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Forgiven by the market: an empirical analysis on the value effects of sin firm acquisitions

By Sil Rutgers (4667573)

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Supervisor: Dirk-Jan Janssen

Radboud Universiteit

Abstract

While the financial economic literature on good CSR behavior seems to be ever-growing, this study focusses on the opposite side for insights on the market perception of unethical investing. Perceived as unethical are so-called ‘sin firms’, which operate in the alcohol, tobacco, weapons and gambling industries. Sin firms have recently been the center of discussion on the question whether or not they could provide investors abnormal returns on their stock portfolios. By analyzing acquirer cumulative abnormal returns for acquisitions that involve sin firm targets, this study shows whether such potential abnormal returns are present in M&A deals. Prior research on this topic is slim and has only focused on short-term returns for acquirers that are not sin firms. It is found that when also looking at the long-term, there seems to be a reversing effect from negative short-term returns to positive long-term returns of sin acquisitions. This supports behavioral economic evidence on the existence of a horn effect and a reversal effect. Additionally, results show no evidence of differences in the returns of sin acquisitions between acquirers that are sin versus those that are not.

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1. Introduction

Current financial economic researchers seem to be obsessed with topics related to corporate social responsibility (CSR), in an attempt to analyze the financial effects of behaving socially responsible for firms. Yet behaving socially responsible is not a possibility for all firms, simply because they are in industries that are seen as socially unresponsible. This is the case for so-called sin firms, that operate in industries generally seen as sinful. A major consequence of this is that many investors exclude the stock of these firms in their portfolios, to avoid being perceived as unethical investors. According to several studies, this positively affects the returns of these sin stocks, finding that they deliver considerably higher risk-adjusted returns than regular stocks (Fabozzi, Ma & Oliphant, 2008), have higher expected returns (Hong & Kacperczyk, 2009), and that exclusion of them can lead to lower expected portfolio returns (Blitz & Swinkels, 2021). But the consensus is shifting since several recent studies have come up with models that can actually explain these high returns. Liston (2016) finds that asset pricing models correcting for investor sentiment (noise trading) make the abnormal returns on sin stock disappear. He suggests that sin stocks are more susceptible to noise trading waves, and are thus harder to arbitrage, resulting in prolonged periods with abnormal returns. The studies of Blitz & Fabozzi (2017) and Richey (2017) both use another approach, and find that the high returns of sin stocks can be explained when using the relatively new Fama-French five-factor model. After controlling for exposures to the five quality factors, no evidence is left that suggests a premium specific to the sin stocks. Thus, the consensus that sin stocks outperform regular stocks is shifting, as reasons are being discovered that can explain the empirically found high returns.

Whether the findings on sin stock returns also apply to other corporate finance topics such as mergers and acquisitions (hereafter referred to as simply acquisitions), is surprisingly less researched. Only the paper of Guidi, Sogiakas, Vagenas-Nanos and Verwijmeren (2020) has studied the value effects of sin firm acquisitions, finding that on average shareholders of the acquiring firm discount sin acquisitions. Yet the paper only focuses on the short-term effects, and only includes non-sin firms acquiring sin targets. Thus the long-term effects, as well as the effects of sin firms acquiring other sin targets, have never been studied before. This thesis will attempt to fill this literature gap, not in the last place because there are numerous arguments that build up to the expectations that the long-term value effects of sin acquisitions differ from the short-term effects, and that the value effects of sin acquisitions for sin acquirers also differ from non-sin acquirers. Next to that, the paper of Guidi et al. (2020) currently forms the only scientific evidence on the short-term value effects of non-sin firms acquiring sin firms, so this thesis will also test whether their findings actually hold for another dataset as well.

Regarding the possible difference between short-term and long-term sin acquisition effects, the efficient market hypothesis (EMH) would prescribe that no difference between these timeframes will exist, because the stocks prices already incorporate the full effect of the acquisition at the time that it is made publicly available (Samuelson, 1965; Fama, 1963, 1965a, 1965b). Yet, psychological and behavioral elements have no place in the EMH, making critics believe such elements can actually affect stock price determination (Malkiel, 2003). Psychological elements that might come into play when looking at long term performance of sin acquisitions, are the tendency that allows overall negative perception to influence specific evaluations (the horn effect, as found by Thorndike, 1920) and the inability of investors to assess complex acquisitions on the short-term (Oler, Harrison & Allen, 2008). These elements could cause a reversal effect between the short-term and the long-term market reaction to an acquisition (Chang & Tsai, 2013).

Regarding the possible difference between the value effects of sin acquisitions for sin acquirers versus non-sin acquirers, a first argument is the diversifying nature the latter has but the first does not have. Multiple studies have found that diversifying acquisitions, in which the target is from another industry than the acquirer, have negative value effects for the acquirers (Harford, 1999; Jensen & Ruback, 1983). The next arguments relate to the multiple empirical findings that only apply to the performance of sin acquisitions made by non-sin acquirers, thus also causing expectations for a difference with sin acquirers. Firstly, the decrease in expropriation risk due to being acquired by a non-sin firm (Beneish et al., 2008). Secondly, the halo effect (which is the positive form of the halo effect) due to being acquired by a high CSR firm (Hong & Liscovich, 2015). Lastly, the possible deterioration of the relationship with stakeholders of the non-sin acquirer caused by entering a sin industry.

To strengthen and expand the literature on the above-named topics this study will focus on both short and long-term value effects of sin acquisitions, made by both sin and non-sin acquirers. This will be achieved by answering the main research question, which is formulated as follows:

What are the value effects of acquiring a sin firm?

The answers to this research question are not only useful for firms that consider such sin acquisitions, but also adds to the existing literature on unethical investing. While one might think it is better not to know whether sin acquisitions could possibly also have positive effects, such findings can also have its implications for ethical investing. Gained knowledge on market perceptions of unethical investing, can also help understand how the market perceives investing in ethical causes. So the implications of this study are extended to the domain of ethical investing as well.

In order to answer the research question, the method of cumulative abnormal returns (CARs) will be used to quantify the market reaction to a sin acquisition. CARs are the difference between the expected returns of a stock, based on estimations of its past relation to the market, and the actual observed stock returns. The CARs will be calculated for a dataset of both sin and non-sin acquisitions, which allows to single out the effect of acquiring a sin firm. Whether or not an acquisition can be categorized as sin, is judged on the target's primary US SIC and NAICS 2017 codes. The acquisition data will be collected from Zephyr, and concerns acquisitions that are announced between 2000 and 2020. Stock and market returns are collected from Refinitiv Eikon. CARs will be calculated over three different timeframes; a short-term event window (-2,2), a long post-event window (3, 130), and a combination of the two windows (-2, 130) used for long-term analysis. To formerly test the hypotheses of this study, ordinary least squares (OLS) regressions are used for which one of the three CARs forms the dependent variable. The main independent variables will be a dummy that distinguishes between sin and non-sin targets, and an interaction variable that shows the effect of sin targets being acquired by sin firms. The regressions will also include control variables for: method of payment, toehold in the target, size of the acquirer, relative size of the acquisition, year of the announcement, industry of the acquirer and country where the acquirer is domiciled.

The results of the short-term CARs show a relatively negative initial market reaction to sin acquisitions made by non-sin acquirers, as also found by Guidi et al. (2020). The effect is consistent over all robustness checks as well, but is not found to be significant. The consistent negative effect does suggest that the arguments of increased litigation risk and deteriorating stakeholder relationships seem to weigh down the returns of sin acquisitions. The results of the long-term CARs reveal a positive market reaction to sin acquisitions made by non-sin acquirers, which is significant on the 10% level. This suggests that the initial negative market reaction is reversed in the months after the sin acquisition, which is in line with the horn effect and the reversal effect. Based on these results the market seems to show forgiveness for sinning acquirers, although it takes some time. Regarding the potential performance difference between sin acquisitions made by either non-sin acquirers or sin acquirers, the results show both negative and positive coefficients across the regressions. Combined with the persistent insignificance, there is no evidence that suggests that this difference exists.

This thesis proceeds by providing an overview of the most relevant literature on the main topics of sin firms and acquisition performance in chapter 2. Resulting from the expectations based on this literature overview, three hypotheses will be formulated. Subsequently, the methodology of this thesis will be discussed, together with the used data and variables, in chapter 3. The results will be described in chapter 4. Lastly, chapter 5 will discuss the implications of the results, describe the limitations of this study, make recommendations for future research and conclude this thesis.

2. Literature review

2.1. Sin stocks

The definition of sin stocks according to Robeco is: "shares in companies involved in activities that are considered unethical" (Robeco, 2022). While this definition is universal, the type of companies that fall within this definition is not universal at all, as it is highly subjective to judge what is unethical. Next to subjectivity, time and culture also have an effect on what people perceive as sinful firms. The alcohol industry for instance is seen as sinful in many cultures due to its addictive properties, but in other cultures brewing beer is seen as a skillful and honorable craft. Time also plays an important role in the perception of sin, as it is only a few decades ago that the bad health effects of tobacco became widely known, meaning that the tobacco industry is only perceived as sinful since then.

In order to still be able to make a selection of sin stocks, it would make most sense to look at the existing literature on the topic. Yet also in the literature there proves to be dispersion on what to regard as a sin industry. Alcohol and tobacco seem to be the only industries on which there is agreement, but there are also several other industries that are often included. The most used (combinations of) other industries are: gambling and weapons (Blitz & Fabozzi, 2017; Richey, 2020; Sagbakken & Zhang, 2022), gaming (Hong & Kacperczyk, 2009; Salaber, 2009; Liston, 2016), gambling, weapons, military or nuclear operations (Guidi et al., 2020; Statman & Glushkov, 2009), and adult services, gaming, weapons, and biotech alterations (Fabozzi, Ma & Oliphant, 2008). As stated, time has effect on what is seen as sinful, and this is also reflected in these papers. Most recent papers include gambling and exclude gaming (except for Liston, 2016), which is why this thesis will do so as well. Next to that, the weapon industry is also included in the majority of the (most recent) papers, so this industry will also be included here. The other named industries, nuclear operations, adult services and biotech alterations, are all limited in both the literature coverage and the deal coverage in case they would be added to the sin selection. So, the selection of sinful industries used by this thesis consists of alcohol, tobacco, gambling and weapons[1].

[1] For completeness of this study, robustness checks will be performed, each excluding one of the sin industries to see whether this affects the results.

2.2. Effect of sin stocks on investor returns

2.2.1.Outperformance of sin stock

Much of the economic research performed on sin firms is aimed at explaining the abnormal returns that some papers have found to be present in portfolios of sin stocks. Fabozzi, Ma and Oliphant (2008) find that their sin stock portfolio produces a 19% annual return, thereby significantly outperforming their benchmark. Similarly, Hong and Kacperczyk (2009) find that sin stocks have higher expected returns, and Blitz and Swinkels (2021) find that exclusion of sin stock from an investment portfolio can result in lower expected returns.

A major reason named for these higher (expected) returns is the systematic undervaluation of sin stocks that is due to the relatively lower demand for them. This lower demand is caused by numerous investors that use exclusion policies in order to refrain themselves from investing in unethical businesses. In line with this, Hong and Kacperczyk (2009) find that sin stocks are to a lesser extend owned by investors that are subject to social norm pressure (such as pension funds, banks and insurance companies). They believe that the systematic undervaluation that they find is due to this lower demand, making it the reason why sin stocks could outperform regular stocks. Blitz and Fabozzi (2017) state that this is indeed the most popular explanation for the outperformance, calling it a reputation risk premium that investors require to invest unethically.

Another reason for the outperformance of sin stocks that is stated in the literature regards the litigation risk surrounding the stock. The litigation risk is supposedly higher due to the products that sin firms produce, which have a relatively higher chance of being negatively affected by government regulation. This risk is higher than for other products, as regulation on addictive products (which the majority of the products of sin firms are) is becoming more common. Hong and Kacperczyk (2009) state that investors want to be compensated for this high litigation risk, thus returns on sin stock tend to be higher.

2.2.2.No outperformance of sin stock

Despite the previously named findings on outperformance of sin stock returns, some recent studies have come up with ways to explain the apparent abnormal returns on sin stock. Blitz and Fabozzi (2017) find that abnormal returns of sin stocks can be explained when using the Fama-French five-factor model. This relatively new model includes two additional quality factors compared to the previous model, namely profitability and investment patterns[2]. The paper runs multiple regressions on the same datasets and finds that most datasets have a significant sin premium when only the original three quality factors are included as independent variables. When the profitability and investment factors are also included, the sin premium consistently becomes insignificant, and thus Blitz and Fabozzi conclude that there is no such sin premium. Richey (2017) has performed an almost identical study that also resulted in the disappearance of sin stock outperformance when using the Fama-French five-factor model. Unfortunately, the authors of both papers do not run regressions with only one of the two new factors included, so no conclusions can be drawn on whether the profitability or the investment patterns (or the combination) contributes most to making the sin premium disappear. A different approach is taken by Liston (2016), who looks at investor sentiment (noise trading) as an explanation for the previously found abnormal returns on sin stock. The paper describes investor sentiment as the combination of rational and irrational components that influence the beliefs of investors about the future, and claims that sin stocks appear more susceptible to waves of investor sentiment than the market. This means that sin stocks are harder to arbitrage, resulting in prolonged periods with prices out of equilibrium and thus with abnormal returns. In all three models used by the paper, the result is that when correcting for waves of investor sentiment, abnormal returns on sin stock disappear. Liston concludes that this suggests that model misspecifications, referring to the ignorance of investor sentiment, have caused the previously found abnormal returns on sin stocks in the literature.

2.3. Sin firms and acquisition performance

2.3.1.Acquisition performance

From the existing literature on sin stock we can conclude that there is some disagreement around the question whether they provide abnormal returns, and also what could explain these (abnormal or not) returns. There is a clear link between stock market performance and acquisition performance, as an acquisition deal regards the purchase of the majority of a firm's stocks. The gains to the shareholders of a firm (the acquirer) that announces the acquisition of another firm (the target), are seen as the acquisition performance (Moeller et al., 2004). When the stock market

[2] The two new Fama-French quality factors imply that profitability has a positive effect on the stock returns of a firm and that the growth of investments has a negative effect on the stock returns of a firm. The three factors that were already part of the previous model concern the size effect (large companies earn lower returns), the value effect (companies with high book-to-market value earn higher returns) and the market risk effect (higher risk earns higher returns).

believes that the acquisition will benefit (drawback) the acquirer, a rise (decline) in the acquirer's stock price will follow, resulting in positive (negative) returns for its shareholders. This stock market reaction is often measured in a short timeframe of a few days around the announcement date, called the event window, in an attempt to only capture the effect of the deal announcement and no other confounding effects (McWilliams & Siegel, 1997). It is also possible to assess the market effects over a longer period, for which post-event windows are used. The length of post-event windows is less typical, as it depends on what the authors want to measure.

The link between stock performance and acquisition performance could mean that the previously named factors affecting sin stock returns also apply to sin acquisition. This creates the first two effects at play, that can be used for formulating the hypotheses. Firstly, the previously named systematic undervaluation of sin stock could cause sin targets to be systematically cheaper, and thus more advantageous for the acquirer (Guidi et al., 2020). Secondly, a reverse effect could be at play, as the previously named litigation risk of sin firms makes the sin targets less advantageous for the acquirer. What must be noted however, is that the similarities between stock market performance and acquirer performance do not imply that the effects of acquiring a sin firm are identical to the effects of purchasing a sin stock. There are other factors that can play a role in the performance of a sin acquisition, that do not play a role for a sin stock purchase. These factors are discussed below, starting with the ones that only affect short-term performance of non-sin acquirers.

2.3.2. Short-term effects for non-sin acquirers

First of such factors is the potential risk sharing benefits that might occur when a sin firm is acquired by a non-sin firm. The paper of Beneish, Jansen, Lewis and Stuart (2008) provides evidence that acquisition deals in the tobacco industry profit from a form of risk-sharing that was not studied before. The paper suggests that when a non-tobacco firm acquires a tobacco target (as opposed to a tobacco firm acquiring a tobacco target), shareholder wealth is better protected against expropriation by politicians[3], which increases the performance of the acquisition. The type of risk here is quite similar to the litigation risk which was named as a reason for sin stock undervaluation in the previous section. Yet the reasoning for its effect is opposite, as Beneish et al. (2008) argue that the increased expropriation risk is indeed present, but can be decreased when the firm is taken over by a firm that is not in the tobacco industry. By this decrease in expropriation risk, the target gains

[3] Beneish et al. believe political expropriation can cause harm to sin target's shareholder wealth by implementing restrictions to sales, increased taxes, punitive damages and other similar regulations.

value and thus makes the performance of the acquisition higher. The underlying cause for this decrease in expropriation risk is that after being acquired, the target firm has more influence in a certain political district due to being part of a non-tobacco firm. This increased influence allows for better lobbying and thus lower risk of expropriation. That this effect is present in the tobacco industry does not mean it is also present in the other sin industries. Yet Guidi et al. (2020) also name political capture (similar to political expropriation) as a possible reason for risk-sharing benefits that can lead to higher performance of sin acquisitions by non-sin firms.

Another factor that can have positive effects on the performance of sin acquisitions by non-sin acquirers, is the use of the halo effect by the management of the acquirer. The halo effect is a cognitive bias that makes people evaluate individual aspects of someone or something more positively, based on an overall positive impression (Thorndike, 1920). Applied to sin acquisitions, this means that the management of the acquirer can create the perception for their shareholders that they will improve the sin target on corporate social responsibility (CSR) areas, thereby increasing the value of the acquisition. This perception can be created solely because the acquirer enjoys a positive CSR reputation, which is (through the halo effect) reflected upon the sin acquisition. This can be the case as the acquirer is expected to maintain the same CSR values on its new acquisition as it does on itself, while there might not even be the commitment to do so. The acquirer is thus effectively using its own brand name to increase the value of the sin acquisition via signaling. Hong & Liscovich (2015) confirm that the halo effect is indeed a likely factor of value creation via CSR. As the halo effect requires the CSR rating of the acquirer to be higher than that of the sin target, the effect should only be observable in the non-sin firm acquiring sin target category of this study.

On the other hand, there can also be negative factors at play when a non-sin firm acquires a sin target, as the relationship with the stakeholders of the acquirer might deteriorate due to association with a sin firm. It can be seen as the flip side of the halo effect, as the acquirer is risking deteriorating its brand name in an attempt to use it for value creation benefits. This is a reputational risk factor, that is similar to the reason why many investors exclude sin stock from their portfolios, as they want to refrain from being seen as unethical by the market. Costs due to reputational damage can become very large, through for instance product boycotts (Pruitt & Friedman, 1986) or via missing out on reputational benefits such as customer loyalty (Walsh, Mitchell, Jackson & Beatty, 2009).

To test whether the negative or the positive factors weigh heavier in a sin acquisition, Guidi et al. (2020) have analyzed the stock market reaction around the announcement of a sin acquisition by a non-sin firm. They do this by taking the cumulative abnormal returns (CARs) of acquirers over the three days symmetrically surrounding the announcement date, and find that market reactions to sin acquisitions are less favorable than that of comparable non-sin acquisitions. To the best of my knowledge, this is the only paper to date that has analyzed acquisition performance for sin firms. To test whether the short-term market reaction to sin acquisitions is also more negative in the dataset of this study, the first hypothesis is formulated accordingly.

Hypothesis 1: *The non-sin acquirer's cumulative abnormal returns around the announcement date of sin acquisitions are more negative than of non-sin acquisitions.*

2.3.3.Long-term effects for non-sin acquirers

A limitation of the paper of Guidi et al. (2020) is that the focus is only on the short term, namely the days around the announcement date of the deal. The efficient market hypothesis (EMH) would however prescribe that no difference between these timeframes will exist, because the stock prices have already incorporated the effect of the acquisition when the announcement is made publicly available (Samuelson, 1965; Fama, 1963, 1965a, 1965b). Yet the EMH is also criticized, often by behavioral economists who argue that psychological traits of investors do not comply with the EMH's assumptions of homogenous expectations and rational investors. They instead argue that there are also behavioral and psychological elements of stock price determination (Malkiel, 2003). These elements will first be discussed in relation to sin acquisitions by non-sin acquirers.

One of the psychological elements that might come into play when looking at long term performance of sin acquisitions, is the so-called horn effect. The horn effect (the negative version of the halo effect) is a cognitive bias that makes people evaluate individual aspects of someone or something more negatively, based on an overall negative impression (Thorndike, 1920). Applying this to the acquisition of a sin firm, it could be that unfavorable stock market reactions upon the announcement of a sin acquisition (as found by Guidi et al.), are caused by negative overall impressions of the target to investors. This negative overall impression is not unlikely for sin firm targets. Investors however, should not let this biased impression influence the evaluation of the economic consequences of the sin acquisition. If they actually do, the relatively negative market reaction to sin acquisitions should only persist on the short-term. For the long-term however, this market reaction should be reversed over time as investors learn more about the target economically, apart from the biased overall impression. Whether this effect is actually present in sin acquisitions, has not been tested before. It can however be expected that when the horn effect is present for sin acquisitions, it is weaker in case the acquirer is also sin, as the acquirer shareholders will likely have a less negative overall impression of sin firms.

Another psychological element could also come into play, even though it has not been studied with regards to sin acquisitions, but it has been studied on other types of acquisitions. Oler, Harrison and Allen (2008) show that in their dataset of horizontal acquisitions, the positive initial market response to the announcement is contradicted by negative long-run post-acquisition returns. They argue that this is due to the complexity of acquisitions, which makes investors initially unable to rightfully assess the economic consequences of the acquisitions. This causes initial positive investor reactions to horizontal acquisitions, to be followed by negative rectifications afterwards. The study of Chang and Tsai (2013) finds a similar reversal effect for acquisitions of privately held targets, where also positive short-term market reactions are followed by negative long-term reactions. The study uses multiple timeframes of varying lengths to measure the overtime change in the market reaction, using windows expanding to 1, 2, 3, 5, 30, 60, 126 and 252 trading days after the announcement. It shows that the initial significantly positive returns turn insignificant at the 60-day window, and subsequently become significantly negative from the 126-day window onwards. This provides evidence that the reversal effect becomes strong enough to counter the initial market reaction, from a six-month event window onwards.

So even though the short-term effects have been researched by Guidi et al. (2020), there are reasons to believe that the short-term relatively unfavorable market reaction to sin acquisitions do not hold on the long-term, as discussed above. The reversal effect could reverse the initial relatively negative reaction to a relatively positive reaction on the long-term. Yet it is unclear whether the effect works this opposite way as well, as it has only shown to change positive reactions to negative in the literature. Theoretically though, the effect should work oppositely as well, since the mechanism of complexity and investor misjudgment still holds. As the long-term market reaction to sin acquisitions has not been tested in the existing literature, this study will do so, as formulated in the second hypothesis. Note that the discussed long-term effects (horn and reversal) could also apply to sin acquisitions made by sin acquirers, but this will be tested by the third hypothesis and not the second hypothesis.

Hypothesis 2: *The non-sin acquirer's cumulative abnormal returns over an extended six-month window, are more positive with sin acquisitions than with non-sin acquisitions.*

2.3.4.Sin versus non-sin acquirers

The study of Guidi et al. (2020) has another limitation, which is that it concerns only non-sin firms acquiring sin targets. This means that sin firms acquiring sin targets are not within the scope of their study, while this difference in acquirer type could prove very interesting. To clarify, acquisition deals can thus be categorized into four different types (as seen in table 1 below), based on the industry of the acquirer in combination with the industry of the target. It must be noted that when making this distinction, the non-sin firm acquiring sin target and the sin firm acquiring non-sin target categories will by definition only consist of diversifying deals, as the acquirer will enter a new industry with the deal. Multiple studies have found that such diversifying mergers have negative value effects for the acquirers (Harford, 1999; Jensen & Ruback, 1983). This is not applicable to sin firms acquiring sin targets, which forms a difference between sin and non-sin acquirers that acquire a sin target.

Table 1: Deal categorization

		Target industry	
		Non-sin	Sin
Acquirer industry	Non-sin	Non-sin firm acquiring non-sin target	Non-sin firm acquiring sin target
	Sin	Sin firm acquiring non-sin target	Sin firm acquiring sin target

Linking back to the earlier mentioned short-term arguments used for the first hypothesis development, it can be concluded that several of them do not apply to sin firms acquiring sin targets. This is the case for the arguments of decreasing expropriation risk (Beneish et al., 2008), the halo effect (Hong & Liscovich, 2015) and the deteriorating relationship with stakeholders. This leaves only the two short-term arguments of possible systematically cheaper sin acquisitions, and increased litigation risk due to sin acquisitions, as applicable to sin firms acquiring sin targets. As these two arguments have opposing effects on the performance of sin acquisitions, it is unclear what the net effect on the short-term will be. Additionally, the long-term arguments used for the second hypothesis, namely the horn effect and the reversal effect, could also apply to sin acquisitions made by sin firms. However, as argued in the previous section, the horn effect will probably be weaker for sin acquisitions made by sin acquirers. In conclusion, the argumentation for sin firms acquiring sin targets is so different from that of non-sin firms acquiring sin targets, that the expectation is that there is significant difference between the two. This expectation is formulated in the third hypothesis and will be tested both on the short-term and long-term.

Hypothesis 3: *The acquirer's cumulative abnormal returns of sin acquisitions differ significantly when the acquirer is also a sin firm, as compared to when the acquirer is a non-sin firm.*

3. Methodological approach and data collection

3.1. Methodology

To examine the value effects of acquiring a sin firm, the stock returns around a sin acquisition will be analyzed. This methodology is adopted in order to be able to see what happens to the value of the acquirer when it decides to acquire a sin firm. To formally test the stock market reaction around the announcement of the acquisition, the method of cumulative abnormal returns (CARs) is used. This method requires a comparison between the actual stock returns around the acquisition and the stock returns that would have been expected in case the acquisition would not have taken place. In order to make such a comparison the expected stock returns need to be constructed based on a counterfactual, while the actual stock market returns can just be empirically observed. The difference between the actual stock return and the expected return is taken as the abnormal return. By accumulating the daily abnormal returns of an acquisition deal in the data sample, the CAR of the deal is determined.

The expected returns $E(R_{i,t})$ of each acquisition deal will be calculated via the market model, which is very similar to the well-known capital asset pricing model (CAPM) and used commonly in the literature (Dodd, 1980; Fuller, Netter & Stegemoller, 2002). The difference is that a constant (alpha) is used as the intercept, instead of the risk-free rate used in CAPM, as depicted in equation 1. The respective stock index that an acquirer is listed in will be used as source for the market returns $(R_{M,t})$. The parameters of the model, alpha and beta, can be estimated by an ordinary least squares (OLS) regression. This OLS regression uses the stock returns as dependent variable and the index returns as independent variable. Thus, it estimates how well the market returns can explain the stock returns that are observed, resulting in a model for each acquisition that contains a coefficient for the intercept and the index return. The estimated intercept coefficient will be used as the alpha, and the estimated index return coefficient will be used as the beta.

$$(1) \qquad E(R_{i,t}) = \alpha_i + \beta_i * R_{M,t}$$

The timeframe over which the estimation is made, is referred to as the estimation window. The estimation window spans from a year before the announcement date of the acquisition to 3 days before the announcement date. A full year generally contains roughly 260 trading days when excluding weekends and public holidays, so the estimation window is denoted as: (-260, -3). This relatively long estimation window (as compared to other studies) is chosen because the effects of the acquisition will be analyzed over a longer term after the announcement date as well. Studies have also shown that when estimation windows consist of at least 100 days, the results are not sensitive to the estimation window length (Armitage, 1995, Park, 2004). Based on the market model that is constructed during the estimation window, it is now possible to determine expected stock returns on a given day after the estimation window. When subtracting the expected stock return on that day from the actual observed stock return, the abnormal return is obtained, as shown by equation 2.

$$(2) \qquad AR_{i,t} = R_{i,t} - E\left(R_{i,t}\right)$$

The abnormal returns will be accumulated over three different timeframes (T), thus forming three different CARs, all captured by equation 3. Firstly, the CARs will be accumulated over a short-term period (hereafter: SCAR) of 5 days symmetrically surrounding the announcement date of the deal, called the event window (-2, 2). According to Oler, Harrison, and Allen (2008), the most commonly used event window consists of 5 days, as used by more than 76% of the papers they reviewed. The event window is included in order to be able to judge whether the findings of Guidi et al. (2020) also hold in the data sample of this research, and will thus be used to test hypothesis 1. What must be noted however, is that Guidi et al. use a 3-day event window. The 5-day window in this thesis is chosen because of its common use and the advantage of better capturing possible information leakage and information processing. As a robustness check, the 3-day event window will be included as well. Secondly, the CARs are examined over a long-term post-acquisition time period (hereafter: PCAR) that expands until six months after the announcement of the deal, which will be called the post-event window (3, 130). The six months have been chosen based on the findings of Chang and Tsai (2013), which show that the CARs of acquisitions of privately held targets become significantly negative (reversal effect) when using a window of six months (or larger). So to be able to capture this reversal effect the six month window is chosen. Thirdly, the CARs are examined over a combined period existing of both the event window and the post-event window (hereafter: CCAR), in order to be able to judge whether the long-term effects in the post-event window offset the short-term effects in the event window. This combined window will thus start two days before the announcement date and end six months after the announcement date (-2, 130), and will be used to test hypothesis 2. The PCAR will not be used to directly test one of the hypotheses, but is used to be able to distinguish the post-event effects separately from the long-term effects of the combined window.

$$(3) \qquad CAR_{i,T} = \sum_{t=t_1}^{T} AR_{i,t}$$

In order to be able to test whether the CARs of sin acquisitions are different than that of non-sin acquisitions, an OLS regression analysis is used on a dataset of both sin and non-sin acquisitions. The data samples will be further explained in the 'Data collection' section. The dependent variable of the

OLS regression will be one of the three CARs, and a sin target dummy will be added as independent variable (which becomes 1 when the target is sin, and 0 otherwise). Additionally, an interaction term between the sin target dummy and a sin acquirer dummy will also be added as independent variable. The interaction term will thus capture the effect of sin firms acquiring sin targets, whereas the sin target dummy will capture the effect of non-sin firms acquiring sin targets. Hence, the coefficient of the interaction term can be used for answering hypothesis 3. The variables will be explained in more detailed in the 'Variables' section below.

3.2. Data

In order to answer the research question, data from both Zephyr and Refinitiv Eikon is used. The data from Zephyr is used to construct the worldwide acquisition deal sample of sin targets, as well as the deal sample of non-sin targets. The data from Refinitiv Eikon is used to acquire insight in the financial performance of the acquirer and the merged firm that are present in the deal samples. The handling and cleaning of the acquired data is done partly in Excel and partly in statistical software program STATA.

As this research looks at the effects of acquiring a sin firm, a distinction must be made whether a deal concerns a sin firm as the target. As stated before, this thesis regards the tobacco, alcohol, weapons and gambling industries as sin industries. To judge whether a firm can be seen as a sin firm, two industry classifications are used, namely the US SIC codes and the NAICS 2017 codes. For both, only the primary industry of the firm is used as identifier. The two classifications each result in a separate dataset of deals, but these will be combined to be able to construct a more extensive dataset that includes all sin acquisitions that comply with the requirements[4]. When deals appear in both classifications, resulting in a double observation in the dataset, one observation will be deleted. Based on the research of Guidi et al. (2020) the following US SIC codes are used: 2080–2085 (Alcohol), 2100–2199 (Tobacco) and 3760–3769, 3795–3795, 3480–3489 (Guns/Defense). A limitation of the US SIC codes is that there are no distinct codes for the gambling industry. The NAICS 2017 classification has a larger number of different industries, which allows for a more detailed selection of sin industries and the inclusion of gambling related industries. Consequently, more codes are included, resulting in the list described in Appendix 1. Concerning the non-sin firms, these

[4] This is in line with the paper of Guidi et al. (2020), who also combine their multiple classifications, and thus include any firm that appears in at least one of the classifications.

firms will thus only be seen as such, when they are not included in both the US SIC and the NAICS 2017 classifications.

Since sufficient stock data is needed in order to analyze merger performance, acquisition deals are only included in the dataset that are confirmed to be completed and have a listed (or delisted) acquirer. The deals must also be specified as acquisitions, and the acquirer's ISIN code should be present. The timeframe used to collect the data is from 2000 to 2020 (both the first of January), in which the deal must be announced. This 20-year timeframe is long enough to construct a sizeable dataset, and also includes multiple economic cycles and merger waves which will make the dataset less sensitive to cyclical movements. The end of the timeframe is chosen so that there is time left to analyze the data of post-merger performance of deals that have been performed close to the end of the timeframe. Additional requirements that are used for deals to be included are based on the restrictions used by Guidi et al. (2020): the deal value needs to be at least 1 million euros (including estimates), and the size of the deal needs to be at least 1% of the acquirer's value (measured in total assets)[5]. These restrictions are used so that the acquisition will actually be large enough to substantially affect the stock returns of the acquirer. Finally, the acquirer needs to hold at least 51% of the target's shares after the deal, and not more than 49% before the deal, so that there is an actual change in ownership. After combining both classification data samples, this ultimately results in 729 unique sin target deals.

What must be noted, is that deals that have a value that amounts to less than 1% of the acquirer's value, cannot be excluded at this stage due to limitations in the Zephyr database. This is also the case for a few extra restrictions that have been used at a later stage when creating the variables in STATA, which will thus decrease the eventual number of deals in the OLS regressions. First of these, is that in order to make the dummy control variables of acquirer country (explained below in the 'Variables' section) not dependent on a too limited number of deals, they must have at least 5 deals. Similarly, the acquirer industry dummy must also contain at least 5 deals. Next to that, deals from tax haven countries are also excluded, as they often concern firms that have complex business structures which are only legally located in the tax haven country. Another excluding restriction is used on deals that result in SCARs or PCARs of exactly 0 (even behind the comma), as this can only be caused by some type of data error. Additionally, restrictions have also been applied to the relative deal size variable (further explained in the 'Variables' section). This includes the minimum of 1% as stated above, but also a maximum of 1000%. This is done because it is assumed to be

[5] Guidi et al. (2020) use another measure for the value of the acquirer, namely the market value of its assets.

unrealistic for 'normal' acquirers to make such big acquisitions, and out of fear that these 'abnormal' deals could have too much effect on the results. Lastly, deals are also excluded that miss more than half of the stock data in their estimation window, meaning that they miss data for at least 129 trading days[6]. This is done to prevent the missings of having a too large effect on the expected returns, which are estimated over the estimation window.

The non-sin target deal dataset (hereafter: non-sin dataset) is constructed based on the same restrictions as the sin target deal dataset (hereafter: sin dataset), except that the deals should not be included in one of the sin firm classifications. In order to make sure the sample size of each of the datasets does not have any influence on the results, the size of the non-sin dataset is chosen so that it is equal to that of the sin dataset. This means that a randomized selection of non-sin deals is made, since the number of non-sin deals completed between 2000 and 2020 is much larger than the number of sin deals. To make the non-sin data sample better workable, a random sample of 1000 deals has been drawn very early in the data handling process. To make sure that the eventual OLS regressions do have exactly similar sized sin and non-sin data samples, a second random sample will be drawn right before the regressions. A more extensive description of how the datasets are acquired and handled is to be found in Appendix 2.

The Refinitiv Eikon database is used to acquire the stock returns and respective index returns of the acquirers involved in the M&A deals that are included in the dataset from Zephyr. This has been done via Excel request tables, and subsequently the sin and non-sin data samples have been combined into one full dataset. The Refinitiv Eikon database is very extensive, yet for several deals the stock and index returns could not be retrieved due to data unavailability. As Zephyr lacks quality data on acquirer total assets, this size measure is also obtained from Refinitiv Eikon.

The full dataset now contains all information needed and is transferred to STATA. Here the variables will be constructed, as described in the next section, but a few additional data changes will be made here as well due to missing data. Firstly, deals are removed when they do not have the required five days of stock data in the event window. This is done because of the short length of the event window, of which the SCARs can be easily affected by one day that misses data. Secondly, as already stated in the previous section, deals that miss more than half of the stock data in their estimation window are removed. Thirdly, deals that miss data for either the acquirer size or the deal size have also been removed, which was the case for 43 deals. Note that the method of dummy variable adjustment could not be performed on these missings because it would create either circular

[6] Corrected for days that also miss index data, so that national holidays are not recorded as missings.

reasoning or extremely high variable correlation[7], which is why the method of listwise deletion has been chosen. The last variable that also contains missing data is the method of payment (MoP) that is used to finance the acquisition. Since a fifth of the deals in the dataset misses data on the MoP, the choice has been made not to remove them as it would substantially decrease the size of the dataset. Alternatively, a dummy variable is added to the regression that captures the effect of the missing MoP data. To make sure that this does not affect the results, a robustness check will also be performed where the deals without MoP data are actually removed. This eventually results in a dataset of 830 deals, of which exactly half regard sin acquisitions and half regard non-sin acquisitions. The next section will discuss the variables of the regression in more detail.

3.3. Variables

As stated in the 'Methodology' section, the main variable of interest will be the sin target dummy, being either sin (1) or non-sin (0). The dependent variable will be one of the three versions of the CARs, thereby enabling the sin target dummy to estimate the difference in value effects between sin and non-sin acquisitions. The additional (control) variables that will be added to the OLS regression are discussed below.

Firstly, the regression will include an interaction effect that allows for testing whether the industry of the acquirer has effect on the CARs of sin target acquisitions. This will be done by splitting the industry of the acquirer into either sin or non-sin, with the use of the sin acquirer dummy (becoming 1 for a sin acquirer and 0 for a non-sin acquirer). This distinction is the same as for the targets, so based on the primary US SIC and NAICS 2017 codes. The sin acquirer dummy is then multiplied with the above-described sin target dummy. As the interaction term is made up of two dummy variables, the interaction term itself will also be a dummy variable. The interaction dummy will become 1 for deals that concern sin firm acquiring sin target, and it will become 0 when either the acquirer or target (or both) is a non-sin firm. A positive coefficient for the interaction dummy will thus mean that the CARs of sin target acquisitions are estimated to be higher when the acquirer is also a sin

[7] The variable Relative size is dependent on both the deal value and the acquirer size, so missings in either one cause the relative size data to be missing as well. Via missing variable adjustment (MVA) the averages of the acquirer size and deal value could be used to replace their own missings, but this would create circular reasoning because in a next step deals are excluded based on their relative size. This exclusion would in turn alter the averages used to replace the missing values, meaning that both steps are interdependent and thus MVA is not useable. Another option would be to replace the missings of Relative size with the average of Relative size itself, and do the same for Acquirer size. This would however cause a correlation of more than 0.95 between the dummy variables that capture the missings of Relative size and Acquirer size, which is why this option has also not been used.

firm. This means that to judge the effect of *non-sin* firms acquiring sin targets on the CARs, the coefficient of the sin target dummy will show this effect. To judge the effect of *sin* firms acquiring sin targets on the CARs, the coefficient of the sin target dummy plus the coefficient of the interaction term will show this effect.

Secondly, three variables are added to the regression that capture the method of payment (MoP) that is used in the acquisition deal. Acquisition financing can generally be divided into three categories: cash payment, shares payment and mixed payment. The first two categories mean that the full acquisition is paid in cash or shares, respectively, while a mixed payment can be any combination of cash, shares and debt. With the inclusion of a cash dummy variable and a shares dummy variable, the three categories are covered as control variables in the regression, leaving the mixed payment as reference category. The effects of the MoP in an acquisition is a much researched topic in the literature, which Sankar and Leepsa (2018) have tried to summarize in their literature review. Multiple studies have shown that the acquirer returns of deals with cash payment are higher than that of deals with stock payment (Travlos, 1987; Ladkani & Banerjee, 2012; Kalinowska & Mielcarz, 2014). Travlov names the theory of the signaling effect as the cause for this difference (1987). This theory states that acquirers are incentivized to use their own shares as payment, when they believe that their shares are overvalued. The model of Shleifer and Vishny confirms this, and they explain that acquirers use this overvaluation to buy less overvalued firms at a discount (2003). This signals to the market that the acquirer might be overvalued (Asquith & Mullins, 1986), making investors want to sell their shares in the acquirer, effectively lowering the share price due to the deal announcement. Contrarily, cash signals better prospects for the acquirer according to the information asymmetry model of Ross (1977). Next to the cash and shares dummy variables, another dummy variable will be added that captures the effect of acquisitions that miss MoP data, as discussed in the previous section.

Thirdly, a variable is added to the regression that captures whether the acquirer already holds a stake (toehold) in the target firm. Empirical research confirms that acquirer returns are higher for firms with a toehold, both on a short time period (Betton, Eckbo & Thorburn, 2008; Hamza, 2011) around the announcement of the deal, and on a long time period after the announcement (Vansteenkiste, 2019). Renneboog and Vansteenkiste state that “Toeholds reduce the target's bargaining power as any increase in the target's share price will also partly accrue to the bidder with a toehold, enabling this bidder to purchase control in the target more cheaply (at a lower premium).” (2019, p. 690). Next to this cost-related hypothesis, the signaling theory is also named in the literature as an explanation for the empirical evidence. Hamza (2011) states that a toehold signals to the market that the acquisition is driven by profit maximization motives, as the acquirer already has an information advantage by being an existing shareholder.

Fourthly, the regression will include the size of the acquirer as a control variable. The size will be measured by the total assets of the latest year preceding the announcement of the acquisition. Studies have shown that the size of the acquirer is of importance to the performance of an acquisition deal. Moeller, Schlingemann and Stulz (2004) find that small acquirers earn an announcement return two percentage points higher than large acquirers, which is not reverted over time. Humphery-Jenner & Powell find a similar relationship between size of the acquirer and acquisition performance (2014). Following the literature, the acquirer size variable will be added to the regression by taking the logarithm of the total assets, as the size effect diminishes in strength when size increases (Moeller et al., 2004).

Fifthly, a variable is added to the regression that controls for differences in relative size of the deal compared to the size of the acquirer. This will be done by dividing the deal value by the total assets (of the latest year preceding the announcement date) of the acquirer (Bao & Edmans, 2011). The study of Fuller et al. (2002) shows that the relative size of a deal does indeed have significant effects on the performance of the deal. They find that for most deals the relative size has a positive effect on the value effects for the acquirer, except for deals with publicly traded targets that are stock financed. Taken into account that the number of stock financed public target deals in the database is very limited (around 3%), and that most literature does not actually make this distinction in the effect (e.g. Guidi et al., 2020; Croci, Petmezas, and Vagenas-Nanos, 2010), this thesis will also not make a distinction for the relative size effect. Thus, the effect will be added to the regression as a continuous variable, with the expectation of having a positive coefficient.

Lastly, in order to make sure that the results are not caused by time-specific, country-specific or industry-specific effects, dummy variables capturing these characteristics will be included in the regression. This will be done by creating dummy variables for the years 2000 to 2019 in which the announcement is made, as well as for the 30 acquirer countries in the dataset, leaving one of the years and countries out of the regression as reference category. Regarding the industry dummies, these will be created based on the first digit of the acquirer primary US SIC code, amounting to a total of 9 industries (including the reference category). As also stated in the previous section, country and industry dummies with less than 5 deals have been removed. This was not needed for year dummies, as they all included more than 5 deals. Fixing the year and industry effects is common practice in studies analyzing acquisition performance (Guidi et al., 2020; Moeller et al., 2004).

4. Results

This chapter contains the results of the main regressions, as well as the results of the robustness checks. The first section regards the descriptive statistics of all variables. Subsequently, the OLS regression assumptions are checked in the second section. After that, the main regression results are discussed in the third section, and lastly the robustness checks are discussed in the fourth section.

4.1. Descriptive statistics

This first result section regards the descriptive statistics of the variables used in the main OLS regressions, which are summarized in Table 2. As discussed in the 'Data' section, acquisitions that miss data for one of the independent variables are removed from the dataset. An exception is made for the acquisitions that have missing data for the method of payment, as these are captured by the Missing MoP dummy. The average short-term cumulative abnormal return (SCAR) is positive, revealing that the initial market reaction to the acquisitions in the dataset is on average positive. Contrarily, the average long-term cumulative abnormal return (CCAR) is negative, which reveals that the positive initial market reaction gets reversed into a negative one, on average. For both the SCAR and the CCAR, especially the maximum is relatively high (118.589 and 568.383, respectively), emphasizing the possible upward potential of an acquisition. The mean of the share ratio shows that 9.3% of the acquisitions is financed with solely shares as payment, while 27.1% is financed with solely cash (Cash ratio), and 20% does not include data on the method of payment (Missing MoP ratio). This means that the remaining 43.6% of the acquisitions is financed by a mixed payment. As seen by the toehold ratio mean, only 4.9% of the acquirors had a stake in the target already before the acquisition. Acquirer size is also summarized in Table 2 for interpretation reasons but note that only Log acquirer size is included in the regressions. The mean of the Acquirer size variable shows that the average total assets of the acquirer are valued at 4.86 billion euros, with a maximum of 856 billion euros. The average relative size of an acquisition as compared to the size of the acquirer, amounts to 35.6%.

Additionally, the distribution of the acquisition deals over the four different categories of acquirer/target and sin/non-sin are shown in Table 3. The number of observations amounts to 830, of which exactly half regard sin targets and half regard non-sin targets. The ratio of deals with sin acquirers and sin targets (0.360) shows that 72% of the sin acquisitions is made by sin acquirers, which means that 28% is made by non-sin acquirers.

Table 2:Descriptive Statistics

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
SCAR	830	1.885	9.700	-40.978	118.589
PCAR	830	-7.799	43.789	-195.768	609.362
CCAR	830	-5.915	44.708	-189.681	568.383
Sin target ratio	830	0.500	0.500	0	1
Sin acquirer ratio	830	0.361	0.481	0	1
Share ratio	830	0.093	0.290	0	1
Cash ratio	830	0.271	0.445	0	1
Missing MoP ratio	830	0.200	0.400	0	1
Toehold ratio	830	0.049	0.217	0	1
Acquirer size (th. Eur)	830	4,865,080	3.29e+07	724	8.56e+08
Log Acquirer size	830	13.081	2.183	6.585	20.568
Relative size	830	0.356	0.812	0.010	7.089

Table 3: Deal categorization ratios

		Target industry	
		Non-sin	Sin
Acquirer industry	Non-sin	0.499	0.140
	Sin	0.001	0.360

4.2. Testing the OLS assumptions

The acquisition deal data that is used for the main regression should be categorized as cross-sectional data, since the CARs of the deals represent a fixed period in time and are not followed over time. For cross-sectional data a few OLS regression assumptions have to be complied with, in order for the regression to be unbiased. These assumptions are discussed below.

4.2.1.Multicollinearity

A Pearson's correlation matrix is included in Table 5 in Appendix 3 to test whether the independent variables of the main regression are heavily correlated with each other. This is important to know as this should not be the case when using an OLS regression, for it causes the assumption of no multicollinearity to be violated and the model to become biased. As seen in the table, none of the independent variables show high correlation with each other, with the correlation between the Cash dummy and the Missing MoP dummy being the highest (-0.305). Correlation coefficients between -0.5 and 0.5 are seen as unproblematic and cause no suspicion of multicollinearity. As seen in the table all independent variables are included in the correlation matrix, except for the interaction term, which cannot be tested in a Pearson's correlation matrix. Interaction terms in general, have high correlation with the two variables for which they show the interaction effect, so including such a term could lead to multicollinearity.

To check whether the interaction term indeed causes multicollinearity, two variance inflation factor (VIF) tests are performed. The first one is performed with the interaction term included and the results are shown by Table 6 in Appendix 3. None of the variables show a problematic VIF value (>5), except for the Sin acquirer dummy and the Interaction term. To make sure the high VIF value of the Sin acquirer dummy is caused by the Interaction term, a second VIF test is performed without the Interaction term. The results of this second test are shown by Table 7 in Appendix 3 and reveal that it is indeed the Interaction term that inflates the variance of the Sin acquirer dummy. As the interaction term forms an essential part of the main regression, it is not possible to consider leaving it out, so no further analysis will be done on the high correlation that it might show.

4.2.2.Heteroscedasticity

Tables 8 to 10 of Appendix 4 show the Breusch-Pagan tests for heteroskedasticity that are performed on the three main regressions (SCAR/PCAR/CCAR). For all three regressions, the p-values are very significant as they amount to 0.0000. As these p-values are smaller than 0.05, the assumption of constant variance of the residuals has to be rejected, meaning that heteroscedasticity is present in the regressions. This can cause the standard errors of the parameters to become biased if not corrected for. The solution used to prevent this, is to use robust standard errors in the regressions.

4.2.3.Normally distributed residuals

Kernel density plots are included in Figures 4 to 6 in Appendix 5. These plots illustrate whether the predicted residuals of the three main regressions are similar to a normal distribution. What can be derived from the figures is that for all three the distribution has a high and sharp central peak, which indicates high kurtosis. However, judging by the plots there are no large departures from normality.

Estimates are also rather robust under departures from normality, so no further action will be taken regarding this.

4.3. Regression results

This third results section regards the main results of the three hypotheses, starting with the first two. The first hypothesis states that the non-sin acquirer's cumulative abnormal returns around the announcement date of sin acquisitions are more negative than of non-sin acquisitions. The second hypothesis states that the non-sin acquirer's cumulative abnormal returns over an extended six-month window, are more positive with sin acquisitions than with non-sin acquisitions. These hypotheses have been studied via the main OLS regressions as described in the previous chapter, of which the results are summarized in Table 4. The results are split up into the three different CARs, each measured over a different timeframe: SCAR (-2, 2), PCAR (3, 130) and CCAR (-2, 130).

The first hypothesis predicts a relatively negative short-term market reaction to a sin acquisition by a non-sin firm. The coefficient of the sin target dummy should be interpreted as the effect of a non-sin firm acquiring a sin target. This means the first hypothesis would translate into a negative coefficient for the sin target dummy on SCAR, which is indeed the case (-0.352). The effect, however, is insignificant. This is different to the results of the study of Guidi et al. (2020), where five out of the six used models showed a significantly negative coefficient for sin targets[8].

The second hypothesis predicts a relatively positive market reaction to a sin acquisition on the long-term, which would translate into a positive coefficient for the sin target dummy on CCAR. This is indeed the case (10.30) as depicted in Table 4, and the coefficient is significant on the 10% level. The significantly positive coefficient is caused by the positive post-event effect (10.66), that outweighs the negative short-term effect. So, the market reaction to sin acquisitions made by non-sin firms seems to show a reversal effect after the initial negative reaction. This is in line with the reversal effect seen in the acquisition of privately owned targets (Chang & Tsai, 2013) and horizontal acquisitions (Oler, Harrison & Allen, 2008). The results are also in line with the expectation based on the horn effect, in which the initial bad impression (in the form of the negative SCAR) gets reversed on the long-term, when the market realizes its initial impression is not based on the full economic story. The EMH however, does not seem to be supported by these results, as it predicts no

[8] Out of the five models that showed significantly negative coefficients for sin targets, two are significant at the 10% level, two at the 5% level and one at the 1% level.

difference between the SCAR and CCAR, because the stock prices have already incorporated the effect of the acquisition in the event window (=SCAR). The second hypothesis can thus be accepted because of the significant sin target dummy coefficient for the CCAR.

Table 4: Main OLS regressions results

VARIABLES	Expected effect	(1) SCAR	(2) PCAR	(3) CCAR
Sin target dummy		-0.352	10.66*	10.30*
		(1.583)	(5.998)	(6.190)
Interaction term (1)		-0.277	-8.759	-9.036
		(1.969)	(10.45)	(10.27)
Share dummy	-	2.060	11.21	13.27
		(2.620)	(10.31)	(10.58)
Cash dummy	+	-0.584	3.921	3.336
		(0.703)	(3.558)	(3.604)
Missing MoP dummy		-0.224	4.223	4.000
		(0.758)	(4.214)	(4.161)
Toehold dummy	+	-0.660	-2.085	-2.745
		(1.140)	(5.158)	(5.373)
Log Acquirer size	+	-0.567***	1.753**	1.185
		(0.192)	(0.891)	(0.897)
Relative size	+	0.166	5.100	5.266
		(0.911)	(6.552)	(6.115)
Sin acquirer dummy		0.427	-1.313	-0.886
		(2.500)	(7.220)	(7.999)
Constant		7.866**	-38.63***	-30.76**
		(3.393)	(13.97)	(14.45)
Observations		830	830	830
R-squared		0.115	0.096	0.105

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

(1): Sin target dummy*Sin acquirer dummy

The third hypothesis predicts that the acquirer's cumulative abnormal returns of sin acquisitions differ significantly when the acquirer is also a sin firm, as compared to when the acquirer is a non-sin firm. As stated, it is possible to distinguish between the effects of a non-sin firm acquiring a sin target and a sin firm acquiring a sin target, via the coefficient of the interaction term. Based on the third hypothesis, the interaction term is expected to have a significant coefficient. As depicted in Table 4, this is not the case for both the short-term (-0.277) and the long-term (-9.036). What can be concluded from the interaction term, however, is that the negative coefficients show that the CARs for sin acquisitions seem to be lower when the acquirer is also a sin firm. Regarding the short-term, this suggests that the argument of increased litigation risk due to a sin acquisition seems to weigh harder than the arguments of a systematically cheaper sin acquisition and a non-diversifying acquisition. Regarding the long-term, this suggests the horn effect does indeed seem to cause a weaker reversing effect over the long-term when the sin acquisitions are made by sin acquirers. The hypothesis, however, cannot be accepted as there is no statistical significance to confirm it.

For the control variables, the expected effects have been indicated in the second column of Table 4, as based on the literature review. Starting from the top, the Share dummy has positive coefficients for both the SCAR and the CCAR, as opposed to the negative expected effect. The Cash dummy, which is based on the same method of payment data, also shows an unexpected negative coefficient for the SCAR. For both dummy variables, the cause for the unexpected coefficients could be the inclusion of the Missing MoP dummy variable. However, this is checked for in a robustness check, which leaves out all acquisitions that miss data on the method of payment. The results of this robustness check are discussed in the next section and are depictured in Table 17 of Appendix 9, which shows that the Shares dummy and Cash dummy coefficients have the same unexpected direction as they do in the main regressions. This listwise deletion of acquisitions that miss data is however based on the assumption that the data misses at complete randomness, while it could be the case that there is some kind of underlying cause for the missing data that affects the results. So there is still a chance that the missing MoP data affects the Cash dummy and Shares dummy coefficients. Yet, as both dummies have coefficients that are insignificant, no further action is taken. The toehold dummy also reveals a negative effect on both SCAR and CCAR, while the opposite was expected. This implies that acquirers that had a stake in the target already before the acquisition, actually enjoy lower CARs on both short-term and long-term. The coefficients however show a very small effect and could also be caused by the small proportion of acquirers with a toehold in the dataset (0.049). Interestingly, the Log acquirer size variable shows a significant negative coefficient for the SCAR and a significant positive coefficient for the PCAR, which results in an insignificant positive CCAR coefficient. This suggests that the size of the acquirer has an initial negative effect on the CARs, but the effect is reversed over the six months after the acquisition. Lastly, the Relative size variable does show an effect that complies with the expectations.

4.4. Robustness checks

In order to make sure that the empirical results actually represent reality, and are not a mere consequence of the choices made in the methodology of this study, multiple robustness checks are performed.

Firstly, robustness checks are performed on the choice of sin industries that are included in the main regression. As discussed in the literature review chapter, the judgement of what to consider a sin industry is quite subjective. To control for this subjectivity, regressions are performed on subsets of data that each exclude one of the four sin categories (alcohol, tobacco, gambling and weapons). The results of these regressions can be found in Tables 11 to 14 of Appendix 6.

Starting from the top, Table 11 shows the regression results when excluding all alcohol related firms, which account for more than half of the sin target data sample used in the main regression. The results however, do not change drastically due to the alcohol exclusion, as the sin target dummy coefficients become -0.377 for the SCAR (instead of -0.352) and 10.31 for the CCAR (instead of 10.30). The standard error of the latter does really change though, making the coefficient insignificant. Regarding the interaction term, the coefficients remain insignificant, yet for the SCAR it does turn from a minor negative coefficient (-0.277) to a positive coefficient (2.548). So, the most notable change is that the reversal effect is not significant anymore when excluding alcohol related targets from the dataset.

Table 12 shows the regression results when excluding all weapons related firms, which account for roughly 10% of the sin target data sample used in the main regressions. Yet, the changes that the exclusion of these firms achieves is noteworthy. The coefficients of the sin target dummy have remained negative for the SCAR (-2.329) and positive for the CCAR (9.782), yet the latter has turned insignificant while the first has turned significant. This still favors the argument of the horn effect (as part of the effect is the initial negative market reaction) but weakens the argument of a reversal effect on the long-term. Regarding the coefficients of the interaction term, these have changed from being negative to being positive, still remaining insignificant. This effect on the interaction term does show that when sin firms acquire weapon targets, the CARs are negatively influenced as compared to non-sin firms acquiring weapon targets.

Table 13 shows the regression results when excluding all tobacco related firms, which account for 7.5% of the sin target data sample used in the main regressions. The exclusion does not cause major changes to the results of the regressions. The only notable change is that for the SCAR the interaction effect changes from a minor negative coefficient (-0.277) to a minor positive coefficient (0.657). The tobacco exclusion causes no alterations to be made to the conclusions of the main regressions.

Table 14 shows the regression results when excluding all gambling related firms, which account for almost 30% of the sin target data sample used in the main regressions. Excluding gambling firms causes all coefficients of the Sin target dummy to become insignificant. This means that without gambling firms there is no statistical evidence anymore for the reversal and horn effect. Next to that, the coefficients for the interaction term have become lower. This would suggest that especially gambling firms cause a potential performance difference between sin acquisitions made by sin firms or by non-sin firms.

Secondly, a robustness check is performed on the choice of window lengths used for the event window. As discussed in the methodology section, the event window of the main regression consists of 5 days (-2,2), but there are also studies that use 3-day event windows (-1,1) (Guidi et al., 2020; Moeller et al., 2004). The rationalization for a shorter event window is that less confounding effects will be picked up that might distort the acquisition effect. This alternative window robustness check will thus include the same estimation (-260,-3) and post-event (3,130) windows, but a shorter event window (-1,1). There will not be robustness checks with alternative estimation or post-event windows, as there are no reasons to do so. Studies have pointed out that regression results are not significantly affected by estimation window length, as long as it exceeds 100 days (Armitage, 1995, Park, 2004). For the post-event window, its length has deliberately been chosen based on the findings of Chang and Tsai (2013) and for the purpose of capturing long-term effects. In the literature there has also not been found any reason that would justify using a different post-event window length. The results of this robustness check are depicted in Table 16 of Appendix 8. The most notable difference is that the coefficient of the sin target dummy on the CCAR is not significant anymore. This weakens the support for the horn effect and the reversal effect between the short-term and long-term market reaction to a sin acquisition made by a non-sin firm. Based on this alternative event window the second hypothesis would actually be rejected. The coefficients of the interaction term remain insignificant but do become positive instead of negative.

Thirdly, a robustness check is performed on the choice for the limit value applied to the allowed missings of stock returns in the estimation window. As discussed, the maximum number of missing stock returns to still include deals in the main regressions, is 129. Which is exactly half of the length of the estimation window (-260,-3). The robustness check tests what effect a stricter limit on the missings will do to the results, by using a maximum number of missings of 64 (a quarter of the estimation window). The reasoning behind this is that missing stock returns can be caused by so-called thin trading, meaning that a certain stock does not get traded for an extended time period. Thin trading can result in CAR calculations showing an abnormal return for a certain day, while the stock has actually not even been traded that day. Along these lines, Tuch and O'Sullivan (2007) argue that thin trading can undermine the reliability of long-run event studies. The results of this additional regression can be found in Table 17 of appendix 9, and are run with 718 deals instead of the 830 deals in the main regressions. Most notably, the coefficient of the sin target dummy for CCAR has become insignificant. This weakens the evidence for the horn effect and the reversal effect, and based on this robustness check, the second hypothesis would actually be rejected. The coefficients of the interaction term remain insignificant but do become positive instead of negative.

Fourthly, a robustness check is performed that does not include the dummy variable that captures missing data on the method of payment of an acquisition (Missing MoP dummy). Instead, the acquisitions that lack this data are excluded from the dataset, and the regressions are ran without them. This has been done because the coefficients of the Missing MoP dummy in the main regressions are reasonably high for the PCAR and CCAR. One would actually expect the coefficients to be very small, since the missing values could be from all three different MoP categories (shares, cash and debt), meaning that the net effect would be near zero. As this is not the case for the PCAR and the CCAR, this additional robustness check is performed without the deals that miss MoP data. Table 14 of Appendix 9 shows the regression results for the robustness check. What can be derived from these results is that the sin target dummy coefficients change barely, but the standard errors change more profoundly. This causes the sin target dummy coefficient for the CCAR to become insignificant. This weakens the evidence for the horn effect and the reversal effect, and based on this robustness check, the second hypothesis would actually be rejected. The coefficients of the interaction term remain insignificant just as in the main regressions.

Lastly, a robustness check is performed that uses Winsorizing to control the influence that CAR outliers have on the results. Winsorizing is a procedure that replaces the values at the tails of the distribution of a variable with less extreme values, so the results are not affected as much by these extreme values (outliers). The procedure thus creates more robust estimators of variability and location (Blaine, 2018). For this study, cutoffs of 5% and 1% are used. For the 5% cutoff this means that 5% of the values at the bottom and the top of the three CAR distributions have been replaced with the values at the 5th and the 95th percentiles, respectively. For the 1% cutoff they are replaced with the values at the 1st and the 99th percentiles. The results of the regressions that are run using this Winsorized dataset, are shown in Table 18 of Appendix 10. Most notably, the significance of the Sin target dummy coefficient for the CCAR disappears, for both 5% and 1% Winsorized regressions. This suggests that the extreme values at the tails of the CCAR distribution have a large effect on the sin target dummy, and that they are partly responsible for its significant effect in the main long-term regression. The coefficient of the Interaction term remains insignificant for all 6 regressions of this robustness check, but notably some of the coefficients are now positive.

5. Discussion and conclusion

While the literature on good CSR behavior seems to be ever-growing in the financial economic domain, looking at the opposite side could prove just as insightful. This study analyses the value effects of acquisitions that involve targets which are bad CSR investments by nature, due to the industry that they operate in. Such ‘sin firms’ in the alcohol, tobacco, weapons and gambling industries have recently been the center of discussion on the question whether or not they could provide investors abnormal returns on their stock portfolios. These possible abnormal returns are however much less researched when they regard the acquisitions of sin firms. Long-term effects of these sin acquisitions have not yet been analyzed in the literature, and knowledge on the short-term effects also proves to be very limited. Analyses of the sin acquisition performance within these two timeframes are performed by testing the first two hypotheses of this study. Additionally, the expectation that the performance differs between sin and non-sin acquirers, is tested via the third hypothesis. Via these hypotheses, this study tries to broaden the literature on the market perception of sinful investment choices.

5.1. Discussion and implications

The results of the short-term cumulative abnormal returns show that the initial negative market reaction to sin acquisitions for non-sin acquirers, as found by Guidi et al. (2020), is not found to be significant in the dataset of this study. This can be caused by the several differences in the methodology, of which the most notable is the use of the Mahalanobis matching method by Guidi et al. (2020). Of the eight robustness checks in this study, only the regression that excludes weapons firms from the sin data sample shows a significant negative short-term market reaction to sin acquisitions. Yet, the direction of the coefficient is consistent over all (robustness) regressions, suggesting that there is some negative market perception of sin acquisitions. Based on this negative coefficient, the downward effects of increased litigation risk and deteriorating stakeholder relationships seem to weigh hardest for sin acquisitions. Overall, the evidence in favor of the findings of Guidi et al. (2020) is not convincing enough, so the first hypothesis of this study needs to be rejected.

The results of the second hypothesis do actually show a significant effect of sin acquisitions for non-sin acquirers, namely a positive one. This is in line with the expectations based on the empirically discovered reversal effect (Chang & Tsai, 2013; Oler, Harrison & Allen, 2008), which makes short-term market reactions to complex information reverse over the long-term. The result also seems to comply with the horn effect, which is a cognitive bias that makes people (investors) evaluate individual aspects of something (sin acquisitions) more negatively, based on an overall negative impression (Thorndike, 1920). This very applicable to sin firms, meaning that the initial market reaction is expected to be negative and subsequently reverse as investors realize the sin aspect is not the only aspect to judge an acquisition on. The evidence to accept the second hypothesis is only partly convincing though, as seven out of the eight robustness checks do not find the same significant negative effect as the main regression. Consistency is however found in the direction of effect, as for all (robustness) regressions the coefficient is positive, often with quite a margin.

The results of the third hypothesis reveal that no significant difference has been discovered between the acquirer's cumulative abnormal returns of sin and non-sin acquirers, when they acquire a sin target. The negative interaction term coefficients however do suggest that sin acquirers enjoy a more negative market reaction to acquiring a sin firm, than non-sin acquirers. Thus, the effect of increased litigation risk due to a sin acquisition seems strong enough to weigh down upward effects on the short-term. These short-term upward effects for sin acquisitions made by sin firms regard the effect of a systematically cheaper sin acquisition and the advantage of a non-diversifying acquisition. For the long-term, it seems that the horn effect causes a weaker reversing effect when the sin acquisitions are made by sin acquirers in comparison to non-sin acquirers. Yet, the robustness checks show that the relatively negative market reaction for sin acquirers is inconsistent over different methodological choices. For several of the robustness checks the coefficients turn out to be positive for sin acquirers, especially on the long-term. The coefficients also do not become significant in any of the (robustness) regressions, so the third hypothesis must be rejected.

Overall, the findings of this study do suggest that there is some form of a reversal effect between the short-term and the long-term performance of sin acquisitions made by non-sin firms. The short-term negative value effects however do not prove significant, while the long-term positive value effects are significant on a 10% level. Both short-term and long-term effects are consistent in their direction, but do not show consistent significance. Thus, the results suggest that the market shows some forgiveness for sinning acquirers, although it takes some time. Regarding a potentially existing difference in the market reaction to sin acquisitions made by sin acquirers versus non-sin acquirers, there seems to be little evidence pointing towards this. The difference is persistently insignificant and shows both negative and positive coefficients across the (robustness) regressions, so based on this study the conclusion can be made that there is no such difference.

5.2. Limitations and recommendations

Although much effort is put into making this study as complete and scientifically correct as possible, there are certain limitations due to the methodological choices made and due to time and data constraints. Firstly, the matching principle that is used to compare sin acquisitions and non-sin acquisitions is the rather simple combining of a randomly sampled non-sin dataset to the sin dataset, based on sample size. There are more sophisticated matching principles, such as the Mahalanobis matching method as used by Guidi et al. (2020), which allows to match the datasets on more characteristics than just the sample size. This would reduce potential selection bias in the dataset, but due to time constraints this method has not been used. Secondly, there are some problems related to performing event studies over a long timeframe. Tuch and O'Sullivan (2007) argue that the reliability of long-term event studies may be undermined by the overlapping of event windows. As the dataset of sin acquisitions contains numerous serial-acquirers, this overlapping is indeed present. The overlapping can cause the long-term CARs of a certain acquisition to be affected by a subsequent acquisition that is performed within six months after the first acquisition. Another long-term event study problem is that the results actually do not only test whether CARs are zero, but also whether the market model is correct. If not completely correct, null hypotheses could be rejected because of biased benchmarks instead of actual abnormal returns (Ang & Zhang, 2011). Thirdly, as already discussed in the 'Results' chapter, part of the data on method of payment (MoP) is missing. This is a consequence of data unavailability within the used databanks, Zephyr and Refinitiv Eikon. Although the regressions have been performed by both including the deals that lack MoP data (via a dummy variable) and excluding the deals (via listwise deletion), it remains unclear whether the missings are completely at random (MCAR). If not, there could be an unknown factor, not corrected for in the model, that causes the missings. This could result in correlation between the missing values and other independent variables, making the estimates biased. As the results of this study show indication of a reversal effect between the short-term and long-term performance of sin acquisitions made by non-sin acquirers, I would recommend further research on this potential effect. Using a more sophisticated matching principle and a more complete dataset, two of the limitations of this study can be overcome, allowing for more conclusive findings. Nonetheless, the results of this study provide helpful insights into the value effects of sin acquisitions, and the forces behind these effects. This can be used both for direction of further research, as well as for practical knowledge to acquirers that consider a sin acquisition.

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7. Appendices

Appendix 1

NAICS 2017 codes classification

- 11191. Tobacco Farming
- 31212. Breweries
- 31213. Wineries
- 31214. Distilleries
- 3122. Tobacco Manufacturing
- 32592. Explosives Manufacturing
- 336414. Guided Missile and Space Vehicle Manufacturing
- 336415. Guided Missile and Space Vehicle Propulsion Unit and Propulsion Unit Parts Manufacturing
- 336419. Other Guided Missile and Space Vehicle Parts and Auxiliary Equipment Manufacturing
- 336992. Military Armored Vehicle, Tank, and Tank Component Manufacturing
- 4248. Beer, Wine, and Distilled Alcoholic Beverage Merchant Wholesalers
- 42494. Tobacco and Tobacco Product Merchant Wholesalers
- 4453. Beer, Wine, and Liquor Stores
- 453991. Tobacco Stores
- 7132. Gambling Industries
- 71321. Casinos (except Casino Hotels)
- 72112. Casino Hotels
- 7224. Drinking Places (Alcoholic Beverages)

Appendix 2

Extensive description of acquiring and handling the dataset(s).

1. Acquiring the sin dataset from Zephyr

Firstly, the dataset including sin target deals (hereafter: sin dataset) has been acquired via Zephyr. Practically, this dataset is chosen to be acquired first, as it will have much less deals than the dataset including non-sin target deals (hereafter: non-sin dataset) and will thus indicate the size both datasets need to have. As stated, two industry classifications have been used to judge whether a target can be seen as a sin firm, namely US SIC and NAICS 2017. For both only the primary codes of the firm have been used. Next to the industry classifications, the following six restrictions have been used in the Zephyr search strategy:

1. Acquirer must be listed or delisted.
2. The percentage of the initial stake has a maximum of 49% and the percentage of the final stake has a minimum of 51% (including unknown minority and majority respectively).
3. Current deal status must be confirmed or completed.
4. The deal must be announced between the first of January 2000 and the first of January 2020.
5. The deal value must be at least 1 million euros, including estimates.
6. The deal type must be an acquisition.

Figure 1: Sin dataset Zephyr restrictions (US SIC)

SEARCH STRATEGY	Step result	Search result
1. Listed/Unlisted/Delisted companies: listed acquiror, delisted acquiror	527,327	527,327
2. US SIC (primary codes): 2082 - Malt beverages, 2083 - Malt, 2084 - Wines, brandy and brandy spirits, 2085 - Distilled and blended liquors, 21 - Tobacco products, ... (Target)	15,019	4,393
3. Time period: on and after 01/01/2000 and up to and including 01/01/2020 (announced)	1,806,563	3,650
4. Current deal status: Completed - confirmed	1,683,309	3,063
5. Deal value (m EUR): min=1 (including estimates)	1,169,500	2,474
6. Percentage of stake: Percentage of initial stake (max: 49 % including unknown minority); Percentage of final stake (min: 51 % including unknown majority)	726,719	352
7. Deal type: Acquisition	825,999	324
Boolean search: 1 And 2 And 3 And 4 And 5 And 6 And 7		TOTAL : 324

Figure 2: Sin dataset Zephyr restrictions (NAICS 2017)

SEARCH STRATEGY	Step result	Search result
1. Listed/Unlisted/Delisted companies: listed acquiror, delisted acquiror	527,422	527,422
2. Time period: on and after 01/01/2000 and up to and including 01/01/2020 (announced)	1,806,583	407,082
3. Current deal status: Completed - confirmed	1,683,653	289,270
4. Deal value (m EUR): min=1 (including estimates)	1,169,757	174,395
5. NAICS 2017 (primary codes): 11191 - Tobacco Farming, 31212 - Breweries, 31213 - Wineries, 31214 - Distilleries, 3122 - Tobacco Manufacturing, ... (Target)	25,264	3,919
6. Percentage of stake: Percentage of initial stake (max: 49 % including unknown minority); Percentage of final stake (min: 51 % including unknown majority)	726,867	746
7. Deal type: Acquisition	826,169	699
Boolean search: 1 And 2 And 3 And 4 And 5 And 6 And 7		TOTAL : 699

For the US SIC classifications this resulted in 324 deals and for the NAICS 2017 classification this resulted in 699 deals. As many of the deals appear in both classifications, the double entries are removed resulting in a dataset of 735 deals. The variables that are selected to be exported as well from Zephyr are listed below:

1. Deal Number
2. Acquiror name
3. Acquiror country
4. Acquiror country code
5. Target name
6. Target country
7. Target country code
8. Deal type
9. Deal value in thousands of Euros
10. Target ISIN number
11. Acquiror ISIN number
12. Announced date
13. Completed date
14. Deal method of payment
15. Deal method of payment value in thousands of Euros
16. Deal financing
17. Target primary US SIC code
18. Acquiror primary US SIC code
19. Target primary NAICS 2017 code
20. Acquiror primary NAICS 2017 code
21. Initial stake in percentages
22. Final stake in percentages
23. Target total assets
24. Acquirer total assets
25. Target net assets
26. Acquirer net assets
27. Target market capitalization
28. Acquirer market capitalization

    (The last six are all from the most recent year before the deal announcement and in thousands of Euros)

The 735 deals have subsequently been exported to Excel and cleaned further by removing the deals without ISIN number of the acquirer (729 remain).

2. Acquiring the non-sin dataset from Zephyr

The restrictions used in Zephyr for the non-sin dataset are very similar to that of the sin dataset, meaning that the six restrictions listed above have also been used. The only difference is that the restrictions of the US SIC and the NAICS 2017 codes have now been added as exclusion, so that no sin target will be included in the non-sin dataset.

Figure 3: Non-sin dataset Zephyr restrictions

SEARCH STRATEGY	Add a search step · Alert me · Save · Clear all steps
1. US SIC (primary codes): 2082 - Malt beverages, 2083 - Malt, 2084 - Wines, brandy and brandy spirits, 2085 - Distilled and blended liquors, 21 - Tobacco products, ... (Target)	15,019
2. Listed/Unlisted/Delisted companies: listed acquiror, delisted acquiror	527,439
3. Percentage of stake: Percentage of initial stake (max: 49 % including unknown minority); Percentage of final stake (min: 51 % including unknown majority)	726,882
4. Time period: on and after 01/01/2000 and up to and including 01/01/2020 (announced)	1,806,586
5. Current deal status: Completed - confirmed	1,683,719
6. Deal value (m EUR): min=1 (including estimates)	1,169,838
7. NAICS 2017 (primary codes): 11191 - Tobacco Farming, 31212 - Breweries, 31213 - Wineries, 31214 - Distilleries, 3122 - Tobacco Manufacturing, ... (Target)	25,264
8. Deal type: Acquisition	826,192
Boolean search: Not 1 And 2 And 3 And 4 And 5 And 6 And Not 7 And 8 · Refresh	TOTAL : 57,173

This results in a dataset of 57.173 deals. (To check whether the sin target deals have actually been removed, the search has also been performed without any US SIC and NAICS 2017 codes, resulting in exactly 735 more deals.) As the number of deals Zephyr is able to export is limited, the deals have been sorted on deal number and subsequently the first 30.000 deals have been exported to Excel. After that the deals from 30.001 to 57.173 have been exported, and later have been added to the first export. The same 28 variables have been selected in Zephyr as for the sin dataset as listed above. Also similarly to the sin dataset, deals have been removed when no ISIN number of the acquirer is available.

3. Preparing datasets for Eikon

As the non-sin dataset is a lot larger than the sin dataset, a sample of 1000 randomly chosen non-sin deals has been made in Stata to make the dataset better workable. The number is chosen so that there will be at least as much deals left as in the sin dataset, even after all missing and invalid deals have been removed. On this new non-sin dataset of 1000 deals, that works a lot faster in Excel, the following deals have been removed:

- Deals that have a deal type of one the following descriptions:
  - Acquisition unknown minority stake %
  - Minority stake increased from % to <50%
  - Minority stake %
  - Joint venture
  - Institutional buy-out
  - Acquisition unknown stake
  - Acquisition increased from >50% to %
- Deals that have more than 49% as initial stake.

This leads to a non-sin dataset of 916 remaining deals. Before this non-sin dataset can be combined with the sin dataset, a sin target dummy is created to keep the datasets distinctive. All sin deals get a dummy value of 1 and all non-sin deals a value of 0. The full dataset is now made up of 1645 deals.

In order to combine the data of Zephyr with that of Eikon, a few steps have to be taken first. Starting with the creation of two new columns, containing the start of the estimation window and the end of the event window. This is done by subtracting 260 workdays (1 year) from the announcement date for the start of the estimation window, and adding 130 workdays (half a year) to the announcement date for the end of the event window. Secondly, a column is added containing the number of trading days between the start of the estimation window and the end of the event window. Thirdly, a column is added containing the cumulative trading days, needed for the next step. Fourthly, the ISIN codes of the acquirers are assigned unique numbers (ID's), as Stata can't handle ISIN codes. As there are acquirers in the dataset that have engaged in multiple deals, the total number of unique ID's is 1237.

4. Retrieving stock & index returns from Eikon

For this part a template from the Radboud NSM Library Team is used, which retrieves the stock and index returns in a format Stata can process easily. This template is a request table, a special Eikon Excel macro to help retrieve data efficiently. The meaning of the various columns of the request table:

B (update): Y (= yes, retrieve data for this line) or N (= no, do not retrieve date for this line)

C: type of request (S = static, TS = time series); here you need TS

D (select format): do you want column headers (C), row headers (R) etc. Here you only need row

headers (R) to get you the daily dates for the stock & index returns

E (series lookup): the ISINs of the firm you want to retrieve data for

F (datatype/expressions): the variables, where PCH#(X(P),1D) = the daily stock return based on the adjusted stock price (P) and PCH#(X(LI),1D) = the daily return of the index the firm is listed (LI('local market index') is the variable Eikon uses to map the ISIN-code to the corresponding index).

G: start dates

H: end dates

I: frequency; here you'll need daily

K: the destination of the data ('='stock & index returns'!C1 = the data of the first firm will load starting cell C1 of the tab stock & index returns. This destination needs to be altered for every line.

When using the cumulative trading days, the retrieved Eikon data will be lined up below each other. This means each line contains the acquirer ISIN code, the date, the stock return and the index return of a day in the estimation or event window of a deal. As Stata can't process the ISIN codes, the previously created acquirer ID's will replace the ISIN codes. These ID's are combined with the announcement date of a deal, to be able to distinguish between multiple deals of the same acquirer.

The data from Eikon is very extensive, however not 100% complete. This causes some missing deal data, which will be excluded during further steps in STATA. This means that still the full dataset of 1645 deals are uploaded to STATA.

5. Calculate the abnormal returns in STATA

Below are the STATA commands used for the calculation of the CARs for the sin dataset. Note that there are also two parts that only apply to one of the robustness checks but not for the main regressions. These parts are marked between asterisks with 'Robustness check' above them.

```
clear all
set more off
```

```
cd"C:\Users\sil\Documents\Uni\jaar 5\Master Thesis\STATA"
use 20_Deal_dates, clear

bys ID: gen sub_id = _n
bys ID: egen max_number_events = max(sub_id)
order sub_id max_number_events
save 21_deal_dates_temp, replace

duplicates drop ID, force
keep ID max_number_events

merge 1:m ID using 20_Returns
keep if _merge==3
drop _merge

expand max_number_events
bys ID Date: gen sub_id = _n
sort ID sub_id Date
order sub_id
merge m:1 ID sub_id using 21_deal_dates_temp
keep if _merge==3
drop _merge max_number_events
egen id = group(ID sub_id)
order id
drop sub_id

gen dummy_announce_before_date = (Announced_date<Date)

bys id: g j = _n if Date < Announced_date
bys id: egen N = max(j)
replace j = j - N - 1
bys id: replace j = _n if Date > Announced_date
replace j = j - N - 1 if Date > Announced_date
replace j = 0 if Date == Announced_date
order id- Announced_date j dummy_announce_before_date

*******************Regular Windows*******************

gen estimationwindow= inrange(j,-260,-3)
gen eventwindow= inrange(j,-2,2)
gen posteventwindow= inrange(j, 3, 130)

gen estimationwindow_day = j if estimationwindow
gen event_window_day = j if eventwindow==1
```

```
gen posteventwindow_day = j if posteventwindow==1

bys id: gen subid = _n
order id subid
bys id: egen count_event_days = count(event_window_day)
bys id: gen max_days_in_window = count_event_days if subid==1
tab max_days_in_window
qui su count_event_days
keep if count_event_days==r(max)

***** Remove estimation windows missing more than half of stock data
bys id: egen missings_estimation_window = count(estimationwindow_day) if Stock_return==0 &
Index_return!=0
ssc install fillmissing, replace
bys id: fillmissing missings_estimation_window, with(any)
replace missings_estimation_window=0 if missings_estimation_window==.
drop if missings_estimation_window>=130

*******************Robustness check********************
*******************Less estimation window missings******
drop if missings_estimation_window>=65
*******************************************************


egen id2 = group(id)
drop id
rename id2 id

***** CARs

gen SAR=.
gen PAR=.
gen alphas=.
gen betas=.

su id
local maxid = r(max)

*****For short (=eventwindow)
forval i=1/`maxid' {
di "Regression " `i' " of " `maxid' " ... To stop click Break button on top of this window"
			cap qui reg Stock_return Index_return if estimationwindow==1 & id==`i'
cap replace SAR = Stock_return - _b[_cons] - _b[Index_return]* Index_return if eventwindow==1 & id==`i'

			cap replace alphas = _b[_cons] if estimationwindow==1 & id==`i'
			cap replace betas = _b[Index_return] if estimationwindow==1 & id==`i'
```

```
}

                        bys id: egen SCAR=sum(SAR) if eventwindow==1

*****For post (=posteventwindow)
forval i=1/`maxid' {
di "Regression " `i' " of " `maxid' " ... To stop click Break button on top of this window"
                        cap qui reg Stock_return Index_return if estimationwindow==1 & id==`i'
cap replace PAR = Stock_return - _b[_cons] - _b[Index_return]* Index_return if posteventwindow==1 & id==`i'

                        cap replace alphas = _b[_cons] if estimationwindow==1 & id==`i'
                        cap replace betas = _b[Index_return] if estimationwindow==1 & id==`i'
}

                        bys id: egen PCAR=sum(PAR) if posteventwindow==1

*****For long (=eventwindow + posteventwindow)
bys id: replace SCAR=SCAR[_n-1] if j==3
bys id: gen LCAR=SCAR+PCAR if j==3



****** Make every deal 1 line
keep if j==3

****** Cleaning up
drop if SCAR==0
drop if PCAR==0

********************Robustness check********************
********************Less estimation window missings*****
save "C:\Users\sil\Documents\Uni\jaar 5\Master Thesis\STATA\33_Estimation_missings_CARs.dta"
export excel ID Announced_date SCAR PCAR LCAR using "C:\Users\sil\Documents\Uni\jaar 5\Master Thesis\CARs export 3.3 Alt Estimation missings.xlsx", firstrow(variables)
*******************************************************


********************Robustness check********************
********************Alternative windows*****************

gen estimationwindow= inrange(j,-260,-3)
gen Alteventwindow= inrange(j,-1,1)
gen posteventwindow= inrange(j, 3, 130)

gen estimationwindow_day = j if estimationwindow
gen Altevent_window_day = j if Alteventwindow==1
gen posteventwindow_day = j if posteventwindow==1
```

```
bys id: gen subid = _n
order id subid
bys id: egen count_event_days = count(Altevent_window_day)
bys id: gen max_days_in_window = count_event_days if subid==1
tab max_days_in_window
qui su count_event_days
keep if count_event_days==r(max)

***** Remove estimation windows missing more than half of stock data
bys id: egen missings_estimation_window = count(estimationwindow_day) if Stock_return==0 &
Index_return!=0
ssc install fillmissing, replace
bys id: fillmissing missings_estimation_window, with(any)
replace missings_estimation_window=0 if missings_estimation_window==.
drop if missings_estimation_window>=130

egen id2 = group(id)
drop id
rename id2 id

***** CARs

gen SAR=.
gen PAR=.
gen alphas=.
gen betas=.

su id
local maxid = r(max)

*****For short (=Alteventwindow)
forval i=1/`maxid' {
di "Regression " `i' " of " `maxid' " ... To stop click Break button on top of this window"
            cap qui reg Stock_return Index_return if estimationwindow==1 & id==`i'
cap replace SAR = Stock_return - _b[_cons] - _b[Index_return]* Index_return if Alteventwindow==1 & id==`i'

            cap replace alphas = _b[_cons] if estimationwindow==1 & id==`i'
            cap replace betas = _b[Index_return] if estimationwindow==1 & id==`i'
}

            bys id: egen SCAR=sum(SAR) if Alteventwindow==1

*****For post (posteventwindow)
forval i=1/`maxid' {
```

```
di "Regression " `i' " of " `maxid' " ... To stop click Break button on top of this window"
            cap qui reg Stock_return Index_return if estimationwindow==1 & id==`i'
cap replace PAR = Stock_return - _b[_cons] - _b[Index_return]* Index_return if posteventwindow==1 & id==`i'

            cap replace alphas = _b[_cons] if estimationwindow==1 & id==`i'
            cap replace betas = _b[Index_return] if estimationwindow==1 & id==`i'
}

            bys id: egen PCAR=sum(PAR) if posteventwindow==1

*****For long (=Alteventwindow + posteventwindow)
bys id: replace SCAR=SCAR[_n-2] if j==3
bys id: gen LCAR=SCAR+PCAR if j==3

save "C:\Users\sil\Documents\Uni\jaar 5\Master Thesis\STATA\37 alt window CARs.dta"

****** Make every deal 1 line
keep if j==3

****** Cleaning up
drop if SCAR==0
drop if PCAR==0

export excel ID Announced_date SCAR PCAR LCAR using "C:\Users\sil\Documents\Uni\jaar 5\Master Thesis\CARs export 3.7 Alt window.xlsx", firstrow(variables)
*********************************************
```

Ultimately Stata was able to calculate the CARs for 1251 deals. The CARs data is exported to the Excel file that contains all data from Zephyr, where they are combined. Then they are transferred back again to STATA to run the regressions.

6. Running the regressions in STATA

The below STATA commands are used to create the variables and run the regressions. Again, some of the commands only apply to robustness checks, which are marked between asterisks with 'Robustness check' above them.

```
clear all

import excel "C:\Users\sil\Documents\Uni\jaar 5\Master Thesis\Oude versies\Fully combined 3.0.xlsx", sheet("1251 master dataset") firstrow

**********For robustness check alternative window***********
import excel "C:\Users\sil\Documents\Uni\jaar 5\Master Thesis\Fully combined 3.7 Alt window.xlsx", sheet("1251 alt window master data") firstrow
```

```
***********************************************************

**********For robustness check Alt estimation missings*******
import excel "C:\Users\sil\Documents\Uni\jaar 5\Master Thesis\Oude versies\Fully combined 3.3 Alt
Estimation missings.xlsx", sheet("1093 master alt est missings") firstrow
***********************************************************

rename LCAR CCAR

***** MoP dummies
gen sharedummy = 0
replace sharedummy = 1 if Dealmethodofpayment=="Shares"
gen cashdummy = 0
replace cashdummy = 1 if inlist(Dealmethodofpayment, "Cash", "Cash assumed")
gen misMoPdummy = 0
replace misMoPdummy= 1 if missing(Dealmethodofpayment)

***** Toehold dummy
destring Initialstake, replace force
gen toeholddummy=1 if Initialstake>0
replace toeholddummy=0 if toeholddummy==.

***** Acquirer size
rename AcquirerTotalAssetsEikonLast Asize
destring Asize, replace force
gen logAsize = log(Asize)
drop if logAsize==.

***** Relative size
destring Correctdealvalue, replace force
gen Relativesize = Correctdealvalue/Asize
***** Drop very high & low relative sizes
drop if Relativesize>10
drop if Relativesize<0.01
drop if Relativesize==.

***** Rename some variables
rename AcquirorprimaryUSSICcode ASIC
rename TargetprimaryUSSICcode TSIC
rename AcquirorprimaryNAICS2017code ANAICS
rename TargetprimaryNAICS2017code TNAICS

***** Industry dummy
gen ASIC2 = substr(ASIC,1,1)
```

```
tab ASIC2
destring ASIC2, replace force
drop if ASIC2==9
tab ASIC2, generate(industrydummy)

***** Sin acquirer dummy
destring ASIC, replace force
drop if ASIC==.
destring ANAICS, replace force
drop if ANAICS==.
gen Sinacquirerdummy=0
*For SIC
replace Sinacquirerdummy=1 if inlist(ASIC, 2080, 2081, 2082, 2083, 2084, 2085)
replace Sinacquirerdummy=1 if ASIC>2099 & ASIC<2200
replace Sinacquirerdummy=1 if ASIC>3759 & ASIC<3770
replace Sinacquirerdummy=1 if ASIC==3795
replace Sinacquirerdummy=1 if ASIC>3479 & ASIC<3490
*For NAICS
replace Sinacquirerdummy=1 if ANAICS>312119 & ANAICS<312150
replace Sinacquirerdummy=1 if ANAICS>312199 & ANAICS<312300
replace Sinacquirerdummy=1 if inlist(ANAICS, 111910, 325920, 336414, 336415, 336419, 336992, 424940,
453991, 721120)
replace Sinacquirerdummy=1 if ANAICS>424799 & ANAICS<424900
replace Sinacquirerdummy=1 if ANAICS>445299 & ANAICS<445400
replace Sinacquirerdummy=1 if ANAICS>713199 & ANAICS<713300
replace Sinacquirerdummy=1 if ANAICS>722399 & ANAICS<722500

***** Year dummy
gen Year=year(Announceddate)
tab Year, generate(yeardummy)

***** Drop 'weird' countries: 1116 remain of 1251
bysort Acquirorcountry: drop if _N<5
drop if inlist(Acquirorcountry, "Cayman Islands", "Bermuda")
***** Country dummy
tab Acquirorcountry, generate(Acountrydummy)

tab Sintargetdummy
sample 415 if Sintargetdummy==0, count

save "C:\Users\sil\Documents\Uni\jaar 5\Master Thesis\STATA\Data for main regressions 3.7.dta"

save "C:\Users\sil\Documents\Uni\jaar 5\Master Thesis\STATA\Data for Alt window regressions 3.7.dta"
```

```
*******************Main Regressions**************

regr SCAR Sintargetdummy sharedummy cashdummy misMoPdummy toeholddummy logAsize Relativesize Sintargetdummy##Sinacquirerdummy yeardummy1 yeardummy2 yeardummy3 yeardummy4 yeardummy5 yeardummy6 yeardummy7 yeardummy8 yeardummy9 yeardummy10 yeardummy11 yeardummy12 yeardummy13 yeardummy14 yeardummy15 yeardummy16 yeardummy17 yeardummy18 yeardummy19 yeardummy20 Acountrydummy1 Acountrydummy2 Acountrydummy3 Acountrydummy4 Acountrydummy5 Acountrydummy6 Acountrydummy7 Acountrydummy8 Acountrydummy9 Acountrydummy10 Acountrydummy11 Acountrydummy12 Acountrydummy13 Acountrydummy14 Acountrydummy15 Acountrydummy16 Acountrydummy17 Acountrydummy18 Acountrydummy19 Acountrydummy20 Acountrydummy21 Acountrydummy22 Acountrydummy23 Acountrydummy24 Acountrydummy25 Acountrydummy26 Acountrydummy27 Acountrydummy28 Acountrydummy29 Acountrydummy30 industrydummy1 industrydummy2 industrydummy3 industrydummy4 industrydummy5 industrydummy6 industrydummy7 industrydummy8 industrydummy9, vce(robust)

regr PCAR Sintargetdummy sharedummy cashdummy misMoPdummy toeholddummy logAsize Relativesize Sintargetdummy##Sinacquirerdummy yeardummy1 yeardummy2 yeardummy3 yeardummy4 yeardummy5 yeardummy6 yeardummy7 yeardummy8 yeardummy9 yeardummy10 yeardummy11 yeardummy12 yeardummy13 yeardummy14 yeardummy15 yeardummy16 yeardummy17 yeardummy18 yeardummy19 yeardummy20 Acountrydummy1 Acountrydummy2 Acountrydummy3 Acountrydummy4 Acountrydummy5 Acountrydummy6 Acountrydummy7 Acountrydummy8 Acountrydummy9 Acountrydummy10 Acountrydummy11 Acountrydummy12 Acountrydummy13 Acountrydummy14 Acountrydummy15 Acountrydummy16 Acountrydummy17 Acountrydummy18 Acountrydummy19 Acountrydummy20 Acountrydummy21 Acountrydummy22 Acountrydummy23 Acountrydummy24 Acountrydummy25 Acountrydummy26 Acountrydummy27 Acountrydummy28 Acountrydummy29 Acountrydummy30 industrydummy1 industrydummy2 industrydummy3 industrydummy4 industrydummy5 industrydummy6 industrydummy7 industrydummy8 industrydummy9, vce(robust)

regr CCAR Sintargetdummy sharedummy cashdummy misMoPdummy toeholddummy logAsize Relativesize Sintargetdummy##Sinacquirerdummy yeardummy1 yeardummy2 yeardummy3 yeardummy4 yeardummy5 yeardummy6 yeardummy7 yeardummy8 yeardummy9 yeardummy10 yeardummy11 yeardummy12 yeardummy13 yeardummy14 yeardummy15 yeardummy16 yeardummy17 yeardummy18 yeardummy19 yeardummy20 Acountrydummy1 Acountrydummy2 Acountrydummy3 Acountrydummy4 Acountrydummy5 Acountrydummy6 Acountrydummy7 Acountrydummy8 Acountrydummy9 Acountrydummy10 Acountrydummy11 Acountrydummy12 Acountrydummy13 Acountrydummy14 Acountrydummy15 Acountrydummy16 Acountrydummy17 Acountrydummy18 Acountrydummy19 Acountrydummy20 Acountrydummy21 Acountrydummy22 Acountrydummy23 Acountrydummy24 Acountrydummy25 Acountrydummy26 Acountrydummy27 Acountrydummy28 Acountrydummy29 Acountrydummy30 industrydummy1 industrydummy2 industrydummy3 industrydummy4 industrydummy5 industrydummy6 industrydummy7 industrydummy8 industrydummy9, vce(robust)

*******************Rubustness checks***************

***** Robustness check 1: Sin selection
destring TSIC, replace force
destring TNAICS, replace force
***** Do the below commands before dropping countries N<5
***** Alcohol
gen Alcoholdummy = 0
replace Alcoholdummy = 1 if inlist(TSIC, 2080, 2081, 2082, 2083, 2084, 2085)
replace Alcoholdummy = 1 if TNAICS>312119 & TNAICS<312150
replace Alcoholdummy = 1 if TNAICS>424799 & TNAICS<424900
replace Alcoholdummy = 1 if TNAICS>445299 & TNAICS<445400
replace Alcoholdummy = 1 if TNAICS>722399 & TNAICS<722500
    ***** All but Alcohol
    drop if Alcoholdummy==1 & Sintargetdummy==1
    tab Sintargetdummy
    sample 195 if Sintargetdummy==0, count
***** Weapons
gen Weaponsdummy = 0
```

```
replace Weaponsdummy = 1 if TSIC>3759 & TSIC<3770
replace Weaponsdummy = 1 if TSIC==3795
replace Weaponsdummy = 1 if TSIC>3479 & TSIC<3490
replace Weaponsdummy = 1 if inlist(TNAICS,325920, 336414, 336415, 336419, 336992)
  ***** All but Weapons
   drop if Weaponsdummy==1 & Sintargetdummy==1
   tab Sintargetdummy
   sample 373 if Sintargetdummy==0, count
***** Tobacco
gen Tobaccodummy = 0
replace Tobaccodummy = 1 if TSIC>2099 & TSIC<2200
replace Tobaccodummy = 1 if inlist(TNAICS, 111910, 424940, 453991)
replace Tobaccodummy = 1 if TNAICS>312199 & TNAICS<312300
         ***** All but Tobacco
   drop if Tobaccodummy==1 & Sintargetdummy==1
   tab Sintargetdummy
   sample 384 if Sintargetdummy==0, count
***** Gambling
gen Gamblingdummy = 0
replace Gamblingdummy = 1 if TNAICS>713199 & TNAICS<713300
replace Gamblingdummy = 1 if TNAICS==721120
         ***** All but Gambling
   drop if Gamblingdummy==1 & Sintargetdummy==1
   tab Sintargetdummy
   sample 294 if Sintargetdummy==0, count


***** Robustness check 3: estimation window missings
tab Sintargetdummy
sample 359 if Sintargetdummy==0, count

***** Robustness check 4: MoP missings verwijderd
drop if misMoPdummy==1
drop misMoPdummy
tab Sintargetdummy
sample 319 if Sintargetdummy==0, count


******************Tables****************************
ssc install asdoc, replace

ssc install outreg2, replace
outreg2 using results, word
outreg2 using results, word append
```

```
****************OLS assumptions********************

***** Multicollinearity
pwcorr Sintargetdummy sharedummy cashdummy toeholddummy logAsize Relativesize
vif

***** Heteroscedasticity (use after regression)
hettest

***** Normal distribution (use after regression)
predict r, resid
kdensity r, normal

***** Check for outliers (use after regression)
ssc install winsor
winsor SCAR, p(.05) gen(SCAR_w5)
winsor PCAR, p(.05) gen(PCAR_w5)
winsor CCAR, p(.05) gen(CCAR_w5)
winsor SCAR, p(.01) gen(SCAR_w1)
winsor PCAR, p(.01) gen(PCAR_w1)
winsor CCAR, p(.01) gen(CCAR_w1)
```

Appendix 3

Table 5: Pairwise correlation matrix

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) Sin target dummy	1.000						
(2) Share dummy	-0.120	1.000					
(3) Cash dummy	0.057	-0.195	1.000				
(4) Missing MoP dummy	0.024	-0.160	-0.305	1.000			
(5) Toehold dummy	-0.039	0.004	0.024	-0.003	1.000		
(6) Log Acquirer size	0.152	-0.156	0.086	0.064	0.050	1.000	
(7) Relative size	0.034	0.242	-0.122	-0.140	-0.013	-0.301	1.000

Table 6: Variance inflation factor (VIF) test

	VIF	1/VIF
Sin target dummy	2.729	0.366
Share dummy	1.407	0.711
Cash dummy	1.306	0.766
Missing MoP dummy	1.371	0.729
Toehold dummy	1.197	0.836
Log Acquirer size	1.538	0.650
Relative size	1.312	0.762
Sin acquirer dummy	198.559	0.005
Interaction term(1)	201.676	0.005

(1): Sin target dummy*Sin acquirer dummy

Table 7: Variance inflation factor (VIF) test: without interaction term

	VIF	1/VIF
Sin target dummy	2.695	0.371
Share dummy	1.407	0.711
Cash dummy	1.306	0.766
Missing MoP dummy	1.363	0.734
Toehold dummy	1.196	0.836
Log Acquirer size	1.538	0.650
Relative size	1.311	0.763
Sin acquirer dummy	3.087	0.324

Appendix 4

Table 8: Breusch-Pagan test for heteroskedasticity of SCAR

Assumption: Normal error terms
Variable: Fitted values of SCAR
H0: Constant variance
chi2(1) = 284.35
Prob > chi2 = 0.0000

Table 9: Breusch-Pagan test for heteroskedasticity of PCAR

Assumption: Normal error terms
Variable: Fitted values of PCAR
H0: Constant variance
chi2(1) = 213.43
Prob > chi2 = 0.0000

Table 10: Breusch-Pagan test for heteroskedasticity of CCAR

Assumption: Normal error terms
Variable: Fitted values of CCAR
H0: Constant variance
chi2(1) = 180.78
Prob > chi2 = 0.0000

Appendix 5

Figure 4: Kernel density estimate plot SCAR

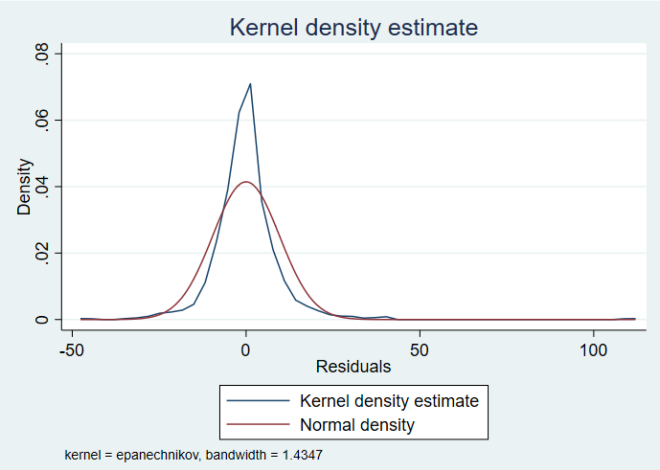


Figure 5: Kernel density estimate plot PCAR

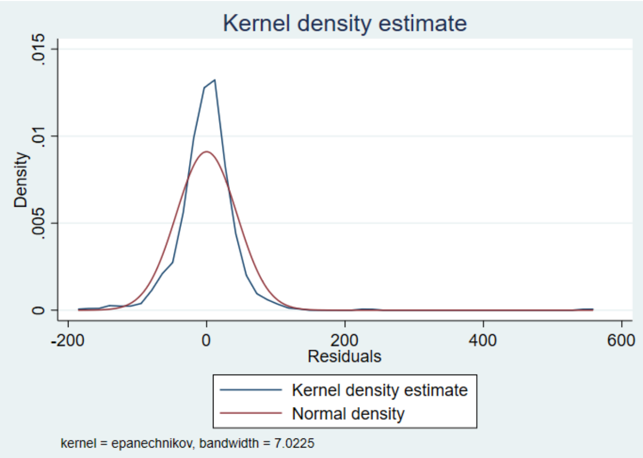
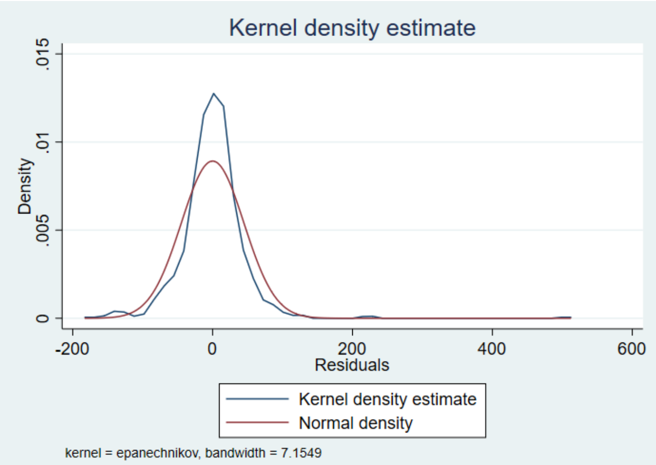


Figure 6: Kernel density estimate plot CCAR



Appendix 6

Robustness checks: Sin selection

Table 11: Regressions without alcohol firms

	(1)	(2)	(3)
VARIABLES	SCAR	PCAR	CCAR
Sin target dummy	-0.377	10.69*	10.31
	(2.233)	(6.264)	(7.189)
Interaction term(1)	2.548	-3.310	-0.762
	(3.411)	(10.48)	(11.44)
Share dummy	5.776	14.14	19.92
	(4.684)	(12.74)	(14.68)
Cash dummy	0.385	-0.124	0.262
	(1.108)	(4.336)	(4.651)
Missing MoP dummy	0.0323	6.269	6.301
	(1.087)	(4.876)	(5.160)
Toehold dummy	-0.608	-7.056	-7.663
	(2.175)	(8.950)	(9.096)
Log Acquirer size	-0.434	1.557	1.123
	(0.335)	(1.219)	(1.329)
Relative size	0.234	-2.577	-2.343
	(0.998)	(2.169)	(2.148)
Sin acquirer dummy	-4.057	-5.636	-9.692
	(3.705)	(8.374)	(10.12)
Constant	7.272	-26.68	-19.41
	(5.754)	(19.77)	(21.23)
Observations	390	390	390
R-squared	0.147	0.148	0.145

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

(1): Sin target dummy*Sin acquirer dummy

Table 12: Regressions without weapons firms

	(1)	(2)	(3)
VARIABLES	SCAR	PCAR	CCAR
Sin target dummy	-2.329*	12.11	9.782
	(1.342)	(7.927)	(7.632)
Interaction term(1)	2.226	3.487	5.714
	(2.119)	(11.57)	(11.25)
Share dummy	1.448	14.61	16.06
	(1.981)	(11.36)	(11.13)
Cash dummy	-0.170	2.372	2.202
	(0.757)	(3.862)	(3.880)
Missing MoP dummy	-0.0201	3.561	3.541
	(0.809)	(4.783)	(4.657)
Toehold dummy	-1.775	1.573	-0.202
	(1.212)	(6.456)	(6.637)
Log Acquirer size	-0.631***	1.324	0.694
	(0.191)	(0.931)	(0.917)
Relative size	0.152	5.854	6.006
	(1.115)	(7.744)	(7.218)
Sin acquirer dummy	1.053	-14.49**	-13.43**
	(1.746)	(6.080)	(6.363)
Constant	8.066**	-36.63**	-28.56*
	(3.625)	(16.45)	(16.99)
Observations	746	746	746
R-squared	0.133	0.099	0.105

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

(1): Sin target dummy*Sin acquirer dummy

Table 13: Regressions without tobacco firms

	(1)	(2)	(3)
VARIABLES	SCAR	PCAR	CCAR
Sin target dummy	-0.344	12.89*	12.54*
	(1.742)	(6.763)	(6.889)
Interaction term(1)	0.657	-1.617	-0.959
	(2.178)	(13.04)	(12.49)
Share dummy	2.069	13.38	15.45
	(2.715)	(10.63)	(10.80)
Cash dummy	-0.286	3.795	3.508
	(0.749)	(3.801)	(3.815)
Missing MoP dummy	-0.0406	6.035	5.995
	(0.805)	(4.502)	(4.410)
Toehold dummy	-1.632	0.0327	-1.599
	(1.298)	(6.346)	(6.472)
Log Acquirer size	-0.776***	1.291	0.515
	(0.221)	(0.954)	(0.942)
Relative size	0.661	4.060	4.721
	(0.994)	(6.496)	(5.994)
Sin acquirer dummy	-0.354	-8.128	-8.482
	(2.008)	(7.986)	(8.151)
Constant	10.10***	-41.84***	-31.74**
	(3.904)	(15.40)	(16.16)
Observations	768	768	768
R-squared	0.121	0.093	0.099

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

(1): Sin target dummy*Sin acquirer dummy

Table 14: Regressions without gambling firms

VARIABLES	(1) SCAR	(2) PCAR	(3) CCAR
Sin target dummy	-0.0663	13.05	12.98
	(2.110)	(8.354)	(8.384)
Interaction term(1)	0.101	-3.517	-3.417
	(2.925)	(15.73)	(15.35)
Share dummy	2.395	12.59	14.99
	(3.952)	(12.49)	(13.29)
Cash dummy	-0.985	0.884	-0.101
	(0.847)	(4.260)	(4.316)
Missing MoP dummy	-0.313	4.411	4.099
	(0.940)	(4.466)	(4.341)
Toehold dummy	-1.239	-1.048	-2.287
	(1.302)	(5.915)	(5.975)
Log Acquirer size	-0.820***	2.110**	1.291
	(0.241)	(0.994)	(0.975)
Relative size	1.150	8.470	9.620
	(1.423)	(9.951)	(9.066)
Sin acquirer dummy	-0.609	-7.856	-8.465
	(2.515)	(10.27)	(10.79)
Constant	12.17***	-45.64***	-33.47*
	(4.107)	(16.73)	(17.71)
Observations	588	588	588
R-squared	0.164	0.115	0.132

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

(1): Sin target dummy*Sin acquirer dummy

Appendix 7

Table 15: Alternative window: regression results

	(1)	(2)	(3)
VARIABLES	SCAR	PCAR	CCAR
Sin target dummy	-0.874	10.40*	9.522
	(1.492)	(6.250)	(6.118)
Interaction term(1)	2.976	1.908	4.884
	(2.094)	(11.79)	(11.04)
Share dummy	1.116	8.424	9.540
	(2.425)	(10.09)	(10.00)
Cash dummy	-0.528	5.335	4.807
	(0.665)	(3.699)	(3.637)
Missing MoP dummy	-0.130	4.490	4.360
	(0.720)	(4.191)	(3.967)
Toehold dummy	-0.864	-0.365	-1.229
	(1.075)	(5.337)	(5.481)
Log Acquirer size	-0.704***	1.438	0.734
	(0.187)	(0.922)	(0.900)
Relative size	-0.376	4.950	4.574
	(1.010)	(6.138)	(5.386)
Sin acquirer dummy	-2.280	-10.75	-13.03*
	(1.661)	(7.642)	(7.689)
Constant	10.60***	-37.27***	-26.67*
	(3.060)	(14.32)	(14.82)
Observations	830	830	830
R-squared	0.100	0.077	0.080

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

(1): Sin target dummy*Sin acquirer dummy

Appendix 8

Table 16: Estimation window missings: regression results

	(1)	(2)	(3)
VARIABLES	SCAR	PCAR	CCAR
Sin target dummy	-0.544	2.759	2.214
	(1.051)	(4.372)	(4.558)
Interaction term(1)	0.566	8.790	9.356
	(2.487)	(7.696)	(8.842)
Share dummy	0.954	4.586	5.540
	(1.601)	(7.835)	(7.968)
Cash dummy	-0.104	4.571	4.466
	(0.782)	(3.153)	(3.333)
Missing MoP dummy	0.263	4.543	4.806
	(0.754)	(3.340)	(3.437)
Toehold dummy	-1.313	-3.918	-5.231
	(1.109)	(5.259)	(5.390)
Log Acquirer size	-0.791***	2.370***	1.580*
	(0.240)	(0.899)	(0.918)
Relative size	1.234	-1.088	0.146
	(0.935)	(1.938)	(1.917)
Sin acquirer dummy	0.254	-11.45*	-11.20
	(2.343)	(6.415)	(7.744)
Constant	8.603**	-55.95***	-47.35***
	(3.896)	(15.22)	(15.98)
Observations	718	718	718
R-squared	0.173	0.119	0.118

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

(1): Sin target dummy*Sin acquirer dummy

Appendix 9

Table 17: Method of payment missings removed: regression results

	(1)	(2)	(3)
VARIABLES	SCAR	PCAR	CCAR
Sin target dummy	-0.345	10.66	10.32
	(1.982)	(7.239)	(7.500)
Interaction term(1)	-1.259	14.36	13.10
	(4.900)	(18.32)	(17.77)
Share dummy	1.039	7.645	8.684
	(3.010)	(10.62)	(11.23)
Cash dummy	-0.425	5.505	5.080
	(0.751)	(3.861)	(3.835)
Toehold dummy	-0.00796	-6.731	-6.739
	(1.366)	(6.314)	(6.512)
Log Acquirer size	-0.892***	1.371	0.479
	(0.242)	(1.044)	(1.036)
Relative size	0.326	6.848	7.173
	(1.036)	(6.758)	(6.287)
Sin acquirer dummy	2.149	-25.03*	-22.88*
	(4.323)	(13.92)	(13.38)
Constant	10.83***	-52.97***	-42.14**
	(3.822)	(16.01)	(16.62)
Observations	638	638	638
R-squared	0.115	0.115	0.117

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

(1): Sin target dummy*Sin acquirer dummy

Appendix 10

Table 18: Winsorized regression results

	5%			1%		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	SCAR	PCAR	CCAR	SCAR	PCAR	CCAR
Sin target dummy	-0.121	2.765	1.894	-0.863	5.196	4.243
	(0.762)	(3.222)	(3.433)	(1.031)	(3.778)	(3.922)
Interaction term(1)	-1.448	4.944	3.252	-0.778	0.0624	-1.409
	(1.376)	(6.386)	(6.843)	(1.826)	(7.362)	(7.769)
Share dummy	1.580	4.962	6.233	1.196	4.246	5.370
	(1.131)	(4.827)	(4.877)	(1.631)	(6.296)	(6.378)
Cash dummy	-0.382	3.387	3.187	-0.477	2.883	2.483
	(0.537)	(2.447)	(2.547)	(0.669)	(2.895)	(3.020)
Missing MoP dummy	-0.335	3.558	3.127	-0.276	2.600	2.316
	(0.564)	(2.715)	(2.822)	(0.683)	(3.201)	(3.295)
Toehold dummy	-0.107	-1.605	-1.938	-0.413	-1.858	-2.179
	(0.972)	(4.267)	(4.423)	(1.051)	(4.653)	(4.878)
Log Acquirer size	-0.399***	1.994***	1.387**	-0.489***	2.181***	1.663**
	(0.132)	(0.568)	(0.591)	(0.169)	(0.697)	(0.713)
Relative size	-0.0267	-1.048	-0.556	0.158	-0.662	-0.182
	(0.500)	(1.693)	(1.675)	(0.754)	(2.115)	(2.125)
Sin acquirer dummy	1.841*	-6.346	-3.554	1.823	-3.543	-0.848
	(1.095)	(5.148)	(5.552)	(1.583)	(6.176)	(6.560)
Constant	6.760***	-37.28***	-27.12**	6.643**	-41.15***	-33.63***
	(2.567)	(10.35)	(10.63)	(3.113)	(12.45)	(12.76)
Observations	830	830	830	830	830	830
R-squared	0.126	0.123	0.120	0.127	0.115	0.115

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
(1): Sin target dummy*Sin acquirer dummy