Norwegian School of Economics

Bergen, spring, 2015

Bidder Returns for Norwegian Acquirers

- A study on how deal- and firm-specific characteristics affect bidder returns for Norwegian acquirers of foreign and domestic companies.

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Master Thesis, MSc, Finance

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Abstract

We examine the announcement returns for Norwegian acquirers of foreign and domestic targets between 1988 and 2014. This is done using panel data in a *random effects* model with stock return data from NHH's Børsprosjektet, and transaction data from SDC's mergers and acquisitions database. We are the first, to our knowledge, to use panel data regression analysis on bidder announcement returns.

Analysing periods around acquisition announcements reveal that only the day of announcement yields significant abnormal returns, which is consistent with the efficient market hypothesis in semi-strong form. Furthermore, we find no significant abnormal returns for firms acquiring public targets, which supports the theory of an efficient market for corporate control. However, we find significant abnormal returns for firms acquiring private targets. The returns from acquiring private targets are greatest when stock is used as the method of payment, while using stock to acquire public targets yields the most negative returns. The acquirer's acquisition experience, the absolute size of the acquirer, and the target being in a related industry all have negative effects on announcement returns. Furthermore, we model the effect of relative size on announcement returns as a cubic function. This reveals a negative relationship until the target is one fourth of the acquirer's size, and a positive relationship beyond this point. Additionally, we are, to our knowledge, the first to account for the possibility of altered marked beta coefficients as a result of acquisitions, through the use of a step-beta approach.

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Acknowledgements

This thesis was written in the spring of 2015 as a part of our Master of Science degree in Economics and Business Administration at the Norwegian School of Economics. First, we would like to thank Johannes Kolberg from Børsprosjektet at NHH for his help with obtaining the stock return data used in our analysis. We would also like to thank Karin Thorburn. Her feedback and input has been very helpful. Most of all, we would like to thank our supervisor, Liam Brunt. His guidance has been essential to our analysis and much appreciated.

Note: the terms “bidder” and “acquirer” are used interchangeably throughout this thesis.

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1. Introduction

«The proposition that a competitive market for corporate control effectively limits managerial divergence from shareholder wealth maximization implies that corporate takeovers are beneficial to shareholders of both firms involved in the transaction» - Eckbo and Thorburn, 2000

Mergers and acquisitions (M&A) can be considered a quest to find a more effective combination of resources in order to create value for shareholders. There is overwhelming evidence that, on average, targets receive substantial abnormal returns from being acquired. For instance, Kengelbach and Roos (2011) found that the average takeover premium from 1990 to 2010 was 36%. However, there is counter evidence on abnormal returns for acquirers, so returns seem to greatly depend on various deal- and company characteristics. This is supported by results found by Bradley and Sundaram (2006), who summarize the bidder announcement returns for each year from 1990 to 2000.

Table 1. Announcement Period Abnormal Returns by year, 1990-2000

Year	All Targets		Public Targets		Private Targets		Difference	
1990	1.17%	(2.58)***	0.18%	(0.16)	1.35%	(2.65)***	1.17%	(1.02)
1991	1.40%	(3.08)***	-1.12%	(-1.33)	2.01%	(2.76)***	3.13%	(2.76)***
1992	1.73%	(5.12)***	-0.60%	(-0.96)	2.20%	(5.76)***	2.80%	(3.11)***
1993	1.25%	(4.41)***	-1.50%	(-2.94)***	1.77%	(5.56)***	3.26%	(4.22)***
1994	1.32%	(5.34)***	-0.03%	(-0.03)	1.61%	(7.83)***	1.58%	(2.50)***
1995	1.25%	(5.04)***	-1.30%	(-3.42)***	1.97%	(6.67)***	3.28%	(5.51)***
1996	1.85%	(8.69)***	-0.05%	(-0.12)	2.28%	(9.33)***	2.33%	(4.31)***
1997	1.54%	(8.84)***	-0.50%	(-1.31)	1.94%	(10.05)***	2.44%	(5.26)***
1998	1.05%	(5.08)***	-0.90%	(-1.90)**	1.47%	(6.44)***	2.36%	(4.40)***
1999	1.63%	(5.17)***	-1.20%	(-2.11)***	2.38%	(6.56)***	3.61%	(4.71)***
2000	2.72%	(3.01)***	-0.60%	(-0.36)	3.32%	(3.25)***	3.91%	(1.56)
All	**1.45%**	**(17.27)*** **	**-0.71%**	**(-3.89)*** **	**1.95%**	**(20.66)*** **	**2.66%**	**(12.31)*** **

Source: Bradley and Sundaram (2006) [t-stats in parenthesis]

Furthermore, Fuller, Netter and Stegemoller (2002) studied the announcement returns for companies that have made five or more acquisitions within three years; their sample consists of 3,135 transactions. They found an Average Cumulative Abnormal Return (ACAR) [-2, 2] (i.e. using a five day window that starts at -2 days and ends at 2 days relative to the acquisition announcement) of 1.8%. Stratifying this sample showed that the ACAR was -1%

when the target was public, 2.1% when the target was private, and 2.8% when the target was a subsidiary. All results were significant at the 10% level.

Moreover, Moeller, Schlingemann and Stulz (2007) analysed returns from a sample of 4,322 acquisitions that occurred between 1980 and 2002. They found an ACAR [-1, 1] of 0.8% for their sample. However, stratifying their sample revealed that the ACAR was -2.3% for public targets that were acquired with stock, 0.7% for public targets that were acquired with cash, and 3.4% for private targets that were acquired with stock. Unfortunately, they did not show the ACAR of private targets that were acquired with cash.

These results indicate that negative- or insignificantly different from zero bidder announcement returns mostly come from the acquisition of public targets. This is supported by research conducted by Andrade, Mitchell and Stafford (2001), which shows the bidder announcement returns for the acquisition of *public* companies by decade from 1973 to 1998.

Table 2. Announcement Period Abnormal Returns 1973-1998

	1973-79	1980-89	1990-98	1973-98
Combined:				
[-1, +1]	1.50%	2.6%*	1.4%*	1.8%*
[-20, Close]	0.1%	3.2%	1.6%	1.9%
Target:				
[-1, +1]	16%*	16%*	15.9%*	16%*
[-20, Close]	24.8%*	23.9%*	23.3%*	23.8%*
Acquirer:				
[-1, +1]	-0.30%	-0.40%	-1.00%	-0.70%
[-20, Close]	-4.50%	-3.10%	-3.90%	-3.80%
No. Obs.	598	1226	1864	3688

Source: Andrade et al. 2001, * Denotes statistical significance at the 5% level.

There are several explanations for negative bidder announcement returns. Moeller, Schlingemann and Stulz (2004) summarize the different explanations. Roll (1986) hypothesizes that managers might overpay for targets due to hubris. The manager may believe that he is more able to realize synergies than others, or believe he is more capable than others of accurately valuing the target. Travlos (1987) finds that acquiring firms with poor returns generally pay with equity, and Myers and Majluf (1984) show that firms who issue equity are

usually signalling to the market that they are overvalued. This is supported by Dong, Hirshleifer, Richardson and Teoh (2008), who show that acquirers with higher valuations receive lower announcement returns. Dong et al. (2008) argue that the market interprets these acquirers as using their overvalued equity to pay for, relatively, less overvalued targets. Furthermore, McCardle and Viswanathan (1994) and Jovanovic and Braguinsky (2004) find that companies usually announce acquisitions when they have exhausted their opportunities for organic growth. This indicates that the market will react negatively to the announcement of an acquisition if they originally thought the company had numerous opportunities for organic growth. Jensen (1986) argues that some managers would rather increase the size of their company than pay out free cash flows as dividends to the company's shareholders. Finally, Mitchell, Pulvino and Stafford (2004) show that there is downward price pressure on the acquirer's stock after they announce an acquisition paid with stock, partly due to the activities of arbitrageurs.

We have decided to focus our research on the acquiring company's announcement returns instead of the announcement returns for the target company. This is because the nature of announcement returns – and drivers for these returns - are more opaque for acquirers than for the targets. Furthermore, the results found in contemporary research on acquirers are also more ambiguous than results found in research on the announcement returns for targets. This makes returns to acquirers a more interesting and worthwhile research subject. Moreover, the majority of similar research focuses on U.S. acquirers and targets, while the research on Norwegian transactions is lacking. There is some research on bidder announcement returns for Norwegian acquirers of Norwegian targets, but, to our knowledge, no research on the bidder announcement returns for Norwegian acquirers of both Norwegian *and* foreign targets. We have chosen the Norwegian market in order to further expand the field of research and to investigate whether there exist any idiosyncratic effects for Norwegian acquirers. In particular, we investigate the abnormal returns for Norwegian acquirers in periods where they have announced a takeover of a foreign or domestic company, and attempt to uncover the major drivers for these returns.

Our initial sample selection process resulted in 1,677 transactions. These transactions were conducted by 383 unique acquirers between 1984 and 2015. We also collected numerous deal- and company characteristics that we used to find an explanation for any abnormal returns to Norwegian acquirers.

The rest of the thesis is structured as follows:

Section two begins with an overview of previous research. This section is structured to briefly explain their sample selection, methodology, results, and inferences.

Section three contains the hypotheses that this thesis seeks to answer.

Section four and five continues with a thorough explanation of our own sample selection data. This includes how and where we acquired our data, and how we modified it in order to conduct our analysis.

Section five describes the deal- and firm-specific variables we use in our analysis.

Section six explains our chosen methodology, focusing on how we calculated the abnormal announcement returns, why we used panel data and a *random effects* model to analyse the abnormal announcement returns, what econometric issues we encountered in our analysis, and what we did in order to rectify these.

Section seven details our analysis, divided into several sub-sections showing regression output with subsequent qualitative interpretation of the implications of our results. First, we report our regression output of abnormal announcement returns across different dates relative to day zero. Next, we analyse abnormal returns in relation to several control variables.

The analysis includes:

- Whether the target being foreign or domestic influences bidder returns.
- Whether industry has any effect on returns, this is done in two forms: (1) whether any industries receive abnormal returns, and (2) whether the fact that the target and acquirer are in the same industry affects bidder returns.
- Whether size has any effect on returns, this is done in two forms: (1) whether the size of the target *relative* to the acquirer influences bidder returns, and (2) whether the absolute size of the acquirer influences bidder returns.
- Whether the acquirer's method of payment affects bidder returns, where the method of payment is stratified into three categories: cash, stock, and hybrid.
- Whether the effects of size and method of payment on bidder returns are dependent on the target being public or private.
- Whether the fact that a bidder has conducted many prior acquisitions affect their announcement returns.
- Whether returns are affected by the acquiring firm receiving a controlling interest.

We also split the sample into public and private targets, and analyse abnormal returns with respect to the control variables mentioned. The choice of explanatory variables are based on variables used in previous research. See section two for details on the previous research that has informed this thesis.

Section eight explains general robustness issues and how we treat them.

Section nine concludes the thesis with a summary of our main findings and some suggestions for further research.

There are two important caveats for these kinds of analyses. First, analyses of announcement returns usually yield attenuated results due to partial anticipation of the acquisitions. Second, the bidder returns at an acquisition announcement date can also be attributable to how the market reassesses the bidders business strategy, not only how the market values the incremental gain of the acquisition itself (Grinblatt & Titman, 2002). We address this by analysing whether there exist any idiosyncrasies for companies that conduct frequent acquisitions. One can argue that the market already takes into account these companies' acquisitive nature so that any abnormal announcement return can be wholly attributed to the incremental gain of a successful takeover.

2. Previous research

2.1 Research on foreign vs. domestic bidders.

Eckbo and Thorburn (2000) conducted research on 1,846 acquisitions of Canadian companies by both U.S. and Canadian bidders between 1945 and 1983. They found that domestic bidders in Canada earned significantly positive monthly announcement returns. They estimated this monthly abnormal announcement return to be 1.13% using a pre-event benchmark, and 1.81% using a post-event benchmark. Both of these results are significant at a 1% level.

Using daily returns yielded a significant two-day return of 0.81%. U.S. bidder announcement returns, however, are substantially lower than the average performance of domestic bidders and indistinguishable from zero.

Their paper discusses several explanations for superior domestic bidder performance. The first explanation is the effect of foreign direct investment (FDI) controls. After 1972, foreign bidders were required to seek prior Government approval prior to acquiring Canadian companies. Intuitively, this gives an advantage to domestic bidders as they are not required to undergo such a process, which can take substantial time and effort to complete.

However, Eckbo and Thorburn found that Canadian bidders outperformed their U.S. counterparts even before this FDI control was put in place. Additionally, the foreign bidders that were exempt from the review process did not earn significant abnormal returns.

Their second explanation is that domestic bidders might be more closely related to the targets they are acquiring. Domestic bidders might have superior information about Canadian targets in the same industry and might be more able to realize synergies. Eckbo and Thorburn compared the announcement returns for related, which was defined as sharing a two-digit SIC-code, and unrelated acquisitions. However, they did not find any evidence that supported this idea; they found that domestic bidders outperformed their U.S. counterparts in both horizontal and conglomerate acquisitions. Consequently, perhaps superior domestic performance could simply be attributed to geographical proximity.

Third, analysing different payment methods (i.e. cash, stock, or hybrid) revealed surprising results. Stock- and hybrid offers generate significantly positive average announcement returns in Canada. This contrasts with the significantly negative market reaction documented by

Travlos (1987) for all-stock mergers in the U.S. They noted that there is substantial evidence that the U.S. market is influenced by adverse selection (Eckbo & Masulis, 1992). This tends to cause a negative market reaction to equity issues. There is less evidence of this for markets outside the U.S.

Finally, comparing bidder announcement returns for transactions where the relative size of the target differed showed that there was a negative relationship between the acquirer's relative size and the significance of the announcement returns. U.S. acquirers were about eight times as large as their Canadian counterparts were. This indicates that the announcement returns for relatively large bidders suffers from an attenuation bias, which could explain the insignificance of the foreign (U.S.) announcement returns.

2.2 Research on industry relatedness

The question of whether the merger or the acquisition of horizontally similar companies yield higher abnormal returns is the subject of many research papers. Akbulut and Matsusaka (2010) investigated the abnormal returns for both related and diversifying mergers and acquisitions. They used a sample of 4,764 mergers of U.S. public firms between 1950 and 2002. Their definition of a related acquisition was that the target and acquirer had to have at least one 4-digit SIC code in common. They measured abnormal returns using the Fama-French three-factor model against a one-year period ending -64 days relative to the acquisition announcement date. Moreover, they used an estimation window of [-1, 1] days relative to the acquisition announcement, and checked the robustness of their result with another estimation using a [-2, 1] window. They found an ACAR [-1, 1], of -1.3% and -0.6% for related mergers and diversifying acquisitions, respectively. Moreover, stratifying these returns based on method of payment revealed that the mean return for stock acquisitions were -2.3% for related acquisitions and -1.7% for diversifying mergers, while for cash payment the returns were 0.5% for related acquisitions and 0.7% for diversifying mergers. All of these ACARs were significant at the 1% level. They also note that the negative returns associated when stock is used as the method of payment cannot be solely attributed to the acquisition itself. Seasoned equity offerings usually result in negative stock price reactions of around 3% (Smith, 1986), and merger announcement returns are usually around 3% lower for stock-financed bids (Andrade et al., 2001). Additionally, bidder announcement returns vary greatly depending on the estimation time period. Akbulut and Matsusaka (2010) compiled bidder return estimates

for several studies, and concluded that timing seems to account for much of the variation within the results. The announcement returns are generally positive during the conglomerate merger wave (1966-1969) and negative during both the surrounding years and the most recent estimation period.

Interestingly, one of the studies summarized by Akbulut and Matsusaka (2010), Morck et al. (1990), reaches the opposite conclusion on the difference in announcement returns for related and unrelated acquisitions. They find a 1.54% and 2.88% announcement return when the bidder and target share a 4-digit SIC-code on data from 1975-1979 and 1980-1987, respectively. The announcement return for transactions where the bidder and target do not share a 4-digit SIC-code are 0.77% and 1.27% for the time periods 1975-1979 and 1980-1987, respectively. However, this study used a sample of only 326 acquisitions. This pales in comparison to the sample of 4,764 acquisitions used by Akbulut and Matsusaka (2010), so the results uncovered in Morck et al. (1990) seems less robust than the results in Akbulut and Matsusaka (2010).

2.3 Research on size

The absolute- and relative size of target and acquirer has been used as an explanatory variable in many papers that conduct research on announcement returns. Moeller et al. (2004)'s main focus is these size effects on acquirer's returns. They analysed the acquirer's return for mergers and acquisitions within the U.S., where the acquirer owned less than 50% before the announcement and ended up with 100% of the company after the transaction was completed. They also included the following criteria:

I. The transaction is listed as completed.
II. The deal value is greater than $1m.
III. The target is a U.S. public-, private- or subsidiary company.
IV. The acquirer is a public firm listed on the Center for Research in Security Prices (CRSP) during the event window.
V. The deal value relative to the acquirer's market value is more than 1%.
VI. The number of days between the announcement and completion of the acquisition is between 0 and 1000.

These criteria yielded a sample of 12,023 transactions between 1980 and 2001. They used the traditional event study methodology with a three day window [-1, 1]. They estimated the benchmark using the CRSP equally weighted index over a period from -205 to -6 days,

relative to the acquisition. They also estimated the abnormal returns by subtracting the market return from each firm's return. Both methodologies yielded the same result. They found a CAR of 1.102% for their entire sample. Large acquirer's, which they define as above the 25th percentile in terms of market cap for the companies listed at the NYSE during the acquisition announcement, received only a 0.076% bidder returns, while acquirer's below the 25th percentile in size received 2.318%, (all significant on the 1% level). This is also supported by results from Bradley and Sundaram (2006), who found that bidder returns were negatively correlated with the acquirer's total size.

There are many different results concerning how the target's relative size affects announcement returns. Studies like Jarrell and Poulsen (1989), Asquith et al. (1983), and Loderer and Martin (1990) find that bidder announcement returns increase with the targets relative size, while studies such as Travlos (1987) find that bidder announcement returns decrease with the relative size of the target. The results found in Travlos (1987) are somewhat supported by Eckbo and Thorburn (2000), who found that that the abnormal returns for domestic (Canadian) bidders decreased as the relative target size increased. However, they found little evidence for significant foreign (U.S.) bidder gains regardless of size. Moreover, Moeller et al. (2004) found that the announcement returns for small acquirers were positively related to the targets relative size, while the returns of large acquirers were negatively related to the targets relative size. They partly attributed this to Roll (1986)'s hubris theory and argued that it is more likely for a manager of a large company to be overconfident and overpay for a target.

The differences in the coefficients found on relative size in the previous paragraph can have several potential explanations. There is substantial evidence that the effect of the targets relative size on announcement returns depends on: (1) whether the target is private or public, and (2) whether the acquirer uses cash or stock to finance the acquisition. Bradley and Sundaram (2006), and Fuller et al. (2002) found that the bidder returns for the acquisition of public targets was negatively correlated with increasing relative target size while the opposite was true for private targets.

2.4 Research on target public status and method of payment

Myers and Majluf (1984) focus on the difference in bidder returns for acquisitions made with different payment methods. They argue that bidders will use stock as the method of payment if they view their stock as overvalued, so announcing takeovers with stock as the method of payment often causes negative announcement returns. Empirical research supports this. Travlos (1987), Fishman (1989), Brown and Ryngart (1991), and Martin (1996) all find that bidders making cash offers have greater abnormal returns at bid announcement. This often causes targets to hesitate to accept stock as a payment method. It is important to note that if the bidder is uncertain about the target's value, the bidder should want to offer stock, since the target will only accept a cash offer if it is above the true value of the target company, which means that the bidder usually overpays.

Chang (1998) analysed bidder returns for acquirers of private companies from 1981 to 1992 and compared it to the bidder returns of acquirers of public companies from 1981 to 1988. His sample selection yielded 281 privately held targets and 255 public targets. He found no significant abnormal returns for bidders who bought private targets with cash, but he did find a significant 2.64% return for bidders who acquired private targets with stock. He attributes this to the formation of blockholders of the acquirer's shares after the acquisition process. Private companies usually have a very concentrated ownership structure, so the formation of blockholders of the acquirer's stock is more likely when acquiring private companies. Post-acquisition blockholders can also arise as a result of the acquisition of a public company, as public companies are generally larger than private companies, but the larger relative size of public companies are most often offset by their dispersed ownership structure.

The implication is that the new owners of the acquirer's stock are better able to monitor the acquirer company's management. Chang tested this hypothesis and found a 4.96% abnormal return in cases where new blockholders were formed versus a 1.77% return in cases with no ex-post blockholders. Fuller et al. (2002) argues that many private managers may use the acquisition as an exit strategy, and are not interested or able to monitor the acquirer's management. Based on this, they conclude that the blockholder formation cannot conclusively explain the difference in abnormal returns for private and public targets.

Hansen and Lott (1996) examined the returns to bidders acquiring 252 private and public targets from 1985 to 1991. They found that bidders gained a 2% higher return when purchasing private companies, and cash offers had announcement returns 0.6 percentage points higher than stock offers, but they did not stratify this difference on public vs. private targets. They offered another explanation for the higher returns for bidders acquiring private targets. They argued that diversified investors are indifferent towards how the synergies are split between the acquirer and target when both companies are public and stockholders own shares in both companies. However, when the target is private, the acquirer stockholders can only benefit by capturing stock gains from the acquisition, assuming the bid is value increasing.

Additionally, Bradley and Sundaram (2006) investigated bidder ACAR's with a sample of 12,476 acquisitions completed by 4116 public companies in the period between 1990 and 2000. They found an ACAR [-2, 2] of 1.4% for their entire sample. Stratifying this sample on both method of payment and target public status revealed that the ACAR for public targets was 0.92% for cash offers, and -1.71% for stock offers. The announcement returns for private targets was 1.1% for cash offers and 1.69% for stock offers. All of these returns were significant at the 1% level.

Furthermore, as mentioned in the introduction, Moeller et al. (2007) used a sample of 4,322 all-cash and all-stock bids from 1980 to 2002. They found an ACAR [-1, 1] of 0.8% for their entire sample. For public targets the ACAR was -2.3% for all stock deals, and 0.7% for all-cash deals. For private targets, the ACAR was 3.4% for all-stock deals.

Savor (2006) used a sample of 1484 merger bids that occurred between 1990 and 2000. He found an ACAR [-1, 1] for the bidder of -3.5% for all-stock bidders and 1% for all-cash bidders. Furthermore, Martin (1996) finds that stock offers are more likely if there is more uncertainty about the bidder's value.

Moreover, Fuller et al. (2002) have studied bidder returns for companies that made five or more successful bids within three years from 1990 to 2000. They calculated ACAR during a five-day window [-2, 2]. They found significantly negative ACAR's (-1%) for public targets and significantly positive ACAR's (2.1%) for private targets. Moreover, they found significantly positive ACAR's of 2.8% for subsidiary targets. They argue that this could be due to the fact that private companies might be priced with an implicit liquidity discount, as

they are not as easily traded as their public counterparts. This is also in line with the evidence that the bidder returns related to the acquisition of a private firm are more positive as the relative size of the target increases, while bidder returns related to the acquisition of public companies are more negative as the relative size of the target increases. Stratifying the sample based on method of payment, in addition to the targets public status, revealed that the bidder announcement returns for public targets were an insignificant 0.34% for cash offers, a significant -1.86% for stock offers, and a insignificant -1.1% for hybrid offers. For private targets the announcement returns were a significant 1.62% for cash offers, a significant 2.43% for stock offers, and 2.48% for hybrid offers. The bidder returns from the acquisition of public companies are consistent with the negative signalling effect of equity issues found in Myers and Majluf (1984), Smith (1986), and Andrade et al. (2011); and the bidder announcement returns for the acquisition of private targets are consistent with the blockholder effect found in (Chang, 1998).

2.5 Research on frequent acquirers

Various research conducted by consulting firms purports that the frequent acquisition of small firms results in superior returns for acquirers' shareholders. These reports include Frick and Torres (2002) from McKinsey & Co.; Harding and Rovit (2004) from Bain & Co.; and Cools, King, Noonan, and Tsusaka (2004) from the Boston Consulting Group. They argue that smaller targets are easier to integrate into the acquiring company's business operations. They also state that frequent acquisitions results in experience benefits, which translates into superior ex-post performance and higher bidder announcement returns. However, one could argue that the researchers from these consulting firms would be inclined to be biased towards reaching results that are favourable towards making frequent acquisitions due to the business they receive when they are consulting with firms that make these acquisitions.

Haleblian and Finkelstein (1999), who used a sample of 449 acquisitions that occurred between 1980 and 1992, found that experience in acquisitive activities were – on a linear basis – negatively correlated with announcement returns. This is somewhat in line with results from Moeller, Schlingemann, and Stulz (2005) that used a sample of 12,023 acquisitions that occurred between 1990 and 2001, who found that the acquirers that experienced the largest losses were previously successful serial-acquirers. However, including a squared control variable revealed that there was a "U" shaped relationship between experience and

performance related to acquisitions. This implies that there are some benefits to having experience in acquisitions beyond a certain point. They argue that this is because of two effects: inappropriate generalisation and experience. Inappropriate generalisation means that managers generalise and use strategies that have worked in the past on new acquisitions without further thought. This can lead to bad decision making and subsequent poor results. The inappropriate generalisation effect dominates until a certain point when the experience effect takes over and becomes the dominant effect.

On the other hand, Bradley and Sundaram (2006), which, as mentioned previously, used a sample of 12,476 acquisitions that occurred between 1990 and 2000, found that frequent acquirers outperformed infrequent acquirers on a general basis. They defined frequent acquirers as firms that acquired more than four firms in their sample period. The fact that this finding deviates from the results uncovered in Moeller et al. (2005) and Haleblian and Finkelstein (1999) is surprising. However, it could be explained by the differences in sample time period.

2.6 Summary of previous research

Our section on previous literature can be summarized as follows:

I. Bidder announcement returns are not significantly different from zero from the acquisition of public targets.
II. Bidder announcement returns are positive for the acquisition of private targets
III. The general negative market reaction to stock issuances in the U.S. makes it hard to infer to what extent the market views the acquisition itself as good or bad.
IV. Specifically for public targets:
    - Acquirer announcement returns are negatively correlated with the relative size of the target
    - Acquirer announcement returns are more negative returns when stock is the method of payment
V. Specifically for private Targets:
    - Acquirer announcement returns are positively correlated with the relative size of the target
    - Acquirer announcement returns are positive for cash offers, but even more positive for stock offers
    - The formation of blockholders in the targets ex-post ownership structure has a positive effect on acquirer announcement returns.

3. Hypotheses

I. On average, there are no significant abnormal returns from the acquisition of public targets.
II. On average, there are significant positive abnormal returns from the acquisition of private targets.
III. Bidder announcement returns for acquirers of public targets are negative when stock is used as the method of payment
IV. Bidder announcement returns for acquirers of private targets are greater when stock is used as the method of payment
V. Bidder announcement returns are negatively related to the absolute size of the acquirer
VI. Bidder announcement returns are greater for unrelated than related acquisitions
VII. There are no acquirer industries that have an idiosyncratic advantage when conducting takeovers
VIII. There are no target industries that have an idiosyncratic advantage that makes them better targets.
IX. Bidder announcement returns for Norwegian acquirers are greater when acquiring domestic targets
X. Acquiring a majority stake in a company increases the acquirer's announcement returns.
XI. Bidder announcement returns for acquirers of public targets are negatively correlated with the relative size of the target, while the bidder announcement returns for acquirers of private targets are positively correlated with the relative size of the target

4. Sample selection

We have collected our data from the Securities Data Corporation's (SDC) Mergers & Acquisitions database. The SDC database has information on 116,000+ U.S. transactions, and 147,000+ non-U.S. transactions. It gathers this information from over 200 English and foreign language news sources; SEC filings and their international counterparts; trade publications; wires; and proprietary surveys of investment banks, law firms, and other advisors (FitzGerald 2015).

Our sample is selected based on the following criteria:

I. The acquirer is listed on the Oslo Stock Exchange (OSE), or on the Oslo Axess Stock Exchange.
II. The deal status is listed as "Completed", which means that the deal has been accepted by both parties and was successfully completed.
III. The deal was announced between January 1st 1962 and April 9th 2015.
IV. The deal is defined as one of the following types: "Disclosed Value Mergers & Acquisitions", "Undisclosed Value Mergers & Acquisitions", "Tender Offer", "Exchange offer", "Minority Stake Purchases", "Acquisition of Remaining Interest".

We have chosen to limit our acquirers to companies listed at the Oslo Stock Exchange and the Oslo Axess Stock Exchange to get a more appropriate basis for comparison of bidder announcement returns. This enables us to use a common benchmark.

The list of deal types are chosen based on the criterion that they are acquisitions of other companies where the acquiring company remains public. Excluded deal types were Leveraged Buyouts, Share Repurchases, and Privatizations.

Other papers, such as Fuller et al (2002) have limited their dataset to deals above a defined minimum deal value. However, we include all values in order to maintain variation in our dataset. Additionally, there is no reason to exclude deals with lower values as we use size as one of our control variable in our analysis. This initial sample selection process yields 1,677 deals with 383 unique acquirers.

The data on daily stock returns are taken from "Børsprosjektet", which is a database at the Norwegian School of Economics. Børsprosjektet houses daily stock prices from 1984 for

companies listed at the Oslo Stock Exchange and Oslo Axess. These returns are adjusted for corporate specific events such as stock splits and dividends.

The intersection of Børsprosjektet's database of daily stock prices and SDC's list of transactions limits our sample time-period to 31 years. This is a longer sample time-period than most earlier research papers on bidder returns, but we choose a longer sample period in order to get as much variation in the data as possible.

One hindrance is that some firms have announced several acquisitions at the same date. Because of our chosen panel regression approach to the analysis (see section on methodology), we are not able to analyse deals announced by the same company at the same date. Specifically, deals announced on the same date by the same company may differ in their characteristics. As such, we are not able to estimate the effects of these characteristics separately, as we cannot determine how much each of these deal affected abnormal returns on the same day for the same company. Consequently, these deals were dropped from the dataset.

Furthermore, some transactions in the SDC database lacked information about *deal value*. This variable is essential to our analysis, which led us to exclude 416 transactions. In addition, Børsprosjektet sometimes lacks historical data on companies that have changed their ticker symbol, while SDC Platinum changes the ticker retroactively to account for the new ticker symbol. Also, in some cases, SDC added an "o" at the end of some ticker symbols to indicate that the ticker was listed at Oslo Børs. This caused some problems with connecting the dataset with the transactions from SDC with the stock returns from Børsprosjektet. Initially, this resulted in the loss of corresponding stock return data for many of the transactions. However, through some additional research, we found many of the incorrect ticker symbols, changed them, and manually added them and their respective stock return data to our sample. We were thus able to increase the number of deals kept in the data set substantially. Finally, there was a minor issue that some stocks had no change in share price at the day of the announcement. There are three prevalent potential reasons for this: a lack of liquidity for the stock in question (no trades completed); a forced trade halt by the stock exchange at the day in question; or simply that the market's expected net present value of the transaction, and other information that entered the market on that day, is zero (closing price turns out to be the same as the day before). We manually checked the trading history of the affected companies around the relevant dates, and found no signs of zero returns due to forced trade halts. As the zero returns

were not caused by trading halts, dropping the transactions with zero stock returns would be wrong, and would bias our results towards overestimating the impact of acquisitions. It is preferable to have a conservative bias, since subsequent significant results will be more robust than if we had dropped the deals with zero return. Our final sample turned out to be 740 transactions conducted by 188 acquirers.

Table 3 shows the number of transactions and the aggregate transaction value for each year from 1988 to 2014. The table shows that the highest level of acquisitive activities – both measured in number of deals and aggregate deal value - took place in the years before the 2007/2008 financial crisis. As could be expected, the number of transactions, and aggregate deal value, dropped after the financial crisis. Furthermore, there is another peak and a subsequent drop during and after the year 2000, which corresponds in time to the *dot-com* bubble of 2000. Somewhat surprisingly, the aggregated deal value is substantially lower during the year 2000 than leading up to the crisis of 2007/2008. The current level of activity seems similar to the level of activity around the year 2002.

Table 3. Sample description – number of deals and aggregate deal value

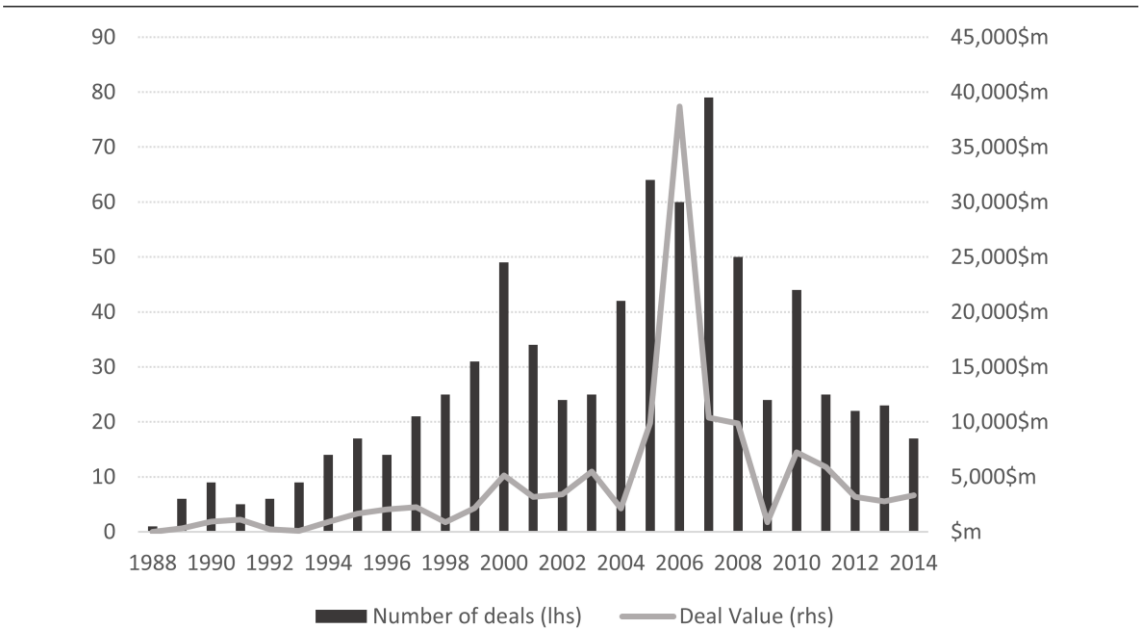


Table 4, 5, and 6 stratify our sample by industry for public targets, private targets, and all targets. The sample contains 135 deals involving public targets and 605 deals where the targets are privately owned. The Mining industry; the Manufacturing industry; and the

Finance, Insurance, and Real Estate industry are the most acquisitive industries of public targets. The most acquisitive industries of private companies are Services and Manufacturing. There is a positive relationship between the number of acquirers and targets within an industry as about half of our sample is of related acquisitions.

The specific details regarding how our final sample is distributed among related and unrelated acquisitions can be found in table 7. The sample is stratified based on SIC-codes, where "2D Common", "3D Common", and 4D Common" refers to acquisitions where the target and acquirer share the first two, three, and four digits in their main SIC-code, respectively. "0D Common" refers to acquisitions where the acquirer and target share zero digits in their main SIC-code. This means that it is an unrelated acquisition.

Table 4. Sample Description – Deals by Industry

Public Target Deals				
	Acquirers		**Targets**	
	N	Percentage	N	Percentage
Agriculture, Forestry & Fishing	4	3.0%	8	5.9%
Mining	25	18.5%	23	17.0%
Construction	3	2.2%	1	0.7%
Manufacturing	37	27.4%	44	32.6%
Transportation, Communications, Electric, Gas and Sanitary	18	13.3%	20	14.8%
Wholesale Trade	6	4.4%	5	3.7%
Retail Trade	0	0.0%	0	0.0%
Finance, Insurance and Real Estate	36	26.7%	17	12.6%
Services	6	4.4%	17	12.6%
Total	**135**	**100%**	**135**	**100%**

Table 5. Sample Description – Deals by Industry

Private Target Deals				
	Acquirers		**Targets**	
	N	Percentage	N	Percentage
Agriculture, Forestry & Fishing	13	2.1%	21	3.5%
Mining	69	11.4%	58	9.6%
Construction	7	1.2%	18	3.0%
Manufacturing	163	26.9%	154	25.5%
Transportation, Communications, Electric, Gas and Sanitary	86	14.2%	77	12.7%
Wholesale Trade	12	2.0%	23	3.8%
Retail Trade	7	1.2%	13	2.1%
Finance, Insurance and Real Estate	56	9.3%	45	7.4%
Services	192	31.7%	196	32.4%
Total	**605**	**100%**	**605**	**100%**

Table 6. Sample Description – Deals by Industry

All Deals				
	Acquirers		**Targets**	
	N	Percentage	N	Percentage
Agriculture, Forestry & Fishing	17	2.3%	29	3.9%
Mining	94	12.7%	81	10.9%
Construction	10	1.4%	19	2.6%
Manufacturing	200	27.0%	198	26.8%
Transportation, Communications, Electric, Gas and Sanitary	104	14.1%	97	13.1%
Wholesale Trade	18	2.4%	28	3.8%
Retail Trade	7	0.9%	13	1.8%
Finance, Insurance and Real Estate	92	12.4%	62	8.4%
Services	198	26.8%	213	28.8%
	740	**100.0%**	**740**	**100.0%**

Table 7. Sample Description - Relatedness

Non-Diversifying Deals						
	Public Target		**Private target**		**Total**	
	N	Percentage	N	Percentage	N	Percentage
2D Common	8	5.9%	69	11.4%	77	10.4%
3D Common	11	8.1%	76	12.6%	87	11.8%
4D Common	42	31.1%	194	32.1%	236	31.9%
0D Common	74	54.8%	266	44.0%	340	45.9%
Total	**135**	**100.0%**	**605**	**100.0%**	**740**	**100.0%**

The majority of transactions in our final sample is from after 2000. This is due to the aforementioned problem of missing deal values for some of the earlier transactions and connectivity issues with Børsprosjektet. The majority of the transactions in our sample were financed using cash as the method of payment. The relative amount of stock financed deals are similar for transactions involving both private and public targets, while the relative amount of hybrid offers are greater for transactions involving private targets. The number of foreign and domestic targets in our sample is relatively similar, as the sample consists of transactions with 240 domestic and 246 foreign targets. See table 8 for specific details regarding how our sample is distributed chronologically and stratified based on the acquirer's method of payment, whether the target is foreign or domestic, and the target's public status.

Table 8. Sample Description – Method of Payment

	Foreign targets						Domestic targets					
	Public Targets			Private Targets			Public Targets			Private Targets		
	Cash	Stock	Hybrid	Cash	Stock	Hybrid	Cash	Stock	Hybrid	Cash	Stock	Hybrid
1988	0	0	0	1	0	0	0	0	0	0	0	0
1989	3	0	0	0	0	1	2	0	0	0	0	0
1990	0	0	0	3	0	2	1	0	0	1	1	1
1991	2	0	0	0	0	0	0	0	1	2	0	0
1992	0	0	0	2	0	1	0	0	0	3	0	0
1993	1	0	0	1	0	1	2	0	0	3	0	1
1994	0	0	0	2	0	1	4	0	0	5	0	2
1995	0	0	0	4	1	2	4	0	0	6	0	0
1996	1	0	0	5	1	0	4	0	0	1	1	1
1997	1	0	0	6	2	1	1	0	0	7	1	2
1998	0	0	0	10	3	2	1	0	2	6	0	1
1999	3	0	0	8	0	2	6	0	0	8	1	3
2000	3	0	0	11	3	12	5	0	0	10	3	2
2001	3	1	0	12	0	3	3	0	0	5	4	3
2002	1	0	0	8	1	4	0	0	0	8	0	2
2003	1	0	0	11	0	3	0	0	1	9	0	0
2004	2	0	0	14	1	8	0	0	0	14	1	2
2005	3	0	0	20	1	5	5	1	0	25	2	2
2006	3	0	0	22	1	5	6	3	2	17	0	1
2007	6	0	0	27	1	5	9	0	0	21	2	8
2008	2	1	0	20	0	6	4	1	1	14	0	1
2009	1	0	0	6	2	1	6	0	0	5	1	2
2010	3	0	0	9	1	6	6	0	0	7	8	4
2011	1	0	0	3	0	2	1	0	1	11	2	4
2012	3	0	0	6	0	0	3	0	0	7	1	2
2013	1	0	0	7	0	2	3	0	0	8	0	2
2014	0	0	0	7	0	3	0	0	0	6	1	0
Total	**44**	**2**	**0**	**225**	**18**	**78**	**76**	**5**	**8**	**209**	**29**	**46**

Table 9 stratifies the initial sample of 1,677 transactions based on transaction experience. "Experience" denotes the number of transactions completed, including the recently announced deal, for the particular company making an announcement.

Table 9. Sample description – acquisition experience

Experience	Kept sample	Dropped sample	Total sample
1	109	315	424
2	79	158	237
3	69	102	171
4	63	65	128
5	47	44	91
6	46	34	80
7	29	30	59
8	22	21	43
9	20	17	37
10	21	14	35
11	20	13	33
12	16	13	29
13	14	13	27
14	10	11	21
15	11	10	21
16	10	10	20
17	9	8	17
18	11	6	17
19	7	5	12
20	9	5	14
21	4	5	9
22	7	5	12
23	4	4	8
24	5	3	8
25	3	3	6
26	4	3	7
27	4	3	7
28	7	3	10
29	6	3	9
30	2	3	5
31	3	2	5
32	3	2	5
33	4	2	6
34	4	1	5
35	2	1	3
36	5	-	5
37	4	-	4
38	3	-	3
39	4	-	4
40	4	-	4
41	4	-	4
42	2	-	2
43	1	-	1
44	2	-	2
45	4	-	4
46	4	-	4
47	3	-	3
48	3	-	3
49	2	-	2
50	2	-	2
51	3	-	3
52	2	-	2
53	2	-	2
54	-	-	-
55	2	-	2
Total	**740**	**937**	**1677**

The loss of 937 deals makes our sample susceptible to a sample selection bias. Bias could arise if the characteristics in the dropped sample (e.g. public status, method of payment, industry, relatedness) are substantially different from the characteristics in our final kept sample. We compared the characteristics of our final sample with the sample of deals that were dropped in order to assess whether the kept sample is likely to suffer from sample selection bias. This comparison, with the differences between the samples given in percentage points, is illustrated in table 10. This reveals that the kept and dropped sample is fairly similar. However, there are some notable deviations with an 11 and 14 percentage point difference in the service industry for targets and acquirers, respectively, and an 8 percentage point difference in the number of diversifying acquisitions. We believe that, even with these discrepancies, the comparison confirms that the sample does not particularly suffer from sample selection bias.

Table 10. Sample Description – Deals by Industry

	Kept Sample	Dropped Sample	Difference
N	740	937	
Domicile:			
Domestic target	50%	53%	-3%
Foreign target	50%	47%	3%
Public status:			
Public target	18%	17%	1%
Private target	82%	83%	-1%
Relatedness:			
2D Common	10%	10%	0%
3D Common	13%	7%	6%
4D Common	32%	30%	2%
0D Common	45%	53%	-8%
Target industry:			
Agriculture, Forestry & Fishing	4%	3%	1%
Mining	11%	10%	1%
Construction	3%	2%	0%
Manufacturing	27%	32%	-5%
Transportation, Communications, Electric, Gas and Sanitary	13%	16%	-3%
Wholesale Trade	4%	4%	0%
Retail Trade	2%	2%	0%
Finance, Insurance and Real Estate	8%	12%	-4%
Services	29%	18%	11%
Acquirer industry:			
Agriculture, Forestry & Fishing	2%	1%	1%
Mining	13%	13%	0%
Construction	1%	2%	-1%
Manufacturing	27%	30%	-3%
Transportation, Communications, Electric, Gas and Sanitary	14%	18%	-3%
Wholesale Trade	2%	3%	-1%
Retail Trade	1%	2%	-1%
Finance, Insurance and Real Estate	12%	18%	-6%
Services	27%	13%	14%
Payment method*:			
N	749	332	
Cash	75%	74%	1%
Stock	7%	13%	-5%
Hybrid	18%	14%	4%
Unspecified	-	605	

**The percentages for the dropped sample excludes 605 transactions where payment method is "Unspecified"*

5. Control variables

The SDC database allows us to collect data on various deal characteristics for each transaction. We use the data on these transactions to test the relationship between announcement returns and these deal characteristics.

The first characteristic we test for is which days surrounding the transaction provide significant abnormal announcement returns. We do this by testing each day for one month, which is 20 trading days, before and after the announcement of the acquisition. We only conduct further research on the days that are significant, as the insignificant days are not relevant to our analysis.

We also test whether the target is Norwegian (domestic) or foreign and how this influences bidder returns. This is easily done because the SDC database lists the target's country of origin. Differences in announcement returns based on the fact that the target is foreign or domestic might indicate that the market believes that the acquirer has more information about a domestic target and is therefore more suited to acquire domestic targets.

We also see whether the target company's industry, which we classify based on the company's main Standard Industrial Classification (SIC) code, affects bidder announcement returns. This is done in two forms: First, we test for whether the fact that the target and acquirer is in the same industry affects bidder returns. This is to assess whether there exists some form of "relatedness" effect. One could argue that an acquiring firm in the same industry as the target is better suited to pick "good" targets than an acquirer from an unrelated industry. Second, we see whether there exists a general relationship between bidder returns and the target's and acquirer's industry separately, Some industries could have idiosyncratic characteristics, which makes them better targets or acquirers, such as lots of fixed assets.

Moreover, we also test whether there is some form of relationship between bidder returns and whether the target is public or private. Private firms are not as easily traded as their public counterparts are, so the price of a private firm might include an implicit liquidity discount. Moreover, public firms should be more efficiently priced than private firms as their market values are the result of continuous transactions between buyers and sellers of shares in the company. Finally, bidder returns could also be affected by the fact that private firms generally have a more concentrated ownership on a general basis.

The next control variable we test for is the relative size between the target and the acquirer. The size of the acquirer is defined as the market capitalization of the acquirer's equity four weeks prior to announcement, while the deal value is used as a proxy for the target's size. Deal value is chosen as a proxy for the target's size because a majority of our targets are private. If the target is too small relative to the acquirer, the bidder returns attributable to the announcement of the takeover might disappear within the regular volatility of the stock.

We also test for whether method of payment has any effect on bidder returns. The most appropriate variable for this is "Consideration Offered". This is the consideration that the acquirer offers at or right after the announcement of the acquisition. SDC had a lot of different categories that described the consideration structure, which included items such as earnouts, assumed liabilities, cash, and different classes of equity. We sorted the different categories used by SDC's and grouped them as either Cash, Stock, or Hybrid. Cash is the consideration structure where the acquirer uses cash exclusively to acquire the target, stock the category where the acquirer only uses equity to acquire the target, and Hybrid is the consideration structure that mixes any different types of considerations, or any method of payment that does not fulfil the criteria of "cash" or "stock".

As mentioned, previous research uncover that the effects of size and method of payment are dependent on the target's public status. In order to test for the same kinds of effects, we include several interaction terms. Specifically, we interact the target's public status with the different size controls and method of payment controls.

Furthermore, we test for whether companies that make frequent acquisitions have some form of experience benefit and receive higher bidder returns than companies that make few acquisitions. We test for this by simply including the acquisition experience of companies, i.e. how many successful acquisitions they have conducted at the relevant point in time.

Moreover, we see whether gaining control of the target company affects the acquirer's return. An acquiring company needs control over the target in order to effectively realize synergies. We have defined "Control" as a 50% ownership stake, or more. That is to say we do not consider an acquisition to involve change of control if the acquirer does not end up controlling a 50% or larger stake, and equally so if the acquirer owned more than 50% ex-ante. It is important to note that owners can in many cases effectively control a company with less than

a 50% ownership stake, but we think that this threshold is less arbitrary than other levels of ownership, as it is the ownership stake needed in order to have a simple majority.

Finally, we analyse whether there is any pattern between bidder announcement returns across time-periods. Economic cycles could be expected to influence how the market reacts to the announcement of an acquisition.

6. Methodology

6.1 Abnormal returns

The central measurement issue in M&A is how to determine the *abnormal return*s caused by such activity. In other words, what are the excess returns to shareholders of acquiring, or target, companies, caused by the acquisition? The most common approach is to look at abnormal returns of a security around the time of the announcement of M&A events, known as *announcement returns*. The rationale behind this approach is that the price reactions of the security at the time of the announcement is the most reliable way to isolate the specific effects of the M&A deal.

A different approach would be to measure company financial performance before and after the consummation of the deal. However, this approach is problematic because it would require the measurement of company performance over long periods, which would subject to large amounts of noise not attributable to the deal. Also, it is impossible to get financials for many firms before the deal (e.g. because they are private).

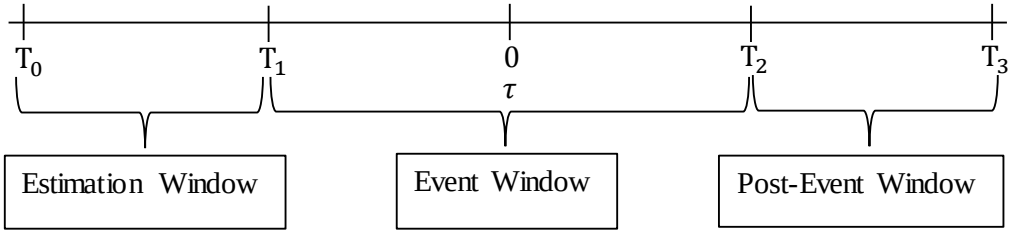
Returning to announcement returns, the idea underlying this approach is that the market reveals its aggregate opinion about the value of the deal. If the market thinks the deal has a positive (negative) value for either the acquirer or the target, the market will bid up (down) the share price of the traded security of the acquirer or target. If security prices incorporate all future expectations about a company's value – which, on average, is a reasonable assumption – then, the security price should change in order to reflect all expected values from any prospective M&A activity. Importantly, security prices react quickly to news affecting the perceived value of a company, which means that the perceived value (positive or negative) from M&A deals should be reflected as soon as news of such activities becomes available. Of course, market expectations about any value from a particular M&A deal are by no means guaranteed to be accurate. Still, the speed with which expectations about a company is incorporated into the security price makes it possible to isolate the effects from M&A in a convincing manner. A more subtle point is that even though the security may appreciate (depreciate) before any real gain (loss) from the deal has actually materialized, a shareholder may choose to sell the security immediately after said appreciation. This means that any

announcement returns may in fact create real value for shareholders, further making the case for why we should care about announcement returns.

A limitation of announcement returns is that they will not accurately reflect the actual consensus opinion about the value of the deal itself. This is because that the share price of both acquirer and target following an announcement will take into account the consensus probability of the deal going through. This can be observed by studying all-stock transactions. If company "A" announces its bid for the acquisition of company "B" in a one share-for-share transaction, the share price of "B" should be equal to "A" immediately after the announcement. This is often not the case, precisely because of the uncertainty of the deal actually going through. Some financial actors specialize in collecting information about announced M&A events, and employ a trading strategy known as "merger arbitrage", seeking to make a profit if they believe the prices of "A" and "B" does not reflect their own, more informed, opinion about the probability of the deal going through. To be clear, this strategy does not exploit an actual arbitrage opportunity, since the strategy is not without risk: the arbitrageur could be wrong about the probabilities. If the arbitrageur thinks the probability of the deal going through is higher than market prices imply, he will want to short-sell the stock of the acquirer, and buy the stock of the acquisition target.

6.2 Classic event study methodology

The normal approach to estimating abnormal returns is through a so-called *event study*, as described in papers like MacKinlay (1997). The traditional approach involves estimating some kind of *normal* return for a company in a pre-event estimation window, and then computing abnormal returns in the actual event window by subtracting the normal return from the actual return in the event window. Typically, this approach entails a simple market model.



$$R_{abn,i,y} = R_{i,y} - \hat{\alpha}_{i,y} - \hat{\beta}_{M,i,y} R_M$$

$R_{abn,i,y}$ is the abnormal *excess* (actual less risk-free) return in the event widow for company *i* from event *y*; $R_{i,y}$ is the total excess return for company *i* in the event window for event *y*; $\hat{\alpha}_{i,y}$ is the estimated alpha for company *i* in the pre-event estimation window before event *y*; $\hat{\beta}_{M,i,y}$ is the pre-event estimated regression coefficient on the market excess return, $R_{M,y}$, before event *y*. The standard approach involves estimating alpha and beta coefficients for each company using a pre-event window, defined by the researcher, respective to each individual deal. Abnormal returns are then calculated by subtracting the estimated alpha and market correlated returns from the total returns in the event window. Finally, in the case of abnormal returns from M&A, the abnormal returns are aggregated for each event (if the event window extends over several periods, e.g. days) to create cumulative abnormal returns (CAR), and then averaged over the sample of deals. An analysis then usually follows, where different average CARs are analysed depending on how the M&A deals are stratified.

6.3 Problems and solutions regarding the classic event study

The approach described above has, in our opinion, three particularly severe limitations. I) the market model allows companies predictable alpha coefficients – alpha is implicitly treated as predictable, since it is consequently subtracted from returns occurring at a later point in time; (II) the pre-event estimation window does not account for the possibility of the same company doing several deals in close time proximity; and (III) event windows are usually defined in an arbitrary way.

If we believe that markets are efficient, or even *somewhat* efficient, share prices should, by definition, not be predictable. Any predictability should be arbitraged away very quickly. This means that a stock should not be able to produce predictable alpha returns over time, which is exactly what the "classic" event-study approach is allowing for. Forcing abnormal returns to account for company specific alphas will bias the results if observed alphas are random, which is our assertion. Our model, which will be discussed in detail later, uses a *random effects* regression model, which assumes that any company specific differences in returns not explained by control variables are random. Furthermore, we also econometrically reject the

hypothesis that a *fixed effects* model is preferable to a *random effects* model. In this way, we remove potential bias that arises from company-specific alphas.

In the "classic" approach, if the pre-event estimation window for event *y* were to overlap with other deals done by a specific company, without accounting for these, then the other deals could potentially bias the estimated coefficients that are only treated as relevant to event *y*. For example, assume that all abnormal returns from M&A for a certain company are in reality positive. If the pre event estimation window for a specific deal overlaps with several other deals in the past, the positive returns from the previous deals would be interpreted as part of the "normal" returns for this company. If this were the case, the abnormal returns for the deal in question would be attenuated, since the estimated coefficients predict that normal returns from this company are always high. Our model uses panel data, with continuous time series data for each company. We include M&A event dummies for each event in each company. In this way, the regression coefficients will not be biased due to previous (or future) deals.

Finally, other research on returns from M&A transactions usually just assume some more or less arbitrary event window around the announcement of an M&A transaction. Minus 2 to plus 2, and minus 5 to plus 5 days before and after an announcement are popular event windows, which are used in the research papers we have cited in the previous research section. This means that there is no testing for the actual significance of abnormal returns for the individual chosen days. The average CAR could be tested for significance, but this would reveal only whether the estimated average abnormal returns for the whole event window are significantly different from zero, i.e. some of the individual days could be strongly significant and others not significant at all. The problem with arbitrary event windows is that a lot of information and insight is lost in the process, when one could alternatively (and easily) determine which days that in fact do produce abnormal returns. This insight could, for instance, help to address the issue of market efficiency in stock markets. That is to say, the more concentrated the abnormal returns are around the actual time of announcement, the more evidence in favour of the theory of efficient markets. Our model, in its initial iteration, adds controls for several days before and after an announcement, in an effort to establish which days that are actually significant with respect to abnormal returns and announcements. This approach makes the best use of the collected data.

6.4 The Model

As mentioned above, we employ a *random effects* model on panel data. The panel data are comprised of deals extracted from the SDC Platinum database, which is merged with data on daily stock prices for the relevant acquirers, provided by Børsprosjektet at NHH. The model can be expressed in the following way:

$$y_{i,t} = c + \beta_i indexreturn_t + \sum_{l \in L_i} \beta_{i,l}\, \omega_{i,l,t} indexreturn_t$$
$$+ \sum_{m \in M} \delta_m\, announcementdummy(m)_{i,t} + \sum_{n \in N} \lambda_n\, control(n)_{i,t} + u_i + e_{i,t}$$

Where $y_{i,t}$ represents the one-day return on stock *i* after closing on day *t*, and is the dependent variable in the model. β_i is the coefficient for company *i*'s interaction with the day *t* one-day return on the chosen index (OSEBX), and is analogous to each company's market beta.

When considering market beta coefficients, an argument could be made that betas are likely to change following acquisitions. Especially so if the transaction target is relatively large, or if the target industry is more (or less) sensitive to market fluctuations than the acquirer industry. In an efficient market, it is reasonable to assume that investors immediately determine how the announced acquisition is likely to affect the market beta of the acquirer, and, consequently, update their opinion on how the equity of the acquirer should be priced relative to movements in the market index. To account for this possibility, we include what we call *step-betas*. Consider the dummy $\omega_{i,l,t}$ which is zero until company *i* announces its deal *l*. After company *i*'s announcement of its deal *l*, this dummy will take the value 1 forever. As such, the dummy represents a *step* (from zero to 1) relative to deal *l*. Note that each company, *i*, makes its own distinct number of L_i deals. The dummy is interacted with the market return, and $\beta_{i,l}$ is the coefficient on this interaction. In other words, $\beta_{i,l}$ is the added effect on overall market beta for company *i* after announcement of its deal *l*, and we refer to this added effect as a step-beta. This allows us to control for any potential changes in market betas caused by acquisition activity, and, as far as we know, we are the first to implement this kind of control into an analysis of abnormal returns. Of course, market betas of companies could also change due to events other than M&A, e.g. after major capital restructurings or change in business segment. Still, we believe that the step-betas are a useful contribution in the context of M&A analysis, and serve as a general improvement to the accuracy of market betas.

In addition, there is an issue with regards to the length of time over which market betas should be estimated. There are no universally accepted standards in this regard, although two or five years of weekly or monthly returns are sometimes used. Since it is not clear what estimation window is optimal for market betas, we retain all our data and estimate betas for the entire sample period. Furthermore, the inclusion of the mentioned step-betas should also serve to correct for changes in market betas.

The δ coefficients express the average effect of different announcement dummies, where announcement dummies are either 0 or 1, depending on the observed day and company. For example, $\delta_{day\ zero}$ is the average effect on daily return on days when $announcementdummy(day\ zero)_{i,t}$ is equal to 1, which is to say that a company *i* announced an M&A event on day *t*. Similarly, $announcementdummy(plus\ 5\ days)_{i,t}$ is equal to 1 when company *i* announced an M&A event on day *t – 5*, and $\delta_{plus\ 5\ days}$ is the average effect on daily return from having made an announcement five days ago, and so on.

The model also allows for controls other than announcement dummies. For example, if we believe that the acquisition target being publicly or privately traded is important for abnormal returns, then the model could include the control variable dummy $control(public\ target)_{i,t}$, which will be equal to 1 when the M&A event announced by company *i* on day *t* involved a public acquisition target. We construct control variables so that they can only be different from zero on corresponding announcement dates. Consequently, since $control(public\ target)_{i,t}$ can only be equal to 1 when there is an announcement, there is no need to interact the control dummy with announcement dummies; and, in the previous example, the coefficients on *announcement day* dummies will now reflect the average effect on *abnormal returns* of making an announcement regarding the acquisition of a privately traded target. Naturally, $\lambda_{public\ target}$ would be the average difference on daily return when announcing the acquisition of a public, rather than a private, company, ceteris paribus.

The model has two error terms. u_i is the unobserved company specific error term attributable to company *i.* Since this is a *random effects* model, u_i is by definition assumed to be random (i.e. there are no company specific persistent alphas). The second error term, $e_{i,t}$, is the idiosyncratic error, which varies with company and time.

This model has some advantages over the classic approach, in addition to those described in the previous section. First, the model allows data for all companies and deals to be run in the same regression, whereas, as mentioned previously, the classic approach is to calculate CARs for each separate deal through individual regressions. This adds an element of simplicity and transparency, making results more readable and intuitive.

The second, and more important, advantage is that the model allows us to include and test several control variables at the same time. In the classic approach, the established way to control for characteristics of M&A deals is to separate deals into different groups and test for differences in CARs between groups. In the classic approach, therefore, it is only practical to analyse the effect of one control variable at the time, and said controls can effectively only be dummies, since deals are only distinguished by the group to which they belong. This severely limits the usefulness of an analysis. Of course, one could in principal create many sub-groups of deals, but interpretation and testing would become increasingly cumbersome as the number of sub-groups increases. Our model makes it easy to interpret the ceteris paribus effects of a large number of control variables, all in one regression.

7. Empirical results

7.1 The event window

As discussed in the section on method, a natural starting point for the analysis of abnormal returns from M&A will be to identify exactly for which days around an announcement abnormal returns are actually significantly different from zero. This is a crucial step towards obtaining insight into the nature of abnormal returns; such insight is lost when using the established method of making an assumption regarding the appropriate event window, and aggregating abnormal returns in the event window into CAR. Our approach in this regard is to include dummies for 20 trading days (approximately four weeks) prior to- and 20 trading days after the day of announcement (day zero).

The regression shown in Table 11 is for all daily stock returns, including the announcement days of 740 deals, regressed on the dummy variables displayed, as well as for company-specific dummies (not shown, available on request) and step-beta dummies (not shown, available on request) interacted with the return on the OSEBX index; all returns are in percentage points. The company-specific, including step-beta, interactions with the index are analogous to company-specific market (Oslo Stock Exchange) beta coefficients. Step-beta interactions, as explained in the methodology section, account for the possibility that company-specific betas may change after acquisitions. The interactions also make it possible to compute average abnormal returns for all deals in one regression, while most other studies do one regression per M&A event, and then compute average abnormal returns subsequently.

The *Day zero* dummy takes the value 1 for days when there was an announcement of an M&A event, and zero otherwise. Similarly *Minus 19 days* takes the value 1 when there will be an announcement in 19 days, and zero otherwise, and so on for the other dummies.

The most striking result is the significance of the coefficient of the *Day zero* dummy, which is significant at the 0.1% level. The coefficient predicts that the daily return on a stock will, on average, be approximately 1.09 percentage points higher on a day when there is an announcement of an M&A event, ceteris paribus.

Table 11. Regression output – 20 days before and after the announcement

	Coef.	Std. err.	t-stat	p-value
Minus 20 days	0.0145	0.132	0.11	0.912
Minus 19 days	0.0695	0.131	0.53	0.597
Minus 18 days	-0.1314	0.131	-1	0.317
Minus 17 days	0.1182	0.131	0.9	0.369
Minus 16 days	0.013	0.131	0.1	0.921
Minus 15 days	0.0192	0.132	0.15	0.884
Minus 14 days	-0.0729	0.132	-0.55	0.579
Minus 13 days	0.0393	0.132	0.3	0.765
Minus 12 days	0.1285	0.131	0.98	0.327
Minus 11 days	-0.1161	0.132	-0.88	0.378
Minus 10 days	0.0017	0.132	0.01	0.99
Minus 9 days	0.0504	0.132	0.38	0.702
Minus 8 days	0.1567	0.132	1.19	0.236
Minus 7 days	0.1682	0.132	1.28	0.202
Minus 6 days	-0.0067	0.132	-0.05	0.959
Minus 5 days	-0.0288	0.132	-0.22	0.827
Minus 4 days	0.2067	0.132	1.56	0.118
Minus 3 days	0.1404	0.133	1.06	0.29
Minus 2 days	0.1075	0.132	0.81	0.417
Minus 1 day	0.2083	0.133	1.57	0.117
Day zero	**1.0915*** **	**0.135**	**8.08**	**0**
Plus 1 day	0.1155	0.132	0.87	0.382
Plus 2 days	-0.1475	0.132	-1.12	0.263
Plus 3 days	-0.0077	0.132	-0.06	0.954
Plus 4 days	-0.0826	0.132	-0.63	0.531
Plus 5 days	-0.1342	0.132	-1.02	0.309
Plus 6 days	0.0527	0.132	0.4	0.69
Plus 7 days	0.1289	0.132	0.98	0.327
Plus 8 days	0.0918	0.132	0.7	0.486
Plus 9 days	-0.1039	0.132	-0.79	0.431
Plus 10 days	0.0633	0.132	0.48	0.631
Plus 11 days	-0.1054	0.132	-0.8	0.424
Plus 12 days	-0.0005	0.132	0	0.997
Plus 13 days	-0.002	0.132	-0.02	0.988
Plus 14 days	0.0072	0.132	0.05	0.957
Plus 15 days	0.187	0.132	1.42	0.155
Plus 16 days	-0.0224	0.132	-0.17	0.865
Plus 17 days	-0.0397	0.132	-0.3	0.763
Plus 18 days	-0.0154	0.132	-0.12	0.907
Plus 19 days	-0.0277	0.131	-0.21	0.833
Plus 20 days	-0.0354	0.131	-0.27	0.788
_cons	0.0355***	0.005	7.55	0
N	615 883			
r2_w	0.0823			
r2_b	0.1359			
r2_o	0.0823			

* p<0.05. ** p<0.01. *** p<0.001

Interestingly, none of the coefficients on the dummies for days before or after announcements are statistically significant at the same level as *Day zero*, or even remotely near this level. The strongest significance, except for at day zero, are for the coefficients on dummies for one and four days before announcement. Coefficients for days *one* and *four* have t-values of 1.57 and 1.56 (p-values of 0.117 and 0.118), and predicted positive abnormal returns of approximately 0.21 percentage points, respectively. However, the coefficient on the dummy for two days before announcement is, while positive, not even slightly significant with a t-value of 0.81. We cannot reasonably expect abnormal returns to be predictably positive four days prior to- but not two days prior to announcement. As such, we cannot reject that positive coefficients on days around, but not including, day zero are random. Still, the fact that coefficients from four days to one day after announcement is somewhat interesting.

Given the results, the day of interest in relation to abnormal returns from M&A is clearly the actual day of announcement, or day zero. In addition, this result is supports the theory of market efficiency. Strong persistent abnormal returns before announcements would suggest some kind of insider trading. Persistent abnormal returns after announcements would suggest that important information about companies is not immediately reflected in the stock price. Since there is no clear and convincing trend in days immediately before or after announcements, these data support the assertion of efficient markets.

The focus from here on will be on explaining the observed abnormal return on day zero, and we conclude that other days surrounding the announcement day does not produce abnormal returns that are significantly different from zero.

7.2 Controlling for the characteristics of the M&A event

The next step of the analysis is to control for different characteristics of the deal, in order to explain what actually drives the abnormal returns. We include several variables in order to control for the hypothesized effects described in the control variables.

The regression shown in Table 12 analyses 740 deals, and includes several control variables that may explain the observed abnormal returns; in addition, company-specific dummies, interacted with the index return, are included, as before (not shown, available on request). All control variables are constructed so that they can only be different from zero if there is an announcement (day zero). This way, controls are de facto interacted with the dummy for announcement, without having to add the actual interactions to the regression.

The coefficient on the dummy for deals that involve foreign acquisition targets is statistically indistinguishable from zero, which means that there is no predicted effect from acquiring a foreign versus domestic company. One could think, ceteris paribus, that acquisitions of domestic targets would yield greater bidder announcement returns than foreign targets because it should be easier to realize synergies with domestic targets, due to factors such as geographical proximity and local knowledge. The fact that the announcement returns are similar for domestic and foreign targets could be due to an offsetting effect of greater potential for returns in foreign markets. It would be interesting to test how acquirers from different countries fared in the same markets as our sample and do a comparison, like Eckbo and Thorburn (2000) did, in order to try and separate the effects of the amount of potential synergies and the potential for synergy realization.

With respect to abnormal returns caused by differences in method of payment, acquisitions of *private* targets where the offered consideration was *all cash* is used as the omitted case. The regression includes interactions between the target's public status and the method of payment, in order to test the hypothesis previously put forward: that the effect of method of payment on abnormal returns is dependent on the targets public status.

Table 12. Regression output – All targets (1/2)

	Coef.	Std. err.	t-stat	p-value
Day zero	1.1497	1.062	1.08	0.279
Foreign target	-0.0605	0.297	-0.20	0.838
All stock offer	3.4694***	0.616	5.63	0.000
Hybrid offer	0.0730	0.402	0.18	0.856
Cash int Public	-0.0589	0.659	-0.09	0.929
All stock offer int Public	-5.5585**	1.853	-3.00	0.003
Hybrid offer int Public	-1.4689	1.768	-0.83	0.406
Acquirer size	-0.0001**	0.000	-2.77	0.006
Acquirer size int Public	-0.0000	0.000	-0.28	0.782
Relative value	-0.0364*	0.016	-2.29	0.022
Relative value^2	0.0008***	0.000	5.62	0.000
Relative value^3	-0.0000***	0.000	-5.89	0.000
Relative value int Public	-0.0407	0.059	-0.69	0.489
Relative value^2 int Public	0.0009	0.001	0.97	0.333
Relative value^3 int Public	-0.0000	0.000	-1.42	0.156
Experience	-0.0319*	0.014	-2.31	0.021
Year 1988	-1.9029	3.749	-0.51	0.612
Year 1989	-1.1102	1.825	-0.61	0.543
Year 1990	-0.8015	1.553	-0.52	0.606
Year 1991	-4.7732*	1.994	-2.39	0.017
Year 1992	-1.5832	1.775	-0.89	0.372
Year 1993	0.1349	1.597	0.08	0.933
Year 1994	-0.7534	1.402	-0.54	0.591
Year 1995	-0.5282	1.340	-0.39	0.693
Year 1996	-2.7372*	1.381	-1.98	0.047
Year 1997	0.4827	1.242	0.39	0.697
Year 1998	-0.1381	1.199	-0.12	0.908
Year 1999	-0.3450	1.155	-0.30	0.765
Year 2000	-0.6484	1.080	-0.60	0.548
Year 2001	-0.6179	1.124	-0.55	0.582
Year 2002	0.5731	1.202	0.48	0.633
Year 2003	0.5394	1.183	0.46	0.649
Year 2004	0.0202	1.097	0.02	0.985
Year 2005	-0.5488	1.042	-0.53	0.599
Year 2006	1.2845	1.046	1.23	0.219
Year 2007	0.4071	1.018	0.40	0.689

Table 12. Regression output – all targets (2/2)

	Coef.	Std. err.	t-stat	p-value
Year 2008	2.0375	1.068	1.91	0.056
Year 2009	0.7904	1.212	0.65	0.514
Year 2010	-0.2526	1.091	-0.23	0.817
Year 2011	1.5708	1.187	1.32	0.186
Year 2012	2.2023	1.228	1.79	0.073
Year 2013	-1.6329	1.224	-1.33	0.182
Year 2014	0.0000	.	.	.
No control	-0.4244	0.402	-1.06	0.291
2D common SIC	-1.5135**	0.505	-3.00	0.003
3D common SIC	-1.0519*	0.496	-2.12	0.034
4D common SIC	-0.8628*	0.348	-2.48	0.013
Agriculture acquirer	1.4944	1.188	1.26	0.209
Mining acquirer	1.1990	0.706	1.70	0.089
Construction acquirer	2.5691	1.413	1.82	0.069
Manufacturing acquirer	0.4692	0.517	0.91	0.364
Transportation acquirer	1.6131**	0.607	2.66	0.008
Wholesale acquirer	-0.9964	1.013	-0.98	0.325
Retail acquirer	0.8028	1.892	0.42	0.671
Finance acquirer	0.2390	0.624	0.38	0.702
Services acquirer	0.0000	.	.	.
Agriculture target	-0.0118	0.968	-0.01	0.990
Mining target	1.9171**	0.704	2.72	0.006
Construction target	-1.6656	1.028	-1.62	0.105
Manufacturing target	0.5665	0.499	1.14	0.256
Transportation target	-0.9139	0.603	-1.52	0.130
Wholesale target	-0.0098	0.805	-0.01	0.990
Retail target	-1.0169	1.459	-0.70	0.486
Finance target	-0.5013	0.681	-0.74	0.462
Services target	0.0000	.	.	.
_cons	0.0367***	0.005	7.97	0.000
N	615,883			
r2_within	0.0828			
r2_between	0.1289			
r2_overall	0.0828			
* p<0.05, ** p<0.01, *** p<0.001				

Note: "int" denotes interaction

The model predicts a 3.47 percentage point positive effect on abnormal returns, on average, when the consideration offered is *all stock* and the target company is *private*, compared to the omitted case mentioned above. This effect is economically large and significant at the 0.1% level. This is consistent with research, such as by Chang (1998), who attributed the greater returns when stock was offered to a private target to the rise of ex-post blockholder ownership in the acquiring company due to the concentrated ownership in private companies. If the target is *publicly* traded and consideration offered is *all stock*, the model predicts, on average, a 5.56 percentage point negative effect on abnormal returns, in *addition* to the effect described for private targets. This additional effect is economically large and significant at the 1% level. This is supported by research such as Bradley and Sundaram (2006), and Moeller et al (2007), who found (compared to stock- and cash deals for private targets and cash deals for public targets) more negative announcement returns for acquirers who used stock to acquire a public target.

The model does not predict any difference in abnormal returns, on average, when the consideration offered is *all cash* and the target is *publicly* traded, compared to the omitted case. This is not consistent with research on the U.S. markets e.g. Fuller et al (2002) and Bradley and Sundaram (2006) who both find greater announcement returns for acquisitions of private targets when cash is the method of payment. There is also no predicted effect from a *hybrid* offer, irrespective of target public status, compared to the omitted case. Fuller et al. found mixed evidence on hybrid offers with significant abnormal returns for private targets, but insignificant abnormal returns for public targets. Given the results, it is worthwhile refitting the model for private and public targets separately.

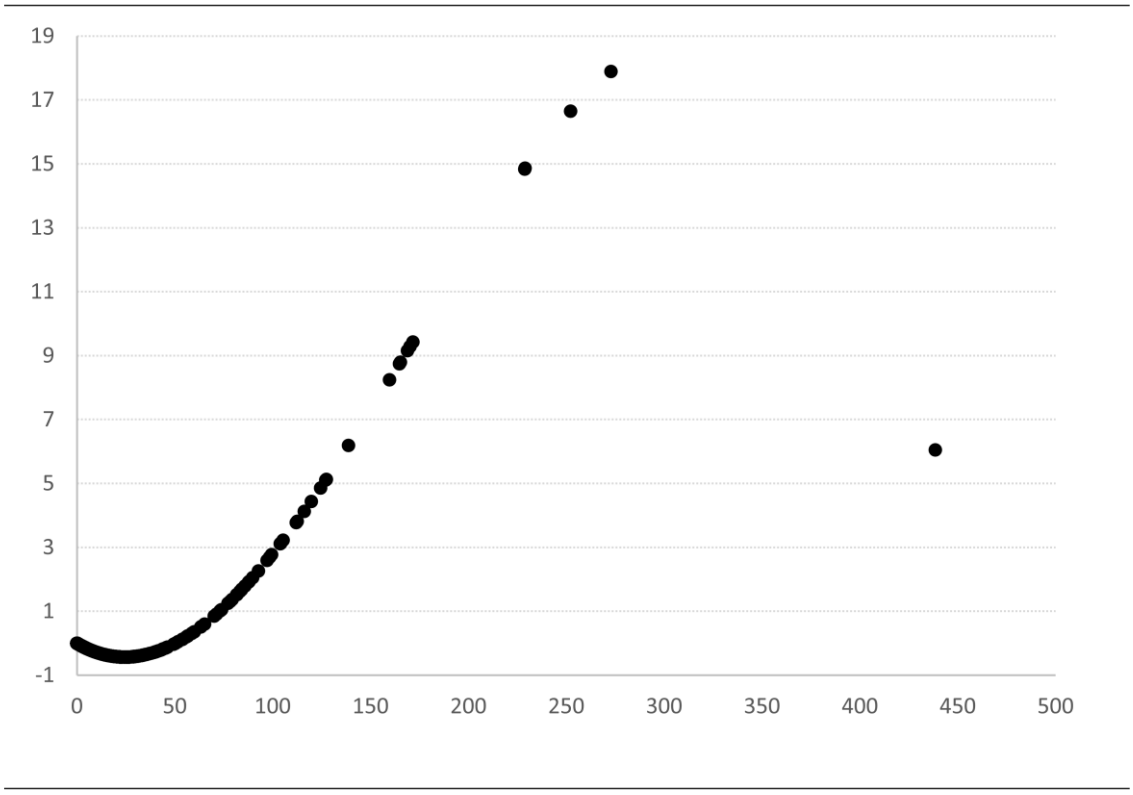
Different measures of size feature heavily in previous research on M&A, and, based on this, we choose to look at the effect of size both with respect to the absolute size of the acquirer, and the relative size of the deal to the acquirer.

The model predicts that absolute size of the acquirer will have a negative effect on abnormal returns, when the target is private, with no significant additional effect predicted for public targets. The coefficient on acquirer size predicts that a one billion USD increase in acquirer market capitalization (four weeks prior to announcement), on average, causes a 0.05 percentage point decrease in abnormal returns, ceteris paribus. The effect is significant at the 1 % level. This is consistent with the research conducted in Moeller et al. (2004), who argued,

as mentioned previously, that large companies are more likely to have managers with hubris and are thus more likely to overpay for targets.

The relative value of deals is calculated as the value of the deal divided by the market capitalization of the acquirer four weeks prior to announcement. This is standard procedure to ensure that value accretion to the acquirer, from the announcement itself, is not included in the market capitalization. The effect on abnormal returns is modelled including squared and cubed terms, and is clearly significant, while the predicted additional effect for public targets is not significant. The actual effect is more obvious when graphing the predicted effects. Figure 1 shows the predicted effect on abnormal returns from relative deal size for all 740 deals. Figure 2 shows the predicted effects for deals with relative value of less than 100%, and gives a more in-depth look into the relative size effect, since most deals (717) have a relative value below this threshold. The estimated effect is extremely large for a few outliers with unusually high relative value, with the largest estimated effect at approximately 18% one-day abnormal return due to relative size, and a relative size of 273%.

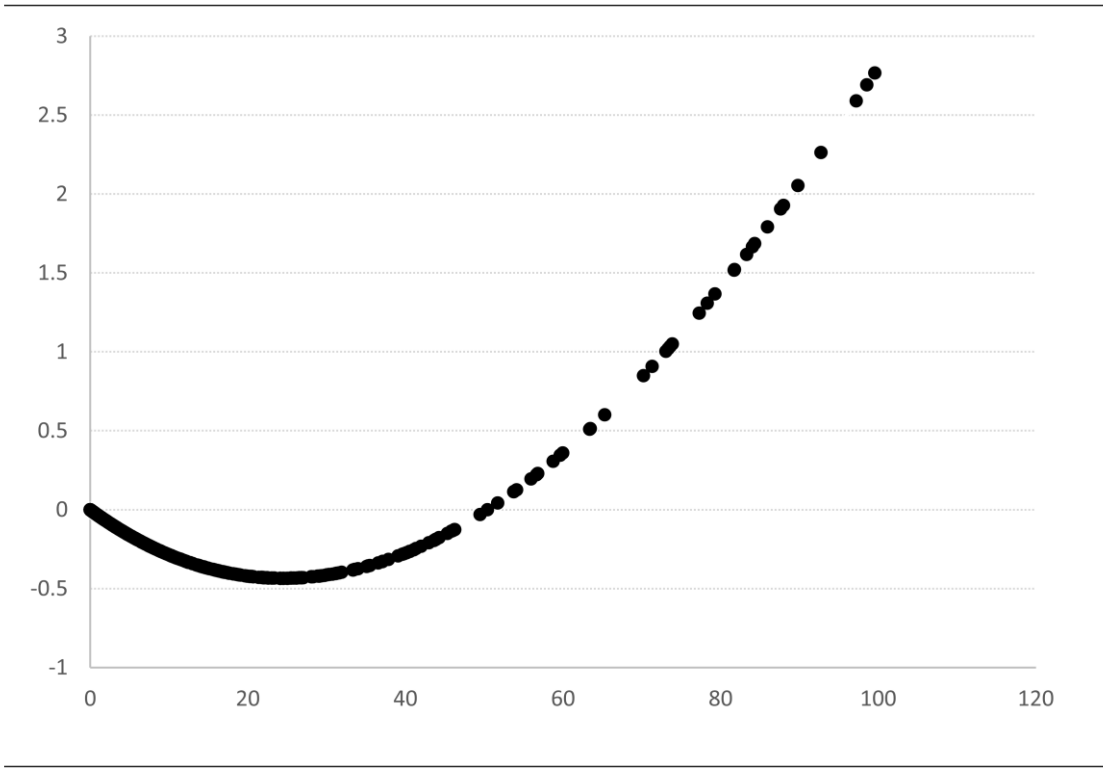
Figure 1. Relative size effect – all targets



Looking at deals with relative size less than 100% of the acquirer reveals a more normalized effect, although the direction of the effect is not straight forward. The model predicts negative

abnormal returns, and increasingly so, from relative value 0% until it bottoms out at approximately 0.43 percentage point negative abnormal return and 25% relative value (not considering the public interaction effects). From here, predicted abnormal returns increase and turn positive at approximately 51% relative value, after which the predicted abnormal returns continue to rise. Computing predicted abnormal returns for deals with public targets, including interaction effects (which are not significant), reveal approximately the same relationship as for private targets, except that the effect on abnormal returns now bottom out at 0.93 percentage point negative return.

Figure 2. Relative size effect – all targets with relative value of 0% to 100%



The fact that the initial effect of relative value on announcement returns are negative is surprising given the fact that our sample contains more private targets (605) than public targets (135), and that announcement returns have been found to more be positively related to relative size for the acquisition of private targets (Fuller et al. 2002), (Bradley & Sundaram, 2006). However, the previous research papers that have informed this thesis on the relative size effect, describes relationship linearly, while we have used a cubic function. This brings additional insight into the relationship between announcement returns and relative size.

One argument for the initial negative effect of relative size could be that it is harder to integrate relatively larger targets into existing operations. The integration costs relative to the incremental gains of an acquisition could yield an initial negative relationship between

relative size and announcement returns until a certain point where the incremental gain of the acquisition outweighs the integration costs. This point, which in our case is at around 25% relative value, could be due to a greater synergy potential with larger targets. It seems reasonable that there would be a higher synergy potential (e.g. cross-selling opportunities, substantial geographical expansion) when the target is beyond a certain size.

The control variable *Experience* is the number of deals a specific company has done at the time, including its latest deal. The experience is calculated using the initial sample extracted from the SDC database, which include more deals than the final sample, and includes deals announced on the same date by the same company. This ensures that the actual acquisition experience is accounted for, even though the deal might not be included in the regression data. The coefficient on this variable is significant at the 1% level and negative, but relatively small at a predicted average of -0.0319 percentage point abnormal return for each one-deal increase in experience. The model has also been fitted with squared terms of experience, but these additional terms were not significant. The negative relationship between acquisition frequency and bidder announcement returns are not in line with Bradley & Sundaram (2006), but it is in line with the negative relationship found in Haleblian and Finkelstein (1999). However, we find no significant "U"-shaped relationship using a squared control variable. This might indicate that inappropriate generalization, which is the idea that managers erroneously base their strategy solely on what has worked in the past, outweighs experience benefits to a greater extent for Norwegian markets compared to the U.S.

Using deals conducted in 2014 as the omitted case, deals in 1991 and 1996 produce significantly negative abnormal returns, ceteris paribus. The model predicts that abnormal returns are 4.77 and 2.74 percentage points lower for deals done in 1991 and 1996, respectively. The number of deals included in our data is five and fourteen for the years 1991 and 1996, which means that these years reflect relatively small parts of the sample, and, it should be considered whether these effects do in fact reflect systematic adverse conditions in the market for acquisitions in these years, or whether the effects are random. There does not seem to be any obvious reason why these particular years should affect abnormal returns negatively.

The *No control* dummy takes the value 1 if an acquirer ended up with a less than 50% stake in the target after the deal, or if an acquirer owned more than a 50% stake in the target before the

deal. This dummy is meant to reflect the fact the deal did not *cause* the acquirer to gain controlling rights that it did not already have. The coefficient for this dummy is statistically indistinguishable from zero. The reason that we find no significant effect of control rights could be due to effective control happening at a different levels ownership stake. It is difficult to find an exact estimate for what ownership stake gives control because this will most likely be different for each company.

Acquirers and targets are considered to be in relating industries based on their SIC-code classifications. The more digits in their SIC-codes acquirers and targets have in common, the more related their industries. Controlling for industry relatedness uses *zero common digits* as the omitted case. Notably, all degrees of relatedness predict negative abnormal returns, all of which are significant, ceteris paribus. The model predicts 1.51, 1.05 or 0.86 percentage points negative abnormal returns from having two, three or four common SIC-code digits, respectively, compared to the omitted case. The effects are significant at the 1% level for two common digits, and at the 5% level for three and four common digits. This in line with the results found in Akbulut and Matsusaka (2010), who found that unrelated mergers were less negative than related mergers in the U.S and argued that diversification can be a value-maximising response to deteriorating industry conditions. It is interesting that related mergers are less negative the more related they are. This could indicate that there is some form of offsetting positive relatedness effect that results in less negative returns for firms that are completely related (four common SIC-code digits) than for those that are slightly related (two- and three common SIC-code digits). The returns are worst for firms that acquire targets that shares their two SIC-code. One could argue that these transactions are "caught in the middle" and the acquirer will neither receive a positive effect from diversification, or relatedness.

Differences in abnormal returns, owing to the acquirer's industry, uses services as the omitted case. The model predicts significant abnormal returns relating to industry when the acquirer is in the "transportation, communications, electric, gas and sanitary" industry. In this case, the model predicts an average increase in abnormal return of 1.61 percentage points, significant at the 1% level. In addition, the model predicts average negative abnormal returns of 0.91 percentage points when the target is in this industry, just outside of 10% significance level. Note that this result is not a consequence of the acquirer's industry being the same as that of the target, since any abnormal returns owing to industry similarities between acquirer and

target are controlled for through different control variables. In the sample for which the model is fitted, there are 104 deals where acquirers, and 97 deals where targets, are within this industry. This supports the assertion that the observed effect is not random.

Being an acquirer in mining predicts average positive abnormal returns of 1.19 percentage points, significant at the 10% level. Furthermore, acquiring targets in mining predicts average positive abnormal returns of 1.92 percentage points, significant at the 1% level. There are 94 deals in the sample where the acquirer is in mining, and 81 deals where the target is in mining, solidifying these results. We expect firms in the mining industry to possess assets such as gold, aluminium, iron, etc., which are all commodities with more intrinsically stable values than assets such as a patent that may or may not prove to have value in the future. These assets are relatively easy to value, so one could argue that the risk of overpaying is relatively lower.

The model also predicts a large positive effect from being an acquirer in the construction industry, at 2.57 percentage points, significant at the 10% level. However, there are only ten deals in the sample where the acquirer is in the construction industry, which leads us to doubt whether this observed effect is well founded. Similarly, there are only nineteen deals where the target is in the construction industry, and the same scepticism applies toward the predicted negative value for this effect, even though the effect is also significant at the 10% level. There are no other statistically significant effects associated with either acquirer's or target's industry.

Notably, the coefficient on the *day zero* dummy is larger when controlling for characteristics of deals, while not statistically significant. However, the larger coefficient is simply a result of the choices made with regards to the omitted cases for the controls described above. If we were to summarize the different coefficients multiplied with the respective average values of control variables, we should get the same overall number for abnormal returns as we did in the regression without characteristic controls.

Table 13 shows the effects of each control variable weighted by the average value of said controls for our sample. The accumulated weighted effect of all controls adds up 1.07 percentage points positive abnormal returns. This corresponds to the estimated effect on day zero from the regression in Table 11 (1.09), with a slight deviation from rounded decimals.

Table 13. Weighted average effect – all targets (1/2)

	Coefficient	Avg. Value	Effect
Day zero	1.1497	1.0000	1.1497
Foreign target	-0.0605	0.4959	-0.0300
All stock offer	3.4694	0.0730	0.2532
Hybrid offer	0.0730	0.1784	0.0130
Cash int. public	-0.0589	0.1622	-0.0096
All stock offer int. public	-5.5585	0.0095	-0.0526
Hybrid offer int. public	-1.4689	0.0108	-0.0159
Acquirer size	-0.0001	3606.1660	-0.1803
Acquirer size int. public	0.0000	828.9764	-0.0092
Relative value	-0.0364	16.1008	-0.5858
Relative value^2	0.0008	1463.4500	1.1710
Relative value^3	0.0000	285515.1000	-0.4463
Relative value int. public	-0.0407	2.7920	-0.1136
Relative value^2 int. public	0.0009	194.7767	0.1828
Relative value^3 int. public	-0.000004	29051.7264	-0.1263
Experience	-0.0319	10.6487	-0.3396
Year 1988	-1.9029	0.0014	-0.0026
Year 1989	-1.1102	0.0081	-0.0090
Year 1990	-0.8015	0.0122	-0.0097
Year 1991	-4.7732	0.0068	-0.0323
Year 1992	-1.5832	0.0081	-0.0128
Year 1993	0.1349	0.0122	0.0016
Year 1994	-0.7534	0.0189	-0.0143
Year 1995	-0.5282	0.0230	-0.0121
Year 1996	-2.7372	0.0189	-0.0518
Year 1997	0.4827	0.0284	0.0137
Year 1998	-0.1381	0.0338	-0.0047
Year 1999	-0.3450	0.0419	-0.0145
Year 2000	-0.6484	0.0635	-0.0412
Year 2001	-0.6179	0.0459	-0.0284
Year 2002	0.5731	0.0324	0.0186
Year 2003	0.5394	0.0338	0.0182
Year 2004	0.0202	0.0568	0.0011

Table 13. Weighted average effect – all targets (2/2)

	Coefficient	Avg. Value	Effect
Year 2005	-0.5488	0.0865	-0.0475
Year 2006	1.2845	0.0811	0.1041
Year 2007	0.4071	0.1068	0.0435
Year 2008	2.0375	0.0676	0.1377
Year 2009	0.7904	0.0324	0.0256
Year 2010	-0.2526	0.0595	-0.0150
Year 2011	1.5708	0.0338	0.0531
Year 2012	2.2023	0.0297	0.0655
Year 2013	-1.6329	0.0311	-0.0508
Year 2014	-	-	-
No control	-0.4244	0.2243	-0.0952
2D common SIC	-1.5135	0.1041	-0.1575
3D common SIC	-1.0519	0.1176	-0.1237
4D common SIC	-0.8628	0.3189	-0.2752
Agriculture acquirer	1.4944	0.0230	0.0343
Mining acquirer	1.1990	0.1270	0.1523
Construction acquirer	2.5691	0.0135	0.0347
Manufacturing acquirer	0.4692	0.2703	0.1268
Transportation acquirer	1.6131	0.1405	0.2267
Wholesale acquirer	-0.9964	0.0243	-0.0242
Retail acquirer	0.8028	0.0095	0.0076
Finance acquirer	0.2390	0.1243	0.0297
Services acquirer	-	-	-
Agriculture target	-0.0118	0.0392	-0.0005
Mining target	1.9171	0.1095	0.2098
Construction target	-1.6656	0.0257	-0.0428
Manufacturing target	0.5665	0.2676	0.1516
Transportation target	-0.9139	0.1311	-0.1198
Wholesale target	-0.0098	0.0378	-0.0004
Retail target	-1.0169	0.0176	-0.0179
Finance target	-0.5013	0.0838	-0.0420
Services target	-	-	-
Weighted average effect			**1.0714**

Note: "int" denotes interaction

As discussed, the next natural step is to divide the sample into two groups, public targets and private targets, and fit the model for each group, separately.

7.3 Controlling for the characteristics of the M&A event with private targets

The regression shown in table 14 now only includes deals where the acquisition target is a private company. This yields a sample of 605 deals.

Since the new sample contains a large proportion of the same deals used in the regression for all deals (table 12), the regressions results are obviously quite similar. The controls that have statistically significant coefficients are the same as before, with quite similar significance levels and sizes, with only a few exceptions.

The coefficients on the dummies for years 2008 and 2012 are now significant at the 5% level. Making an acquisition 2008 or 2012 is predicted to result in positive abnormal returns of 2.30 and 2.66 percentage points, respectively, ceteris paribus. The sample contains 41 deals that were announced in 2008 and the target was private, making the observed effect for this year quite robust. Further, the sample contains only 16 deals from 2012 involving private targets, which is to say that the observed effect from this year might be less well founded.

A seemingly likely explanation for the observed effects of 2008 and 2012 is the fact that these were both times of serious economic recessions. The year 2008 saw the recession triggered by the American housing bubble and the fall of Lehman Brothers; and 2012 witnessed the European "credit crunch" with several European states unable to fulfil their economic commitments. During these economic downturns, it might have been that companies under financial duress were acquired by financially sound firms at a low valuation relative to valuations in more stable times. This is sometimes referred to as a "fire sale", implying that firms have no choice but to sell, increasing supply, and lowering prices.

The effect of relatedness in the form of three common SIC-code digits is now slightly less negative, with a predicted negative abnormal return of 0.83 percentage points (compared to negative 1.05 percentage points for all deals). Interestingly, the estimated effect is no longer significant, with a t-value of 1.52. There is no obvious reason why the significance of this

effect should change so drastically while the significance of other degrees of relatedness stay approximately the same, simply as a result of excluding public targets from the analysis.

The predicted effect from being a transportation, communications, electric, gas and sanitary industry acquirer is now larger, at 1.97 percentage points abnormal return (compared to 1.61% percentage points when looking at all deals). In addition, acquiring targets in the same industry predicts slightly more negative returns than before, at 1.14 percentage points negative abnormal return (compared to 0.91 percentage points before), and now significant at the 10% level. There are 86 deals with acquirers, and 77 deals with targets within this industry, which means that we can be still be confident in these results.

Finally, being a mining industry acquirer is predicted to be slightly more beneficial when only analysing private targets. The effect of being an acquirer in mining is predicted to cause 1.97 percentage points positive abnormal returns (compared to 1.61 percentage points from the full sample of private and public targets). Similarly, the model predicts that acquiring private targets in the mining industry causes positive abnormal returns of 2.42 percentage points (compared to an average effect of 1.92 percentage points when looking at both private and public mining targets). There are 69 deals with acquirers, and 58 deals with targets in the mining industry, supporting the validity of these results.

Table 15 shows the weighted average effects for the regression that only includes private targets. The estimated overall effect on abnormal returns is now 1.24 percentage points (compared to 1.07 for all deals). Notably, using stock as payment method drives the average abnormal returns positively to a large degree. Acquirers in the transportation, communications, electric, gas and sanitary industry, as well as well as mining targets, also heavily drive the abnormal returns positively. In addition, effects from all degrees of industry relatedness drives average abnormal returns for the sample in a noticeably negative direction.

Table 14. Regression output – private targets (1/2)

	Coef.	Std. err.	t-stat	p-value
Day zero	1.1972	1.093	1.10	0.273
Foreign target	-0.2806	0.324	-0.87	0.387
All stock offer	3.4115***	0.622	5.48	0.000
Hybrid offer	-0.0037	0.407	-0.01	0.993
Acquirer size	-0.0001**	0.000	-2.92	0.004
Relative value	-0.0401*	0.016	-2.49	0.013
Relative value^2	0.0008***	0.000	5.84	0.000
Relative value^3	-0.0000***	0.000	-6.10	0.000
Experience	-0.0329*	0.015	-2.17	0.030
Year 1988	-2.2333	3.754	-0.59	0.552
Year 1989	-0.0960	3.776	-0.03	0.980
Year 1990	-1.1622	1.621	-0.72	0.473
Year 1991	-10.6287***	2.841	-3.74	0.000
Year 1992	-1.6989	1.783	-0.95	0.341
Year 1993	-2.7416	1.878	-1.46	0.144
Year 1994	0.0050	1.541	0.00	0.997
Year 1995	-0.5597	1.428	-0.39	0.695
Year 1996	-4.2873**	1.567	-2.74	0.006
Year 1997	0.3420	1.280	0.27	0.789
Year 1998	-0.1599	1.228	-0.13	0.896
Year 1999	-0.1593	1.236	-0.13	0.897
Year 2000	-0.5151	1.104	-0.47	0.641
Year 2001	-1.0252	1.180	-0.87	0.385
Year 2002	0.6511	1.222	0.53	0.594
Year 2003	0.7799	1.202	0.65	0.516
Year 2004	-0.0485	1.114	-0.04	0.965
Year 2005	-0.5797	1.065	-0.54	0.586
Year 2006	1.4767	1.077	1.37	0.170
Year 2007	0.3939	1.043	0.38	0.706

Table 14. Regression output – private targets (2/2)

	Coef.	Std. err.	t-stat	p-value
Year 2008	2.3009*	1.095	2.10	0.036
Year 2009	0.5846	1.298	0.45	0.653
Year 2010	0.2707	1.126	0.24	0.810
Year 2011	1.6744	1.223	1.37	0.171
Year 2012	2.6570*	1.328	2.00	0.045
Year 2013	-1.7805	1.274	-1.40	0.162
Year 2014	0.0000	.	.	.
No control	-0.8539	0.466	-1.83	0.067
2D common SIC	-1.5816**	0.539	-2.93	0.003
3D common SIC	-0.8279	0.546	-1.52	0.129
4D common SIC	-0.8523*	0.384	-2.22	0.027
Agriculture acquirer	1.9703	1.401	1.41	0.160
Mining acquirer	1.4145	0.773	1.83	0.067
Construction acquirer	3.1107	1.714	1.81	0.070
Manufacturing acquirer	0.6941	0.555	1.25	0.211
Transportation acquirer	1.9654**	0.656	3.00	0.003
Wholesale acquirer	-1.3820	1.178	-1.17	0.241
Retail acquirer	0.5487	1.905	0.29	0.773
Finance acquirer	-0.1238	0.707	-0.17	0.861
Services acquirer	0.0000	.	.	.
Agriculture target	-0.0359	1.151	-0.03	0.975
Mining target	2.4235**	0.781	3.10	0.002
Construction target	-2.0145	1.081	-1.86	0.062
Manufacturing target	0.8472	0.554	1.53	0.126
Transportation target	-1.1435	0.665	-1.72	0.085
Wholesale target	-0.0255	0.881	-0.03	0.977
Retail target	-0.5353	1.478	-0.36	0.717
Finance target	-0.8047	0.764	-1.05	0.292
Services target	0.0000	.	.	.
_cons	0.0367***	0.005	7.97	0.000
N	615748.0000			
r2_w	0.0828			
r2_b	0.1302			
r2_o	0.0828			

* p<0.05, ** p<0.01, *** p<0.001

Table 15. Weighted average effect – private targets (1/2)

	Coefficient	Avg. Value	Effect
Day zero	1.1972	1.0000	1.1972
Foreign target	-0.2806	0.5306	-0.1489
All stock offer	3.4115	0.0777	0.2650
Hybrid offer	-0.0037	0.2050	-0.0007
Acquirer size	-0.0001	3397	-0.1866
Relative value	-0.0401	16	-0.6530
Relative value^2	0.0008	1552	1.3040
Relative value^3	0.0000	313691	-0.5129
Experience	-0.0329	10.1521	-0.3344
Year 1988	-2.2333	0.0017	-0.0037
Year 1989	-0.0960	0.0017	-0.0002
Year 1990	-1.1622	0.0132	-0.0154
Year 1991	-10.6287	0.0033	-0.0351
Year 1992	-1.6989	0.0099	-0.0168
Year 1993	-2.7416	0.0099	-0.0272
Year 1994	0.0050	0.0165	0.0001
Year 1995	-0.5597	0.0215	-0.0120
Year 1996	-4.2873	0.0149	-0.0638
Year 1997	0.3420	0.0314	0.0107
Year 1998	-0.1599	0.0364	-0.0058
Year 1999	-0.1593	0.0364	-0.0058
Year 2000	-0.5151	0.0678	-0.0349
Year 2001	-1.0252	0.0612	-0.0627
Year 2002	0.6511	0.0380	0.0248
Year 2003	0.7799	0.0380	0.0296
Year 2004	-0.0485	0.0661	-0.0032
Year 2005	-0.5797	0.0909	-0.0527
Year 2006	1.4767	0.0760	0.1123
Year 2007	0.3939	0.1058	0.0417
Year 2008	2.3009	0.0678	0.1559

Table 15. Weighted average effect – private targets (2/2)

	Coefficient	Avg. Value	Effect
Year 2009	0.5846	0.0281	0.0164
Year 2010	0.2707	0.0579	0.0157
Year 2011	1.6744	0.0364	0.0609
Year 2012	2.6570	0.0264	0.0703
Year 2013	-1.7805	0.0314	-0.0559
Year 2014			
No control	-0.8539	0.1388	-0.1186
2D common SIC	-1.5816	0.1140	-0.1804
3D common SIC	-0.8279	0.1256	-0.1040
4D common SIC	-0.8523	0.3207	-0.2733
Agriculture acquirer	1.9703	0.0215	0.0423
Mining acquirer	1.4145	0.1140	0.1613
Construction acquirer	3.1107	0.0116	0.0360
Manufacturing acquirer	0.6941	0.2694	0.1870
Transportation acquirer	1.9654	0.1421	0.2794
Wholesale acquirer	-1.3820	0.0198	-0.0274
Retail acquirer	0.5487	0.0116	0.0063
Finance acquirer	-0.1238	0.0926	-0.0115
Services acquirer			
Agriculture target	-0.0359	0.0347	-0.0012
Mining target	2.4235	0.0959	0.2323
Construction target	-2.0145	0.0298	-0.0599
Manufacturing target	0.8472	0.2545	0.2157
Transportation target	-1.1435	0.1273	-0.1455
Wholesale target	-0.0255	0.0380	-0.0010
Retail target	-0.5353	0.0215	-0.0115
Finance target	-0.8047	0.0744	-0.0599
Services target			
Weighted average effect			**1.2391**

7.4 Controlling for the characteristics of the M&A event with public targets

The regression shown in table 16 now include only deals where the acquisition target is a public company. This yields a sample of 135 deals.

Interestingly, when fitting the model only for deals that involved a publicly traded target company, none of the coefficients on control variables are statistically significant, not even at the 10% level. The coefficient on the day zero announcement dummy is not close to being significant, which implies that there are no abnormal returns the model is failing to explain. Based on these results, the natural conclusion to be drawn is that there are no abnormal returns to Norwegian acquirers when acquiring a publicly traded company.

This is consistent with efficient markets for corporate control, as competition in such a market will not allow any firm to acquire another firm at a price which will give them abnormal returns. Therefore, the expected announcement returns in such a market should be indistinguishable from zero (Travlos (1987).

Table 17 show the weighted average effects on abnormal returns for the regression that only include public targets. The estimated overall effect on abnormal returns is now a modest 0.19 percentage points (compared to 1.07% for all targets). This coincides with the assertion that abnormal returns from acquisitions of public targets are indistinguishable from zero. To further investigate this result, we also performed a regression with public targets only and without other controls than dummies for days surrounding day zero. This regression (not shown) reveals no significant abnormal returns for any day around acquisitions of public targets, supporting the assertion that abnormal returns from acquiring public targets does not produce abnormal returns.

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Table 16. Regression output – Public targets (1/2)

	Coef.	Std. err.	t-stat	p-value
Day zero	0.6828	4.311	0.16	0.874
Foreign target	0.6377	0.871	0.73	0.464
All stock offer	-1.0062	2.107	-0.48	0.633
Hybrid offer	1.2233	2.107	0.58	0.561
Acquirer size	-0.0001	0.000	-0.95	0.342
Relative value	-0.0168	0.083	-0.20	0.839
Relative value^2	0.0007	0.001	0.51	0.612
Relative value^3	-0.0000	0.000	-0.60	0.551
Experience	0.0101	0.046	0.22	0.825
Year 1988	0.0000	.	.	.
Year 1989	-1.1637	4.238	-0.27	0.784
Year 1990	0.0000	.	.	.
Year 1991	-1.0931	4.610	-0.24	0.813
Year 1992	0.0000	.	.	.
Year 1993	5.1844	4.354	1.19	0.234
Year 1994	-2.2724	4.282	-0.53	0.596
Year 1995	-0.6757	4.370	-0.15	0.877
Year 1996	0.3877	4.220	0.09	0.927
Year 1997	2.7479	4.594	0.60	0.550
Year 1998	0.2554	4.683	0.05	0.957
Year 1999	-0.5606	3.992	-0.14	0.888
Year 2000	0.2562	4.296	0.06	0.952
Year 2001	0.7796	4.118	0.19	0.850
Year 2002	0.5069	5.667	0.09	0.929
Year 2003	-3.0898	5.237	-0.59	0.555
Year 2004	1.0567	4.735	0.22	0.823
Year 2005	-0.3619	4.115	-0.09	0.930
Year 2006	0.8802	4.075	0.22	0.829
Year 2007	0.8403	4.020	0.21	0.834

Table 16. Regression output – Public targets (2/2)

	Coef.	Std. err.	t-stat	p-value
Year 2008	0.1536	4.066	0.04	0.970
Year 2009	0.8544	4.195	0.20	0.839
Year 2010	-0.8768	4.100	-0.21	0.831
Year 2011	-0.2444	4.619	-0.05	0.958
Year 2012	0.7143	4.340	0.16	0.869
Year 2013	-0.4808	4.487	-0.11	0.915
Year 2014	0.0000	.	.	.
No control	1.1143	0.887	1.26	0.209
2D common SIC	-0.5027	1.999	-0.25	0.801
3D common SIC	-2.3318	1.794	-1.30	0.194
4D common SIC	-1.0859	1.017	-1.07	0.286
Agriculture acquirer	-1.8125	3.232	-0.56	0.575
Mining acquirer	-0.7226	2.638	-0.27	0.784
Construction acquirer	0.4453	3.506	0.13	0.899
Manufacturing acquirer	-2.7056	2.186	-1.24	0.216
Transportation acquirer	0.2450	2.488	0.10	0.922
Wholesale acquirer	-2.7762	2.800	-0.99	0.321
Retail acquirer	0.0000	.	.	.
Finance acquirer	-1.2013	2.271	-0.53	0.597
Services acquirer	0.0000	.	.	.
Agriculture target	0.5454	2.192	0.25	0.804
Mining target	1.8126	2.210	0.82	0.412
Construction target	-0.2887	5.010	-0.06	0.954
Manufacturing target	-0.1270	1.442	-0.09	0.930
Transportation target	-0.0515	1.862	-0.03	0.978
Wholesale target	-1.5629	2.565	-0.61	0.542
Retail target	0.0000	.	.	.
Finance target	0.6704	1.961	0.34	0.732
Services target	0.0000	.	.	.
_cons	0.0366***	0.005	7.96	0.000
N	615278.0000			
r2_w	0.0821			
r2_b	0.1369			
r2_o	0.0821			
* p<0.05, ** p<0.01, *** p<0.001				

Table 17. Weighted average effect – public targets (1/2)

	Coefficient	Avg. Value	Effect
Day zero	0.6828	1.0000	0.6828
Foreign target	0.6377	0.3407	0.2173
All stock offer	-1.0062	0.0519	-0.0522
Hybrid offer	1.2233	0.0593	0.0725
Acquirer size	-0.0001	4544	-0.2327
Relative value	-0.0168	15	-0.2569
Relative value^2	0.0007	1068	0.7564
Relative value^3	-0.0000026	159247	-0.4179
Experience	0.0101	12.8741	0.1295
Year 1988	-	-	-
Year 1989	-1.1637	0.0370	-0.0431
Year 1990	-	-	-
Year 1991	-1.0931	0.0222	-0.0243
Year 1992	-	-	-
Year 1993	5.1844	0.0222	0.1152
Year 1994	-2.2724	0.0296	-0.0673
Year 1995	-0.6757	0.0296	-0.0200
Year 1996	0.3877	0.0370	0.0144
Year 1997	2.7479	0.0148	0.0407
Year 1998	0.2554	0.0222	0.0057
Year 1999	-0.5606	0.0667	-0.0374
Year 2000	0.2562	0.0593	0.0152
Year 2001	0.7796	0.0519	0.0404
Year 2002	0.5069	0.0074	0.0038
Year 2003	-3.0898	0.0148	-0.0458
Year 2004	1.0567	0.0148	0.0157
Year 2005	-0.3619	0.0667	-0.0241
Year 2006	0.8802	0.1037	0.0913
Year 2007	0.8403	0.1111	0.0934
Year 2008	0.1536	0.0667	0.0102

Table 17. Weighted average effect – public targets (2/2)

	Coefficient	Avg. Value	Effect
Year 2009	0.8544	0.0519	0,0443
Year 2010	-0.8768	0.0667	-0,0585
Year 2011	-0.2444	0.0222	-0,0054
Year 2012	0.7143	0.0444	0,0317
Year 2013	-0.4808	0.0296	-0,0142
Year 2014	-	-	-
No control	1.1143	0.6074	0,6768
2D common SIC	-0.5027	0.0593	-0,0298
3D common SIC	-2.3318	0.0815	-0,1900
4D common SIC	-1.0859	0.3111	-0,3378
Agriculture acquirer	-1.8125	0.0296	-0,0537
Mining acquirer	-0.7226	0.1852	-0,1338
Construction acquirer	0.4453	0.0222	0,0099
Manufacturing acquirer	-2.7056	0.2741	-0,7415
Transportation acquirer	0.2450	0.1333	0,0327
Wholesale acquirer	-2.7762	0.0444	-0,1234
Retail acquirer	-	-	-
Finance acquirer	-1.2013	0.2667	-0,3203
Services acquirer	-	-	-
Agriculture target	0.5454	0.0593	0,0323
Mining target	1.8126	0.1704	0,3088
Construction target	-0.2887	0.0074	-0,0021
Manufacturing target	-0.1270	0.3259	-0,0414
Transportation target	-0.0515	0.1481	-0,0076
Wholesale target	-1.5629	0.0370	-0,0579
Retail target	-	-	-
Finance target	0.6704	0.1259	0,0844
Services target	-	-	-
Weighted average effect			**0.1862**

7.5 Summary of empirical results

We have analysed the nature of the event window around M&A announcements with Norwegian acquirers; and we have modelled abnormal returns to acquirers from a sample of 740 deals with information that allow us to control for various factors, distinguishing between private and public targets.

Controlling for 20 trading days before and after announcement reveals that the only day when abnormal returns from M&A announcements are statistically significant is on the actual announcement day. Consequently, further analysis of abnormal returns were conducted only with respect to day zero. As far as we know, this is the first study to show this.

We further show that abnormal returns can be explained by differences in the consideration offered to the owners of targets, specifically that there is a positive effect from offering stock to private targets, and a negative effect from offering stock to public targets. Moreover, acquirers obtain lower abnormal returns when their absolute size is larger. Also, relative size between the deal value and the acquirer negatively affects abnormal returns when relative size is lower than approximately 50%, while relative value above this threshold affects abnormal returns positively. Abnormal returns are also negatively affected when a company has more acquisition experience (i.e. the company has done more deals in the past). We also show that deals done in 2008 and 2012 result in increased abnormal returns compared to other years. Acquiring targets which are in related industries affect abnormal returns negatively, compared to diversifying acquisitions. Finally, abnormal returns are higher when the acquiring company is in the mining and transportation, communications, electric, gas and sanitary industries; abnormal returns are higher when the target is in the mining industry, and lower when the target is in transportation, communications, electric, gas and sanitary industries

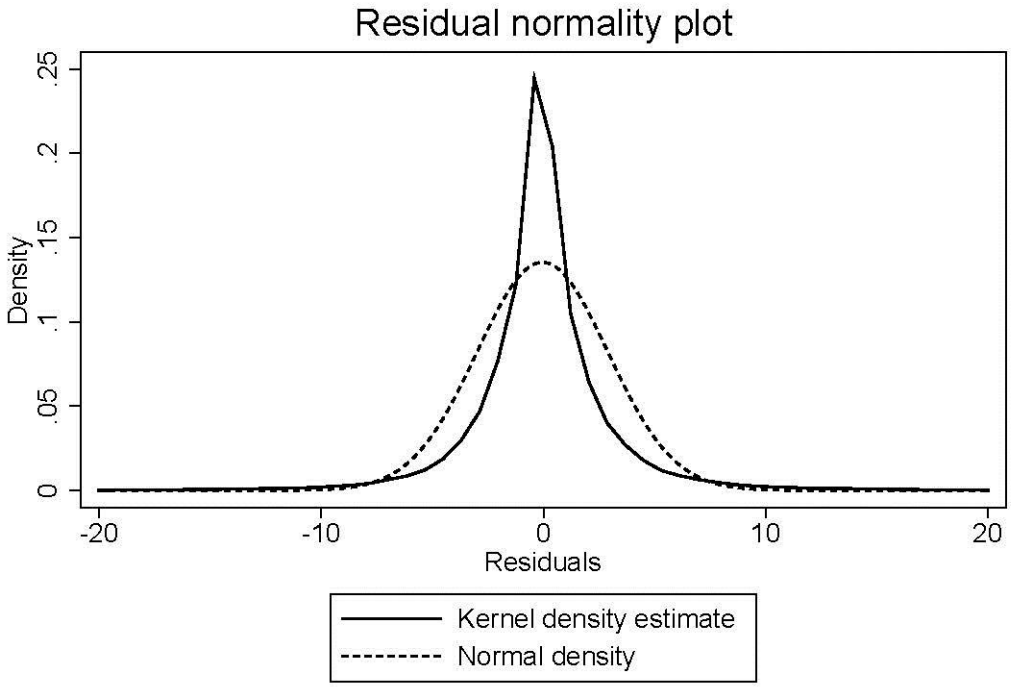
Modelling abnormal returns for deals where targets were private yields very similar results as those described in the above paragraph. Notable differences include: increased positive effects from deals done in 2008 and 2012; less negative returns from acquisitions of companies related through three digit common SIC-codes; and increased sizes in abnormal returns (both positive and negative) when acquirers or targets belong to the industries described in the above paragraph.

Modelling abnormal returns for deals where targets were publicly traded yields very different results than those described above. In fact, no coefficients in the estimated model are statistically significant. This leads to the conclusion that abnormal returns from deals, where the acquisition target is a publicly traded company, are, on average, indistinguishable from zero.

8. Robustness issues

As mentioned before, we employ a *random effects* model in order to estimate our results. To test whether this is in fact correct, we also ran regressions specified as *fixed effects* models, and subsequently performed Hausman specification tests to see whether the underlying assumptions of *random effects* estimation are met. We were not able to reject the null-hypothesis that there are no systematic differences in coefficients, which mean that we choose to continue using the *random effects* model, which is also more efficient, provided underlying assumptions are met.

The validity of statistical inference through t-tests rely on assumptions of approximately normally distributed error terms. It is the case with financial data that distribution of returns are almost always peaked near zero with fat tails at either end. The plot below shows the distribution of predicted errors from our regression including all deals and all characteristic controls. In principle, fat tail causes the problem that conventional limits for test statistics are no longer valid. However, we did not encounter any reference to this issue in published research on abnormal returns, which implies that researchers do not consider this issue crucial. As such, we do not address this issue in our analysis.



An issue when conducting analysis on a part of a sample is the risk of sample selection bias. The SDC database keep extensive records of M&A transactions that have taken place, and is widely used in published research. Therefore, we feel confident that the initial sample represents, at minimum, the large majority of deals conducted by Norwegian publicly traded acquirers. Still, the analysis we performed was, by necessity, only for a part of this sample. However, as shown in the section describing the sample, the characteristics of the deals we had to leave out of the analysis matches our kept sample, for the most part, quite closely. As such we do not expect that the estimated results are materially affected by sample selection bias.

Finally, it is always necessary to consider if the model is likely to suffer from endogeneity bias. One could imagine development in share price would affect control variables in our model. For instance, an acquirer could be more likely to offer stock as payment method after a recent surge in share prices. However, it does not seem reasonable that daily stock *returns* should affect control variables included in our model. This line of reasoning is further supported by our results, which show that there are no significant correlations between daily returns prior to an M&A announcement. As such, we are comfortable in the assertion that the model does not suffer from endogeneity.

9. Conclusion

This thesis examines the announcement returns for Norwegian acquirers of foreign and domestic targets. We find results consistent with the view that the public market for corporate control is efficient, as we find no significant abnormal returns for companies that acquire public targets. However, we find significant abnormal returns for companies that acquire private targets. This confirms hypothesis I and II.

Furthermore, testing a 41-day window around acquisition announcement reveals that the only day that yields significant abnormal returns is day zero (the announcement date). This is consistent with the efficient market hypothesis in semi-strong form, which says that the market reflects all publicly known information. As far as we know, we are the first to show this.

We also find consistency with many of the documented effects that exist for U.S. acquirers. This includes a negative relationship between the acquirer's absolute size and announcement returns; that acquiring public targets with stock is more negative than with cash and that the opposite is true for acquiring private targets; and that unrelated acquisitions have more positive announcement returns than related acquisitions. This confirms hypothesis III, IV, V, and VI.

Certain acquiring and target industries display abnormal returns, contrary to hypothesis VII and VIII. In addition, we can neither conclude that Norwegian acquirers receive greater returns from domestic acquisitions nor that a controlling ownership stake yields greater returns, which reject hypothesis IX and X.

Furthermore, we bring additional insight to the effect of the relative size of the target on bidder announcement returns using a cubic relationship that reveals a negative relationship up to the point where the target is one fourth of the acquirer's size and a positive relationship beyond this point. This relationship means that we cannot conclude either way with regards to hypothesis XI.

We use a *random effects* model on panel data to conduct our analysis, which allows us to control for numerous firm- and deal specific variables simultaneously. This makes it more

informative than the traditional event study method, which stratify their sample in order to test for the effect of each variable. As far as we know, we are the first to apply this approach for analysing bidder returns. Additionally, we are, to our knowledge, the first to account for the possibility of altered marked beta coefficients as a result of acquisitions, through the use of a step-beta approach.

Finally, we suggest two main topics for further research. First, it would be interesting to conduct research on the announcement returns for domestic acquirers in each country the Norwegian firms in our sample have done M&A and compare their returns to the returns in our sample. This could potentially clarify whether the effect truly is insignificant, or whether there are two opposing effects (e.g. geographical proximity vs. higher returns in foreign markets) that cancel each other out. Second, it would be interesting to conduct further research on the relative size effect in the U.S. using a cubic relationship to see whether the cubic effect on relative size is similar to what we find for Norwegian acquirers.

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