now reduced to a single collective one per event.

If even unadjusted significance tests (such as the t-test described above), which ignore clustering, fail to reject the null hypothesis, then, of course, the issue should no longer be of concern. Any adjusted, and hence more cautious, tests which account for the cross-sectional correlation would only lead to even more conservative results. That is to say, they would be even further from rejecting the null hypothesis. In these cases, conducting sophisticated clustering-proof tests will therefore not yield any insightful additional information.

Follow-up Regression

The central purpose of this thesis is to investigate if there are any abnormal returns for LBO targets' rivals. This, we aim to achieve by means of the outlined event study. However, “to provide a more complete picture ... cross-sectional tests are a standard part of almost every event study” (Kothari and Warner, 2007, p.19). Typically, this means regressing the observed abnormal returns against firm characteristics in pursuance of further insights into how these returns may be explained. Such a follow-up regression analysis is relevant even in the case of insignificant, zero-mean abnormal returns (Kothari and Warner, 2007).

In order to get the most out of this additional investigation, it makes sense to base the exact specification of the regression model on the actual results from the event study. We will therefore revisit this subject in Chapter 5 following the presentation of the outcomes of our primary inquiry.

4 EVENT STUDY RESULTS

In order to be able to report comprehensive and, more importantly, robust results, we introduced a number of variables to this event study. For example, as previously mentioned, we employ both a market model and a constant mean model to estimate normal returns. In the case of the former, we use both individual country indices and the MSCI Europe index as separate benchmarks. Furthermore, we calculate abnormal returns based on various estimation periods and for every conceivable single- and multi-day event window within the ten days surrounding the event day. Finally, on top of the official LBO announcement dates we also investigate valuation effects around the rumor dates. As a consequence, the combinatoric realities stemming from our investigative rigor would make an exhaustive presentation of all results prohibitively voluminous.

The core outcomes reported herein are therefore limited to four sets of results: abnormal returns based on the market model (using country-specific indices) for event windows of [0], [-1;1], [-1;3], and [-3;3] days. The market model is widely accepted as a standard estimation model and benefits from a higher level of sophistication compared to the constant mean model. Country indices were preferred as benchmarks over a single European index because of their higher explanatory power as indicated by the higher average R^2 achieved in the normal return estimation regressions. The relatively short event windows reflect the expectation that the information from transaction announcements should disseminate rather rapidly. There is no reason to believe why it would take capital markets, which for the purposes of this thesis are assumed to be at least semi-efficient, several days to react. Insights from calculations using other variables will sporadically complement the core sets where appropriate.

4.1 Sample

All results are based on 124 leveraged buyouts which have been announced between January 2010 and April 2016. Figure 5 illustrates how the included transactions are distributed over time. The sample achieves a relatively even balance across all full years.

A total of 393 rival firms have been identified, for an average of 3,2 core competitors per LBO target.

Figure 5

LBOs in Sample - Temporal Distribution

(by announcement date)

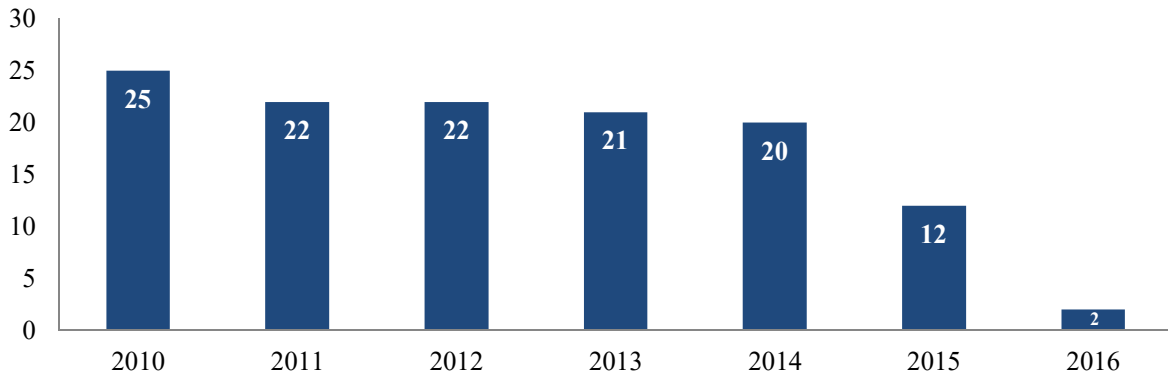
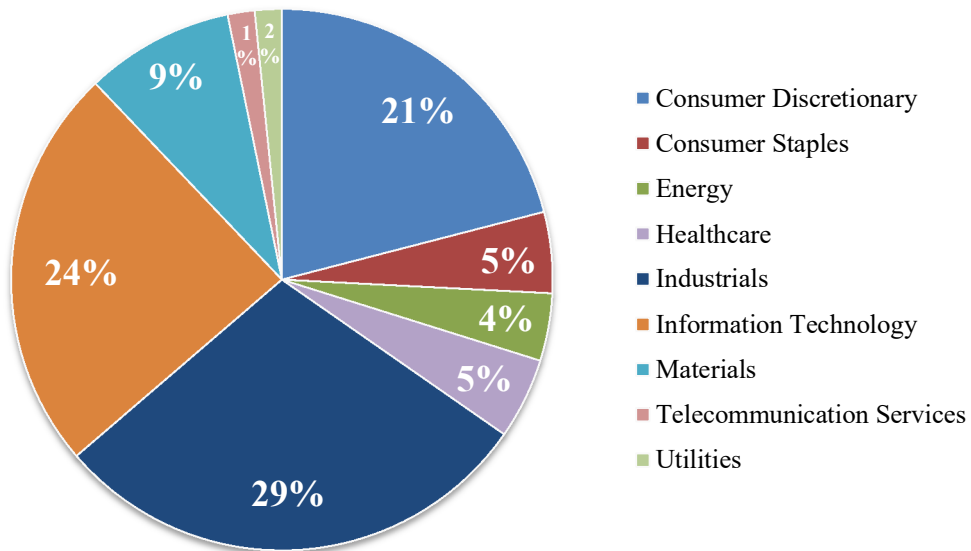


Figure 6 provides information about which industries the LBOs occurred in. The three most represented sectors are Industrials, Information Technology, and Consumer Discretionary. In total, every sector from the GICS classification system is represented in the sample with the exception of Financials, which has been excluded for reasons previously outlined.

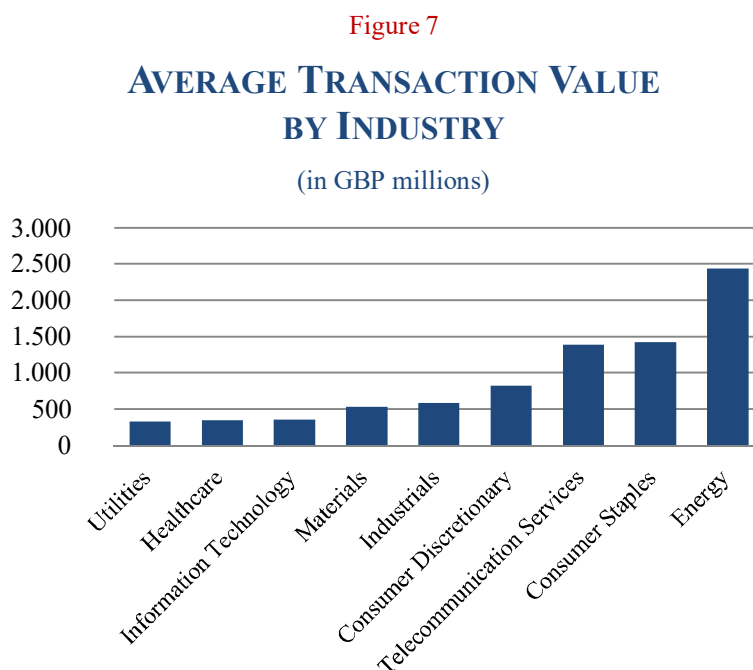
Figure 6

LBOs in Sample - Industry Classification



In terms of transaction values, the sample comprises a broad spectrum of observations, ranging from GBP 27,7 million to GBP 6,8 billion. As explained in Chapter 3, a filter was put in place to exclude any deals smaller than GBP 25 million, which explains the bottom threshold in our sample. Table 1 and Figure 7 provide a detailed overview of the transaction sizes.

Table 1	
LBO TRANSACTION VALUES	
(in GBP million)	
Min. Transaction Value	27,7
Median Transaction Value	249,1
Mean Transaction Value	692,0
Max. Transaction Value	6.792,8



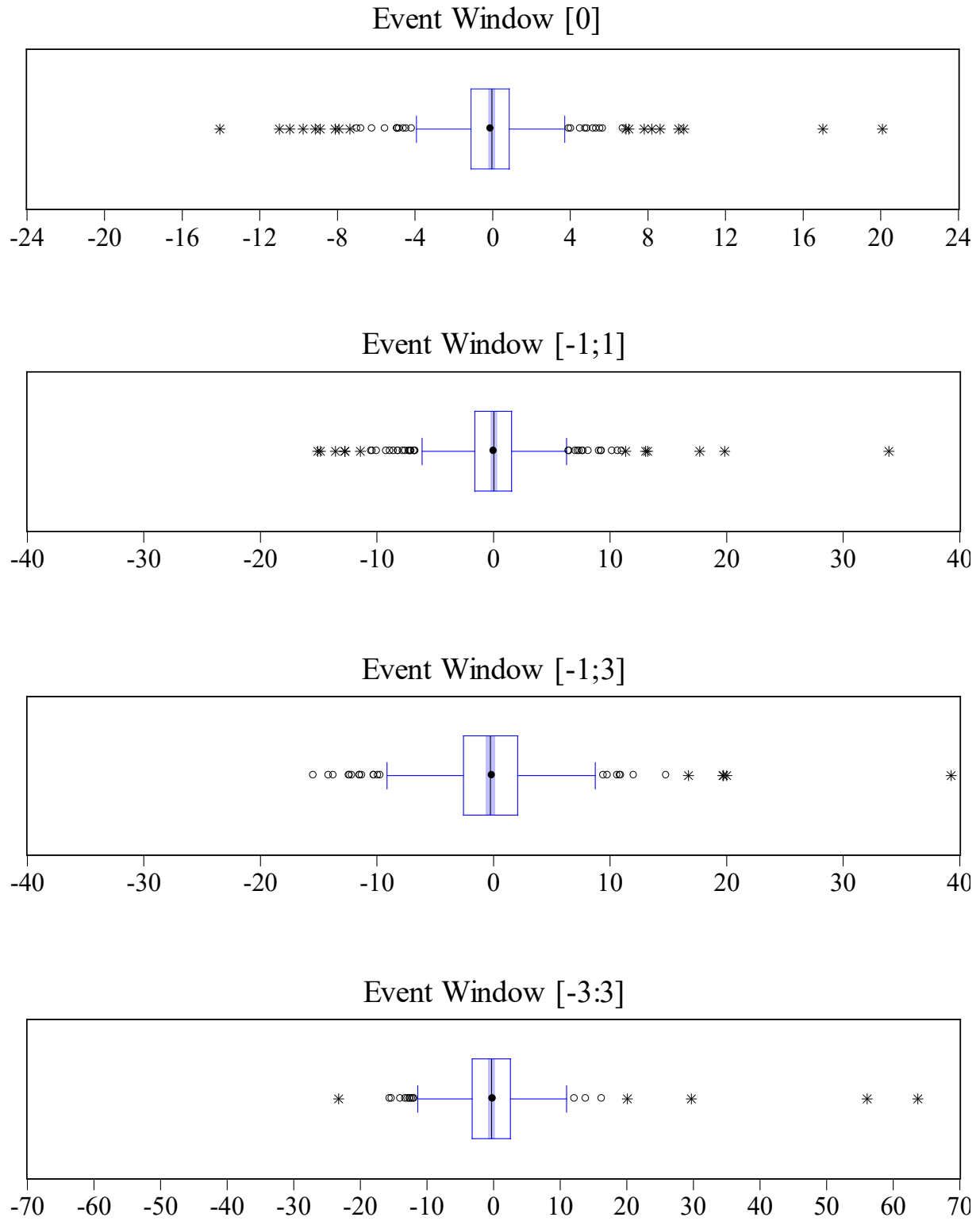
4.2 Results – Descriptive Statistics

On an aggregate level, following LBO announcements, abnormal returns for target rivals are close to zero, with a mean average values of -0,1% for the announcement date [0], 0,05% for event window [-1;1], -0,09% for [-1;3], and -0,08% for [-3;3]. There is some dispersion around these values with standard deviations ranging from 3% for the single announcement day [0] to 7% for the cumulative seven day period [-3;3].

The boxplots in Figure 8 illustrate in detail the distribution of all individual abnormal returns across the sample. The dot and the line within the central blue box represent the mean and median values, respectively. The outer borders of the box delineate the first and third quartiles. Put differently, 50% of all observations by definition lie within the box. Values outside of the staples (the outermost blue vertical lines) fall outside the range of +/- 1,5 x IQR, where IQR is the interquartile range defined as the breadth of the box. Circles represent near outliers, whereas stars are considered far outliers. The data displayed is based on LBO announcement dates. In unreported results, we found virtually identical outcomes using rumor dates instead.

Figure 8

CUMULATIVE ABNORMAL RETURNS DISTRIBUTION [%]



4.3 Results – Inferential Statistics

In order to test for significance, we employed the standard t-test as presented in Chapter 3. In a first round of analysis, we applied it to every firm in the sample on an individual basis. In these instances, the numerator of the t-statistic is the (cumulative) abnormal return for the individual firm, and the denominator is the corresponding standard error from the estimation period regression. Out of 393 observations, the vast majority of firms, namely 349 or 89%, display insignificant abnormal returns. In contrast, 44 results show significance at a 10% level, out of which 31 are also significant at 5%. Interestingly, we find that the significant results are split 50-50 between positive and negative abnormal returns¹⁴.

While such an individual analysis may provide some first insights, event studies of this nature are much more concerned about results in the aggregate (MacKinlay, 1997). We now turn our attention to collective effects across the entire sample. To do so, we apply the t-test to cumulative average abnormal returns (CAAR). Here, too, we find zero-mean abnormal returns which are not significant¹⁵. Tables 2 and 3 provide detailed results, including p-values (reported in brackets), for the four focal event windows based on different normal return estimations.

Table 2

CAAR – INDIVIDUAL RIVAL FIRMS

based on LBO announcement dates

Event Window	Normal Return Estimation Model		
	Market (Country Indices)	Market (MSCI Europe)	Constant Mean
[0]	-0,096% <i>(0,521)</i>	-0,012% <i>(0,938)</i>	0,057% <i>(0,718)</i>
[-1;1]	0,054% <i>(0,806)</i>	0,138% <i>(0,538)</i>	0,289% <i>(0,223)</i>
[-1;3]	-0,092% <i>(0,720)</i>	-0,017 % <i>(0,948)</i>	0,077% <i>(0,779)</i>
[-3;3]	-0,080% <i>(0,815)</i>	0,057% <i>(0,870)</i>	0,220% <i>(0,547)</i>

¹⁴ These observations were based on event window [0]; however, we find virtually identical results for other periods as well.

¹⁵ In unreported results, the few significant abnormal returns we do find are weeks away from the actual event and thus too far removed to be plausibly associable therewith.

Table 3

CAAR – INDIVIDUAL RIVAL FIRMS

based on LBO rumor dates

Event Window	Normal Return Estimation Model		
	Market (Country Indices)	Market (MSCI Europe)	Constant Mean
[0]	-0,120%	-0,076%	0,000%
	<i>(0,365)</i>	<i>(0,571)</i>	<i>(0,997)</i>
[-1;1]	0,004%	0,022%	0,062%
	<i>(0,988)</i>	<i>(0,931)</i>	<i>(0,819)</i>
[-1;3]	-0,287%	-0,261%	-0,130%
	<i>(0,314)</i>	<i>(0,361)</i>	<i>(0,660)</i>
[-3;3]	-0,186%	-0,232%	-0,117%
	<i>(0,571)</i>	<i>(0,485)</i>	<i>(0,734)</i>

In Chapter 3 we discussed the issue of clustering, which causes the classic t-test, when applied to the CAAR across the sample, to be misspecified, since it fails to account for the cross-correlation between firms affected by the same event. We have argued that ignoring cross-sectional dependencies will lead to inferences biased towards rejecting the null hypothesis of zero abnormal returns. While such a rejection did not occur in this case, we still conduct a second significance test which takes care of the clustering issue in order to validate our findings. By using cumulative abnormal returns from equally-weighted rival portfolios instead of individual rivals, clustering concerns are circumvented. As expected, though, this does little to change the interpretation of the overall results. Tables 4 and 5 illustrate how, on a portfolio level as well, abnormal returns are in aggregate not significantly different from zero. Additional detailed results for all relevant event windows may be found in the appendix.

Finally, while not the focus of this investigation, we also checked abnormal returns for the LBO targets themselves (the ones which were publicly traded prior to the buyout). We find a highly significant CAAR of 12,4% on the day of the announcement – much to the delight of many a shareholder, we presume.

Table 4

CAAR – PORTFOLIOS OF RIVAL FIRMS

based on LBO announcement dates

Normal Return Estimation Model			
Event Window	Market (Country Indices)	Market (MSCI Europe)	Constant Mean
[0]	---	0,206%	0,036%
		<i>(0,337)</i>	<i>(0,866)</i>
[-1;1]	---	0,336%	0,082%
		<i>(0,385)</i>	<i>(0,831)</i>
[-1;3]	---	0,034%	0,091%
		<i>(0,938)</i>	<i>(0,836)</i>
[-3;3]	---	0,155%	0,078%
		<i>(0,791)</i>	<i>(0,894)</i>

Table 5

CAAR – PORTFOLIOS OF RIVAL FIRMS

based on LBO rumor dates

Normal Return Estimation Model			
Event Window	Market (Country Indices)	Market (MSCI Europe)	Constant Mean
[0]	---	0,065%	0,044%
		<i>(0,758)</i>	<i>(0,837)</i>
[-1;1]	---	0,267%	0,094%
		<i>(0,455)</i>	<i>(0,792)</i>
[-1;3]	---	0,215%	0,104%
		<i>(0,598)</i>	<i>(0,799)</i>
[-3;3]	---	0,428%	0,079%
		<i>(0,458)</i>	<i>(0,891)</i>

4.4 Discussion

The heavily quantitative preceding section provided us with a great deal of numbers – some of them remarkably close to zero (the returns), others remarkably far away from it (the p-values). In this concluding part of the chapter, we move from a purely numerical to a more contextual discussion of the results. Given the findings, this will be a fairly straightforward affair.

What does the data tell us then about rivals' stock price reactions to leveraged buyouts? Essentially, what we find is the capital market equivalent of a collective shrug. In other words, we do not find any support for our first hypothesis H_1 in the data. This stands in stark contrast to previous studies which have investigated the same phenomenon and found (highly) significant abnormal returns, albeit in both directions. What makes this particularly curious is the fact that the sample used for this study focuses explicitly on core competitors instead of a broader range of rivals. They would be expected to display the most intense reactions to private equity backing of one of their peers. This, interestingly, is not at all reflected in the data at hand – at least not at first sight. There is, however, an alternative interpretation that would reconcile our findings more with the extant literature. Casting aside the notion of rivals' apparent indifference, one could argue that it is not the valuation effects that are zero, but specifically the *net* valuation effects. Based on this line of reasoning, abnormal returns are on average zero not because there is no impact on competitors, but because simultaneously occurring positive and negative effects cancel each other out. While this notion has intuitive appeal and finds theoretical support, with regards to solely the results from this event study it is pure speculation. The follow-up cross-sectional test described in the next chapter will attempt to shed more light on this issue.

5 REGRESSION ANALYSIS

As briefly mentioned in Chapter 3, a regression analysis following an event study is relevant even if abnormal returns are insignificant (Kothari and Warner, 2007), as is the case in our study. While it may seem that there is little left to explain after finding, essentially, nothing, the data at hand, coupled with firm characteristics variables may, in fact, allow us to disentangle some of the opposing effects that influence rivals' stock price reactions. From the descriptive statistics of our results we know that even though most of our observed abnormal returns reside close to zero, there are outliers on both sides. Furthermore, the rather symmetrical distribution around zero illustrates how positive and negative effects appear to cancel each other out to some extent. Looking for qualitative differences between the firms with lower abnormal returns versus the firms with higher ones could highlight through which dynamics competitors may benefit or suffer from an LBO of one of their peers.

5.1 Regression Specification

In order to specify an appropriate regression model, we turn back to the theoretical foundation of this thesis as laid out in Chapter 2. It has been suggested that competitive forces, unleashed by financial, governance, and operational changes in the LBO target, should hurt its rivals and put downward pressure on their stocks upon the LBO announcement. On the other hand, signaling effects have been posited to boost share prices. If variables are included in the regression which capture these dimensions, it may be possible to test the validity of these claims and gain further insight into the intra-industry dynamics of LBOs. Doing so, however, requires a certain degree of creativity, as it is not entirely straightforward to package two such multifaceted issues into quantifiable proxy observations ready for use in a regression. In the following, we delineate our approach to overcoming this obstacle.

First, for the competition effects perspective, we turn to the construct of financial constraint as a backdoor through which we arrive at an operationalizable substitute factor. The underlying logic is this: a shift in competitive behavior of the LBO target is expected to elicit a response from its peers. How the exact dynamics play out is actually not all that relevant. No matter how the competitive behavior in an industry is influenced by an LBO or how rivals will want to react to these changes, it seems axiomatic that financially constrained firms will always be at a disadvantage because their relative lack of access to capital will allow them fewer strategic responses. Based on this line of reasoning, and if we accept the hypotheses from the

product market competition literature, we would therefore expect financially constrained firms to exhibit lower abnormal returns. Put differently, if we regress abnormal returns against a proxy for financial constraint, we would expect to see a negative coefficient associated with the financial constraint variable. If that is the case, this may be interpreted as support of the arguments based on competitive dynamics following an LBO.

There has been much debate about how to best operationalize financial constraint. As a construct that is not directly observable, most workaround solutions have used proxy measures “based on what firms say, do, or look like” (Farre-Mensa and Ljungqvist, 2016, p.303). For example, it has been suggested that firms which pay no dividends or have no credit rating (implying no access to public debt markets) may be classified as financially constrained (Faulkender and Petersen, 2006). Another early, misleadingly intuitive approach was to look at the cash flow sensitivity of firms, i.e. how sensitive a company’s investment decisions are to the availability of internal capital (Fazari et al., 1988). It was proposed that a higher sensitivity is associated with a higher degree of financial constraint. However, in a seminal response to this proposition that triggered an influential debate between the two sets of authors, Kaplan and Zingales (1997) argue that the opposite holds true. Employing a different approach (qualitative analysis of firms’ annual reports), they conclude that, in fact, less constrained firms’ investments are more sensitive to cash flows. Building on the work of Kaplan and Zingales (1997), Lamont et al. (2001) then developed what they termed the KZ index, which consists of five linearly combined accounting ratios. This index has proved immensely popular and has been widely used since (Farre-Mensa and Ljungqvist, 2016). There have also been attempts to improve upon it over the years. Most notably, Hadlock and Pierce (2010) in a comprehensive analysis compare various common financial constraint measures. Their findings cast doubt on their validity. Arguing that the two unequivocally useful predictors of constraint are firm size and age, they develop a new index based on solely these two variables. Appropriate functional form and coefficients are derived from ordered logit models. In its final form, the SA index looks like this:

$$SA_Index = (-0,737 * Size) + (0,043 * Size^2) - (0,040 * Age)$$

... where size is the log of inflation-adjusted total assets and age is the number of years the company has been public. In accordance with their regression data, two modifications to the inputs apply: size is windsorized at \$4,5bn and age is windsorized at 37 years (Hadlock and

Pierce, 2010). The older and bigger a firm is, the more negative the SA index, reflecting lower financial constraints. In contrast, smaller and younger firms will have a less negative SA value (i.e. closer to zero), indicating higher financial constraints¹⁶.

While relatively simplistic in nature, the authors assert that “this index would appear to be a reasonable choice for measuring financial constraints in many contexts” (p.1929). The SA index has subsequently proved popular, and has been used extensively in other studies via out-of-sample extrapolation. Of course, like any other proposed measure for financial constraint, the SA index has not been exempt from criticism (e.g. Farre-Mensa and Ljungqvist, 2016). However, its wide-spread use and well-founded origins make us confident in its suitability for the purpose at hand. Furthermore, what makes it a particularly fitting measure for this thesis is the fact that, much like our sample, Hadlock and Pierce’s estimation sample also consisted of a broad range of exclusively public firms.

Besides the competitive dimension proxied through the financial constraint construct, the second concept we aim to capture in our regression analysis is industry signaling effects. To do so, we employ a dummy variable which takes on the value 1 if the LBO in question has been a secondary (or tertiary etc.) buyout, and the number 0 if it is the first time the target has been bought out. The required data to apply this classification is obtained from the Zephyr database and has been validated by checking if the sellers in the respective transactions have indeed been private equity firms. The rationale for using the secondary buyout dummy is as follows. The signaling argument posits that rivals will experience positive abnormal return because the buyout signals good news for the industry as a whole and also suggests that the rivals may become future acquisition targets themselves. It is to be expected that such signals should have the strongest effect the first time they occur. Subsequent to this initial signal, further information effects will automatically be “watered down” to some extent, lacking an element of surprise. Accordingly, it can be hypothesized that rivals witnessing a secondary LBO of their peer will react relatively less positively than rivals to a first time LBO target¹⁷. In other words, the coefficient associated with the secondary LBO dummy would be expected to take on a negative value.

In addition to the two primary independent variables of interest, we also include measures to capture other firm and transaction characteristics. Specifically, we control for LBO transaction

¹⁶ In Hadlock and Pierce’s (2010) sample, the mean value for non-constrained firms was -3,678, and for constrained firms -1,495.

¹⁷ It is readily apparent that this specification is not ideal, as there could have been LBOs targeting other firms outside of our data set in an industry, potentially distorting the measured effects. However, given the available data, it is a suitable approximation.

value, leverage (debt-to-capital ratio), pre-LBO target status (private/public), and industry; the latter two of which are dummy variables. The public target dummy is of particular interest as it may complement the purpose of the secondary LBO dummy. If the LBO target is a public firm as well, this may intensify signaling effects to other public companies (which all rivals in our sample are).

We test for possible multicollinearity issues by checking the correlation between the independent variables, but find no reasons for concern in this regard. The final specification of the regression model applied to the cross-section of all rival firms is thus:

$$\begin{aligned}
 CAR[T_1; T_2] = & \beta_1 SA_INDEX + \beta_2 DUM_SEC_LBO + \beta_3 \ln(LBO_VALUE) + \beta_4 LEVERAGE \\
 & + \beta_5 DUM_PUBL_T + \beta_6 DUM_CD + \beta_7 DUM_EN + \beta_8 DUM_HC \\
 & + \beta_9 DUM_IND + \beta_{10} DUM_IT + \beta_{11} DUM_TEL + \beta_{12} DUM_UTI + C
 \end{aligned}$$

<i>CAR[T₁;T₂]</i> :	(Cumulative) abnormal returns for different values of T ₁ and T ₂
<i>SA_INDEX</i> :	The measure proxying for financial constraint
<i>DUM_SEC_LBO</i> :	Takes the value 1 for secondary LBOs
<i>LBO_VALUE</i> :	Measures the size of the LBO transaction (GBP, million)
<i>LEVERAGE</i> :	Debt-to-Capital ratio
<i>DUM_PUBL_T</i> :	Takes the value 1 for LBOs where the target was a public company
<i>DUM_CD</i> :	Industry dummy, Consumer Discretionary
<i>DUM_EN</i> :	Industry dummy, Energy
<i>DUM_HC</i> :	Industry dummy, Healthcare
<i>DUM_IND</i> :	Industry dummy, Industrials
<i>DUM_IT</i> :	Industry dummy, Information Technology
<i>DUM_TEL</i> :	Industry dummy, Telecommunications
<i>DUM_UTI</i> :	Industry dummy, Utilities
<i>C</i> :	Constant term

5.2 Diagnostic Tests & Regression Results

The use of OLS regressions such as the one in this thesis implicitly presupposes certain data characteristics that need to be fulfilled in order to achieve statistical soundness. Before we turn to the results of the cross-sectional tests, we therefore conduct the standard assessments to ensure model integrity and output quality. The procedures we employ are based on the recommendations laid out by Brooks (2014).

First, we check for heteroskedasticity using the White test. While no significant heteroskedasticity is found (the p-values for the test exceed 5%), the results can be conservatively interpreted as slightly ambiguous. In unreported results, we make use of White's standard errors to account for this contingency. The effects for any subsequent inferential efforts, however, are negligible.

To evaluate whether the normality of residuals assumption is fulfilled, we turn to the Jarque-Bera test statistic. The results indicate that normality is clearly rejected. However, given the size of our sample, "violation of the normality assumption is virtually inconsequential" (Brooks, 2014, p.210).

Finally, the suitability of the assumed functional form is tested. The Ramsey RESET test results unequivocally reject non-linearity, meaning that the linear model employed is appropriate.

The results of the regression, reported for the four core event windows, are displayed in Table 6. Overall, the findings suggest overwhelming insignificance of the independent variables. One observation that stands out ever so slightly is the significance at a 10% level of the SA index for event windows [-1;3] and [-3;3]. Given the negative coefficients, this would be in line with the hypothesized relationship where being financially constrained leads rivals to react more negatively to an LBO of one of their peers. However, caution needs to be exercised when interpreting this result. It is a mere hint; tentatively supportive of the outlined theoretical argument, but a far cry from anything resembling empirical proof. The fact that the same variable shows opposite signs for other event windows casts further doubt on the result. The same goes for the other instances that see two variables, the industry dummy Consumer Discretionary and the constant term, being significantly different from zero for a single event period.

Table 6

Dependent Variable: (Cumulative) Abnormal Returns $[T_1;T_2]$; in percent

Method: Least Squares

Observations: 368

Coefficients reported in **bold**, p-values reported in (*italics*)

Significance at the 10% level indicated by an asterisk

Coefficients	Event Window			
	[0]	[-1;1]	[-1;3]	[-3;3]
<i>SA_INDEX</i>	0,191	0,450	-0,955	-1,249
	(0,529)	(0,198)	(0,074)*	(0,084)*
<i>DUM_SEC_LBO</i>	-0,295	0,505	0,133	0,062
	(0,385)	(0,236)	(0,824)	(0,939)
<i>ln(LBO_VALUE)</i>	0,020	0,184	0,266	0,187
	(0,870)	(0,265)	(0,225)	(0,528)
<i>LEVERAGE</i>	0,736	1,343	1,626	3,025
	(0,415)	(0,525)	(0,308)	(0,161)
<i>DUM_PUBL_T</i>	-0,159	0,792	-0,700	-0,685
	(0,765)	(0,186)	(0,456)	(0,590)
<i>DUM_CD</i>	-0,566	0,721	-1,047	-2,084
	(0,244)	(0,463)	(0,222)	(0,072)*
<i>DUM_EN</i>	-0,786	1,068	-0,801	-2,315
	(0,274)	(0,716)	(0,527)	(0,177)
<i>DUM_HC</i>	0,726	1,263	-0,694	-2,950
	(0,393)	(0,226)	(0,643)	(0,146)
<i>DUM_IND</i>	-0,176	0,719	-0,516	-1,803
	(0,716)	(0,663)	(0,546)	(0,119)
<i>DUM_IT</i>	0,067	0,749	0,198	-0,824
	(0,894)	(0,496)	(0,824)	(0,493)
<i>DUM_TEL</i>	-0,475	1,729	-0,043	-0,557
	(0,683)	(0,799)	(0,983)	(0,841)
<i>DUM_UTI</i>	-1,547	2,200	2,116	0,482
	(0,296)	(0,504)	(0,418)	(0,891)
<i>C</i>	0,522	2,167	-4,406	-4,146
	(0,720)	(0,206)	(0,087)*	(0,233)
R-squared	0,020	0,028	0,022	0,027
F-test (p-value)	0,839	0,593	0,771	0,614
Durbin-Watson stat	1,962	1,924	2,013	2,033

The R^2 values, hovering between 2% and 3%, are further testament to the limited explanatory power of the regression. Overall, the data provides no support for hypotheses H_{2a} or H_{2b} . Of course, all this does not come entirely as a surprise. There is little variation in the dependent variable (the cumulative abnormal returns) in the first place. Trying to explain variation where there barely is any is by design a fairly tricky endeavor, if not an exercise in futility altogether.

6 CONCLUSION

This thesis set out to investigate how the stock prices of competitors to LBO targets react upon news of the buyout. The corporate finance and industrial organization literature has, over the past three decades, provided insightful answers to this and related other questions; they are, however, not without ambiguity. Conflictive theoretical arguments coupled with mixed empirical findings have left this research area one in need of further research efforts. As Bernstein et al. (2014) succinctly put it, “it is important to understand the mechanisms by which the presence of private equity-backed firms affects their peers ... [and] much more remains to be explored here” (p.27).

To do so, we employed an event study methodology, supported by a follow-up cross-sectional regression analysis. Inputs are derived from an original data set, meticulously constructed through both quantitative and qualitative assessment which arguably manages to capture market realities more accurately than previous investigations. To the best of our knowledge, no previous study within this field of research has employed the data collection approach described herein. It is, *inter alia*, this data set which sets this thesis apart and makes it a valuable contribution to the literature.

We report insignificant, zero-mean abnormal returns across all relevant event windows. These findings appear robust insofar as they apply for a variety of parameters. For example, we have employed both constant mean and market models to estimate normal returns; in the latter case we used individual country indices as well as a single pan-European one. Furthermore, we investigated abnormal returns both for LBO announcement dates and for preceding rumor dates. Based on the empirical evidence, we do not find support for any of the hypotheses laid out in Chapter 2.

At first sight, the unequivocally insignificant results may appear sobering. At the same time, considering the theoretical foundation upon which this study has been built, they do not necessarily come as a surprise; nor are they without meaning. The literature suggests that there are various, mutually opposing forces at play when rival firms’ stock prices react to the announcement that a peer is being bought out by a private equity investor. On the one hand, in expectation of having a prospectively more formidable competitor in the LBO target, share prices among rivals drop as they instantaneously capture the downward pressure on future performance these firms may experience. On the other hand, LBO announcements have also been argued to constitute good news for all firms in an industry, as they can be interpreted as

signals that the sector in question is in good shape and more M&A activity may be on the horizon. This should lift share prices upon deal announcement.

Previous empirical findings have, separately, supported both of these perspectives. Evidently, our results fall firmly in between. Two possible interpretations come to the fore. Either rival firms in our sample tend to be fundamentally indifferent to LBO announcements (and rumors thereof) within their industries; or they have reason to both worry and celebrate at the same time, and the effects ultimately cancel each other out to a large extent. The regression analysis performed subsequent to the event study was designed to dig deeper into such dynamics that may help explain the results. Specifically, we tested whether financially constrained firms react more negatively to peer LBOs, a notion rooted in the logic of competitive dynamics, and whether secondary buyouts lack some of the signaling punch commonly ascribed to LBOs. The outcomes of this cross-sectional test are statistically insignificant for the most part. The hypothesis of financial constraint being associated with lower abnormal returns finds light support for some event windows. However, great caution is required when interpreting such results. Only the faintest of signals is detected in the data; barely enough to warrant speculation, let alone concrete inferences.

In conclusion, while the non-significant results this study has produced may seem unspectacular, they do constitute a novel verdict which simultaneously both challenges and supports previous empirical findings and neatly fits into the existing theoretical framework. Overall, they provide insights that further our understanding within this research domain.

In terms of managerial implications, our findings seem to indicate that looking for rival reactions to private equity investments may be of limited value. Such an interpretation, based solely on a single study, however, could be short-sighted. It is our belief that private equity funds should continue to be observant of how the competitors of their prospective investment targets react to rumors and announcements of the transaction. Their stock movements may, after all, be a proxy feedback mechanism of how the market views the proposed deals.

With regards to opportunities for future scientific inquiry, we can see a number of exciting and important contributions that would further the research agenda of the subject at hand. First of all, it would be beneficial to have other event studies investigating core sets of rivals instead of the broader competition. While we conducted our data collection with utmost care and diligence, certain biases or inaccuracies can never be completely ruled out. It would be insightful to see if other data samples constructed with a hybrid quantitative/qualitative

procedure yield similar results. Furthermore, the exclusively European setting of our study leaves room for a shift in geographical focus.

We would also like to see a more in-depth, potentially purely qualitative, empirical take on the topic. Most studies so far, including this one, have primarily been concerned with the average abnormal returns observed over a great number of LBOs. Case studies of fewer or even just a single LBO could place exclusive focus on the dynamics behind these abnormal returns (or the lack thereof) and explore how rival firms expect to be affected by buyouts in their industries.

Yet another research opportunity will gradually appear as the post-financial-crisis time period expands, making long-term observations of the LBOs that occurred within it possible. Incorporating long-term performance data in this study would have been either premature or outright impossible. Going forward, however, it will be interesting to see how, if at all, LBOs in this period affect rival firms in terms of revenue growth, profitability measures, and overall competitive intensity. Also, did the industries targeted by private equity subsequently really exhibit higher growth rates? Was there an increase in M&A activity which benefited rivals by becoming takeover targets themselves? Such questions may have been addressed in previous studies already. However, recalling what Bernstein et al. (2014) said about the extreme differences and potential incomparability between LBO waves, it seems imperative to keep researching these issues.

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APPENDIX I – DETAILED RESULTS (ANNOUNCEMENT DATES)

CAAR – INDIVIDUAL RIVAL FIRMS

Normal Return Estimation: Market Model (Country Indices)

Event Window	0	1	2	3	4	5
0	-0,10%	0,14%	0,17%	0,00%	0,25%	0,12%
	<i>(0,521)</i>	<i>(0,455)</i>	<i>(0,427)</i>	<i>(0,999)</i>	<i>(0,333)</i>	<i>(0,678)</i>
-1	-0,19%	0,05%	0,08%	-0,09%	0,16%	0,03%
	<i>(0,323)</i>	<i>(0,806)</i>	<i>(0,737)</i>	<i>(0,720)</i>	<i>(0,556)</i>	<i>(0,932)</i>
-2	-0,06%	0,19%	0,22%	0,05%	0,30%	0,17%
	<i>(0,795)</i>	<i>(0,473)</i>	<i>(0,446)</i>	<i>(0,881)</i>	<i>(0,355)</i>	<i>(0,627)</i>
-3	-0,18%	0,06%	0,09%	-0,08%	0,17%	0,04%
	<i>(0,439)</i>	<i>(0,834)</i>	<i>(0,769)</i>	<i>(0,815)</i>	<i>(0,625)</i>	<i>(0,921)</i>
-4	-0,21%	0,03%	0,06%	-0,11%	0,15%	0,01%
	<i>(0,408)</i>	<i>(0,909)</i>	<i>(0,838)</i>	<i>(0,761)</i>	<i>(0,680)</i>	<i>(0,972)</i>
-5	-0,19%	0,05%	0,08%	-0,09%	0,16%	0,03%
	<i>(0,473)</i>	<i>(0,880)</i>	<i>(0,809)</i>	<i>(0,801)</i>	<i>(0,661)</i>	<i>(0,939)</i>

SHARE OF POSITIVE ABNORMAL RETURNS

Among Observations with Significance at 10%

Event Window	0	1	2	3	4	5
0	47,62%	51,28%	55,26%	44,74%	59,38%	57,14%
-1	52,78%	54,05%	58,06%	59,38%	62,07%	56,25%
-2	53,13%	58,06%	56,25%	56,67%	69,23%	60,61%
-3	53,85%	57,14%	48,28%	53,85%	60,00%	44,44%
-4	50,00%	45,83%	52,00%	47,83%	60,00%	51,85%
-5	45,83%	42,86%	43,48%	40,00%	56,52%	48,28%

CAAR – PORTFOLIOS OF RIVAL FIRMS

Normal Return Estimation: Market Model (MSCI Europe)

Event Window	0	1	2	3	4	5
0	0,21%	0,47%	0,42%	0,19%	0,24%	0,27%
	<i>(0,337)</i>	<i>(0,179)</i>	<i>(0,344)</i>	<i>(0,641)</i>	<i>(0,556)</i>	<i>(0,517)</i>
-1	0,07%	0,34%	0,28%	0,03%	0,08%	0,10%
	<i>(0,795)</i>	<i>(0,385)</i>	<i>(0,558)</i>	<i>(0,938)</i>	<i>(0,849)</i>	<i>(0,807)</i>
-2	0,11%	0,37%	0,32%	0,08%	0,13%	0,15%
	<i>(0,782)</i>	<i>(0,465)</i>	<i>(0,600)</i>	<i>(0,892)</i>	<i>(0,819)</i>	<i>(0,784)</i>
-3	0,19%	0,45%	0,40%	0,16%	0,21%	0,23%
	<i>(0,653)</i>	<i>(0,401)</i>	<i>(0,525)</i>	<i>(0,791)</i>	<i>(0,717)</i>	<i>(0,686)</i>
-4	0,12%	0,39%	0,33%	0,09%	0,14%	0,16%
	<i>(0,776)</i>	<i>(0,484)</i>	<i>(0,607)</i>	<i>(0,885)</i>	<i>(0,814)</i>	<i>(0,784)</i>
-5	0,22%	0,49%	0,43%	0,19%	0,24%	0,26%
	<i>(0,628)</i>	<i>(0,394)</i>	<i>(0,512)</i>	<i>(0,766)</i>	<i>(0,694)</i>	<i>(0,666)</i>

SHARE OF POSITIVE ABNORMAL RETURNS

Among Observations with Significance at 10%

Event Window	0	1	2	3	4	5
0	72,73%	69,23%	33,33%	36,36%	40,00%	27,27%
-1	40,00%	50,00%	25,00%	23,08%	33,33%	30,00%
-2	50,00%	36,36%	30,00%	30,00%	40,00%	41,67%
-3	33,33%	40,00%	33,33%	23,08%	33,33%	40,00%
-4	50,00%	58,33%	63,64%	41,67%	40,00%	33,33%
-5	66,67%	50,00%	44,44%	36,36%	50,00%	46,15%

APPENDIX II – DETAILED RESULTS (RUMOR DATES)

CAAR – INDIVIDUAL RIVAL FIRMS

Normal Return Estimation: Market Model (Country Indices)

Event Window	0	1	2	3	4	5
0	-0,12%	0,10%	-0,09%	-0,20%	-0,10%	0,05%
	<i>(0,365)</i>	<i>(0,663)</i>	<i>(0,713)</i>	<i>(0,457)</i>	<i>(0,708)</i>	<i>(0,879)</i>
-1	-0,21%	0,00%	-0,18%	-0,29%	-0,19%	-0,05%
	<i>(0,251)</i>	<i>(0,988)</i>	<i>(0,505)</i>	<i>(0,314)</i>	<i>(0,513)</i>	<i>(0,876)</i>
-2	-0,12%	0,10%	-0,09%	-0,19%	-0,10%	0,05%
	<i>(0,583)</i>	<i>(0,732)</i>	<i>(0,764)</i>	<i>(0,532)</i>	<i>(0,758)</i>	<i>(0,882)</i>
-3	-0,12%	0,10%	-0,08%	-0,19%	-0,09%	0,06%
	<i>(0,625)</i>	<i>(0,730)</i>	<i>(0,794)</i>	<i>(0,571)</i>	<i>(0,788)</i>	<i>(0,873)</i>
-4	-0,15%	0,07%	-0,12%	-0,23%	-0,13%	0,02%
	<i>(0,568)</i>	<i>(0,834)</i>	<i>(0,712)</i>	<i>(0,498)</i>	<i>(0,705)</i>	<i>(0,965)</i>
-5	-0,16%	0,06%	-0,13%	-0,23%	-0,13%	0,02%
	<i>(0,596)</i>	<i>(0,858)</i>	<i>(0,728)</i>	<i>(0,536)</i>	<i>(0,723)</i>	<i>(0,966)</i>

SHARE OF POSITIVE ABNORMAL RETURNS

Among Observations with Significance at 10%

Event Window	0	1	2	3	4	5
0	53,87%	50,52%	50,00%	52,32%	53,35%	48,97%
-1	52,20%	48,84%	52,20%	52,71%	51,16%	50,65%
-2	47,80%	48,32%	49,61%	52,45%	51,68%	50,65%
-3	50,39%	48,58%	52,71%	50,65%	52,71%	50,65%
-4	50,90%	50,65%	54,26%	53,75%	54,26%	52,97%
-5	50,65%	49,87%	52,71%	52,71%	52,97%	51,94%

CAAR – PORTFOLIOS OF RIVAL FIRMS

Normal Return Estimation: Market Model (MSCI Europe)

Event Window	0	1	2	3	4	5
0	0,07%	0,34%	0,52%	0,29%	0,53%	0,40%
	<i>(0,758)</i>	<i>(0,303)</i>	<i>(0,208)</i>	<i>(0,455)</i>	<i>(0,172)</i>	<i>(0,309)</i>
-1	-0,01%	0,27%	0,44%	0,21%	0,45%	0,32%
	<i>(0,980)</i>	<i>(0,455)</i>	<i>(0,309)</i>	<i>(0,598)</i>	<i>(0,259)</i>	<i>(0,427)</i>
-2	0,24%	0,51%	0,69%	0,47%	0,71%	0,58%
	<i>(0,504)</i>	<i>(0,290)</i>	<i>(0,220)</i>	<i>(0,390)</i>	<i>(0,180)</i>	<i>(0,280)</i>
-3	0,20%	0,47%	0,65%	0,43%	0,67%	0,54%
	<i>(0,610)</i>	<i>(0,360)</i>	<i>(0,270)</i>	<i>(0,460)</i>	<i>(0,240)</i>	<i>(0,340)</i>
-4	0,09%	0,37%	0,55%	0,32%	0,56%	0,43%
	<i>(0,810)</i>	<i>(0,470)</i>	<i>(0,350)</i>	<i>(0,570)</i>	<i>(0,320)</i>	<i>(0,440)</i>
-5	0,10%	0,37%	0,55%	0,32%	0,57%	0,44%
	<i>(0,820)</i>	<i>(0,490)</i>	<i>(0,360)</i>	<i>(0,590)</i>	<i>(0,330)</i>	<i>(0,450)</i>

SHARE OF POSITIVE ABNORMAL RETURNS

Among Observations with Significance at 10%

Event Window	0	1	2	3	4	5
0	31,25%	63,64%	40,00%	50,00%	63,64%	36,36%
-1	50,00%	70,00%	50,00%	50,00%	55,56%	37,50%
-2	50,00%	57,14%	66,67%	83,33%	66,67%	50,00%
-3	50,00%	62,50%	57,14%	50,00%	44,44%	37,50%
-4	50,00%	66,67%	60,00%	57,14%	62,50%	44,44%
-5	60,00%	62,50%	60,00%	57,14%	57,14%	44,44%