Payout Policy and Event Study Methodology

Inauguraldissertation zur Erlangung des akademischen Grades
eines Doktors der Wirtschaftswissenschaften
der Universität Mannheim

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vorgelegt im Herbstsemester 2013

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Tag der mündlichen Prüfung: 25. September 2013

Acknowledgements

This thesis would not have been possible without the guidance, help, and support of many different people. I am indebted to all of them.

First, I would like to thank Prof. Dr. Erik Theissen for offering me the possibility to work under his guidance.

I would also like to thank Prof. Dr. Stefan Ruenzi for co-refereeing this thesis.

Many thanks go to my current and former colleagues: Thomas Johann, Christopher Fink, Ekaterina Zatonova, Christian Speck, Marion Baierlein, Dr. Christian Westheide, Dr. Maria Kasch, Dr. Volker Vonhoff, Dr. Raphael Paschke, Dr. Mark van Aachter and our student assistants. My gratitude is in particular addressed to my office mates Erik Fernau and Inga van den Bongard. Im a also indebted to Philipp Schreiber who continuously helped me with organizing and reading the 'Fiwi' lectures.

I am very thankful to my co-authors and former colleagues Prof. Dr. Christian Andres and Prof. Dr. André Betzer. Together with Dr. Peter Limbach they were always available for fruitful and encouraging discussions. Beyond that Prof. Dr. André Betzer and the IT-deparment of the BUW - Bergische Universität Wuppertal provided me with a dedicated server farm. Without their invaluable support I would not have been able to successfully conduct my simulation study.

Most of all, I would like to thank my family for their endless encouragement, patience, and support over the past years.

Mannheim, Juli 2013 *Markus Doumet*

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Chapter 1

Introduction

Payout policy can be defined as the recurring managerial decisions and actions that set the amount and form of payouts to shareholders. Allen and Michaely (2003, p.340) further assume that these decisions imply "[...] some consistency over time, and that payouts, and dividends in particular, do not simply evolve in an arbitrary and random manner.'

While most researchers would agree on this definition of payout policy, the issue itself remains one of the most challenging and open topics in corporate finance. Despite an enormous amount of research, there is still no unanimity on the most fundamental questions (Black, 1976; Allen and Michaely, 2003): Does payout policy affect the overall firm value? How do firms set the amount and form of payout?

Most empirical research dealing with these questions has a focus on US capital markets. However, due to the differences in the institutional setting of the US and continental Europe it is questionable if the findings for the US can be applied to European firms. In fact the German institutional setting exhibits some deviations worth mentioning: First, opposed to the US, repurchases where essentially prohibited in Germany until 1998. Second, Germany underwent a major tax reform in 2001 affecting the relative tax advantage of repurchases over dividends. Finally, German firms operate in a corporate governance system that substantially differs from the US (e.g. Da Silva et al., 2004; Gugler, 2003).

The purpose of this dissertation thesis is to investigate how these changes in the institutional setting have affected the payout policy of German firms. This dissertation thesis further contributes to the literature on event study methodology by analyzing the measurment of abnormal credit default swap spreads. In the following, we will present the main ideas and concepts of this dissertation thesis.

In contrast to many theoretical studies, most empirical studies equate payout policy with dividend policy. This can be explained by the fact that regular dividends where the predominant method to disburse cash to shareholders until the mid 1980s. However, US corporations have increasingly used share repurchases in the recent past. Grullon and Michaely (2002) document that aggregate share repurchases exceed aggregate dividend payments for the first time in 1999. As a reaction to the increasing importance of share repurchases researchers have (i) analyzed how markets react to the announcement of a repurchase program and (ii) tried to model how managers decide on conducting a repurchase program.

It is a stylized fact, at least since the early studies by Dann (1981) and Vermaelen (1981), that stock prices react positive to repurchase announcements. Different hypotheses have been brought forward that try to explain these findings, but there is still an ongoing debate about which hypotheses are consistent with the empirical findings. Even though these empirical findings contradict the seminal Miller and Modigliani (1961) irrelevancy theorem, the latter is a good starting point for the identification of imperfections possibly explaining the influence of payout policy on firm value. Since capital markets are neither perfect nor complete, payout policy interacts with investment decisions and thereby potentially affects the market value of a firm. Asymmetric information, principal agent conflicts and the taxation of distributions can explain the positive stock price reaction, but also affect the managers' decision on changing their payout policies. This is explained in the following:

First, managers might possess private information about the true value of their firm. According to the classical signal models brought forward by Bhattacharya (1979) and Miller and Rock (1985) managers should adjust their payouts in order to convey their private information to the public. In both models dividends and repurchases are implicitly treated as perfect substitutes. If managers believe that their firm is currently undervalued, they can either increase dividends or initiate a repurchase program. Thus, these signalling models are a possible explanation for the positive market reaction on repurchase announcements. The signalling models further imply that managers in firms with higher information asymmetries are more likely to announce a repurchase (or dividend increase) if they possess positive information on their firms' future prospects.

Second, payout policy can be used to mitigate potential conflicts between shareholders and managers. Jensen (1986) argues that managers might allocate funds to investment projects that increase the managers' private benefits but decrease the shareholders' value. Jensen (1986) and Easterbrook (1984) suggest that reducing cash available to managers can mitigate the potential misuse of funds. Announcing a repurchase program or a dividend increase should therefore yield to a positive market reaction. As the management of a firm effectively sets the payout policy, this theory lacks an explanation why managers should voluntarily disburse cash to shareholders. A potential solution to that problem is a large shareholder possessing enough power to enforce a value-maximizing payout policy. However, a large and powerful shareholder should be able to monitor the management, which itself is sufficient to align the managers' interests. Moreover, Gugler and Yurtoglu (2003) argue that the existence of a large blockholder can cause an additional agency conflict between large and small shareholders. German firms are an interesting object to gain further insights on the relation between ownership structure and payout policy, not least by the fact that many German firms are more closely held than US firms.

While the aforementioned hypotheses treat dividends and repurchases as perfect substitutes, tax-based arguments lead to the conclusion that investors can have a clear preference for either dividends or repurchases. In the US, dividends are more heavily taxed than capital gains. Thus, (individual) investors should have a clear preference for repurchases over dividends. If managers care about their investors' tax preferences, they should substitute repurchases for dividends. In contrast to this prediction, US corporations used dividends as their primary method of payout until the mid 1980s. Black (1976) describes this phenomenon as the 'dividend puzzle'.

Jagannathan et al. (2000) provide evidence that repurchases differ from dividends not just because of a different tax treatment. In line with Lintner's (1956) findings they document that dividends are smoothed over time. In contrast, repurchases are much more volatile and vary with the business cycle. They conclude that dividends are used to disburse permanent earnings while repurchases are used to pay out temporary earnings. This contradicts the assumption that repurchases should replace dividends because of their relative tax advantage, but rather implies that repurchases and dividends are complementary payout methods.

The institutional setting in Germany allows us to further investigate some important research questions. In contrast to the US, share repurchases had almost entirely been prohibited before 1998. At that time US firms had already reached a new phase of the gradual transition process from solely paying dividends to a situation where repurchase were as important as dividends. Therefore, it is interesting to study how German firms make use of the possibility to repurchase their own shares. In addition to the introduction of stock repurchases in 1998, Germany underwent a major tax reform affecting the relative tax advantage of repurchases over dividends in 2001. These particularities of the German institutional setting allows us to shed further light on the relation between dividends and repurchases. Especially the two aformentioned regulatory changes are a favourable setting to test the various hypothesis against each other.

While most empirical studies that investigate the effect of payout policy on firm value employ an equity event study, some studies have examined the corresponding effect on bondholders' wealth. A central problem is that, in contrast to stocks, the event study methodology is less developed for other assets. As a consequence, researchers often adopt the stock event study methodology to other assets such as bonds and credit default swaps (CDSs). In their recent paper Bessembinder et al. (2009) provide evidence that this adoption to bonds is problematic because the related test statistics are poorly specified and lack the power to reliably detect event-induced changes in bond prices. A relatively new asset used in event studies originates from credit derivates. In particular, a growing literature analyzed the effect of corporate and regulatory events on credit default swap spreads. In contrast to bonds, research can employ CDSs to analyze the effect of an event on credit risk. Another difference is the fact that CDS are mainly traded by instituinal investors. Since Daniels and Jensen (2005) and Zhu (2006) show that price discovery occurs first in the CDS market and subsequently in the bond market, analyzing CDS is likely to provide cleaner results. Despite these and other potential advantages of CDSs over bonds (e.g. liquidity, standardized maturities) no guidline that describes how to conduct a meaningful event study with CDS exists. We therefore use a large international dataset with CDS spreads to conduct a simulation study similar to Brown and Warner (1980, 1985). This allows us to evaluate the specification and power of different spread change models and test statistics.

The rest of this dissertation is organized as follows: Chapter 2 (joint work with Christian Andres, André Betzer and Erik Theissen) investigates the decision to announce an open market share repurchase and the share price reaction to the subsequent announcement. To the best of our knowledge, this is one of the first studies that analyze the decision to repurchase stock and the market reaction in a joint econometric framework. Previous studies have either focused on the managerial decision to announce a repurchase (e.g. Jolls, 1998; Dittmar, 2000; Jagannathan et al., 2000; Kahle, 2002; Von Eije and Megginson, 2008; Oswald and Young, 2010; Andriosopoulos, 2010) or solely analyzed the market reaction to repurchase announcements (e.g. Vermaelen, 1981; Comment and Jarrell, 1991; Stephens and Weisbach, 1998). The former strand of the literature emphasizes that managers decide rationally whether or not to announce a repurchase program. The latter strand of the literature employs the event study methodology and a subsequent cross-sectional regression to explain abnormal returns. A potential problem with this approach is that it implicitly assumes that the set of announcing firms is a random sample of the population of all listed firms. If firms self-select to announce a repurchase, a simple cross-sectional regression on abnormal returns can lead to biased results. To bridge the gap between the different strands of the literature and to account for the potential selection bias, we employ an econometric model in the spirit of Heckman (1979). This approach requires a "non-event sample" of firms that could reasonably be expected to announce a repurchase but finally did not. To the best of our knowledge, only two papers have applied this approach to the case of share repurchases. Li and McNally (2007) use a sample of Canadian firms that announce a repurchase program. They create their non-event sample by matching a non-repurchasing firm to each repurchasing based on different firm characteristics such as size and industry. They thereby assume that non-repurchasing firms that are 'similar' to the repurchasing firms should exhibit a non-trivial probability of announcing a repurchase program. Schremper (2003) analyzes German firms. He does not apply a matching approach but defines all non-repurchasing firms as the control group. He thereby assumes that investors assign a non-trivial probability of announcing a repurchase program to all listed firms.

However, the specific institutional rules for Germany allow us to construct a clean sample of non-repurchasing firms. German firms that intent to conduct a repurchase program need to follow a standardized two-step procedure. First, the shareholders' meeting has to grant the managerial board the permission to conduct a repurchase program. This permission is valid up to 18 month (5 years from 2009 onwards). This permission is basically an option because it grants the right, but not the obligation, to conduct a repurchase program. German firms that finally conduct a repurchase program have to disclose this fact to the public. This two-step procedure allows us to identify repurchasing firms and non-repurchasing firms that intended to do so, but finally did not.

The empirical analysis is based on 438 ad-hoc announcements published between May 1998 and December 2008. Non-event firms are defined as firms that have a valid approval by the shareholders' meeting but do not make use of it. We find that the conditional approach yields results that are qualitatively comparable but differ in detail from those obtained using a non-conditional approach. We confirm earlier findings of negative share price performance prior to the repurchase announcement and positive and significant announcement day abnormal returns. The results of our probit models are consistent with the free cash flow hypothesis and provide at least partial support for the rent extraction, signalling and capital structure hypotheses. The results of the cross-sectional regressions provide strong support for the signalling hypothesis once we control for the selection bias.

Chapter 3 (joint work with Christian Andres, Erik Fernau, and Erik Theissen) also deals with the introduction of share repurchases as an additional method to payout cash to shareholders. The scope of this chapter is to shed further light on the relation of repurchases and dividends as well as the effect of taxes on payout policy. We therefore define total payout as the sum of regular dividends, special dividends, and repurchases. Employing Lintner's (1956) partial adjustment models for dividends and total payouts, we analyze the effect of the introduction of repurchases and a major tax reform on the overall payout policy of German firms. Our sample covers 424 non-financial firms at any time during the 21-year period 1988-2008. This yields an unbalanced panel with 4,363 firm-year observations.

As it is questionable whether repurchases and dividends are substitutes or complements, we define different model specifications. We further allow for changes in the payout policy of German firms due to the two aforementioned institutional changes by introducing structural breaks for the introduction of repurchases and the

2001 tax reform. If dividends and repurchases are perfect substitutes, the parameters of a Lintner (1956) model based on total payouts should not change by the introduction of repurchases in 1998. In contrast to this prediction, we document a substantial decrease for the target payout ratio and an increase in the speed of adjustment. These results are inconsistent with the hypothesis that dividends and repurchases are close substitutes.

We further investigate wether firms adjust their payout policy due to a change in the relative tax advantage of repurchases over dividends. After the 2001 tax reform repurchases were taxed at a lower rate than dividends. This holds for both individual and corporate investors. We would hence expect that firms replace dividends by share repurchases. Nevertheless, we do not observe a decrease in the target ratio of a partial-adjustment model based on regular dividends. We therefore conclude that our results are also inconsistent with the prediction that tax considerations are a major driver of payout decisions.

Because our results do not support the assumption that repurchases and dividends are substitutes, we also test the validity of the flexibility hypothesis broad forward by Jagannathan et al. (2000). For this purpose we decompose earnings into a permanent and a transitory component. Our results support the flexibility hypothesis, which predicts that dividends are used to disburse permanent earnings while repurchases are used to disburse transitory earnings.

In Chapter 4 (joint work with André Betzer and Christian Andres) we examine the size and power of test statistics designed to detect abnormal changes in credit risk as measured by CDS spreads. In the spirit of Brown and Warner (1980, 1985) and Bessembinder et al. (2009), we follow a simulation approach to examine the statistical properties of normal and abnormal CDS spreads and assess the performance of normal return models and test statistics. Using daily CDS data, we find that parametric test statistics are generally inferior to non-parametric tests, with the rank test performing best. Some of the classical normal return models, such as the market model, are found to be poorly specified. A CDS factor model based on factors identified in the empirical literature is generally well specified and more powerful in detecting abnormal performance. If factor information is not available, a simple mean-adjusted approach should be used. Finally, we examine performance in the presence of event-induced variance increases and bootstrapped p-values. Our inferences hold for US and European

CDS data and are not affected by reference entities' credit quality.

Chapter 2

Open Market Share Repurchases in Germany: A Conditional Event Study Approach

2.1 Introduction

Since the early studies by Dann (1981) and Vermaelen (1981) it is a stylized fact that share prices react positively to the announcement of share repurchases. Academic research has proposed a considerable number of hypotheses (to be briefly reviewed in section 3) aiming to explain this finding, and a large number of empirical papers have tested them.

The usual approach in these studies is to construct a sample of firms announcing repurchases, to estimate the announcement period abnormal returns using event study methodology, and to finally regress the abnormal returns on a set of explanatory variables. A potential problem with this approach is that it implicitly assumes that the set of announcing firms is a random sample of the population of all listed firms. However, managers decide rationally whether or not to announce a repurchase program. Evidence from empirical studies that model the decision to repurchase (e.g. Jolls, 1998; Dittmar, 2000; Jagannathan et al., 2000; Kahle, 2002; Von Eije and Megginson, 2008; Oswald and Young, 2010; Andriosopoulos, 2010) suggests that repurchasing firms are systematically different from non-repurchasing firms. Similarly, the significant negative pre-announcement abnormal returns documented in previous studies (e.g. Vermaelen, 1981; Comment and Jarrell, 1991; Stephens and Weisbach, 1998) indicate that the announcements are 'timed'; i.e., are contingent upon the share price performance. Thus, there is a potential selection bias.

Acharya (1988) develops an econometric methodology that corrects for the potential selection bias. It is similar in spirit to Heckman (1979). Prabhala (1997) analyzes under what conditions this procedure performs well. He states (p. 32) that this is the case 'only when one has, in addition to data on firms announcing the event, a set of non-event firms, that is, firms that were partially anticipated to announce but chose not to announce the event in question'. He then concludes (p. 33) that 'when the necessary non-event data are available, inference should be based on conditional methods'.

Unfortunately, non-event data is unavailable in most applications. In this regard, the institutional rules for share repurchases in Germany are an exception. As explained in more detail in section 2, the shareholders' meeting first has to approve a share repurchase program. The approval is valid for up to 18 months (up to 5 years since 2008) and allows the managerial board to initiate a repurchase program. The board is, however, not obliged to do so. If the managerial board decides to repurchase shares this fact is publicly announced. This two-step process allows us to construct an event sample (firms with approval from the shareholders' meeting that did announce a repurchase program) and a non-event sample (firms with approval from the shareholders' meeting that did not announce a repurchase program).

We use this specific setup to estimate a joint model of a) the decision to initiate a repurchase program and b) the determinants of the event date abnormal returns. We also compare the results obtained using this conditional model to those obtained using the traditional non-conditional approach.

We are aware of two papers that use a conditional approach to analyze the information content of repurchase announcements. Li and McNally (2007) use a sample of Canadian firms that announced a repurchase program (the event sample) and a size- and industry-matched sample of non-repurchasing firms (the nonevent sample). This sample selection procedure is based on the assumption that market participants assign a non-trivial repurchase probability to the sample of matched firms. Schremper (2003) analyzes German firms. He uses a sample of all non-repurchasing firms as non-event sample. This approach implicitly assumes that the market attaches a non-trivial repurchase probability to all listed firms.

Our paper contributes to the literature in several ways. First, our paper is one of the first studies to analyze the determinants of repurchase announcements and the determinants of the announcement date abnormal returns jointly. Second, as outlined above the specific institutional setting in the German stock market allow us to construct a non-event sample of firms that already obtained shareholders' approval to initiate a repurchase program. With such a non-event sample at hand the conditional event study methodology we employ is appropriate. We do not have to rely on a matched sample approach as used by Li and McNally (2007), and neither do we have to assume that all firms are expected to repurchase with a non-trivial probability as in Schremper (2003). Third, we improve on the methodology used in previous papers by estimating the first-stage probit model and the second-stage cross-sectional regression simultaneously. This procedure increases the efficiency of the estimates. We find that the conditional estimation approach yields results that are qualitatively comparable but differ in detail from those obtained using a non-conditional approach. We further confirm earlier findings of negative share price performance prior to the repurchase announcement and positive and significant announcement day abnormal returns. The results of our probit models are consistent with the free cash flow hypothesis and provide at least partial support for the rent extraction, signalling and capital structure hypothesis. In addition, the results of the cross-sectional regressions provide strong support for the signalling hypothesis once we control for the selection bias. We find only weak support for the free cash flow and rent extraction hypothesis.

The remainder of the paper is structured as follows. In section 2.2 we describe the institutional setting in Germany. In section 2.3 we develop our hypotheses. Section 2.4 describes the methodology and the data set. We present our results in section 2.5, section 2.6 concludes.

2.2 Institutional Background

Approving Repurchases

Until 1998 share repurchases were essentially prohibited in Germany.[1] In 1998 a new law came into force that allows share repurchases. Under this law firms are allowed to buy back up to 10% of their shares. A firm wishing to buy back shares has to follow a standardized two-step procedure. As a first step the shareholders' meeting (with simple majority) has to grant the managerial board the permission to buy back shares. This permission has to specify the maximum number of shares to be bought back (not more than 10% of shares outstanding), the minimum and maximum price to be paid per share, and the time of validity of the permission (initially not longer than 18 months; since 2008 no longer than 5 years).

This permission gives the managerial board the right, but not the obligation, to buy back shares. Once the board decides to actually initiate a repurchase program the firm has to communicate this fact to the public. This is mandated by the German securities trading act (Wertpapierhandelsgesetz), which requires that listed firms immediately disclose information that is likely to materially affect security prices ('ad-hoc disclosure'). Empirical studies analyzing the impact of repurchase announcements on share prices typically use the date of the ad-hoc disclosure as the event date (e.g. Gerke et al., 2002; Schremper, 2003; Seifert and Stehle, 2003; Hackethal and Zdantchouk, 2006; Bessler et al., 2009).

The two-step approval procedure with the permission from the shareholders' meeting at the first stage and the decision of the managerial board at the second stage is important for our analysis. Our simultaneous estimation procedure requires a control group of firms that did not initiate a repurchase program but could reasonably be expected to do so. We choose firms that got approval from the shareholders' meeting but did not announce a repurchase program. The managerial board of these

[1] Firms could acquire their own shares only under restrictive conditions (e.g. to prevent damage). Although there is some disagreement in the literature as to the actual number of repurchases in Germany prior to 1998 (see Seifert 2006 for a discussion) it is safe to conclude that share repurchases were not used as a means of disbursing cash to shareholders prior to 1998.

firms could have initiated a repurchase program at any time. Therefore, investors would reasonably attach a non-trivial probability of announcing a repurchase program to these firms. This claim is supported by empirical results reported in Hackethal and Zdantchouk (2006). These authors analyze the share price reaction on the day on which it becomes known that the management seeks shareholders' approval for a repurchase program. They find a positive abnormal return of 1.47% on the event date and a cumulative abnormal return of 2.53% [5.21%] in a symmetric 3-day [11-day] window around the event date. The result that share prices increase when the management seeks approval for a repurchase program from the shareholders' meeting supports our claim that market participants attach a non-trivial probability of initiating a repurchase program to firms that obtained approval from the shareholders' meeting.

Implementation of Repurchase Programs

Firms are required to treat all shareholders equally. This precludes negotiated repurchases from large shareholders. Open market repurchases, repurchase tender offers and transferrable put rights are admissible, though open market repurchases are the dominating form.[2] As is the case in the U.S., the announcement of a share repurchase still does not require the managerial board to actually repurchase shares. The actual amount of repurchases is published in the firm's financial statement.
Since 2004 new European Union regulation imposes additional restrictions on repurchases. Individual transactions made as part of a repurchase program now have to be reported within seven trading days. Further, there are restrictions on the prices at which open market repurchases can be made (not higher than the price of the previous transaction) and on the maximum daily repurchase volume (not more than 25% of the average daily volume on the market on which the trade is made).

There are two ways in which a firm can handle the repurchased shares. First, it can treat them as an asset on the asset side of the balance sheet. They can then be used to cover outstanding convertible bonds or executive stock options. The maximum

[2] Out of 589 repurchase announcements in our sample, only 17 (less than 3%) do not concern open market repurchases.

number of shares a firm can hold on its balance sheet is 10% of the shares outstanding. Alternatively, the firm can reduce the number of shares outstanding. In this case the firm's book equity is reduced accordingly.

Tax Treatment

The tax treatment of dividends and repurchases underwent a major change in 2001. Until 2001 Germany operated a full imputation system. Dividends paid to domestic investors were essentially taxed at the investor's personal tax rate.[3] Retained earnings were taxed at a corporate tax rate. Consequently, investors with a personal tax rate below the corporate rate favored dividends over repurchases while investors with a tax rate above the corporate rate favored repurchases.[4] Corporations should have been indifferent because their 'personal' tax rate is the corporate rate. Foreign investors did not receive the tax credit and may therefore have had a preference for repurchases.

Since 2001 dividends and retained earnings are taxed at the same rate at the corporate level. At the investor level half of the gross dividend is taxed at the investor's personal tax rate. Capital gains are not taxed when the shares are held for more than one year. When this condition is met investors should thus have a clear preference for repurchases over dividends.

In summary, while the preference for dividends versus repurchases depended on the status (domestic versus foreign) and the personal tax rate of the investor prior to 2001, there should be a clear preference for repurchases after 2001.

2.3 Hypotheses

Starting with the seminal work of Dann (1981) and Vermaelen (1981) a large number of authors have empirically analyzed share repurchase programs. Three main questions

[3] Dividends were first taxed at the firm level. Domestic investors received the gross dividend plus a tax credit equal to the tax paid by the firm. The gross dividend was taxed at the investor's personal tax rate. The resulting tax liability was then offset against the tax credit.

[4] This statement implicitly assumes that capital gains are not taxed. This was indeed the case when the shares were held longer than 6 months (one year from 1999 onwards).

are addressed in this literature: (1) why do firms repurchase shares, (2) how does the share price react to repurchase announcements and (3) on what determinants does the price reaction depend. The theoretical and empirical literature proposes several hypotheses that are not mutually exclusive. We briefly discuss these hypotheses in this section, and we summarize them in Table 2.1.

According to the *signalling hypothesis*, managers repurchase shares in order to signal private information implying that the firm is currently undervalued. By this argument, the likelihood for a repurchase should be higher for firms with lower valuation (as measured by Tobin's Q, the market-to-book ratio or previous share price performance), and it should be higher when informational asymmetries between managers and investors are more pronounced. This is likely to be the case for smaller firms. The share price reaction caused by a repurchase announcement should be inversely related to these measures of valuation and informational asymmetries. Further, larger repurchase programs and repurchase announcement made by firms with higher managerial ownership in combination with a poor stock performance should trigger larger share price reactions because they provide more credible signals.[5]

The starting point of the *free cash flow hypothesis* is the agency conflict between shareholders and managers. Repurchases reduce the free cash flow and may thereby reduce agency costs. They should thus be more likely in firms in which the agency problem is more severe. By this argument, firms with higher levels of free cash flow, firms with fewer profitable investment opportunities (as measured by Tobin's Q or the market-to-book ratio) and firms with lower leverage should be more likely to announce a repurchase program. The market should also react more positively to repurchase announcements made by these firms. However, this effect may be limited or even reversed provided that managers use a repurchase programm to finance future acquisitions instead of reducing the firm's equity. Moreover, the latter case is less likely since self-interested managers may not voluntarily initiate repurchase programs that limit their investment opportunities. Therefore, firms with large shareholders (who can exert pressure on managers) are more likely to initiate a repurchase program.

[5] In case a repurchase is conducted by means of a tender offer the share price reaction should be increasing in the offer premium. Our empirical analysis is confined to open market repurchases to which this argument does not apply.

The *rent extraction hypothesis* (Gugler and Yurtoglu, 2003) starts from the observation that, in countries with concentrated ownership structures such as Germany, there may not only be agency conflicts between shareholders and managers but also agency conflicts between large and small shareholders (e.g. La Porta et al., 2000). These conflicts are likely to be more pronounced when a large shareholder holds voting rights in excess of cash flow rights. A repurchase program deprives a firm of cash that otherwise might be diverted by large shareholders. Thus, if a firm with a strong blockholder announces a repurchase program, the share price should react favorably to the announcement. Consequently, the announcement date abnormal return should increase in the stake of the largest shareholder and decrease in the cash-flow-to-voting-rights ratio.[6] By the same argument, large blockholders may be opposed to repurchase programs. Therefore, the likelihood of a repurchase announcement should decrease in the stake of the largest shareholder and increase in the cash-flow-to-voting-rights ratio.

A large second shareholder may contain the power of the largest shareholder (see Gugler and Yurtoglu, 2003). Consequently, the likelihood of a repurchase announcement should increase in the stake of the second largest shareholder while the price reaction to the announcement should be decreasing in the stake of the second largest shareholder.

Several hypotheses make predictions about the determinants of the choice between dividends and repurchases. We subsume them under the header *choice of payout method*. A firm's choice of the payout method should be governed by the relative tax treatment of dividends and repurchases and by the tax preferences of the firm's shareholders. As outlined in section 2 the 2001 tax reform favored repurchases over dividends. We therefore expect a higher probability for a repurchase in the post-reform period. Prior to 2001 investors in high tax brackets favored repurchases while those in low tax brackets favored dividends. According to the tax clientele hypothesis one would therefore expect firms with high dividend yields to predominantly have investors in low tax brackets and firms with low dividend yields to have investors in high tax brackets. As the latter investors favor repurchases over dividends we thus expect an inverse relation between dividend yield and the probability of a repurchase.

[6] A low cash-flow-to-voting-rights ratio indicates deviations from the one-share-one-vote principle. Consequently, the higher the cash-flow-to-voting-rights ratio the better aligned are the incentives of small and large shareholders.

Jagannathan et al. (2000) provide evidence that dividends are paid out of permanent cash flows while repurchases are paid out of transitory cash flows. Firms with more volatile cash flows are more likely to experience transitory changes in cash flows and should thus be more likely to repurchase shares. This argument implicitly assumes that managers prefer to smooth dividends. If a firm - for whatever reason - prefers not to smooth its dividends, there is no reason for this firm to use repurchases to disburse transitory cash flows. Consequently, we expect that firms with a history of volatile dividends are less likely to initiate a repurchase.

Managerial stock options are typically not dividend-protected. Consequently, their value decreases when a firm pays dividends. Managers in firms with stock option plans may therefore prefer repurchases over dividends (Jolls, 1998; Kahle, 2002). As noted earlier, repurchased shares can be used to service existing stock-option plans. Since repurchases conducted with this intention do not signal positive information, repurchase announcements made by firms with stock option plans should trigger lower abnormal returns.[7]

The *capital structure hypothesis* posits that repurchases may be used as a means to adjust a firm's capital structure to its target level. Accordingly, firms with below-target leverage levels should be more likely to announce a repurchase (Hovakimian et al., 2001). To the extent that firm value depends on the distance between the actual and the target capital structure the abnormal return triggered by a repurchase announcement should be increasing in this distance.

Survey evidence presented by Brav et al. (2005) suggests that managers are concerned about earnings per share (EPS). We therefore include earnings per share in our empirical model. As there is no economic rationale for the *EPS hypothesis* we do not expect a particular sign for the coefficient.

[7] Unfortunately, data on the existence of stock option plans is unavailable for our sample. We are therefore unable to test this hypothesis empirically.

Table 2.1: Summary of Hypotheses

Table 2.1 provides an overview of the main hypotheses on share repurchases, the variables which proxy for them, and the expected signs of the related coefficients' estimates of the first-stage probit model and the second-stage cross-sectional regression.

Hypothesis	**Variable**	**Expected sign**	
		Probit	Cross-section
Signalling	Tobin's q / Market-to-book ratio	-	-
	Share price performance	-	-
	Size of repurchase program		+
	Managerial ownership x Share price performance		-
	Firm size	-	-
Free cash flow	Free cash flow	+	+
	Free cash flow x Reason 'acquisition'		-
	Cash holdings	+	+
	Tobin's q / Market-to-book ratio	-	-
	Cash holdings x Low market-to-book ratio	+	+
	Ownership concentration	+	
	Cash holdings x ownership concentr.	+	
	Leverage	-	-
Rent extraction	Stake of largest shareholder	-	+
	Stake of second largest shareholder	+	-
	Cash-flow-to-voting-rights ratio	+	-
Choice of payout method	2001 Tax reform dummy	+	
	Dividend yield	+	
	Volatility of cash flows	+	
	Volatility of dividends	-	
Capital structure	Leverage	-	-
Earnings per share	Earnings per share	?	?

2.4 Methodology and Data

The objective of our regression analysis is a joint estimation of (a) the likelihood to initiate a repurchase program and (b) the determinants of the event date abnormal returns. Step (b) requires event study cumulative abnormal returns as an input. Therefore we first describe the event study that we perform. We then describe the joint conditional estimation approach we employ. While doing so we also discuss the traditional non-conditional approach and highlight its potential disadvantages. The final subsection describes our data set and presents descriptive statistics.

Event Study

We measure the stock price reaction to open-market repurchase announcements applying standard event-study methodology. The abnormal return of firm i on day τ is defined as the difference of the realized return and the expected return based on the market model (Brown and Warner, 1985):[8]

$$AR_{i,\tau} = R_{i,\tau} - (\widehat{\alpha}_i + \widehat{\beta}_i R_{m,\tau}) \tag{2.1}$$

where $AR_{i,\tau}$ is the abnormal return of firm i on day τ and $R_{m,\tau}$ is the return of the proxy for the market portfolio on day τ. The coefficients $\widehat{\alpha}_i$ and $\widehat{\beta}_i$ in equation (2.1) are OLS estimates obtained from a regression of firm i's daily returns on the market portfolio (and a constant) over a period of 160 trading days ending 21 days before the announcement. We use the CDAX index as our proxy for the market portfolio.

Daily average abnormal returns are then calculated for each day of the event period as the cross-sectional arithmetic mean of the abnormal returns:

$$AAR_{\tau} = \frac{1}{N} \sum_{i=1}^{N} AR_{i,\tau} \tag{2.2}$$

where N is the total number of firms in the sample. The cumulative average abnormal

[8]In an unreported robustness check we alternatively use the constant mean return model. The results are virtually identical.

return from day τ_1 to day τ_2 is given by:

$$CAAR_{\tau_1,\tau_2} = \sum_{\tau=\tau_1}^{\tau_2} AAR_{\tau} \tag{2.3}$$

We test the statistical significance of AARs and CAARs applying a simple time-series test (Brown and Warner, 1985). Since deviations from the iid normal assumption of the aforementioned test are highly likely in event studies, we additionally apply various robust test statistics. We calculate the Patell (1976) standardized residuals test that is robust to heteroscedastic event-period abnormal returns. Moreover, we apply the standardized cross-sectional test introduced by Boehmer et al. (1991) that is additionally robust to event-induced variance increases. In case of non-normality of the abnormal returns the former three parametric tests may be poorly specified. Therefore, we also apply the non-parametric Corrado and Zivney (1992) rank test and the Cowan (1992) generalized sign test.

We additionally measure the abnormal trading volume applying the methodology described in Brav and Gompers (2003). We expect that the abnormal trading volume is virtually zero over the pre- and post-announcement periods, but significantly increases during the announcement period.

The conditional estimation approach

The traditional approach to analyze the determinants of the event study CARs is to regress individual abnormal returns on a set of explanatory variables using OLS. The corresponding cross-sectional regression equation can be written as:

$$CAR_i = X_i\beta + \epsilon_i \tag{2.4}$$

where CAR_i denotes the cumulative abnormal return of event i, X_i is a vector of explanatory variables, and ϵ_i an error term assumed to be normally distributed. The traditional approach implicitly assumes that the sample of firms announcing a repurchase is a random sample from the population of all listed firms. However, provided that the shareholders' meeting has granted permission to buy back shares managers decide rationally on whether or not to announce a repurchase. In order to account for the resulting selection bias we adopt the general selection model proposed by Acharya (1988) and analyzed in detail by Prabhala (1997). The cross-sectional regression is augmented by a selection equation that models a firm's decision whether or not to announce a repurchase. Managers are assumed to announce a repurchase ($REP_i = 1$) when the marginal utility U_i^* of doing so is strictly positive. Otherwise they do not announce a repurchase ($REP_i = 0$). U_i^* is modeled as a linear function of exogenous, publicly observable variables W_i:

$$U_i^* = W_i\gamma + \eta_i \tag{2.5}$$

where η_i is an error term assumed to be normally distributed and orthogonal to W_i. Since market participants only observe the binary outcome REP, and since announcement day abnormal returns are only observed for announcing firms, we finally obtain the system:

$$REP_i = 1 \Leftrightarrow U_i^* = W_i\gamma + \eta_i > 0 \tag{2.6}$$

$$REP_i = 0 \Leftrightarrow U_i^* = W_i\gamma + \eta_i \leq 0 \tag{2.7}$$

$$CAR_i = X_i\beta + \epsilon_i \;\; if \; REP_i = 1 \tag{2.8}$$

Estimation of the selection model requires a sample of event firms (firms that announced a repurchase) and a sample of non-event firms.

When estimating the abnormal return equation (2.8) we explicitly account for the fact that the dependent variable (the CAR following the repurchase announcement) is only observed for the subsample of repurchasing firms. In order to demonstrate under which circumstances the traditional approach leads to inconsistent estimates we take the conditional expectation of equation (2.8).

$$E\left[CAR_i \mid REP_i = 1\right] = X_i\beta + E\left[\epsilon_i \mid REP_i = 1\right] = X_i\beta + E\left[\epsilon_i | \eta_i > -W_i\gamma\right] \tag{2.9}$$

Following Heckman (1979) we further assume that ϵ_i and η_i follow a bivariate normal distribution:

$$\begin{aligned} \eta_i &\sim N(0,1) \\ \epsilon_i &\sim N(0,\sigma_\epsilon) \\ corr(\eta_i,\epsilon_i) &= \rho \end{aligned} \tag{2.10}$$

Given this assumption we can express the expected value of ϵ_i given η_i as:

$$E\left[\epsilon_i \mid \eta_i > -W_i\gamma\right] = \rho\sigma_\epsilon E\left[\eta_i \mid \eta_i > -W_i\gamma\right] = \rho\sigma_\epsilon \frac{\phi(W_i\gamma)}{\Phi(W_i\gamma)} = \rho\sigma_\epsilon\lambda_i(W_i\gamma) \tag{2.11}$$

Inserting equation (2.11) into equation (2.8) yields:

$$E[CAR_i \mid REP_i = 1] = X_i\beta + \rho\sigma_\epsilon\lambda_i(W_i\gamma) = X_i\beta + \beta_\lambda\lambda_i(W_i\gamma) \tag{2.12}$$

A comparison of equation (2.12) with equation (2.4) shows that applying the traditional approach leads to inconsistent estimates whenever the error terms of the selection model and the abnormal return equation are correlated.

Heckman (1979) argues that self-selection can be interpreted as an omitted variable problem. He therefore proposes a two-step estimator for eqs. (2.6)-(2.8). In a first step he estimates the selection equation by means of a probit model. He then calculates the fitted correction factor, the inverse Mill's ratio:

$$\widehat{\lambda}_i(W_i\gamma) = \frac{\phi(W_i\widehat{\gamma})}{\Phi(W_i\widehat{\gamma})} \tag{2.13}$$

Finally, he estimates the parameters of the abnormal return equation with the fitted correction factor as an additional explanatory variable. The second-stage regression is then estimated by least squares. Assuming bivariate normality, this approach yields consistent estimates. However, a joint estimation of eqs. (2.6)-(2.8) using maximum likelihood (ML) estimation results in more efficient estimates and allows to test if the correlation coefficient ρ is significantly different from zero.

So far we have treated self-selection as a purely econometric issue. However, Li and Prabhala (2007) emphasize the 'dual nature' of the inverse Mill's ratio. In our self-selection mechanism we assumed that is the part of U_i^* that is not explained by the publicly observable regressors W_i. Because η_i is an error term orthogonal to the publicly observable regressors W_i we can interpret it as private information of the managers. From the point of view of investors who cannot observe the managers' private information, the unconditional expected value of η_i is zero. When investors observe that a firm announces a repurchase, they can update their expectation of the managers' private information η_i. This private information revealed by the repurchase announcement should affect the stock price reaction to the announcement. We can test whether this is the case by including $E[\eta_i \mid REP_i = 1]$ as an additional explanatory variable in the abnormal return equation (2.4). According to equation (2.13) this additional variable is equivalent to the inverse Mill's ratio. Hence, a correction for self-selection coincidently allows to test for the influence of private information on announcement period abnormal returns. If the managers' private information that affected their decision to initiate a repurchase program is at least partially revealed by the repurchase announcement, we expect a positive relation between the private information and the event period abnormal return.

Data and descriptive statistics

In this section, we provide a description of the sample selection and event identification process. To analyze the decision to initiate a repurchase program and to quantify the share price reaction after the subsequent announcement, we consider all non-financial firms included in the German Composite DAX index (CDAX).[9] Our sample period extends from May 1998 to December 2008.

As outlined above, our methodology requires a sample of firms announcing a repurchase program and an additional sample of firms that could reasonably be expected to announce a repurchase program but did not. To construct these two samples, we apply the following criteria throughout our analyses. First, we only consider firms with a valid approval of the shareholders' meeting that allows the managerial board to initiate a repurchase program. These approvals have to be reported both in the annual reports and have to be filed with the 'Bundesanstalt fuer

[9] The CDAX is a broad stock index that contains all German firms listed in the two major market segments 'General Standard' and 'Prime Standard'

Finanzdienstleistungsaufsicht' (BaFin).[10] The BaFin maintains a database of reported approvals. We use this database as our starting point. Because it is incomplete, we hand-collect additional cases from annual reports.

In a second step we divide all firms with a valid approval into a sample of repurchasing and a sample of non-repurchasing firms. A firm is identified as a repurchasing firm if it publicly announces the initiation of a repurchase program through an ad-hoc disclosure announcement. We collect the announcements from the electronic databases of DGAP[11], Euroadhoc[12], and firms' websites. We obtain announcement dates and timestamps. This procedure results in an initial sample of 589 announcements. We exclude 17 announcements that relate to non-open-market repurchases. To avoid biases induced by confounding events we further exclude 134 announcements where other price-relevant information is disclosed during the event period. The remaining 438 announcements made by 238 different firms constitute our sample of repurchasing firms. Firms often state explicit reasons for the repurchase program in the announcement. Results of previous research (Gerke et al., 2002; Seifert and Stehle, 2003; Hackethal and Zdantchouk, 2006) suggest that the stated reason has an impact on the share price reaction to the announcements. We therefore record the stated reasons and code them into a set of dummy variables.[13] These are included in the abnormal return equation.

In a final step we randomly assign a non-repurchasing firm to each firm announcing a repurchase program. The non-repurchasing firms are selected from the population of firms that possess an approval from the shareholders' meeting but did not announce a repurchase program during the whole period for which the approval was valid. Information for the matched firm is recorded on the day on which the event firm made its repurchase announcement.

Additional data is obtained from various sources. We collect share price data, trading volume, and annual accounting data from *Thomson Reuters Datastream*. All

[10] The Bundesanstalt fuer Finanzdienstleistungsaufsicht (BaFin), the German analogue to the Securities and Exchange Commission, is the federal authority charged with the supervision of securities trading.

[11] DGAP- 'Deutsche Gesellschaft fuer Ad-hoc PublizitÃ¤t mbH': www.dgap.de

[12] Euroadhoc: www.euroadhoc.com

[13] We use the categories 'underperformance', 'excess cash', 'capital structure', 'acquisition' and 'stock option program'. Firms that do not state a reason in their announcements (all remaining) are the base case.

book values are as of the fiscal year before the announcement, whereas market values are based on the third trading day before the announcement. Following Comment and Jarrell (1991) and Bessler et al. (2009) we measure individual past share price performance over the 50 trading days ending on the third trading day before the announcement.

The *free cash flow hypothesis* and the *rent extraction hypothesis* predict that the ownership structure of a firm potentially affects the likelihood of a repurchase announcement as well as the share price reaction to such an announcement. We therefore collect data on disclosed holdings of voting shares for the two largest shareholders from *Hoppenstedt Aktienfuehrer*.[14] We further calculate the ratio of cash flow rights to voting rights for the largest shareholder. This variable proxies for deviations from the one-share-one-vote principle.

Table 2.2 provides a detailed description of all explanatory variables we use in our regressions. Table 2.3 contains descriptive statistics for the event firms and the non-event firms. The event firms have lower market-to-book ratios, higher free cash flows, are less leveraged and have less concentrated ownership (as evidenced by a slightly lower share of the largest shareholder). There are no significant differences with respect to share price performance, size, and dividend yield.

[14]We only consider shareholdings larger than 5% since this is the legal reporting threshold for the most part of our observation period. German listed firms typically exhibit a complex structure of corporate ownership. Pyramiding and cross ownership as well as the use of dual-class shares can induce a wedge between cash flow and voting rights. Hence, we do not rely on shareholdings on the first-tier but follow the procedure proposed by Da Silva et al. (2004) to identify the ultimate controlling shareholder. Based on this methodology, the ultimate controlling shareholder is situated at the first-tier if (i) there is no shareholder holding at least 25% of the voting shares, or (ii) the largest shareholder holding more than 25% is a bank, insurance company, the German state, a foreign company or institution, or a family/individual. In all other cases, the ultimate controlling shareholder is said to be at a higher tier which is reached if criteria (i) or (ii) are satisfied. If a widely held firm is reached at a higher layer, the ultimate control lies with this corporation. In order to track shareholdings from the first-tier to ultimate controlling levels we use (in addition to the *Hoppenstedt Aktienfuehrer*) *Commerzbank-Wer gehoert zu Wem*, a publication on ownership of German firms.

Table 2.2: Description of Key Variables

Table 2.2 provides an overview of our key variables and their definitions.

Variable	**Definition**
Market-to-book ratio	The market value of equity plus debt to the book value of assets of the fiscal year prior to the announcement. The calculation is based on market values 3 trading days before the announcement.
Share price performance	The share price performance is measured by individual buy-and-hold returns over the 50 day period ending 3 trading days prior to the announcement.
Firm size	The natural logarithm of the firm's total assets in the fiscal year before the repurchase announcement.
Free cash flow	The free cash flow is defined as EBIT + depreciation - taxes + delta def. taxes - minority interest - interest - dividends + extra items.
Cash holdings	The firm's cash and cash equivalents relative to the firm's total assets. All items are based on the fiscal year before the repurchase announcement.
Ownership concentration	The Herfindahl index of the firm's ownership structure.
Leverage	The difference of the firm's market leverage ratio and the median ratio of the corresponding industry.
Stake of largest shareholder	The voting rights of the largest ultimate owner ($> 5\%$).
Stake of second largest shareholder	The voting rights of the 2nd largest ultimate owner ($> 5\%$).
Cash-flow-to-voting-rights ratio	The cash flow to voting rights ratio is calculated for the ultimate controlling shareholder.
Managerial ownership	The cumulative voting rights of the managerial board members ($> 5\%$).
2001 Tax reform dummy	Dummy variable that obtains a value of one for announcements made after the 2001 tax reform.
Dividend yield	The firm's dividend per share divided by the share price three trading days before the announcement.
Volatility of cash flows	The standard deviation of operating cash flows over the past five years.
Volatility of dividends	The standard deviation of cash dividends over the past five years.
Earnings per share	The firm's earnings per share on a diluted (adjusted) basis.

Table 2.3: Summary Statistics for Event and Non-event firms

Table 2.3 provides summary statics for the main characteristics of all event and non-event firms. Each sample consists of 438 observations. Tests of difference give the test statistics for the T-test and Wilcoxon rank test.

	Event firms		**Non-event firms**		**Tests of difference**	
	Mean	Median	Mean	Median	T-test	Wilcoxon
Mrket-to-book ratio	2.13	1.49	2.67	1.70	2.34**	2.48**
Share price performance	−0.07	−0.04	−0.04	−0.02	1.25	1.39
Firm size	12.33	12.00	12.47	11.83	0.95	−0.48
Free cash flow	0.05	0.05	0.02	0.04	−2.10**	−2.95***
Leverage	0.00	−0.02	0.05	0.02	3.53***	2.60***
Stake of largest shareholder	0.36	0.33	0.40	0.37	2.71***	2.38**
Dividend Yield	2.24	1.39	2.65	1.30	1.01	0.10

*** significant at 1%; ** significant at 5%;* significant at 10%

2.5 Results

We present and discuss our empirical results in three steps. We start by presenting the results of the probit model. We then describe the results of our event study and finally those of the cross-sectional regression. The objective of the discussion is twofold. First, we wish to analyze whether our conditional approach yields results that are different from those obtained using the traditional approach. Second, we are interested in the economic motives underlying the repurchase decision and in the determinants of the share price reaction to the repurchase announcement.

Probit model - the decision to repurchase

The results of the probit models are shown in Table 2.4. The column labelled ‘Probit’ reports the results that we obtain when we estimate the probit model separately. The column labelled ‘ML’ contains the results that we obtain when we estimate the probit model and the cross-sectional regression jointly. The two sets of results are qualitatively similar. We find that the probability for a repurchase is decreasing in the market-to-book ratio and in leverage, and is increasing in the free cash flow, the interaction between cash holdings and the low market-to-book dummy, and the cash-flow-to-voting-rights ratio. We further find that the component stocks of the high-technology index Nemax are more likely to initiate a repurchase.

Table 2.4: Selection Equation (First-Stage Regressions)

Table 2.4 contains the regression result of the determinants of the decision to repurchase. Results are reported for the probit model and the joint maximum likelihood estimation (ML). In addition to our key analyses variables we employ industry (based on the ICB classifications) and year dummies. T-statistics are based on cluster-robust standard errors.

	Probit	**ML**
Market-to-book ratio	−0.0384***	−0.0426***
Share price performance	−0.1015	−0.1575
Firm size	0.0259	0.0287
Free cash flow	0.7671**	0.5946***
Cash holdings	0.2020	0.4638
High Cash holdings x Low market-to-book ratio	0.2974**	0.2565***
Ownership concentration	−0.1606	−0.0145
Cash holdings x ownership concentr.	−0.0464	−0.0097
Leverage	−0.6890***	−0.6975***
Stake of largest shareholder	−0.2727	−0.2751
Stake of second largest shareholder	0.1212	0.4065
Cash-flow-to-voting-rights ratio	0.8487*	0.8232**
2001 Tax reform dummy	−0.0654	−0.1101
Dividend yield	−0.0044	−0.00462
Volatility of cash flows	−0.0778	0.0019
Volatility of dividends	0.0028	−0.0081
Earnings per share	−0.0046	−0.0047
Nemax	0.2838*	0.3213**
Constant	−0.1015	−0.0583
#Obs.	876	876

*** significant at 1%; ** significant at 5%;* significant at 10%

The variables included in the probit model proxy for the signalling hypothesis, the free cash flow hypothesis, the rent extraction hypothesis, the choice of payout method hypothesis and the capital structure hypothesis. Table 2.5 visualizes the results in a way which facilitates their interpretation. It shows the competing hypotheses, the independent variables which proxy for them, their expected sign and the actual results.

These results are consistent with the free cash flow hypothesis, which correctly predicts that the repurchase probability depends positively on the free cash flow and the interaction between cash holdings and the low market-to-book dummy and depends negatively on the market-to-book ratio and leverage. The result that the repurchase probability is positively related to the cash-flow-to-voting-rights ratio is consistent with the rent extraction hypothesis. The evidence with respect to the signalling and capital structure hypothesis is difficult to interpret because the negative coefficients on the market-to-book ratio and the leverage ratio are also predicted by the free cash flow hypothesis. We find no support for a choice of repurchases that is motivated by the tax system or the tax preferences of the firm's shareholders.

Table 2.5: Expected and Actual Results (Selection Equation)

Table 2.5 shows the competing hypotheses, the independent variables which proxy for them, their expected sign and the actual results for the first-stage regression. An actual result that is conform to its prediction is denoted by a '✓'. A deviation from the prediction is denoted by an 'X'. Statistically insignificant parameters are denoted by a '0'.

Hypothesis	Variable	Expected sign	Probit	ML
Signalling	Tobin's q / Market-to-book ratio	-	✓	✓
	Share price performance	-	0	0
	Firm size	-	0	0
Free cash flow	Free cash flow	+	✓	✓
	Cash holdings	+	0	0
	Tobin's q / Market-to-book ratio	-	✓	✓
	Cash holdings x Low market-to-book ratio	+	✓	✓
	Ownership concentration	+	0	0
	Cash holdings x ownership concentr.	+	0	0
	Leverage	-	✓	✓
Rent extraction	Stake of largest shareholder	-	0	0
	Stake of second largest shareholder	+	0	0
	Cash-flow-to-voting-rights ratio	+	0	✓
Choice of payout method	2001 Tax reform dummy	+	0	0
	Dividend yield	+	0	0
	Volatility of cash flows	+	0	0
	Volatility of dividends	-	0	0
Capital structure	Leverage	-	✓	✓
Earnings per share	Earnings per share	?	0	0

Event study results

The event study results are shown in Table 2.6 and Figure 2.1. Consistent with the previous literature we find large positive abnormal returns. The event day abnormal return is 3.21% and is significant at better than the 1% level. The three-day cumulative abnormal return is slightly larger at 3.55% and is also highly significant. As can be seen from Figure 2.1 the trading volume is abnormally high on the event day (at more than 250% of its normal level) and stays at an elevated level for about eight trading days.

Table 2.6: Cumulative Average Abnormal Returns

Table 2.6 presents the average abnormal returns (AAR) on the announcement date and the cumulative average abnormal return (CAAR) for different periods. Panel A contains the CAARs of different pre announcement periods, Panel B focuses on periods centered on the announcement day, and Panel C reports results for the post announcement period. Abnormal returns are calculated applying the market model. The market index is the CDAX performance index. The estimation period ranges from $\tau = -181$ to $\tau = -21$.

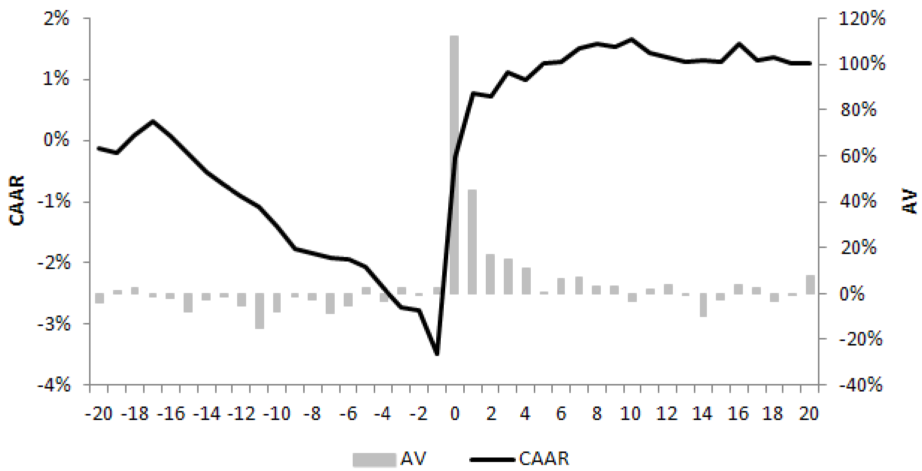
(τ, τ)	**CAAR**	**pos:neg**	**t-test**	**Patell Z**	**BMP**	**Corrado**	**Gen. Sign**
Panel A:Pre Announcement Windows							
(-20;-3)	−2.73%	168:270	-3.78^{***}	-5.52^{***}	-4.61^{***}	-2.75^{***}	-3.90^{***}
(-10;-3)	−1.64%	177:261	-3.49^{***}	-4.52^{***}	-3.83^{***}	-2.26^{***}	-3.04^{***}
Panel B: Announcement Windows							
(-2;2)	3.45%	293:145	8.42^{***}	10.62^{***}	8.12^{***}	4.81^{***}	8.06^{***}
(-1;1)	3.55%	317:121	9.42^{***}	14.93^{***}	11.14^{***}	6.74^{***}	10.26^{***}
(0;0)	3.21%	341:097	12.52^{***}	22.53^{***}	12.86^{***}	10.24^{***}	12.65^{***}
Panel C: Post Announcement Windows							
(3;10)	0.92%	225:213	2.13^{**}	2.14^{**}	2.15^{**}	1.54	1.55
(3;20)	0.55%	223:215	0.94	0.80	0.86	1.22	1.36

*** significant at 1%; ** significant at 5%;* significant at 10%

Consistent with previous results we find significantly negative abnormal returns prior to the event date. In the 18 [8] day window extending from day -20 [-10] until day -3, the cumulative abnormal return is -2.73% [-1.64%], significant at the 1% level. This pattern is consistent with timing attempts. Managers announce a repurchase after a period of negative share price performance. If such timing occurs, the occurrence of the event (the repurchase announcement) is non-random, and a conditional estimation approach is warranted.

Figure 2.1: Cumulative Average Abnormal Returns

Figure 2.1 depicts the cumulative average abnormal return for open market repurchase announcements over the 41 day period (-20; 20). Abnormal returns are calculated applying the market model. The market index is the CDAX performance index. The estimation period ranges from $\tau = -181$ to $\tau = -21$. Abnormal trading volume is calculated as described in Brav and Gompers (2003).



Cross-sectional regression

The results of the cross-sectional regression are shown in Table 2.7. The column labelled 'LS' shows the results of a simple OLS regression without correction for the selection bias. The column labelled 'Heckman' displays the results that we obtain when we estimate the cross-sectional regression separately but include the Mill's ratio from the first-stage probit model as an additional explanatory variable. The column labelled 'ML' contains the results of the simultaneous maximum likelihood estimation.

The results of the three models are qualitatively similar in many respects. However there are differences in detail which are worth mentioning. These differences lead us to other interpretations in the identification of potential value drivers after the announcement of open market share repurchases. Most importantly, the stake of the

Table 2.7: Abnormal Return Equation (Second-Stage Regressions)

Table 2.7 presents the results on the determinants of abnormal returns for repurchase announcements. Abnormal returns are calculated for the single announcement day. In addition to our key analyses variables we consider the reasons stated by repurchasing firms within their ad-hoc messages. Moreover, we employ industry (based on the ICB classifications) and year dummies. T-statistics are based on cluster-robust standard errors.

	LS	**Heckmann**	**ML**
Reason 'underperformance'	0.0097	0.0094	0.0089
Reason 'excess cash'	0.0172	0.0173*	0.0175*
Reason 'capital structure'	−0.0066	−0.0067	−0.0062
Reason 'acquisition'	0.0143***	0.0142***	0.0144***
Reason 'stock option program'	−0.0007	−0.0003	−0.0007
Market-to-book ratio	−0.0003	−0.0003	0.0005
Share price performance	−0.0217**	−0.0199*	−0.0204**
Managerial ownership	−0.0161	−0.0159*	−0.0181**
Managerial ownership x Share price performance	−0.3193	−0.0323*	−0.0333**
Firm size	−0.0016	−0.0020	−0.0019
Free cash flow	0.0297	0.0211	0.0180
Free cash flow x Reason 'acquisition'	−0.1221***	−0.1233***	−0.1189***
Cash holdings	0.0146	0.0081	0.0083
High Cash holdings x Low market-to-book ratio	0.0001	0.0040	0.0078
Leverage	0.0064	0.0190	0.0036
Stake of largest shareholder	0.0207	0.0272	0.0283**
Stake of second largest shareholder	0.0286	0.0207	0.0214
Cash-flow-to-voting-rights ratio	0.0214	0.0244	0.0145
Earnings per share	−0.0018*	−0.0017*	−0.0017
Nemax	0.0277***	0.0222**	0.0223***
Constant	0.0155	0.0014	0.0343
Mill's ratio	–	0.0281***	–
Standard error σ	–	–	0.0506***
Correlation ρ	–	–	0.5623***
#Obs.	438	438	876

*** significant at 1%; ** significant at 5%;* significant at 10%

largest shareholder and the interaction term between managerial ownership and share price performance are insignificant in the LS model but turn significant once we control for selection bias. Furthermore, the coefficient on the Mill's ratio as proxy for the private information of managers is significant in the Heckman and the ML estimation. In the sequel we present and discuss the results of the ML estimation.

The CARs are significantly negatively related (minimum 5%-level) to the prior share price performance, to managerial ownership, the interaction term between managerial ownership and share price performance, and the interaction term between free cash flow and reason 'acquisition' dummy. Repurchase announcements by firms that are included in the high-tech index Nemax trigger higher CARs. With respect to the self-reported repurchase reasons we find that the CARs are larger when the firm announces that it repurchases shares because the management want to use the raised money to finance future acquisitions. Furthermore, firms that are controlled by large blockholders exhibit significantly higher returns after repurchase announcements.
Table 2.8 (constructed in the same way as Table 2.5 above) is intended to facilitate the interpretation of our results. Our main finding is that once controlling for selection bias, the results provide strong support for the signalling hypothesis. The negative coefficient on the prior share price performance as well as the negative coefficient on the interaction term between managerial ownership and share price performance support the signalling hypothesis. We use the interaction term between managerial ownership and share price performance because when we employ simply managerial ownership as proxy we cannot distinguish the following two opposing effects: On the one hand, the signal on undervaluation seems to be more credible when the management owns a greater fraction of the shares. However, on the other hand, a greater managerial ownership could also mitigate potential agency problems and therefore reduce potential benefits of the repurchase to that end. Therefore, the more appropriate test for signalling credibility is in our view a test of the interaction term of underperformance and managerial ownership. The support for the signalling hypothesis is corroborated by the fact that the coefficient on the Mill's ratio in the Heckman model is positive and significant. As discussed in section 4, this finding can be interpreted in two different ways. First, in the original spirit of the Heckman approach, it indicates that the sample of firms announcing a repurchase is not random. Put differently, there is selection bias. Second, the significant coefficient implies that the repurchase announcement reveals private information (previously only held by the managers of the firm) to investors.

As is apparent from equation (2.9) above, the coefficient on the Mill's ratio is the product of the standard deviation of the error term ϵ_i and the correlation between

Table 2.8: Expected and Actual Results (Abnormal Return Equation)

Table 2.8 shows the competing hypotheses, the independent variables which proxy for them, their expected sign and the actual results for the second-stage regression. An actual result that is conform to its prediction is denoted by a '✓'. A deviation from the prediction is denoted by an 'X'. Statistically insignificant parameters are denoted by a '0'.

Hypothesis	Variable	Expected sign	LS	Heckman	ML
Signalling	Tobin's q / Market-to-book ratio	-	0	0	0
	Share price performance	-	✓	0	✓
	Managerial ownership x Share price performance	-	0	0	✓
	Firm size	-	0	0	0
Free cash flow	Free cash flow	+	0	0	0
	Free cash flow x Reason 'acquisition'	-	✓	✓	✓
	Cash holdings	+	0	0	0
	Tobin's q / Market-to-book ratio	-	0	0	0
	Cash holdings x Low market-to-book ratio	+	0	0	0
	Leverage	-	0	0	0
Rent extraction	Stake of largest shareholder	+	0	0	✓
	Stake of second largest shareholder	-	0	0	0
	Cash-flow-to-voting-rights ratio	-	0	0	0
Capital structure	Leverage	-	0	0	0
Earnings per share	Earnings per share	?	0	0	0

the error terms in the probit model and the cross-sectional regression. The joint ML estimation approach allows us to decompose the coefficient into these two components and to separately test them for statistical significance. The results, reported in the third column of Table 2.7, indicate that both components are significant. As discussed in section 4 the significant correlation implies that the parameter estimates of the traditional (i.e., non-conditional) cross-sectional regression are inconsistent.

Furthermore, the significant and negative coefficient on the interaction term between free cash flow and reason 'acquisition' dummy and the positive and significant coefficient on the variable stake of largest shareholder lend partial support to the free cash flow and the rent extraction hypotheses. The latter finding cannot be confirmed by the LS and the Heckman specifications.

Overall, we can summarize that using a conditional estimation approach in the context of repurchase announcements lead not only to more efficient estimates but also, at least to a certain extent, to the identification of different value drivers.

2.6 Conclusion

In this paper we jointly analyze the decision to announce an open market share repurchase and the share price reaction to the announcement. We use a conditional estimation approach. This approach takes into account that the repurchase decision is made rationally and that, consequently, there is a potential selection bias.

According to Prabhala (1997, p. 32) a conditional approach is warranted 'when one has, in addition to data on firms announcing the event, a set of non-event firms, that is, firms that were partially anticipated to announce but chose not to announce the event in question'. The institutional rules for share repurchases in Germany allow us to construct such a non-event control sample. The shareholders' meeting first has to approve a share repurchase program. The approval allows the managerial board to initiate a repurchase program but does not require it to do so. This two-step procedure allows us to construct an event sample (firms with approval from the shareholders' meeting that did announce a repurchase program) and a non-event sample (firms with approval from the shareholders' meeting that did not announce a repurchase program).

Our results demonstrate that a conditional estimation approach (which is preferable from a theoretical point of view) yields results that are qualitatively comparable but differ in detail from those obtained using a non-conditional approach. Most importantly, we find strong support for the signalling hypothesis once controlling for the selection bias. We therefore conclude that a conditional approach should be used whenever the requirements (i.e., the existence of a suitable non-event control group) are met.

Of course one could take an alternative point of view here and argue that the differences we document are not important enough to merit the additional complexity of the conditional estimation approach. Irrespective of the point of view, though, our paper makes one important contribution. It considers a setting that is well-suited for the (theoretically superior) conditional approach and documents how the results of the traditional and the conditional approach differ. This enables researchers to make a more informed choice of methodology.

Our event study results confirm earlier findings of negative share price performance prior to the repurchase announcement and positive and significant announcement day abnormal returns. The results of our probit models are consistent with the free cash flow hypothesis and provide at least partial support for the rent extraction, signalling and capital structure hypothesis. In addition, the results of the cross-sectional regressions provide strong support for the signalling hypothesis once we control for selection bias. We find only weak support for the free cash flow and rent extraction hypothesis.

Chapter 3

Dividends, Stock Repurchases, and the Lintner Model: A Dynamic Panel Data Analysis of German Firms

3.1 Introduction

The question of how firms decide on the amount of cash to be disbursed to shareholders has attracted the attention of financial economists for decades. Lintner's (1956) partial adjustment model still is the workhorse of empirical investigations of corporate payout decisions.[1] He developed his model in a period when dividends were the dominant form of payouts. More recently, however, the volume of stock repurchases has caught up, and there have been years in which the volume of stock repurchases by listed U.S. firms has surpassed the volume of dividends (Grullon and Michaely, 2002). Skinner (2008, p. 608) concludes that his empirical evidence "suggests that repurchases have become the dominant form of payout".

In spite of this, however, in most empirical applications Lintner's (1956) partial adjustment models of payout policy is applied to dividend payouts rather than to total payouts. There are only very few exceptions in the literature. Grullon and Michaely (2002) estimate a traditional dividend-based Lintner model and then relate the resulting dividend errors (the difference between actual and predicted dividends) to the repurchase volume. The only paper we are aware of that estimates a Lintner model based on total payouts is Skinner (2008). He uses two restricted samples, one consisting

[1] For recent applications see, among others, Andres et al. (2009), Chemmanur et al. (2010), and Skinner (2008). For a recent theoretical paper that builds on Lintner's model see Lambrecht and Myers (2012). There are also critical voices, though. De Angelo et al. (2008) argue that the model has lost some of its descriptive ability, mostly because the number of firms that have a well-defined target payout ratio has decreased.

of firms that repurchase and pay dividends in most years, and one consisting of firms that repurchase and never pay dividends. So far no paper estimates a Lintner model on full payouts for a comprehensive sample that permits to draw general conclusions on the choice between dividends and repurchases.

The correct specification depends on the economic reasons that drive the choice between dividends and stock repurchases. When both are good substitutes for each other (as argued by Miller and Modigliani (1961), and as would be the case in a world of perfect capital markets) the model should be better at explaining total payouts rather than dividends. Tax-based explanations predict that firms choose the payout method that receives the more favorable tax treatment. In this case the correct model specification may depend on the tax regime. The financial flexibility hypothesis brought forward by Jagannathan et al. (2000) states that dividends are used to pay out permanent earnings while stock repurchases are used to pay out transitory earnings. In this case the estimation of a Lintner-type model requires the decomposition of earnings into a permanent and a transitory component.

In this paper we put the cart before the horse. We estimate different versions of Lintner-type partial adjustment models. The results then allow us to draw inferences on the motives underlying the choice between dividends and stock repurchases. Our sample is a large panel of German firms covering the period 1988-2008. This sample has two distinct advantages. First, stock repurchases were essentially prohibited until 1998. Therefore, we can analyze how the introduction of an alternative to dividends affects corporate payout decisions. Second, a major change in the tax system in 2001 affected the relative attractiveness of dividends and stock repurchases. This allows us to investigate the importance of tax considerations for corporate payout decisions in general, and the choice between dividends and stock repurchases in particular.

Our results can be summarized as follows. The introduction of repurchases in 1998 has materially affected the payout policy of German firms. This is inconsistent with the substitutes hypothesis. We find no evidence that German firms have altered their payout policy in response to the 2001 tax reform. Our results provide support for the flexibility hypothesis. Our results imply that dividends are more sticky than total payouts. This is consistent with the prediction of the flexibility hypothesis that dividends are predominantly paid out of permanent earnings. We further document that, after the introduction of repurchases, the responsiveness of dividends to changes in transitory earnings is reduced substantially. This corroborates the evidence in favor of the flexibility hypothesis.

Our paper contributes to the literature in several ways. First, it is the first paper that uses a partial adjustment model to analyze how the introduction of stock repurchases affects the magnitude and determinants of dividend payouts. Second, we test whether a Lintner-type partial adjustment model is better suited to model dividend payouts or total payouts. Different from Skinner (2008), we do not restrict our sample to firms with a particular history of payout decisions. Third, we decompose earnings into a permanent and a temporary component. We then integrate both components in a partial adjustment model in order to test the hypothesis (brought forward by Jagannathan et al., 2000) that dividends are used to disburse permanent earnings while stock repurchases are used to pay out temporary earnings. To the best of our knowledge, this is the first paper that tests the flexibility hypothesis within a Lintner-type partial adjustment model. In our empirical analysis we use GMM-in-systems estimations, and we explicitly consider the role of special dividends (which, prior to the introduction of stock repurchases, might have been used to disburse temporary earnings).

The remainder of the paper is organized as follows. Section 3.2 describes the institutional setting in Germany. Section 3.3 develops our hypotheses. Section 3.4 presents the sample and descriptive statistics. In section 3.5 we describe the econometric methodology and the results, section 3.6 concludes.

3.2 The Institutional Setting

As pointed out above, the development of the institutional framework in Germany provides an ideal environment for our research questions. During the first half of our sample period (1988-1997) stock repurchases were effectively prohibited. In 1998 a new law came into force which allowed stock repurchases. Besides this change in regulation there was also a major change in the taxation system. Until 2001 Germany operated a full imputation system that favored dividend payouts over repurchases for most investor types. After the 2001 tax reform the tax preference of most investors shifted towards repurchases.

In the following, we describe several aspects of the institutional environment of German firms as well as relevant changes over the sample period. These issues are dividends (section 3.2.1), stock repurchases (3.2.2) and the tax treatment of dividends and stock repurchases (3.2.3).

3.2.1 Dividends

German firms pay annual (rather than quarterly) dividends. The payout decisions of German Stock Corporations (Aktiengesellschaften) are governed by §58 of the Stock Corporation Act (Aktiengesetz, AktG).[2] The executive and the supervisory board can decide to retain up to 50% of the profits. The decision on whether to retain or to pay out the remaining amount is taken by the shareholders' meeting by simple majority vote. In practice it is almost always the case that the shareholders' meeting votes in favor of the proposal made by the executive board. The payment date is usually the business day following the day of the annual shareholders' meeting.[3]

Firms may pay special designated dividends (Sonderdividenden). They are of particular interest for our study because special designated dividends may be close substitutes for stock repurchases (De Angelo et al., 2000).[4] This, in turn, is of particular importance because repurchases were essentially prohibited until 1998.

[2] The following description relates to the standard case. The articles of incorporation may allow for deviations from this standard procedure. In addition, §150 AktG prescribes that a firm has to retain at least 5% of its earnings as long as the total amount of retained earnings amounts to less than 10% of the dedicated capital (Grundkapital).

[3] Some firms have issued both common shares and non-voting preferred shares. The preferred shareholders are entitled to a cumulative minimum dividend (§139 AktG). These claims have priority over dividend payments to common shareholders. If the minimum preferred dividend is not paid in a given year, it is cumulated and has to be paid out in later years. If the dividend is not paid for two consecutive years, owners of preferred shares are entitled to a temporary voting right (§140 AktG), until the cumulated minimum dividend has been paid. In addition, non-voting shares are often entitled to an excess dividend, i.e. a dividend that is larger by a specified amount than the dividend paid to common shareholders.

[4] This view is supported by empirical results in Brickley (1983). For a sample of U.S. firms he finds higher dividend payouts in the year following a dividend increase than in the year following a special designated dividend. This indicates that special designated dividends are weaker signals of higher future payouts than increases of regular dividends.

3.2.2 Stock Repurchases

Until 1998, stock repurchases were essentially prohibited in Germany.[5] In 1998, a new law came into force that allowed stock repurchases. Under this law firms are allowed to buy back up to 10% of their shares.[6] A firm wishing to buy back shares has to follow a standardized procedure. As a first step the shareholders' meeting (with simple majority) has to grant the managerial board the permission to buy back shares. This permission has to specify the maximum number of shares to be bought back (not more than 10% of shares outstanding), the minimum and maximum price to be paid per share, and the time of validity of the permission (initially not longer than 18 months; increased to 5 years in 2008).

This permission gives the managerial board the right, but not the obligation, to buy back shares.[7] Once the board decides to actually initiate a repurchase program the firm has to communicate this fact to the public. This is mandated by the German securities trading act (Wertpapierhandelsgesetz) which requires that listed firms immediately disclose information that is likely to materially affect security prices ("ad-hoc disclosure"). Empirical studies analyzing the impact of stock repurchase announcements on share prices typically use the date of the ad-hoc disclosure as the event date (e.g. Gerke et al., 2002; Schremper, 2003; Seifert and Stehle, 2003; Hackethal and Zdantchouk, 2006; Bessler et al., 2012).

Firms are required to treat all shareholders equally. This precludes negotiated repurchases from large shareholders. Open market repurchases, repurchase tender offers and transferrable put rights are admissible, though. Open market repurchases are the dominating form.

As is the case in the U.S., the announcement of a stock repurchase still does not require the managerial board to actually repurchase shares. The actual amount of

[5] Firms could acquire their own shares only under restrictive conditions (e.g. to prevent damage). Although there is some disagreement in the literature as to the actual number of repurchases in Germany prior to 1998 (see Seifert, 2006, for a discussion) it is safe to conclude that stock repurchases were not used as a means of disbursing cash to shareholders prior to 1998.

[6] The 10% threshold applies to an individual repurchase program, not to the total amount of repurchases during the life of the firm.

[7] Given permission through the annual meeting, the decision to initiate a repurchase program is taken by the executive board and approved by the supervisory board.

repurchases is published in the firm's financial statement. This information thus allows us to identify the repurchase volume in a fiscal year.

Since 2004 new regulation adopted by the European Union imposes additional restrictions on stock repurchases. Individual transactions made as part of a repurchase program now have to be reported within seven trading days. Further, there are restrictions on the prices at which open market repurchases can be made (not higher than the price of the previous transaction) and on the maximum daily repurchase volume (not more than 25% of the average daily volume on the market on which the trade is made).

Finally, there are two ways in which the firm can handle the repurchased shares. First, it can treat them as an asset on the asset side of the balance sheet. They can then be used to cover outstanding convertible bonds or executive stock options. The maximum number of shares a firm can hold on its balance sheet is 10% of the shares outstanding. Alternatively, the firm can reduce the number of shares outstanding. In this case the firm's book equity is reduced by the repurchase volume.

3.2.3 Taxation of Dividends and Repurchases

The tax treatment of dividends and repurchases underwent a major change in 2001. Until 2001 Germany operated a full imputation system. Dividends paid to domestic investors were essentially taxed at the investor's personal tax rate.[8] Retained earnings were taxed at a corporate tax rate. Capital gains were tax exempt when the shares were held for more than six months (twelve months from 1999 onwards). Consequently, investors with a personal tax rate below the corporate rate on retained earnings favored dividends over repurchases while investors with a tax rate above the corporate rate favored repurchases. The latter group was usually small as the corporate tax rate on retained earnings was very close to the highest marginal tax rate on personal income. Corporate shareholders had a preference for dividends, as they received dividends tax free while capital gains were taxed at the corporate tax rate. Foreign investors did not

[8]Dividends were first taxed at the firm level. Domestic investors received the gross dividend plus a tax credit equal to the tax paid by the firm. The gross dividend was taxed at the investor's personal tax rate. The resulting tax liability was then offset against the tax credit.

receive the tax credit and may therefore have had a preference for repurchases.

Since 2001 dividends and retained earnings are taxed at the same rate at the corporate level. At the personal investor level, half of the net dividend is taxed at the investor's personal tax rate. Capital gains are not taxed when the shares are held for more than one year. When this condition is met individual investors should thus have a clear preference for repurchases over dividends. For corporate investors, dividend payments were essentially tax-free.[9] At the same time, capital gains from the sale of shares held in another company were also tax-exempt. Corporate investors were thus largely indifferent between dividend and repurchases.

In summary, while the preference for dividends versus repurchases depended on the status (domestic versus foreign) and the personal tax rate of the investor prior to 2001, there should be a clear preference for repurchases after 2001. We thus expect a shift from dividends to repurchases after the 2001 tax reform.

3.3 Hypotheses

Lintner's (1956) model is based on the presumption that firms have a target payout ratio. Therefore, changes in earnings translate into payout changes. The adjustment is not immediate, though. Rather, firms adjust their payout only partially towards the new target level. In its simplest form the model thus yields the adjustment process

$$\Delta D_{i,t} = \alpha_i + c_i(D^*_{i,t} - D_{i,t-1}) + u_{i,t} \tag{3.1}$$

$$D^*_{i,t} = r_i P_{i,t} \tag{3.2}$$

where $D_{i,t}$ denotes the dividend of firm i in period t, $P_{i,t}$ denotes profits, $D^*_{i,t}$ are the desired dividend payments, r_i is the target payout ratio for firm i and c_i is the speed-of-adjustment coefficient.

The model was developed at a time when stock repurchases were very rare. Therefore, it only considered dividend payouts. Despite the growing importance of

[9] Since 2004 5% of the received dividend had to be declared as revenue and was therefore subject to the corporate tax rate.

repurchases most researchers have continued to use Lintner's framework to model dividend payouts only. Two notable exceptions are Grullon and Michaely (2002) and Skinner (2008). Whether it is appropriate to model total payouts rather than dividend payouts is an open question, though. It hinges on the motives why firms choose dividends or repurchases.

In a world without differential tax treatment of dividends and repurchases or other frictions, the two payout methods would be perfect substitutes.[10] Grullon and Michaely (2002) report empirical evidence that US firms increasingly use repurchases as substitutes for dividends. If indeed dividends and repurchases were close substitutes for each other it would be appropriate to apply the Lintner model to total payouts rather than to dividends only.

An implication of perfect substitutability would be that a firm's payout decision does not depend on the available menu of payout methods. Consequently, under perfect substitutability the introduction of repurchases should not affect the payout policy. This leads to our first hypothesis.

H3.1 (substitutes): The introduction of repurchases in 1998 does not affect the parameters of a Lintner model of total payout.

When the tax system treats dividends and repurchases differently, firms have an obvious reason to prefer one payout method over the other. As explained in section 3.2, the German tax reform in 2001 has made repurchases more attractive. We thus have

H3.2 (taxes): The 2001 tax reform results in a reduction of the (target) dividend payout ratio and a corresponding increase in the amount of repurchases.

So far we have assumed that (absent differential tax treatment) dividends and repurchases are good substitutes. This need not be the case, though. The earnings of a firm may consist of a permanent component and a transitory component. Managers

[10] The assumption of a frictionless world is not a necessary condition for the substitute hypothesis to hold. The principal-agent models of Easterbrook (1984) and Jensen (1986) imply that managers pay dividends in order to disburse free cash flow and thus to reduce agency costs. A similar argument can be made in favor of repurchases. In signaling models of payout decisions (Bhattacharya, 1979; Miller and Rock, 1985) managers use dividends to signal information about future profitability. In a similar way, repurchases could be used as signals. Thus, both the principal-agent models and the signaling models are consistent with the substitute hypothesis.

may be reluctant to increase dividends in response to high transitory earnings because the higher dividend level may not be sustainable, and managers typically try to avoid dividend cuts.[11] Against this background, Jagannathan et al. (2000) have argued that firms use dividends to disburse permanent earnings but use repurchases to pay out transitory earnings. This is referred to as the flexibility hypothesis. The survey results reported in Brav et al. (2005) as well as the empirical evidence in Guay and Harford (2000) support the flexibility hypothesis.

H3.3 (flexibility / payout): Changes in dividend payouts are caused by changes in permanent earnings but unrelated to changes in transitory earnings.

The flexibility hypothesis implies that repurchases track the more volatile transitory component of earnings. Consequently, we should expect that repurchases are adjusted quickly to changes in (transitory) earning. This implies the following hypothesis.

H3.4 (flexibility / speed of adjustment): The speed of adjustment coefficient will be larger in a Lintner model of total payout than in a Lintner model of dividends.

As noted in section 3.2, firms can use special designated dividends to disburse transitory cash flows. The flexibility hypothesis implies that firms used special dividends for that purpose prior to 1998 when repurchases were essentially prohibited. With the introduction of repurchases the importance of special dividends should decline. This should hold in particular after the 2001 tax reform which puts dividends at a disadvantage relative to repurchases.

H3.5 (special dividends): Special designated dividends lose importance after 1998.

We note that De Angelo et al. (2000) reject the hypothesis that special dividends were displaced by repurchases. We believe, though, that the German setting, where repurchases were prohibited prior to 1998 warrants a reconsideration of this hypothesis.

Young firms tend to have volatile earnings and may therefore be reluctant to initiate dividend payments (Fama and French, 2001). They may, however, be willing

[11] Michaely et al. (1995) show that the negative market reaction after dividend cuts is stronger than the positive market reaction after dividend increases.

to repurchase shares.[12] We therefore expect that the fraction of firms that do not distribute cash to shareholders (using either dividends or repurchases) decreases after 1998.

H3.6 (fraction of non-payers): The fraction of firms that do not pay out cash to shareholders decreases after 1998.

3.4 Sample and Data Description

In this section, we describe the construction of our sample and present summary statistics. The descriptive analysis will already provide a first indication of the validity of some of our hypotheses.

3.4.1 Sample Selection

Our sample covers all non-financial firms listed on the Frankfurt Stock Exchange that were among the largest 200 non-financial firms in Germany (as measured by total assets[13]) at any time during the 21-year period 1988-2008. This results in an initial sample of 424 firms. Our sample on average covers 67.2% of the aggregate market capitalization of all listed firms in Germany.

We drop firms-years in which a control agreement was in place.[14] The reason is that firms that are subject to a control agreement do not decide independently on their payout. We further restrict our sample to firms with at least two consecutive firm-year observations. The resulting data set is an unbalanced panel with 4,363 firm-year observations.

Until 1998 domestic firms had to prepare their accounts according to German accounting standards. Between 1998 and 2004 they were allowed (but not required) to apply international accounting standards (IAS/IFRS or US-GAAP) instead. Since

[12] Typically, special dividends are declared *in addition* to regular dividends. Therefore, special designated dividends are not an alternative for these firms.

[13] We measure total assets at year-end. If the fiscal year of a firm is not the calendar year, we estimate the year-end value of total assets as a time-weighted average of the total assets in the previous and following fiscal year.

[14] A control agreement implies that the firm is effectively controlled by a parent company. For a more detailed discussion see Andres et al. (2009).

2005 application of IAS/IFRS is mandatory. A change in the accounting standards can affect reported earnings significantly. We therefore use dummy variables to control for the accounting standards that were applied. In a robustness check, we exclude the first firm-year after a change in accounting standards.[15] The results are similar and are thus omitted.

Information on balance sheet items, items from the income statement and dividends were collected from *Saling/Hoppenstedt Aktienfuehrer*. This is a yearly publication that provides detailed information (e.g., ownership structure, board composition, financial report information) on German listed firms. Values denominated in Deutsche Mark were converted to Euros at the official conversion rate.[16]

The dividend information we collect contains the nominal value and the tax credit (under the imputation system in effect until 2001) as well as any special designated dividend. We further obtain information on the number of shares outstanding. If a firm has several classes of shares (typically common shares and non-voting preferred shares) we calculate the total dividend payout. All values are adjusted for stock splits, stock dividends and changes in the dedicated capital (e.g. due to seasoned equity issues).

We further collect data on stock repurchases for the period 1998-2008. As outlined in section 3.2, the initiation of a repurchase has to be publicly announced. Subsequently, the actual amount of repurchases has to be published in the annual report. We use this information to infer the amount of repurchases in each fiscal year.

German firms typically pay the annual dividend in the second quarter of the fiscal year. These dividends, however, are paid out of earnings of the previous year. Therefore, we link each dividend payment to the fiscal year preceding the year in which the dividend was paid. Thus, as an example, we link the dividend paid in 2004 to earnings in 2003.

Matters are more complicated for repurchases. For example, repurchases occurring early in 2004 are likely to be related to 2003 earnings, while repurchases later in the year may well be made in response to interim earnings figures for 2004. In our baseline specification we treat repurchases like dividends, i.e., repurchases made in 2004

[15] In the first year after a change in accounting standards, first-differenced earnings figures are calculated from two financial reports prepared according to different rules.
[16] The official conversion rate of 1998 is 1.95583 Deutsche Mark per Euro

are linked to earnings for 2003. As a robustness check, we implement an alternative specification. We link repurchases to the earnings of the year in which the repurchase occurs (i.e., repurchases made in 2004 are linked to 2004 earnings).[17]

3.4.2 Descriptive Statistics

Table 3.1 shows summary statistics for per-share earnings, total payouts and regular dividends. Total payout is the sum of regular dividends (including the tax credit until 2001), special dividends (also including the tax credit when applicable) and repurchases (from 1998 onwards).

Table 3.1: Summary Statistics

Table 3.1 provides summary statistics for dividends, total payout and earnings in Euros per share. Total payout is defined as the sum of regular (gross) dividends, special dividends, and stock repurchases. The sample consists of 4,363 firm-year observations over the sample period from 1988 to 2008. Since we do not have information on special dividends for 21 firm-year observations, the number of observation for total payout is reduced accordingly.

	Earnings	Total Payout	Dividends
Mean	15.35	10.13	7.95
Standard Deviation	83.34	43.08	21.98
Coefficient of Variation	5.43	4.86	2.77
Median	5.03	3.99	3.91
Maximum	2,278.00	1,566.00	399.42
Minimum	-1.078.43	0.00	0.00
No. Observations	4,363	4,342	4,363

On average, firms pay out about two thirds of their earnings. Regular dividends on average account for 51.8% of earnings. Total payouts are almost as volatile as earnings (coefficient of variation 4.86 as compared to 5.43). Regular dividends, on the other hand, are much less volatile. Their coefficient of variation is 2.77, about half the corresponding value for earnings. These results are consistent with the stylized fact that "regular dividends are what is smoothed, and not total payouts" (de Angelo et al. 2008, p. 158). The finding that total payouts are much more volatile than dividends is inconsistent with the substitutes hypothesis. If dividends and repurchases were indeed close substitutes there would be no reason to smooth dividends but not total payouts.

[17] The results are similar and are not reported.

Figure 3.1: Composition of Total Payout

Figure 3.1 depicts the composition of total payout over the sample period from 1988-2008. All ratios are based on gross payouts relative to earnings.

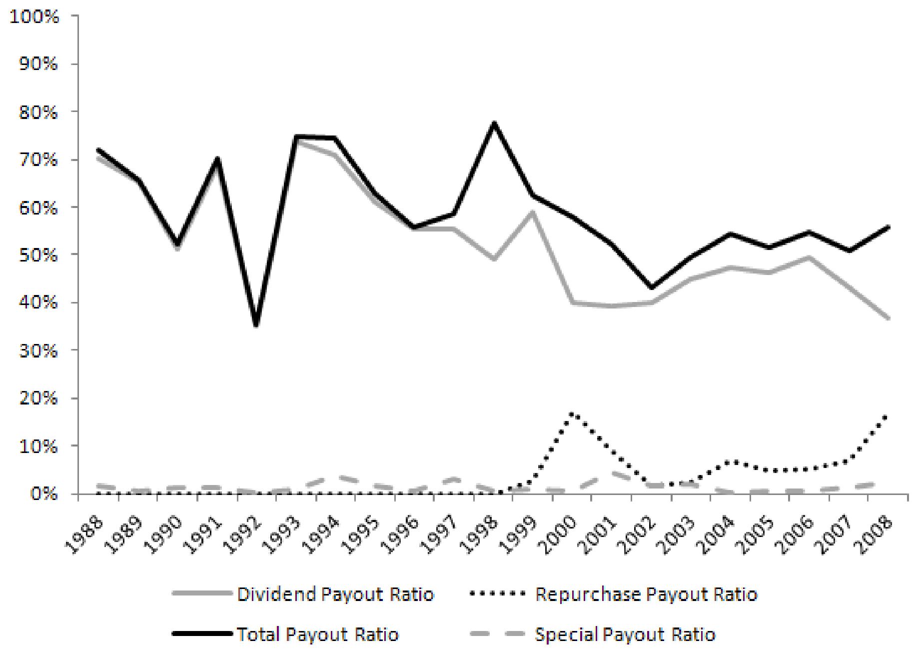


Table 3.2 shows the evolution of dividend payout ratios, special dividend payout ratios, repurchase ratios, and total payout ratios over time. The payout ratios are also displayed in Figure 3.1. Dividend ratios appear to decrease over time. In particular, the average ratio for the pre-repurchase period 1988-1997 is 56.4% while the corresponding value for the repurchase period 1998-2008 is only 44.2%. It is also noteworthy that (contrary to hypothesis H3.2) dividend payout ratios do not decrease after the 2001 tax reform.

The total payout ratio, on the other hand, changed only slightly, from 57.7% to 53.7%. These shifts are consistent with dividends being substituted by repurchases. Interestingly, though, the special dividend ratio does not decrease but rather increases, from 1.3% to 1.5%. This is consistent with De Angelo et al. (2000) but clearly inconsistent with our hypothesis H3.5.

Stock repurchases are much less important in Germany than they are in the U.S. The highest repurchase ratio is 17.2%, observed in 2000. Repurchase ratios are

Table 3.2: Aggregate Payout Ratios

Table 3.2 provides annual information on payout ratios. The data consist of all firm-year observations with positive earnings (therefore, the number of observations is lower than in Table 1). Yearly payout ratios are obtained by relating aggregate payouts (dividends and/or repurchases) to aggregate earnings $\sum_i EARN$. $\sum_i DIV$ is the aggregate dividend payout per year expressed in millions of Euros. Accordingly, $\sum_i SPECIAL$ is defined as the aggregate payout of special dividends, $\sum_i REP$ is the aggregate repurchase volume, and $\sum_i TP$ is the sum of the three aforementioned items. We dropped two special dividends from the sample. Heidelberg Druckmaschinen AG paid a special dividend of €27.71 in 1997. Altana AG paid a special dividend of €33.50 in 2007. This corresponds to a special payout volume of 2,833 Mio. €and 4,732 Mio. €, corresponding to 77.54% and 61.98% of the pre-dividend market value of equity, respectively. We additionally report average payout ratios for the overall sample period (1988-2008), the period before the introduction of stock repurchases (1998-1997), and the period thereafter (1998-2008).

Year	# OBS	$\frac{\sum_i DIV}{\sum_i EARN}$	$\frac{\sum_i SPECIAL}{\sum_i EARN}$	$\frac{\sum_i REP}{\sum_i EARN}$	$\frac{\sum_i TP}{\sum_i EARN}$
1988	147	70.34%	1.73%	-	72.07%
1989	158	65.42%	0.36%	-	65.78%
1990	164	51.16%	1.15%	-	52.31%
1991	170	68.97%	1.11%	-	70.08%
1992	136	35.08%	0.12%	-	35.20%
1993	149	73.89%	0.87%	-	74.76%
1994	151	70.89%	3.66%	-	74.55%
1995	170	61.22%	1.57%	-	62.79%
1996	164	55.33%	0.51%	-	55.84%
1997	155	55.48%	3.16%	-	58.64%
1998	181	62.44%	0.50%	0.02%	62.96%
1999	193	58.91%	1.04%	2.51%	62.46%
2000	227	39.95%	0.63%	17.15%	57.73%
2001	224	39.09%	4.24%	9.05%	52.39%
2002	179	39.90%	1.69%	1.64%	43.24%
2003	169	45.04%	1.98%	2.43%	49.44%
2004	157	47.47%	0.26%	6.81%	54.53%
2005	182	46.21%	0.55%	4.62%	51.38%
2006	184	49.27%	0.41%	5.06%	54.74%
2007	192	43.06%	1.15%	6.71%	50.92%
2008	179	36.90%	2.33%	16.56%	55.79%
1988-97	1,417	56.43%	1.31%	-	57.74%
1998-08	2,067	44.24%	1.53%	7.90%	53.67%
1988-08	3,484	4 7.12%	1.49%	6.16%	54.77%

much lower than dividend ratios in each single year. The low repurchase ratios might be explained by the fact that repurchases were prohibited before 1998 and firms only slowly adopted this additional method of payout. Note, though, that the fact that the largest repurchase ratio in our sample is observed already in 2000 casts doubt on that explanation. An alternative explanation of the low repurchase ratios rests on the restrictive regulation which requires advance approval by the shareholders meeting and limits each individual repurchase program to no more than 10% of the shares outstanding.

Table 3.3 shows the fraction of firms that increased, decreased, or held constant their dividend and total payout, respectively. As the figures for dividends and total payouts are almost identical we concentrate on the former. Dividends are unchanged in more than 35% of the cases. We observe much more increases (about 40%) than decreases and omissions (together 28.3%). This pattern is consistent with managers being reluctant to cut dividends (and total payouts). A similar asymmetry between increases and decreases has also been reported for the U.S. (e.g. Jagannathan et al., 2000; Skinner, 2008) and for Germany (Andres et al., 2009).

As argued above, we expect the fraction of firms that do not distribute earnings to shareholders to decrease after the introduction of stock repurchases in 1998. We therefore divide our sample firms into two groups, those that pay dividends in a particular year and those that do not. The latter group is further decomposed into two subgroups, firms that pay no dividend in a particular year but have paid a dividend in earlier years, and firms that never paid a dividend. The fraction of sample firms in these four groups is depicted in Figure 3.2.

The fraction of dividend-paying firms decreased steadily until about 2003 and then started to rebound. It is noteworthy that the fraction of dividend-paying firms did not decrease in 1998 when repurchases were introduced; if anything, it *increased*. The fraction of non-paying firms is the complement of the fraction of paying firms and is thus not interesting in itself. What is interesting, though, is the decomposition into former payers and firms that never paid out dividends to shareholders. The fraction of the latter group has been close to zero until 1997. It started to increase in 1998 and then reached a plateau in 2001 where it stayed for several years. Since 2005 we observe a slight decline.

The increase in the fraction of firms that never paid out dividends coincides with the introduction of repurchases in 1998 and with the hot IPO market at the end of the 1990s. Thus, the newly listed firms either use repurchases to disburse cash to

Table 3.3: Type of Payout Change

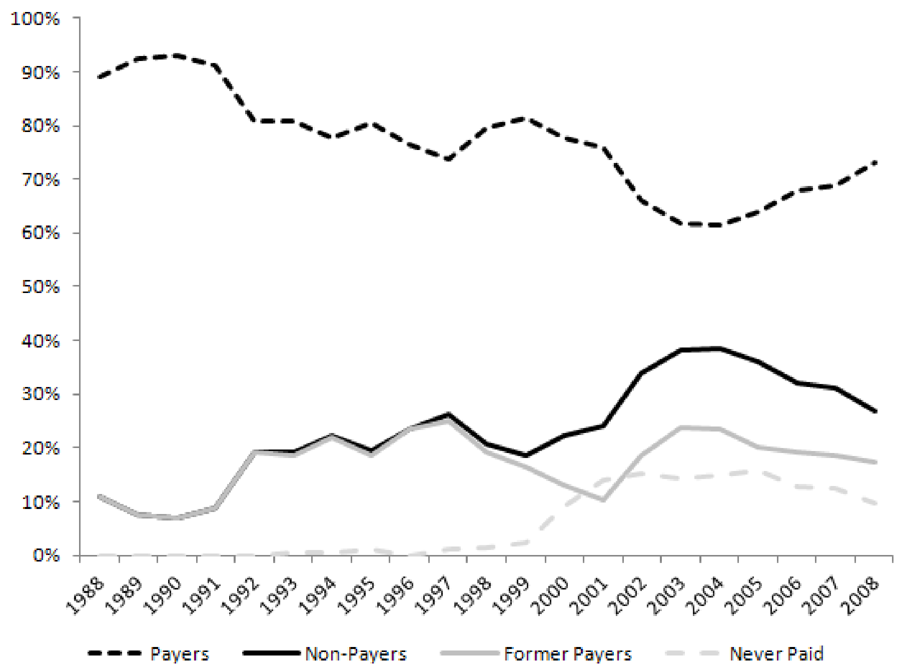
Table 3.3 shows the type and number of payout changes for each year of our sample. A firm can *increase, decrease* or *maintain* its payout relative to the previous year. In case of a decrease, a firm can either reduce or *omit* payouts. Total payout is defined as the sum of regular (gross) dividends, special dividends, and stock repurchases.

	Dividends				*Total Payout*			
Year	Increase	Maintain	Decrease	Omit	Increase	Maintain	Decrease	Omit
1988	45	77	24	4	38	65	22	4
1989	65	72	19	3	67	70	19	3
1990	76	65	19	3	76	65	19	3
1991	72	58	40	10	76	58	36	10
1992	53	53	48	19	53	52	49	19
1993	40	71	39	11	42	70	38	11
1994	67	32	83	15	67	31	84	15
1995	87	63	35	5	83	62	40	5
1996	80	58	47	19	74	58	53	19
1997	72	61	52	16	73	60	52	14
1998	97	62	28	6	94	63	30	6
1999	82	82	39	6	81	79	43	6
2000	86	54	66	8	92	50	64	7
2001	130	48	61	8	124	47	68	7
2002	67	58	108	25	66	56	111	25
2003	61	113	55	23	62	108	59	23
2004	115	70	36	17	109	66	46	19
2005	86	73	58	8	90	67	60	9
2006	107	97	16	6	102	92	26	8
2007	100	98	23	11	106	82	33	10
2008	108	91	19	11	115	73	30	12
1988-97	657 (39.3%)	610 (36.5%)	406 (24.3%)	105 (6.3%)	649 (39.3%)	591 (35.8%)	412 (24.9%)	103 (6.2%)
1998-08	1039 (43.4%)	846 (35.3%)	509 (21.3%)	129 (5.4%)	1041 (43.5%)	783 (32.7%)	570 (23.8%)	132 (5.5%)
1988-08	1696 (41.7%)	1456 (35.8%)	915 (22.5%)	234 (5.8%)	1690 (41.8%)	1374 (34.0%)	982 (24.3%)	235 (5.8%)

their shareholders, or they do not disburse cash at all. We find that the latter is the dominant case. Most of the firms that never paid dividends (287 firm-year observations in the 1998-2008 period) do not repurchase shares either. Our data set only contains 35 firm-year observations (12.2%) in which a firm that never paid a dividend repurchases shares.

Figure 3.2: Percent of Sample Firms in Different Dividend Groups

Figure 2 depicts the distribution of the different dividend groups. A firm-year observation is defined as '*payer*' if a firm pays a regular dividend in the relevant year. Otherwise, the observation is defined as '*non-payer*'. For each non-payer we additionally track the whole history of dividend payments. If a company has never paid a regular dividend since its IPO, we define this firm-year observations '*never paid*' A '*former payer*' is defined as a firm that is currently not paying a regular dividend, but did so in at least one firm-year after going public.



3.5 Methodology and Results

The descriptive statistics presented in the previous section already give some indication about the validity of our hypotheses. To draw further conclusions, we run a set of multivariate regressions that are derived from Lintner's (1956) model of dividend payouts. In the following, we explain in detail how the original model is adapted to test changes in the payout policy of our sample firms.

3.5.1 Model specifications

The starting point of our analysis is the Lintner (1956) model in its simplest form[18]

$$\Delta D_{i,t} = \alpha_i + c_i(D^*_{i,t} - D_{i,t-1}) + u_{i,t} \tag{3.3}$$

$$D^*_{i,t} = r_i P_{i,t} \tag{3.4}$$

where α_i is a constant, c_i is the speed of adjustment coefficient, $P_{i,t}$ are after-tax earnings, $D_{i,t}$ are dividend payments, $\Delta D_{i,t}$ is the change in dividend payments, $D^*_{i,t}$ are the desired dividend payments and r_i is the target payout ratio for firm i. Equation (3.3) models partial adjustment towards the desired level of dividends $D^*_{i,t}$, provided that $0 \leq c_i \leq 1$. The two polar cases correspond to complete adjustment ($c_i = 1$) and no adjustment ($c_i = 0$) towards the desired payout level.

Substitution of (3.4) into (3.3) yields

$$\Delta D_{i,t} = \alpha_i + b_i P_{i,t} + d_i D_{i,t-1} + u_{i,t} \tag{3.5}$$

where $b_i = c_i r_i$ and $d_i = (1 - c_i)$. It is common to assume that the target payout ratio and the speed of adjustment coefficient are constant across firms (Andres et al., 2009; Fama, 1974; Skinner, 2008). Adding year-fixed effects ($YEAR_t$)[19] and firm-fixed effects (η_i, to capture firm-specific heterogeneity) yields the baseline specification

$$D_{i,t} = bP_{i,t} + dD_{i,t-1} + YEAR_t + \eta_i + \upsilon_{i,t} \tag{3.6}$$

This specification considers (regular) dividends only. Denoting special dividends by $S_{i,t}$ and repurchases by $R_{i,t}$ we obtain a model based on total payouts

$$(D_{i,t} + S_{i,t} + R_{i,t}) = bP_{i,t} + d(D_{i,t-1} + S_{i,t-1} + R_{i,t-1}) + YEAR_t + \eta_i + \upsilon_{i,t} \tag{3.7}$$

[18] We also estimate a model in which we additionally include lagged earnings as suggested by Fama and Babiak (1968). The results are similar and are not reported.

[19] We re-estimated all models without the year-fixed effects. The results remain qualitatively unchanged.

Under the substitute hypothesis model (3.7) is a reasonable specification.

Hypothesis H3.1 states that the introduction of repurchases in 1998 does not affect the parameters of a Lintner model of total payouts. To test this hypothesis we define a dummy variable which is set to 0 before 1998 and set to 1 from 1998 onwards. This dummy variable is interacted with the independent variables of the total payout model (3.7). The coefficient estimates allow us to test whether the target total payout ratio and/or the speed of adjustment changed after the introduction of repurchases in 1998.

Hypothesis H3.2 states that the 2001 tax reform should result in a reduction of dividend payout ratios. To test this hypothesis we augment our baseline model (the dividends-only model (3.6)) with a dummy variable which is set to 0 before the tax reform and set to 1 thereafter. The dummy variable is interacted with the independent variables. The coefficient estimates allow us to test whether the target dividend payout ratio and/or the speed of adjustment changed after the tax reform.

The flexibility hypothesis (our hypothesis H3.3) states that dividends are paid out of permanent earnings while repurchases (and special dividends) are paid out of transitory earnings. Model (3.7) is then an inappropriate specification because it does not differentiate between the two components of earnings.

Testing the flexibility hypothesis requires decomposing earnings into a permanent and a transitory component. We use the following simple procedure. We define permanent earnings $PermP_{i,t}$ to be the three-year moving average of earnings.[20] Transitory earnings $TransP_{i,t}$ is defined to be the deviation between total and permanent earnings.

As a robustness check we implement two alternative specifications. First, we use a five-year moving average instead of a three-year moving average. Second, we estimate an AR(1)-model for each firm. The predicted values are then interpreted as the permanent component of earnings while the residual is interpreted as the transitory component. The results for these alternative specifications are similar to those shown in Table 3.7

[20] Our choice of three-year moving averages is inspired by the definition of cash flow shocks in Guay and Harford (2000). They consider shocks in cash-flows as the average of cash-flows in years $t = 0$ and $t = -1$ and measure the permanence in cash flow shocks as the difference between a three-year post-shock cash-flows period ($t = 1$, $t = 2$ and $t = 3$) and a three-year pre-shock cash-flows period ($t = -4$, $t = -3$ and $t = -2$).

and are therefore omitted.

We estimate the following model

$$D_{i,t} = bPermP_{i,t} + kTransP_{i,t} + dD_{i,t-1} + YEAR_t + \eta_i + \upsilon_i \quad (3.8)$$

If dividend changes reflect changes in permanent earnings the coefficient k in equation (3.8) should, according to hypothesis H3.3, be zero. Moreover, firms could not use repurchases to disburse temporary earnings prior to 1998. Consequently, there may be a structural break in 1998. We address this issue by including a shift variable that measures differences between the period prior to 1998 and the period from 1998 onwards.

Under the flexibility hypothesis positive transitory earnings are expected to result in repurchases or special dividends. To test this hypothesis we define the variable $TranP^+_{i,t}$ which equals the transitory earnings as defined above when they are positive, and which equals zero when the transitory earnings are negative. We then estimate the following model based on repurchases and special dividends

$$S_{i,t} + R_{i,t} = bPermP_{i,t} + kTransP^+_{i,t} + d(S_{i,t} + R_{i,t}) + YEAR_t + \eta_i + \upsilon_i \quad (3.9)$$

We expect k to be positive and b to be zero.

3.5.2 Estimation methods

The models we estimate are dynamic panel data models with a relatively short time dimension (T=21) and a relatively large number of firms (N=424).[21] It is well known that in this case the OLS estimator yields upward-biased estimates of the coefficient on the lagged dependent variable. The within-group estimator (WG) (which is obtained by subtracting the firm-specific mean from all observations), on the other hand, yields downward-biased estimates (e.g. Bond, 2002; Nickel, 1981). Consistent estimates can

[21] $T = 21$ is the maximum number of firm-years for an individual firm. As our dataset is an unbalanced panel, the average number of firm-years is much smaller and amounts to 11 years. Similarly, the average number of firm observations per year amounts to 208 and is thus smaller than the number of different firms in our sample

be obtained by GMM. We therefore implement the GMM-in-systems (GMM-SYS) estimator (Blundell and Bond, 1998). It simultaneously estimates the equation in first differences with lagged levels as instruments and the equation in levels with lagged first differences as instruments.

When implementing the GMM-SYS estimator we apply Roodman's (2009) rule of thumb. It states that the number of instruments should not exceed the number of cross-sectional units (firms in our case). We impose this restriction and then choose the instrument matrix with the highest p-value for the Hansen-test of overidentifying restrictions.

Besides the GMM estimator we also report the OLS and WG estimators. The coefficient on lagged payout obtained when using the GMM-SYS estimator should lie in between the estimators obtained from the OLS and the WG estimators.

3.5.3 Results

For all model specifications, we report estimates based on OLS, alongside with within-group (WG) and GMM-in-systems (GMM(SYS)) estimators. We start the analysis by estimating original specification as a benchmark model. Columns (1) to (3) of Table 3.4 contain the coefficient estimates of the baseline model specification (3.6). The coefficients on the lagged dependent variable vary between 0.67 (WG) and 0.80 (OLS), with a GMM(SYS) coefficient estimate (0.68) that is much closer to the within-groups estimator. These results confirm the prediction of an upward bias in OLS. The parameter estimates result in a speed of adjustment in the range of [0.20, 0.34], which is roughly in line with other studies on German data (Andres et al., 2009; Behm and Zimmermann, 1993). The estimated target payout ratio ($b/(1-d)$) varies between 0.23 (WG) and 0.48 (GMM(SYS)). Accordingly, estimates obtained via OLS and GMM (SYS) are very close to the average dividend payout ratio over the full sample period (46.6%, as documented in Table 3.2).

Table 3.4: Classical Lintner model & Total payout model

Table 3.4 shows the results of OLS, within-groups (WG), and GMM-in-Systems (GMM-SYS) regressions with dividends per share as dependent variable (regression models 1-3). In addition we report the results with total payout as the dependent variable (regression models 4-6). The number of observations is slightly lower for models 4-6 because in some cases we were unable to identify whether a special dividend was paid in addition to the regular dividend. The first column shows the independent variables. $D_{i,t-1}$ and $S_{i,t-1}$ are dividends and special dividends per share paid out in the previous year, respectively. $R_{i,t-1}$ corresponds to the repurchase volume per share in the previous year.P represents after-tax earnings per share. For the within-group models the coefficient for *Constant* is the average value of the fixed effects as obtained from Stata 12. Each cell shows the estimated coefficient and t-value (in parentheses). The superscripts *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively. The statistics m1 and m2 are tests for the absence of first-order and second-order serial correlation in the residuals, asymptotically distributed as $N(0,1)$ under the null of no serial correlation. The Hansen statistic is a test of the over-identifying restrictions, asymptotically distributed as $\chi^2(k)$ under the null of valid instruments, with k degrees of freedom reported in parentheses. All regressions include year dummies and a dummy indicating a change in the accounting standards. The *Speed of Adjustment* is calculated as one minus the coefficient for $D_{i,t-1}$ (or $D_{i,t-1} + S_{i,t-1} + R_{i,t-1}$, respectively). The *Target Ratio* equals the coefficient for $P_{i,t}$ divided by the *Speed of Adjustment*.

	Regular dividends			*Total payout*		
	OLS	WG	GMM (SYS)	OLS	WG	GMM (SYS)
Constant	1.111	1.675	−0.168*	2.937***	1.955	7.404
$D_{i,t-1}$	0.802***	0.666***	0.681***	–	–	–
	(6.89)	(6.16)	(5.95)			
$D_{i,t-1} + S_{i,t-1} + R_{i,t-1}$	–	–	–	0.252	0.139	0.183
$P_{i,t}$	0.083**	0.078**	0.154***	0.339***	0.320**	0.429***
	(2.14)	(2.11)	(2.79)	(2.68)	(2.40)	(4.10)
m1	–	–	−2.59	–	–	−1.98
m2	–	–	−1.08	–	–	−1.30
Hansen (d.f)	–	–	334.22	–	–	339.90
			(316)			(313)
Observations	3960	3960	3960	3909	3909	3909
Target Ratio	0.419	0.234	0.483	0.453	0.372	0.525
Speed of Adj.	0.198	0.334	0.319	0.748	0.861	0.817

Not surprisingly, the estimates of the target payout ratio are higher for the full payout model (columns (4)-(6)). These estimates are based on model specification (3.7), where (regular) dividends, special dividends and repurchases are added up to total payout. Again, the results of the GMM(SYS) estimation (52.5%) are very close to the average total payout ratio (55.8%). Compared to the estimates in columns (1)-(3), the target payout ratio is only slightly higher, though. This points to the importance of dividends as the main form of payout for German firms. When comparing the speed of adjustment, the total payout model yields substantially higher estimates than the dividends-only model. This finding is consistent with hypothesis H3.4 and indicates that (regular) dividends are indeed more sticky than total payouts.

The models discussed thus far implicitly assume that the target payout ratios and the speed of adjustment are constant throughout the sample period. However, with the introduction of stock repurchases, (regular) dividends lost in importance. Our descriptive results in Table 3.2 show a decrease in the average dividend payout ratio from 56.4% to 44.1%.

Therefore we now turn to a model specification that allows for a structural break in 1998. The results are shown in Table 3.5. We first consider the dividends-only model (columns (1)-(3) in Table 3.5). As expected, we find a negative and highly significant (at the 5% level or better) change in the target dividend payout ratio. Before 1998, the estimated target payout ratio varies between 0.48 and 0.70 and drops significantly once stock repurchases became legal (range between [0.19, 0.47]). Estimates for the speed of adjustment are also lower for the period after 1997. This implies that dividend payouts became even more sticky once repurchases were allowed. A possible explanation for this finding is that firms, to a certain extent, used dividends to disburse transitory earnings prior to 1998 but ceased to do so once repurchases were allowed.

Columns (4)-(6) of Table 3.5 report the estimates for the total-payout model.[22] The results show a substantial decrease in the estimated target total payout ratio (from 0.79 to 0.49) and a strong increase in the speed of adjustment (from 0.51 to 0.89, all figures relate to the GMM(SYS) estimation) after 1997. These results are inconsistent with the substitutes hypothesis (H3.1). They rather imply that dividends and repurchases are not perfect substitutes. Stock repurchases (and potentially also special dividends) allow for a faster adjustment to temporary changes in earnings, which is reflected in the faster speed of adjustment during the second half of the sample period.

[22] There are 53 cases in which a firm announces that it repurchases shares in order to use the shares as a means of payment in future acquisitions ("acquisition currency"). When we eliminate the corresponding 53 firm-year observations we obtain results that are similar to those presented in the text.

Table 3.5: The Introduction of Stock Repurchases

Table 3.5 shows the results of OLS, within-groups (WG), and GMM-in-Systems (GMM-SYS) regressions with dividends per share as dependent variable (regression models 1-3). In addition we report the results with total payout as the dependent variable (regression models 4-6). The number of observations is slightly lower for models 4-6 because in some cases we were unable to identify whether a special dividend was paid in addition to the regular dividend. The first column shows the independent variables. $D_{i,t-1}$ and $S_{i,t-1}$ are dividends and special dividends per share paid out in the previous year, respectively. $R_{i,t-1}$ corresponds to the repurchase volume per share in the previous year.P represents after-tax earnings per share. For the within-group models the coefficient for *Constant* is the average value of the fixed effects as obtained from Stata 12. Each cell shows the estimated coefficient and t-value (in parentheses). The superscripts *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively. We introduce a structural break to account for the introduction of stock repurchases in 1998. We report the coefficient for the period from 1998 onwards which is the sum of the pre-break period (1988-1997) parameter and a shift term. We test if the sum of the pre-break period parameter and the shift term is statistically different from zero. We also report the standard t-test for the shift parameter and the t-value (in parentheses; please note that the coefficient is the sum of the pre-break coefficient and the shift parameter while the t-statistic is for the shift parameter. There can thus be cases where the parameter is positive while the t-statistic is negative). The superscripts †,†† ,††† denote significance at the 10%, 5%, and 1% levels, respectively. The statistics m1 and m2 are tests for the absence of first-order and second-order serial correlation in the residuals, asymptotically distributed as $N(0,1)$ under the null of no serial correlation. The Hansen statistic is a test of the over-identifying restrictions, asymptotically distributed as $\chi^2(k)$ under the null of valid instruments, with k degrees of freedom reported in parentheses. All regressions include year dummies and a dummy indicating a change in the accounting standards. The *Speed of Adjustment* is calculated as one minus the coefficient for $D_{i,t-1}$ (or $D_{i,t-1} + S_{i,t-1} + R_{i,t-1}$, respectively). The *Target Ratio* equals the coefficient for $P_{i,t}$ divided by the *Speed of Adjustment.*

	Regular dividends			*Total payout*		
	OLS	WG	GMM (SYS)	OLS	WG	GMM (SYS)
Constant	1.102	1.048	0.423	1.276	−0.075	8.807***
	(1.50)	(0.96)	(0.42)	(1.02)	(−0.04)	(2.13)
$D_{i,t-1}(88-97)$	0.669***	0.613***	0.608***	–	–	–
	(16.77)	(27.3)	(47.44)			
$D_{i,t-1}(98-08)$	0.886***,††	0.735***	0.768***	–	–	–
	(2.26)	(0.91)	(1.14)			
$D_{i,t-1}+S_{i,t-1}+R_{i,t-1}(88-97)$	–	–	–	0.577***	0.476***	0.494***
				(15.10)	(4.49)	(9.11)
$D_{i,t-1}+S_{i,t-1}+R_{i,t-1}(98-08)$	–	–	–	0.179††	0.080††	0.117†††
				(−2.30)	(−2.25)	(−3.07)
$P_{i,t}(88-97)$	0.196***	0.188***	0.274***	0.284***	0.274***	0.397***
	(3.23)	(2.93)	(25.65)	(3.24)	(3.05)	(3.90)
$P_{i,t}(98-08)$	0.054**,††	0.051**,††	0.107***,†††	0.346**	0.329**	0.431***
	(−2.56)	(−2.26)	(−4.04)	(0.38)	(0.33)	(0.28)
m1	–	–	−2.74	–	–	−1.98
m2	–	–	−0.37	–	–	−1.23
Hansen (d.f)	–	–	330.67	–	–	339.87
			(307)			(313)
Observations	3960	3960	3960	3909	3909	3909
Target Ratio (88-97)	0.592	0.486	0.699	0.671	0.523	0.785
Target Ratio (98-08)	0.474	0.192	0.461	0.421	0.358	0.488
Speed of Adj (88-97)	0.331	0.387	0.392	0.423	0.524	0.506
Speed of Adj. (98-08)	0.114	0.265	0.232	0.821	0.920	0.883

When comparing the dividends-only model (columns (1)-(3)) to the total payout model (columns (4)-(6)), we find that (as in Table 3.4) the total payout model yields higher estimates of the speed of adjustment as compared to the dividends-only model. This adds to the evidence in support of hypothesis H3.4.

In sum, the findings in Table 3.5 indicate that dividends and repurchases are not considered as (perfect) substitutes by German firms.

As pointed out in section 3.2 a change in taxation in 2001 made repurchases relatively more desirable for the vast majority of investors. We therefore expect that target dividend payout ratios decrease after 2001 (hypothesis H3.2). To test this hypothesis we extend the dividends-only model of Table 3.5 to allow for a tax-induced structural break in 2002 in addition to the structural break in 1998. The results are shown in Table 3.6. The coefficient estimates show substantial variation across sub-periods. We find payouts in later years to be much more rigid, as evidenced by a significantly lower speed of adjustment after 2001. The estimates of the target payout ratio are not within an economically meaningful range. The GMM-in-systems estimator implies a target payout ratio above 100%. Because the model specification with two structural breaks yields implausible results (possibly because the second sub-period is very short) we abstain from modeling two structural breaks in our further analysis and rather focus on the main structural break in 1998.

To gain further insight into the impact of the tax reform on payout decisions we re-estimate the model for the first sub-period (1988-97) and the last sub-period (2002-08) separately. The results are also shown in Table 3.6 (specification (4) and (5)). The target dividend payout ratio is 0.62 in 1988-97 and 0.72 in 2002-08. Thus both the joint estimation and the separate estimations for the sub-periods yield results which are inconsistent with H3.2. This hypothesis predicts *lower* target dividend payout ratios after the tax reform. Our results thus imply that tax considerations do not seem to be a (first order) determinant of the payout policy of German firms. This corroborates evidence reported in Andres et al. (2012).

The analysis thus far showed that the speed of adjustment is generally higher in a total payout model than in a dividends-only model. This is consistent with the flexibility hypothesis of Jagannathan et al. (2000). It predicts that changes in dividends

Table 3.6: The Tax Reform

Table 3.6 shows the results of OLS, within-groups (WG), and GMM-in-Systems (GMM-SYS) regressions with dividends per share as dependent variable. The number of observations is slightly lower for models 4-6 because in some cases we were unable to identify whether a special dividend was paid in addition to the regular dividend. The first column shows the independent variables. $D_{i,t-1}$ represents dividends per share paid out in the previous year. P represents after-tax earnings per share. For the within-group model the coefficient for *Constant* is the average value of the fixed effects as obtained from Stata 12. Each cell shows the estimated coefficient and t-value (in parentheses). The superscripts *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively. We introduce two structural breaks to account for the introduction of stock repurchases in 1998 and the tax reform in 2001. We report the coefficient for the period from 1998 to 2001 and the period from 2002 to 2008 which is the sum of the pre-break period (1988-1997) parameter and a shift term in both cases. We test if the sum of the 1988-1997-period parameter and the shift term is statistically different from zero. We also report the standard t-test for the shift parameter and the t-value (in parentheses; please note that the coefficient is the sum of the pre-break coefficient and the shift parameter while the t-statistic is for the shift parameter. There can thus be cases where the parameter is positive while the t-statistic is negative). The superscripts †,††,††† denote significance at the 10%, 5%, and 1% levels, respectively. The statistics m1 and m2 are tests for the absence of first-order and second-order serial correlation in the residuals, asymptotically distributed as $N(0, 1)$ under the null of no serial correlation. The Hansen statistic is a test of the over-identifying restrictions, asymptotically distributed as $\chi^2(k)$ under the null of valid instruments, with k degrees of freedom reported in parentheses All regressions include year dummies and a dummy indicating a change in the accounting standards. The Speed of Adjustment is calculated as one minus the coefficient for $D_{i,t-1}$ in the respective period. The Target Ratio equals the coefficient for $P_{i,t}$ divided by the Speed of Adjustment in the respective period.

	Regular dividends				
	OLS	WG	GMM (SYS)	GMM (SYS)	GMM (SYS)
Constant	1.092	1.129	−0.550	0.221	−2.833***
	(1.49)	(1.03)	(−0.63)	(0.55)	(−2.60)
$D_{i,t-1}(88-97)$	0.671***	0.580***	0.582***	0.538***	–
	(16.52)	(28.81)	(43.20)	(30.20)	
$D_{i,t-1}(98-01)$	0.484***	0.299**,†	0.322***,†	–	–
	(−1.35)	(−1.90)	(−1.80)		
$D_{i,t-1}(02-08)$	1.008***,†††	0.859***,††	0.941*,†††	–	0.880***
	(3.73)	(2.88)	(3.43)		(7.78)
$P_{i,t}(88-97)$	0.196***	0.180***	0.280***	0.2888***	–
	(3.23)	(2.85)	(31.47)	(44.30)	
$P_{i,t}(98-01)$	0.041††	0.033†††	0.250***	–	–
	(−2.49)	(−2.34)	(−0.37)		
$P_{i,t}(02-08)$	0.048**,††	0.044**,††	0.078**,†††	–	0.086**
	(−2.35)	(−2.01)	(−6.00)		(2.30)
m1	–	–	−2.77	−1.67	−1.47
m2	–	–	−1.02	−1.04	−0.93
Hansen (d.f)	–	–	308.57	105.87	118.73
			(232)	(90)	(77)
Observations	3960	3960	3960	1648	1494
Target Ratio (88-97)	0.596	0.429	0.607	0.623	–
Target Ratio (98-01)	0.079	0.047	0.381	–	–
Target Ratio (02-08)	−6.000	0.312	1.322	–	0.717
Speed of Adj (88-97)	0.329	0.420	0.418	0.462	–
Speed of Adj. (98-01)	0.516	0.701	0.678	–	–
Speed of Adj. (02-08)	−0.008	0.141	0.059	–	0.120

are related to changes in permanent earnings but are unrelated to changes in temporary earnings. We now turn to a direct test of this hypothesis (our H3.3). As described above, we decompose earnings into a permanent and a transitory component (see model (3.8) above). The model is estimated for dividends only and allows for a structural break in 1998.

The results are shown in Table 3.7. During the first half of the sample period, the coefficients of both permanent and transitory earnings are positive and highly statistically significant. The estimated target payout ratios are only slightly lower for transitory earnings than for permanent earnings. This implies that, prior to the introduction of repurchases, firms used regular dividends to disburse transitory earnings.

With the introduction of stock repurchases this picture changes. We observe a statistically significant structural break for both earnings components. While the target payout ratio for permanent earnings decreases moderately and insignificantly (from 0.68 to 0.51 for the GMM(SYS) estimation), we observe a substantial and significant (at the 1% level) decrease for temporary earnings, from 0.66 to 0.26 (GMM (SYS)). Thus, in the period after 1997, the reaction of dividend payouts to changes in transitory earnings is much weaker than in the pre-1998 period. In addition, the speed of adjustment decreases after 1997. Both results are consistent with the flexibility hypothesis (H3.3).

Since the introduction of stock repurchases in 1998 firms are equipped with a more flexible method to disburse transitory earnings and thus do no longer use regular dividends for this purpose.

By definition, our measure of the transitory component of earnings can be negative. The flexibility hypothesis, however, implies that only positive deviations in earnings (i.e. positive transitory earnings) result in (temporary) payouts. We therefore run additional regressions in which only *positive* transitory earnings are considered to explain changes in special dividends and repurchases.

Table 3.7: Financial Flexibility: Dividends

Table 3.7 shows the results of OLS, within-groups (WG), and GMM-in-Systems (GMM-SYS) regressions with dividends per share as dependent variable. The first column shows the independent variables. $D_{i,t-1}$,$PermP$ represents represents the three year moving average of after-tax earnings per share based on the years $t, t-1$ and $t-2$. $TransP$ is equal to the difference between after tax earnings per share and $PermP$. For the within-group model the coefficient for *Constant* is the average value of the fixed effects as obtained from Stata 12. Each cell shows the estimated coefficient and t-value (in parentheses). The superscripts *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively. We introduce two structural breaks to account for the introduction of stock repurchases in 1998 . We report the coefficient for the period from 1998 to 2008 which is the sum of the pre-break period (1988-1997) parameter and a shift term in both cases. We test if the sum of the 1988-1997-period parameter and the shift term is statistically different from zero. We also report the standard t-test for the shift parameter and the t-value (in parentheses; please note that the coefficient is the sum of the pre-break coefficient and the shift parameter while the t-statistic is for the shift parameter. There can thus be cases where the parameter is positive while the t-statistic is negative). The superscripts †,†† ,††† denote significance at the 10%, 5%, and 1% levels, respectively. The statistics m1 and m2 are tests for the absence of first-order and second-order serial correlation in the residuals, asymptotically distributed as $N(0,1)$ under the null of no serial correlation. The Hansen statistic is a test of the over-identifying restrictions, asymptotically distributed as $\chi^2(k)$ under the null of valid instruments, with k degrees of freedom reported in parentheses All regressions include year dummies and a dummy indicating a change in the accounting standards. The *Speed of Adjustment* is calculated as one minus the coefficient for $D_{i,t-1}$ in the respective period. The *Target Ratio Perm (Trans)* equals the coefficient for $PermP_{i,t}(TransP_{i,t})$ in the respective period divided by the *Speed of Adjustment* in the respective period.

	Regular dividends		
	OLS	WG	GMM (SYS)
Constant	0.592	0.668	−0.041
	(1.17)	(0.83)	(−0.04)
$D_{i,t-1}(88-97)$	0.643***	0.567***	0.589***
	(11.58)	(21.94)	(20.60)
$D_{i,t-1}(98-08)$	0.793***	0.603***	0.673***
	(1.30)	(0.25)	(0.59)
$PermP_{i,t}(88-97)$	0.206***	0.218***	0.279***
	(2.98)	(2.96)	(10.30)
$PermP_{i,t}(98-08)$	0.112**,†	0.131*,†	0.168***,†††
	(−1.84)	(1.55)	(−3.13)
$TransP_{i,t}(88-97)$	0.194***	0.182***	0.273***
	(3.30)	(2.86)	(75.16)
$TransP_{i,t}(98-08)$	0.013†††	0.006†††	0.085**,†††
	(−2.67)	(−2.40)	(−5.07)
m1	–	–	−2.99
m2	–	–	−0.41
Hansen (d.f)	–	–	337.28
			(315)
Observations	3581	3581	3581
Target Ratio Perm (88-97)	0.577	0.503	0.679
Target Ratio Perm (98-08)	0.541	0.330	0.514
Target Ratio Trans (88-97)	0.543	0.420	0.664
Target Ratio Trans (98-08)	0.063	0.015	0.260
Speed of Adj. (88-97)	0.357	0.433	0.411
Speed of Adj. (98-08)	0.207	0.397	0.327

Table 3.8: Financial Flexibility: Special Dividends & Repurchases

Table 3.8 shows the results of OLS, within-grpoup, and GMM-in-Systems regressions with with the sum of special dividends and stock repurchases per share as dependent variable. The first column shows the independent variables. $S_{i,t-1}$ are special dividends per share paid out in the previous year, $R_{i,t-1}$ corresponds to the repurchase volume per share in the previous year. PermP represents the three year moving average of after-tax earnings per share based on the years t, t-1 and t-2. $PositiveTransP$ is equal to the maximum of the difference between after tax earnings per share and $PermP$ and zero. For the within-group model the coefficient for *Constant* is the average value of the fixed effects as obtained from Stata 12. Each cell shows the estimated coefficient and t-value (in parentheses). The superscripts *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively. We introduce two structural breaks to account for the introduction of stock repurchases in 1998. We report the coefficient for the period from 1998 to 2008 which is the sum of the pre-break period (1988-1997) parameter and a shift term in both cases. We test if the sum of the 1988-1997-period parameter and the shift term is statistically different from zero. We also report the standard t-test for the shift parameter and the t-value (in parentheses; please note that the coefficient is the sum of the pre-break coefficient and the shift parameter while the t-statistic is for the shift parameter. There can thus be cases where the parameter is positive while the t-statistic is negative). The superscripts †,††,††† denote significance at the 10%, 5%, and 1% levels, respectively. The statistics m1 and m2 are tests for the absence of first-order and second-order serial correlation in the residuals, asymptotically distributed as $N(0,1)$ under the null of no serial correlation. The Hansen statistic is a test of the over-identifying restrictions, asymptotically distributed as $\chi^2(k)$ under the null of valid instruments, with k degrees of freedom reported in parentheses All regressions include year dummies and a dummy indicating a change in the accounting standards. The *Speed of Adjustment* is calculated as one minus the coefficient for $D_{i,t-1}$ in the respective period. The *Target Ratio Perm (Trans)* equals the coefficient for $PermP_{i,t}(TransP_{i,t})$ in the respective period divided by the *Speed of Adjustment* in the respective period.

	Regular dividends		
	OLS	WG	GMM (SYS)
Constant	-1.350^{**}	-2.605^{**}	2.740
	(-2.43)	(-2.36)	(0.52)
$\mathrm{S_{i,t-1}} + R_{i,t}(88-97)$	0.167^{**}	0.074	0.157
	(2.43)	(0.66)	(2.11)
$\mathrm{S_{i,t-1} + R_{i,t}}(98-08)$	$-0.034^{\dagger\dagger\dagger}$	-0.083	$-0.038^{\dagger\dagger\dagger}$
	(-3.12)	(-1.47)	(-2.61)
$\mathrm{PermP_{i,t}}(88-97)$	0.140^{***}	0.195^{***}	0.142^{***}
	(4.40)	(3.87)	(5.36)
$\mathrm{PermP_{i,t}}(98-08)$	0.124	0.143	0.009
(-0.18)	(-0.58)	(-0.12)	
$\mathrm{PositiveTransP_{i,t}}(88-97)$	0.060^{**}	0.060	0.073^{***}
	(2.21)	(1.33)	(7.54)
$\mathrm{PositiveTransP_{i,t}}(98-08)$	0.407	0.444	$0.634^{*,\dagger}$
	(1.18)	(1.21)	(1.71)
m1	–	–	-1.37
m2	–	–	-1.63
Hansen (d.f)	–	–	296.22
			(177)
Observations	3353	3353	3353
Target Ratio Perm (88-97)	0.168	0.211	0.168
Target Ratio Perm (98-08)	0.120	0.132	0.009
Target Ratio Trans (88-97)	0.072	0.065	0.087
Target Ratio Trans (98-08)	0.394	0.410	0.611
Speed of Adj. (88-97)	0.833	0.926	0.843
Speed of Adj. (98-08)	1.034	1.083	1.038

Table 3.8 contains the results of this specification (model (3.9)). As expected, we find the speed of adjustment to be very high. This lends further support to the notion that special dividends and repurchases are used as very flexible means of payouts. In fact, the estimated speed of adjustment further increases with the introduction of stock repurchases. Surprisingly, the permanent earnings component has a significant and positive impact on special dividends before 1998. The coefficient for positive transitory earnings is also positive (and partly significant), but consistently lower in magnitude. This implies that special dividends were partly used to pay out permanent earnings. For the second half of the sample period, though, special dividends and stock repurchases are not influenced by the permanent component of earnings.

Accordingly, the estimated target payout ratio of permanent earnings falls to almost zero (for GMM(SYS)). On the other hand, the target payout ratio for the (positive) transitory component of earnings increases strongly and significantly (at the 10% level), from 0.09 to 0.61 (GMM(SYS)). This can again be interpreted as evidence in favor of the flexibility hypothesis.

3.6 Conclusion

The Lintner (1956) model, the workhorse of empirical research on corporate payout decisions, is usually applied to dividend payouts. Against the background of the strong increase in repurchases this is not necessarily appropriate, though. We argue that a comparison of Lintner models of dividend payout and total payout can yield insights into the drivers of the payout decision. In particular it allows us to discriminate among alternative theories of corporate payout, namely, the substitutes hypothesis, tax-based explanations, and the flexibility hypothesis. These theories make specific predictions about the target payout ratios and speed of adjustment coefficients in Lintner models of dividend payout and total payout.

A distinguishing feature of our dataset is that it spans the introduction of stock repurchases in Germany in 1998 as well as a tax reform in 2001. This allows us to analyze how these events affected payout policy. We find that the introduction of repurchases in 1998 has materially affected the payout policy of German firms. In particular, both dividend and total target payout ratios decrease. The speed of adjustment for dividend payout decreases while the speed of adjustment for total payout increases. This is inconsistent with the substitutes hypothesis which predicts that the introduction of repurchases should not alter total payouts. Interestingly, special designated dividends do not lose importance after the introduction of repurchases.

We find no evidence that German firms have changed their payout policy in response to the 2001 tax reform. This finding, although surprising at first sight, is consistent with previous evidence. Andres et al. (2012) document that the tax preferences of the largest shareholder have no impact on the dividend payout ratios of German firms.

Our results provide clear support for Jagannathan et al.'s (2000) financial flexibility hypothesis. We find that dividends are more rigid than total payouts. This is consistent with the prediction of the flexibility hypothesis that dividends are predominantly paid out of permanent earnings. We further document that, after the introduction of repurchases, the responsiveness of dividends to changes in transitory earnings is reduced substantially. This finding is also supportive of the flexibility hypothesis.

Chapter 4

Measuring Abnormal Credit Default Swap Spreads

4.1 Introduction

Academics, financial regulators, and practitioners have shown an increasing interest in the effect of firm specific, regulatory and macroeconomic events on credit risk pricing. To that end the event study methodology has been employed on credit default swaps (CDSs) because this relatively young asset exhibits some distinct advantages over other credit risk measures. However, the current literature is lacking a clear understanding of the particularities of event studies employing CDS. Applying a simulation approach similar to that of Brown and Warner (1985) and Bessembinder et al. (2009), we therefore examine the statistical properties of CDS data and the performance of current methods and models used to test for the presence or absence of abnormal changes in CDS spreads. Furthermore, based on the findings of the literature on the determinants of CDS changes, we test a CDS four-factor model that captures all potential relevant pricing factors identified in the literature.

Measuring the impact of any event on stock prices is among the most common empirical techniques in corporate finance. Brown and Warner (1980, 1985) show that under many conditions the market model and standard parametric tests are well specified in this research context. In addition, an important strand of the literature investigates the influence of firm-specific events on debtholder wealth.[1] In a recent paper, Bessembinder et al. (2009, p. 4256) examine the different methodologies used to detect abnormal bond returns and conclude that "the inferences drawn in previous studies could in fact be incorrect, depending on the sample size, the magnitude of the event... and whether parametric or non-parametric tests were used."

[1]For an overview, see Bessembinder et al. (2009).

However, since reliable CDS data has become available from vendors such as Bloomberg and Markit, a growing literature measuring the impact of corporate events on debtholder wealth prefers to employ CDS data instead of bond data, among other things, for the following reasons: First, many bond event studies suffer from the fact that firms have a variety of bonds outstanding, with different maturities and credit ratings and differences in liquidity. In such cases, it is not obvious how the different value changes should be aggregated to gauge the total effect of the event. In contrast, only one CDS per firm needs to be valued and contract maturities are typically set to five years[2] (Bessembinder et al., 2009). Second, CDS spreads are a pure measure of credit risk, whereas bond spreads include factors that are unrelated to default risk, such as interest risk and illiquidity (Longstaff et al., 2005; Callen et al., 2009). Third, empirical studies such as those of Daniels and Jensen (2005) and Zhu (2006) show that price discovery occurs first in the CDS market and subsequently in the bond market. In addition, CDS contracts are more liquid than corporate bonds (Bessembinder et al., 2009). Given these differences between CDS and corporate bonds, it is understandable that researchers might—under certain circumstances—favor CDS event studies over bond event studies.

Table 4.1 gives an overview of previous CDS event studies. This strand of the literature is relatively young due to the fact that CDS data only became available about 10 years ago. We aim to provide researchers using CDS data with detailed knowledge of the performance of a variety of methods that are used to detect abnormal CDS changes. The evidence presented in this paper should help avoid biased inferences similar to those documented by Bessembinder et al. (2009) in the context of abnormal bond returns.

As already investigated by Brown and Warner (1980, 1985) and Bessembinder et al. (2009) in the case of stock and bond returns, respectively, the performance of the different methods depends essentially on two features: The method under scrutiny should avoid excessive Type I (falsely rejecting the null of no abnormal CDS spread

[2] In general, data on CDS contracts are available for maturities between six months and 30 years. However, CDS contracts with a maturity of five years are the most standard and most liquid contracts. Due to the limited availability of spread data for other maturities, we restrict our analyses to five-year contracts.

Table 4.1: CDS Event Studies in the Literature

Study authors, year	No. of spread observations	Time period	Benchmark model	Spread change calculation	Test statistic	Data source
Hull et al. (2004)	29,032	Oct. 1998–May 2002	Index based on average CDS spread in rating category	Absolute	Bootstraped t-Test	GFI
Micu et al. (2004)	694 financial and non-financial firms	Jan. 2001–Dec. 2003	Index based on average CDS spread in rating category	Absolute	t-Test, sign test	Markit
Norden and Weber (2004)	60,827	July 1998–Dec. 2002	Index based on CDS spread in rating category	Absolute	Wilcoxon sign(rank) test	Unknown European bank
Daniels and Jensen (2005)	72 firms	Jan. 2002–Dec. 2002	Constant mean model	Absolute	t-Test	JP Morgan Chase
Lehnert and Neske (2006)	100 major European firm	Aug. 2000–Aug. 2003	Average daily percentage change of CDS spreads within a particular investment-grade category	Absolute	t-Test (not clearly stated)	Trac-X Europe index
Jorion and Zhang (2007)	512,292	Jan. 2003–Dec. 2004	Rating-adjusted CDS spread	Absolute	t-Test	Markit
Greatrex (2008)	413,844	Jan. 2001-Apr. 2006	Index based on CDS spread in rating category Market (index) model	Absolute	Cross-sectional t-test	Markit
Callen et al. (2009)	20,328	2002–2005	None	Relative	None	Lombard Risk
King (2009)	28 banks	Jan. 2008–Jan. 2009	Multifactor model Market Model	Absolute	none	Markit
Pop and Pop (2009)	31 financial institutions	Jan. 2003–Jan. 2004	Constant mean model	Absolute	t-Test	Markit and Credit Market Analysis
Galil and Soffer (2011)	1,176,640	Jan. 2002–June 2006	Index based on average CDS spread in rating category	Absolute	t-Test Wilcoxon sign (rank) test	Markit
Shivakumar et al. (2011)	846,261	2001–2008	Matched basket of CDS contracts, selecting the CDS contracts with the same credit rating category	Relative	None	Markit
Dittmann et al. (2012)	108 firms	2000–2010	Index based on average CDS spread in rating category	Absolute	t-Test, sign test Wilcoxon sign (rank) test	Markit

change when it is in fact true) and Type II (not rejecting the null of no abnormal CDS spread change when it is in fact false) errors.

Our descriptive results show that the distributional properties of our CDS dataset are characterized by positive mean spread changes and a positive excess kurtosis independent of the reference entities' ratings (investment or non-investment grade), the region (United States or Europe), and the applied spread change measure (absolute or relative). These observations indicate that the distribution of realized spread changes is not normal. The distributional characteristics of abnormal spread changes similarly show deviations from normality (positive skewness and excess kurtosis). Consequently, parametric test statistics may lead to biased inferences in CDS event studies. Furthermore, our findings, (1) that not just the spread level but also the absolute spread change and (2) the CDS standard deviation are both negatively related to the reference entities' ratings, support the notion that relative and not (!) absolute spread changes should be applied in CDS event studies.

Our main simulation results indicate that only the non-parametric rank test is well specified when assessing the size of the tests (Type I errors). All other test statistics—parametric tests in particular—suffer from disproportionate Type I errors. In addition, similar to the findings of Bessembinder et al. (2009) in the context of abnormal bond returns, non-parametric test statistics are more powerful in detecting abnormal spread changes compared to standard parametric tests. However, in contrast to the work of Bessembinder et al. (2009), our simulations show that the matched portfolio approach used to calculate abnormal spread changes is the least powerful of all tested models. Hence, using this model in CDS event studies may lead to biased inferences. Across all parametric and non-parametric test statistics, the CDS factor model seems to be the best specified and most powerful model since it exhibits the highest detection rates. Surprisingly, the simple mean-adjusted approach also leads to very reasonable results and is recommended if factor information is not available. Our simulation results are very similar for the investment-grade and non-investment-grade samples. In additional analyses we follow Boehmer, Musumeci and Poulsen (BMP) (1991) and reexamine the performance of the different test statistics when introducing an event-induced variance increase. As expected, only the test of BMP (1991) is consistently well specified under these conditions. However, at least for relatively small variance increases, the generalized sign and the cross-sectional t-test seem to be well specified.

Several previous studies (e.g. Hull et al., 2004) use a bootstrap approach to control for the bias that arises if the empirical distributions of test statistics are skewed. Accordingly, we present simulation results that account for erroneous assumptions about the empirical distribution of employed test statistics. Overall, the bootstrap approach only leads to a slight shift of empirical rejection rates toward the theoretical rates expected under the absence of abnormal performance. The power of the different test statistics is not affected when using bootstrapped p-values. The previous results are mainly confirmed since the new CDS four-factor model still outperforms all other normal return models.

The remainder of the paper is organized as follows. Section 2 describes the CDS data and specifies the methodology following Brown and Warner (1985). Section 3 describes the different spread measures used in previous papers and discusses their suitability. Furthermore, we present the models used in our subsequent simulation study to calculate expected credit default spreads. Section 4 describes the distributional properties of our initial dataset and analyzes the size and power for each combination of test statistic and normal CDS spread change model. Section 5 discusses specific problems in the context of event studies with CDSs, namely, the event-induced variance increase (Harrington and Shrider, 2007) and the skewness of the empirical distributions of the test statistics. Section 6 concludes the paper.

4.2 Data and Simulation Methodology

4.2.1 Data

Credit default swaps are contractual agreements between two parties that protect against the default risk of a reference entity (in most cases a company). The protection buyer pays a periodic fee, the spread, to the protection seller. In return, the protection seller is obligated to compensate the protection buyer if a predefined credit event (e.g., default or bankruptcy) happens to the reference entity. Generally, CDS contracts are standardized under the rules set by the International Swaps and Derivatives Association

(ISDA) and are traded over the counter. They therefore lack the legal reporting and disclosure requirements of regulated exchanges. Consequently, reliable and complete information on spreads is not directly observable. This difficulty is compounded by the fact that neither transnational legal reporting requirements nor international clearing and settlement standards exist.[3] In contrast to stock event studies, a critical examination of the employed spread data is therefore an inevitable step prior to measuring abnormal CDS spreads.

Empirical studies on CDS data are based on either a limited set of transaction and quote data or use composite spreads derived from periodical surveys of major market participants. Transaction spreads are provided by interdealer platforms (e.g., GFI) or directly out of the book of a single market participant (e.g. Norden and Weber, 2004) but generally suffer from low reporting frequencies. While it is obvious that spread data from a single market maker are not representative of the overall CDS market, Mayordomo et al. (2010) argue that the same may apply to interdealer platform data. Their major concern is that interdealer data are incomplete, since CDSs are commonly negotiated via voice transactions. Due to this lack of reliable transaction and quote data, most empirical studies on CDSs employ composite spreads provided by specialized data vendors (see column 7 of Table 4.1). The general approach of these data vendors is based on a collection of book of record data from numerous major market participants. After cleaning these data for outliers and stale spreads, the remaining data are aggregated into composite spreads. By construction, these spreads are neither pure transaction nor quoted spreads. Nevertheless, we believe that composite spreads are a more representative valuation of the overall CDS market and have an advantages over spreads derived from interdealer platforms or a single market participant:

In addition to the credit risk of the reference entity, the level of CDS spreads is potentially affected by the counterparty default risk, i.e. the risk that the swap parties fail to meet their payment obligations (e.g. Hull and White, 2001; Leung and Kwok, 2004). The amount and direction of the effect will depent on (i) the expected costs of

[3] Regulatory agencies in the United States and Europe started discussions about the introduction and/or information sharing of one (or more) central CDS counterparties in 2009.

replacing a contract (ii) the expected double default[4] costs (iii) the default correlation between the reference entity and the swap parties. In contrast to transaction prices, the composite spread is not affected by the counterparty risk based on a single market participant, but is affected by the average counterparty risk in the market. While the average counterparty risk cleary affects spread levels, it might not affect spread changes if it is stable over time. We think that this stability assumption rather applies to the average counterparty risk than to the counterparty risk based on a single set of swap parties. However, if the event itself has not only an effect on the credit risk of the reference entity but also on the average counterparty risk, it is difficult to disentangle both effects.

Even though most data vendors use a similar approach to derive their composite spreads, Mayordomo et al. (2010) show that composite spreads from different databases exhibit deviations across firms and in the time-series dimension. Additionally, spreads reported by different vendors seem to adjust to new information at different speeds. According to Mayordomo et al. (2010), data from Markit Ltd. and CMA Datavision lead all other databases in terms of price discovery for European entities. The authors report comparable results for US entities, except that price data from CMA Datavision also lead data from Markit Ltd. While differences in price discovery are of minor importance in a simulation study with artificial abnormal effects, the database selection can have a substantial influence on the results of an applied event study. Since Markit Ltd. provides supplementary information for each contract, such as the contractual standards, the firm's country of domicile, and the average rating of both Standard and Poor's and Moody's, we use their daily CDS database as our main data source. Markit Ltd. employs the aforementioned collection and data cleaning processes to construct their composite spreads. As an additional restriction, they only report composite spreads whenever the pricing information is based on at least three different market participants.

Within their daily CDS database, Markit Ltd. provides composite spreads over the period from January 2001 to July 2011. However, since the ISDA published its

[4] A double default describes the case were both the reference entity and the protection seller default simultaneously.

refined contractual standards for CDSs on February 11, 2003, we do not consider CDS quotes prior to that date. Within their contractual standards for CDSs, the ISDA provides alternative definitions of feasible restructuring credit events. While most CDSs on North American entities follow the so-called modified (frequently referred to as Mod R) restructuring, CDSs on European entities follow the modified-modified (Mod Mod R) restructuring convention. Packer and Zhu (2005) show that differences in the restructuring clause of CDSs can non-negligibly affect their pricing. We therefore split our sample into two regional subsamples: North America and Europe. Within each subsample, we only consider corporate single-name contracts with a maturity of five years, since these are the most actively traded. We further restrict our sample to contracts written on senior unsecured debt to avoid any bias due to differences in seniority.

On April 8, 2009, the ISDA issued a supplement to the 2003 ISDA contractual standards that primarily led to a refinement of the credit event definitions and the terms of settlement. Two of these changes potentially affect our simulation results: (i) CDS contracts on North American entities do not include debt restructuring as a credit event. We therefore consider ex restructuring (XR) contracts for the North American subsample from the introduction of the supplement onwards. We further exclude contracts with an event window containing spread changes based on mixed restructuring clauses. (ii) CDSs are traded with a fixed coupon. For instance, CDSs on North American entities can be traded with a fixed coupon of 100 or 500 basis points. Additionally, a variable upfront payment can be used to account for the specific credit risk of a certain reference entity. However, fixed coupons and upfront payments can easily be converted into the previously used variable par spread and should therefore not affect our results. The final North American subsample consists of 2,146,519 daily quotes denominated in US dollars. The European subsample consists of 1,186,072 daily quotes denominated in Euros.

Additional data on stock market indices, volatility indices, and swap zero curves are obtained from Bloomberg. As stock market indices we use the Standard & Poor's (S&P) 500 for North America and the DJ Euro Stoxx for Europe. Stock market volatility is measured by the VIX Index for the North American subsample and the VSTOXX for the European subsample. As a proxy for the overall level of the zero curves we use five-year swap rates denominated in US dollars and Euros. The slope of each curve is estimated by the difference between the 10-year and one-year swap rates.

4.2.2 Simulation Methodology

We apply Brown and Warner's (1985) simulation approach to investigate the statistical properties of different test statistics and normal CDS spread change models. Based on 5,000 randomly drawn samples of realized spread changes, we examine the size and power for each combination of test statistic and normal CDS spread change model. We further assess the robustness of our results by varying the sample size and level of abnormal spread changes. In additional tests we examine how well the different test statistics behave in the face of event-induced variance and determine the suitability of bootstrapped p-values as a remedy for the non-normality of spread changes.

In a first step, we generate 5,000 samples consisting of 200 CDS contracts each. The contracts are randomly selected from our initial dataset. We further assign a random event date to each contract. There is no general rule as to whether the asset and event date should be drawn sequentially (Barber and Lyon, 1997; Brown and Warner, 1980, 1985) or simultaneously (Bessembinder et al., 2009). Moreover, related studies applying the sequential approach also differ in the order of selecting the asset and event date. Bessembinder et al. (2009) argue that the optimal random sampling procedure depends on the specific structure of the underlying database. For the case of CDSs, the number of tradable contracts as well as the quote frequency increase over time. We therefore follow the approach suggested by Bessembinder et al. (2009) and simultaneously select contract–event date combinations. This procedure leads to samples that are weighted toward later dates and firms that exhibit longer spread histories.

In unreported results available upon request, we conduct all simulations applying both sequential methods, but the results are virtually unchanged. We also control for contracts with insufficient spread data during the event and estimation period. Each combination of contract and event date must meet the following additional requirements to remain in a sample: (i) Spread changes must be observable for each day of the event window, which includes the 41 trading days, from -20 to 20; (ii) spread changes must be observable for at least 50% of the estimation window, which includes the 150 trading days, from -170 to -21; and (iii) the percentage of zero spread changes should not exceed 10%. If a combination does not meet all the criteria, the observation is dropped and a new random combination is drawn. Similar to Bessembinder et al. (2009) this restricition should address potential biases caused by illiquid contracts.[5] We repeat this procedure for each sample until we reach the final sample size of 200 contracts.

4.3 Measuring Abnormal CDS Spreads

In this section, we discuss the methodological particularities of event studies employing CDSs. Column 5 of Table 4.1 shows that different spread change measures are used in the literature, namely, relative and absolute spread changes. Therefore, we discuss the suitability of both measures and provide a rationale for the use of relative spreads. We also briefly review normal return models applied in the classical event study framework and discuss their adaptation to CDSs. Column 4 of Table 4.1 describes the various models used to calculate expected spread changes in the prior literature. In addition, we go beyond these classical models and introduce a (four-) factor model for CDSs. In our view, it is appropriate to use a specific factor model for expected (i.e., normal) CDS spread changes, since the recent empirical literature documents the importance of several determinants not accounted for in classical models. Accordingly, we build on the factors identified in the empirical literature on CDS spreads when specifying our factor model. Finally, column 6 of Table 4.1 displays the different test statistics employed in previous studies. The most prevalent parametric and non-parametric test statistics used to assess the presence of abnormal spread changes are discussed in the Appendix B.

4.3.1 Spread Change Measures

In stock or bond event studies, abnormal returns are calculated to measure the impact of any event on security prices. In contrast, so-called abnormal spread changes are

[5] We also run all simulations without the third restriction. All results are qualitatively similiar.

calculated whenever researchers quantify the effect of a firm-specific event on a firm's credit risk with the help of CDS data. A spread change measures the change in the premium of newly issued default swap contracts with constant maturity.[6] Independent of the way spread changes are calculated, an abnormal spread change can be defined as the realized spread change minus the normal spread change:

$$\Delta AS_{i,\tau} = \Delta S_{i,\tau} - E\left[\Delta S_{i,\tau}|\Omega_\tau\right] \tag{4.1}$$

where $\Delta AS_{i,\tau}$, $\Delta S_{i,\tau}$, and $E\left[\Delta S_{i,\tau}|\Omega_\tau\right]$ are the abnormal, realized, and normal spread changes, respectively, for contract i at event date τ. The normal spread change is the expected spread change conditional on the information set Ω_τ (e.g., past spread changes). In the following we present the different methods to calculate spread changes. After that we discuss different models for normal spread changes.

Both absolute (Hull et al., 2004; Norden and Weber, 2004; Galil and Soffer, 2011) and relative spread changes (Callen et al., 2009; Shivakumar et al., 2011) are used in the literature. The daily absolute spread change is simply the daily difference in the CDS spread:

$$\Delta_{abs} S_{i,\tau} = S_{i,\tau} - S_{i,\tau-1} \tag{4.2}$$

The daily relative CDS spread is the percentage change of daily spreads. We apply continuous compounding by calculating the difference in the logarithm of daily spreads:

$$\Delta_{rel} S_{i,\tau} = ln\left(S_{i,\tau}\right) - ln\left(S_{i,\tau-1}\right) \tag{4.3}$$

Even though relative spread changes are closer to the concept of returns, early CDS event studies in particular use absolute spread changes (Hull et al., 2004; Norden and Weber, 2004). In principle, the application of both calculation methods is correct. However, the distributional properties using one or the other may be quite different. This can potentially affect the size and power of the test in the presence of abnormal spread changes. Furthermore, the economic interpretations of absolute and relative

[6] Spreads are only reported for newly issued contracts. However, a time series with transaction prices on a specific contract is necessary to calculate CDS returns. Furthermore, CDS returns differ for protection buyers and sellers. For a detailed discussion on the calculation of CDSs, see Berndt and Obreja (2010).

average abnormal spread changes are different. Therefore, the choice of the right measure also depends on the expected effect on the firm's credit risk. For most corporate events it seems more plausible to expect an event-induced effect on credit risk that is proportional to the firm's initial default probability and its loss, given default. In line with this argument, we recommend the use of relative spread changes as an appropriate measure in most applications.

4.3.2 Naïve Models

In this section we define two naïve models based on models employed in current stock and bond event studies.

Mean-Adjusted Spreads

The mean-adjusted model is possibly the simplest normal return model. Brown and Warner (1985) discuss this model in the context of stock returns. The normal return is the arithmetic average of realized returns in the estimation period. Accordingly, the abnormal return of stock i at event date τ is the difference between the realized return and the estimated mean return. Handjinicolaou and Kalay (1984) use a similar model for bonds. Instead of realized bond returns, they calculate returns in excess of matched Treasury securities. Applying the event study methodology to CDS data, we estimate the normal return calculating the sample mean spread change of firm i in the estimation period:

$$\overline{\Delta S_i} = \frac{1}{150} \sum_{t=-170}^{-21} \Delta S_{i,t} \tag{4.4}$$

Assuming that the normal spread can differ by firm but is constant over time, the abnormal spread change of firm i at event date τ is

$$\Delta AS_{i,\tau} = \Delta S_{i,\tau} - \overline{\Delta S_i} \tag{4.5}$$

Brown and Warner (1985) show that short-term stock event studies based on the mean adjusted model yield similar results to event studies based on more sophisticated models such as the market model or the capital asset pricing model (CAPM). However, Bessembinder et al. (2009) cannot confirm this finding for bond event studies. They document that the mean-adjusted model is the least powerful of all examined approaches. Therefore, the performance of this model in the context of CDS event studies is an open question.

Market Model

The market model is the workhorse of stock event studies. This model is a single-factor asset pricing model based on the assumption of a stable linear relation between individual stock returns and the return of a broad market index. We adapt this model to the case of CDS spreads and define the model equation

$$\begin{aligned} \Delta S_{i,\tau} &= \alpha_i + \beta_i \Delta S_{index,\tau} + \epsilon_{i,\tau} \\ E\left[\epsilon_{i,\tau}\right] &= 0 \quad VAR[[\epsilon_{i,\tau}] = \sigma^2_{\epsilon_i} \end{aligned} \tag{4.6}$$

where $\Delta S_{index,\tau}$ is the spread change of the CDS index and $\epsilon_{i,\tau}$ is the zero mean disturbance term. The daily index spread at time τ is equal to the mean credit default spreads of all firms in our dataset at time τ.[7] The parameters of the market model are α_i, β_i, and $\sigma^2_{\epsilon_i}$. We estimate the model parameters for each firm based on the spread changes in the estimation period. The abnormal spread change of firm i at event date τ is, accordingly,

$$\Delta AS_{i,\tau} = \Delta S_{i,\tau} - \widehat{\alpha}_i + \widehat{\beta}_i \Delta S_{index,\tau} \tag{4.7}$$

MacKinlay (1997) points out that the ability to detect event effects increases with the R^2 of the market model regressions. This implies that the market model dominates the mean-adjusted model in terms of size and power if it also exhibits greater explanatory power for spread changes.

[7]We calculate separate indices for North America and Europe because of the different restructuring clauses. We additionally construct an equally weighted index of all (non-)investment-grade contracts that is used for robustness tests based on the (non-)investment-grade subsample.

4.3.3 Matching Portfolios

Another technique often applied in bond and long-term stock event studies to generate normal returns is the matched portfolio approach. The abnormal return is the difference between the respective firm's realized return and the return of a reference portfolio. The reference portfolio contains firms that resemble the event firm in certain risk characteristics but are assumed to be unaffected by the particular event. While matching on firm characteristics such as size or the market-to-book ratio is common for long-term stock event studies (Ritter, 1991; Loughran and Ritter, 1995; Barber and Lyon, 1997), the bond rating is the most important matching criterion in bond event studies (Asquith and Kim, 1982; Bessembinder et al., 2009). Since CDS spreads should depend on the expected default probability and the expected loss of the reference obligation, we adapt the matching to ratings. Therefore, we define the abnormal spread change as the difference between the realized spread change and the spread change of a rating-equivalent reference portfolio:

$$\Delta AS_{i,\tau} = \Delta S_{i,\tau} - \Delta S_{RE,\tau} \qquad (4.8)$$

where $\Delta S_{RE,\tau}$ is the spread change of the rating-equivalent portfolio. For each rating letter we calculate daily spread changes as the average spread change of all available contracts with the corresponding rating while we exclude event firms for their entire event period. The rating for each contract is the average issuer rating of S&P and Moody's as reported by Markit Ltd.

Bessembinder et al. (2009) recommend applying a value-weighted matched portfolio approach for bond event studies. Based on their simulation results, they conclude that this approach combined with non-parametric test statistics is well specified and dominates all other approaches in terms of power. The reluctance of rating agencies to make timely rating adjustments (also referred to as rating stickiness), as documented, for example, by Posch (2011), could be seen as a potential disadvantage of adopting the portfolio approach to CDS spreads.

4.3.4 The CDS Factor Model

The three aforementioned approaches are an adaptation of the classical event study methodology. In them, we implicitly assume that spread changes are sufficiently explained by common pricing factors. However, researchers have identified additional important determinants of credit spread changes. Collin-Dufresne et al. (2001) investigate the impact of possible determinants of credit risk on bond spread changes. They derive theoretical determinants of spread changes from structural models of default. Their findings imply that aggregate factors exhibit a higher explanatory power than firm-specific factors. Ericsson et al. (2009) conduct a similar regression analysis with CDSs. They identify leverage, equity implied volatility, and the level of the Treasury yield curve as major determinants of credit spread changes. Alexander and Kaeck (2008) apply a regime-switching model to identify the regime-dependent determinants of CDS index spread changes. In addition to the previous findings, they document a statistically significant relation between the slope of the risk-free yield curve and spread changes. Furthermore, they provide evidence that the factor loadings of major determinants significantly differ in regimes of high-/low-volatility CDS markets.[8]

Based on these findings, we consider the following market-wide factors as potential explanatory variables in our factor model: (i) the level of the risk-free yield curve, (ii) the slope of the risk free yield curve, (iii) the equity implied volatility, and (iv) stock market performance. We use five-year swap rates as a proxy for the level of the risk-free yield curve. The difference between 10- and one-year swap rates serves as a proxy for the slope. We use swap zero curves instead of Treasury zero curves since the results of Hull et al. (2004), Blanco et al. (2005), and Houweling and Vorst (2005) indicate that the swap zero curve seems to be the relevant risk-free rate on credit derivative markets. Following the literature on the determinants of spread changes (e.g. Collin-Dufresne et al., 2001; Ericsson et al., 2009), we measure the equity implied volatility by the VIX index for the North American subsample and the VSTOXX for the European subsample. The stock market index for the North American subsample is the S&P 500.

[8] We estimate individual model parameters for each firm and consider short time periods. We therefore do not employ a regime-switching approach. However, researchers conducting long-term event studies should allow for time-varying model parameters.

The stock market index for the European subsample is the Dow Jones Euro Stoxx Index.

An important step in the derivation process of our final factor model is the identification of potentially redundant variables. Within the estimation window, multicollinearity does not reduce the predictive power of the model as a whole. It only affects the estimate of the individual factor loadings. However, if the pattern of multicollinearity in the estimation window differs from that in the event window, the out-of-sample prediction (abnormal spread changes of the event window) may suffer from large prediction errors. We therefore calculate the pairwise correlation coefficients of all the determinants of credit spread changes identified in the literature and test for multicollinearity using variance inflation factors. Table 4.2 provides an overview of the pairwise correlation coefficients for relative spread changes. We find that stock market returns and equity implied volatility are highly correlated, with a correlation exceeding -70% in both subsamples. Since our tests for multicollinearity based on all variables also indicate a problem with the variable stock market returns, we drop this variable.[9]

Table 4.3 provides the variance inflation factors based on the remaining variables. Since all variance inflation factors are close to one, we find no signs of serious multicollinearity. Based on the remaining variables, we define the abnormal spread change of firm i at event date τ as

$$\Delta AS_{i,\tau} = \Delta S_{i,\tau} - \widehat{\alpha}_i - \widehat{\beta}_{1,i}\Delta S_{index,\tau} - \widehat{\beta}_{2,i}\Delta Level_\tau - \widehat{\beta}_{3,i}\Delta Slope_\tau - \widehat{\beta}_{4,i}\Delta Vola_\tau \quad (4.9)$$

where $\Delta Level_\tau$ and $\Delta Slope_\tau$ are the proxies for the level and slope of the risk-free yield curve and $\Delta Vola_\tau$ is the proxy of the equity implied volatility.

[9]In unreported results we also conduct all simulations with the full factor model. The additional factor does not improve the results.

Table 4.2: Factor Correlation

Panel A: North America

	ΔS_{Index}	$\Delta Level$	$\Delta Slope$	$\Delta Vola$	$\Delta Stock$
ΔS_{Index}	1.00				
$\Delta Level$	-0.14	1.00			
$\Delta Slope$	-0.01	0.10	1.00		
$\Delta Vola$	0.26	-0.25	0.01	1.00	
$\Delta Stock$	-0.29	0.32	0.01	-0.74	1.00

Panel B: Europe

	ΔS_{Index}	$\Delta Level$	$\Delta Slope$	$\Delta Vola$	$\Delta Stock$
ΔS_{Index}	1.00				
$\Delta Level$	-0.17	1.00			
$\Delta Slope$	0.03	0.16	1.00		
$\Delta Vola$	0.36	-0.25	-0.02	1.00	
$\Delta Stock$	-0.41	0.36	0.02	-0.75	1.00

Table 4.3: Variance Inflation Factors

Panel A: All Variables

	ΔS_{Index}	$\Delta Level$	$\Delta Slope$	$\Delta Vola$	$\Delta Stock$
North America	3.10	1.14	1.01	2.23	4.01
Europe	3.22	1.18	1.03	2.33	5.04

Panel B: Factor Model Variables

	ΔS_{Index}	$\Delta Level$	$\Delta Slope$	$\Delta Vola$
North America	1.08	1.09	1.01	1.14
Europe	1.17	1.11	1.03	1.20

4.4 Results

4.4.1 Distributional Properties of CDS Spreads

Before we analyze the size and power for each combination of test statistic and normal CDS spread change model, we provide a basic description of the distributional properties of our initial dataset. We examine the distributional characteristics of realized CDS spread changes and abnormal CDS spread changes to obtain a first impression about which test statistics might be best suited for the detection of any firm-specific event's effect on credit risk.

Panel A of Table 4.4 shows descriptive statistics for the entire sample based on relative spread changes. Statistics are further broken down into two subsamples: investment grade and non-investment-grade reference entities. As a striking first result, we observe positive mean spread changes for all subsamples. This effect is independent of the references entities' rating, region, and applied spread change measure, indicating a general widening of spreads over our observation period. Because all median spread changes are equal to zero, the resulting distributions exhibit positive skewness. Furthermore, we observe a positive excess kurtosis similar to daily stock returns (e.g. Cont, 2001). These features suggest that realized spread changes follow a non-Gaussian distribution. As can be seen in Panel B of Table 4.4, the aforementioned distributional properties largely apply to absolute spread changes, too. However, skewness and excess kurtosis are much larger when considering absolute compared to relative spread changes.

Table 4.4: Descriptive Statistics (Realized Spread Changes)

Panel A: ***Statistics of Relative CDS Spread Changes* (%), 0.01 = 1%**

North America

	Number of observations	Mean	Median	Standard deviation	Skewness	Excess kurtosis	Positive spreads	Zero spreads
All	2,091,244	0.0002	0.0000	0.0466	1.1860	324	38.4%	22.9%
Investment grade	1,352,614	0.0002	0.0000	0.0158	1.5441	219	39.4%	21.8%
Non-investment grade	738,630	0.0003	0.0000	0.0480	0.6150	480	37.8%	24.9%

Europe

	Number of observations	Mean	Median	Standard deviation	Skewness	Excess kurtosis	Positive spreads	Zero spreads
All	1,083,294	0.0004	0.0000	0.0461	1.8442	500	39.3%	19.4%
Investment grade	922,296	0.0004	0.0000	0.0454	1.7884	183	39.3%	19.7%
Non-investment grade	160.998	0.0003	0.0000	0.0495	2.0791	1,786	39.0%	18.0%

Panel B: ***Descriptive Statistics of Absolute CDS Spread Changes* (basis points (bps)), 0.01 = 1 bps**

North America

	Number of observations	Mean	Median	Standard deviation	Skewness	Excess kurtosis	Positive spreads	Zero spreads
All	2,091,244	0.0032	0.0000	0.9520	25.4	18,116	38.4%	22.9%
Investment grade	1,352,614	0.0002	0.0000	0.1570	6.4	5,621	39.4%	21.8%
Non-investment grade	738,630	0.0087	0.0000	1.5884	15.5	6,627	37.8%	24.9%

Europe

	Number of observations	Mean	Median	Standard deviation	Skewness	Excess kurtosis	Positive spreads	Zero spreads
All	1,083,294	0.0011	0.0000	0.4336	184.1	167,924	39.3%	19.4%
Investment grade	922,296	0.0008	0.0000	0.1659	347.2	226,328	39.3%	19.7%
Non-investment grade	160,998	0.0034	0.0000	1.0600	78.3	31,309	39.5%	18.0%

Notes: This table provides summary statistics for daily spread changes for the North American and European samples, as well as for subsamples by issuer rating. Daily CDS spread data are from the daily CDS database of Markit Ltd. and cover the period from February 11, 2003, to July 31, 2011. Panel A reports the results for the relative spread change measure. Panel B reports the results for the absolute spread change measure.

As discussed in Section 4.2.1, the selection of the correct spread change measure depends on its distributional properties and the assumed event effect. While mean relative spread changes do not change with the rating, we observe an increase in mean absolute spread changes as the rating decreases from investment grade to non-investment grade. This suggests that not just the spread level but also the absolute spread change is statistically negatively related to the reference entities' rating. We also find a negative statistical relation between the rating and the standard deviation. As already pointed out in Section 4.2.1, these findings support the notion that relative spread changes should be applied in CDS event studies. This does not necessarily imply that abnormal CDS spread changes and the related test statistics also suffer from non-Gaussian distributions. Table 4.5 displays the distributional properties of abnormal spread changes obtained by using the normal return models described above.

At this point, no abnormal performance has been introduced. Nevertheless, the results in Table 4.5 show deviations from zero, both for mean and median daily abnormal spread changes. Compared to the spread changes discussed above, departures from normality are a little less pronounced for relative abnormal spread changes (Panel A). However, daily abnormal spread changes still exhibit positive skewness and excess kurtosis. This indicates that parametric test statistics may yield biased results. In addition, the distribution of abnormal CDS spread changes seems to differ substantially across the different normal return models. The CDS factor model seems to produce the lowest level of standard errors, skewness and excess kurtosis. In contrast, the matching portfolio approach results in abnormal spreads that exhibit the highest levels of skewness and excess kurtosis. Concerning absolute CDS spread changes (Panel B), all models except the CDS market model show comparatively large positive abnormal spread changes. Skewness and excess kurtosis remain very high for absolute CDS spread changes.

As discussed above, our main analysis focuses on relative CDS spread changes.[10] Moreover, all simulations yield very similar results for the subsamples of investment-grade and non-investment-grade reference entities.Accordingly, we do not present

[10]We report results for the empirical size and power of absolute CDS spread changes in Appendix A.

detailed results for these subsamples. They are available upon request.

Table 4.5: Descriptive Statistics (Abnormal Spread Changes)

Panel A: ***Statistics of Relative CDS Spread Changes (%), 0.01 = 1%***

North America

	Mean	Median	Standard deviation	Skewness	Excess kurtosis	Positive spreads
Mean-adjusted spreads	-0.0001	-0.0003	0.0379	1.1	111	48.6%
Market model	0.0002	-0.0001	0.0357	1.1	139	49.3%
CDS factor model	0.0001	-0.0002	0.0062	1.0	59	49.2%
Portfolios (rating)	-0.0002	-0.0003	0.0491	1.4	227	49.4%

Europe

	Mean	Median	Standard deviation	Skewness	Excess kurtosis	Positive spreads
Mean-adjusted spreads	-0.0001	-0.0003	0.0378	0.6	85	48.5%
Market model	-0.0003	-0.0003	0.0323	1.2	162	48.7%
CDS factor model	-0.0001	-0.0002	0.0054	1.1	62	48.9%
Portfolios (rating)	-0.0001	-0.0002	0.0493	1.3	267	49.3%

Panel B: ***Descriptive Statistics of Absolute CDS Spread Changes (basis points (bps)), 0.01 = 1 bps***

North America

	Mean	Median	Standard deviation	Skewness	Excess kurtosis	Positive spreads
Mean-adjusted spreads	0.0044	0.0001	1.15	2.2	10,528	50.6%
Market model	0.0047	0.0001	1.16	1.9	10,657	50.8%
CDS factor model	0.0012	0.0002	3.59	1.8	5,373	50.5%
Portfolios (rating)	0.0056	0.0000	1.21	1.6	9,486	48.8%

Europe

	Mean	Median	Standard deviation	Skewness	Excess kurtosis	Positive spreads
Mean-adjusted spreads	0.0017	0.0000	0.50	343.2	163,039	50.2%
Market model	0.0009	-0.0001	0.49	351.6	168,677	49.5%
CDS factor model	0.0007	0.0000	1.12	334.9	81,364	50.2%
Portfolios (rating)	0.0024	0.0000	0.52	303.4	139,298	47.7%

Notes: This table provides summary statistics for daily relative abnormal spread change for each model of abnormal spread changes. The results are based on 5,000 replications of 200 randomly drawn CDS contract-event day combinations. We do not add any abnormal performance. Panel A reports the results for the relative spread change measure. Panel B reports the results for the absolute spread change measure.

4.4.2 Size of Tests

We start our main analysis by estimating the empirical size of the different test statistics. Tables 4.6 and 4.7 contain results for our simulation of 5,000 randomly drawn samples of 200 abnormal CDS returns and document the probabilities with which the null hypothesis of no abnormal performance is rejected. Probabilities are based on a standard t-test (both time series and cross-sectional), the test of BMP (1991), a rank test, and a generalized sign test. If a test statistic is well specified, the empirical rejection rate should not deviate significantly from the assumed theoretical significance level. Considering a two-sided test at the 5% level of significance, this corresponds to a rejection rate of 2.50% in the lower tail and 2.50% in the upper tail. We follow the common assumption that the underlying binominal distribution of success is approximately normal for a large number of trials (Bessembinder et al., 2009; Campbell et al., 2010) to test whether the empirical rejection rates deviate significantly from 2.50%. On the basis of 5,000 random samples, the test statistic should be between 2.07% and 2.93% in 95% of cases.[11] As Bessembinder et al. (2009), we are primarily concerned with rejection rates that are too high in the absence of abnormal spread changes, that is, Type I errors, or overrejections. All combinations of normal return models and test statistics that yield rejection rates that are significantly higher than 2.93% erroneously show a significant effect of a (non-existent) event on spread changes and should therefore not be used in CDS event studies. Even though too low a rejection rate is not directly a problem for the specification of the test statistics, we also mark significant underrejection in the tables. A rejection rate that is too low under validity of the null hypothesis of no abnormal return may indicate low power. The results are reported by region in Table 4.6, with North American spreads in Panel A and European data in Panel B.

[11] Under the assumption that the outcomes of each test of the 5,000 trials are independent, the underlying Bernoulli process implies a mean rejection rate of 0.025 (lower or higher tail of 2.50%), with a standard deviation of 0.0022 ($= \sqrt{0.025 \times 0.975}/\sqrt{5,000}$). The proportion of rejections should hence be between $0.025 \pm (1.96 \times 0.0022) = 2.07\%$ and 2.93% in 95% of the cases for a significance test at the 5% level.

Table 4.6: Size of Tests (Relative Spread Changes)

Panel A: North America

Lower tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
Mean-adjusted spreads	1.98%*	2.08%	1.60%*	2.16%	2.46%
Market model	2.06%*	1.56%*	1.48%*	2.02%*	3.06%*
CDS factor model	2.76%	1.96%*	1.68%*	2.80%	2.86%
Portfolios (rating)	2.42%	2.66%	3.26%*	2.50%	6.98%*

Upper tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
Mean-adjusted spreads	3.10%*	2.32%	2.76%	2.70%	1.86%*
Market model	3.64%*	2.28%	2.90%	2.82%	1.70%*
CDS factor model	5.02%*	2.38%	2.82%	2.78%	2.38%
Portfolios (rating)	3.12%*	1.66%*	1.60%*	2.22%	0.68%*

Panel B: Europe

Lower tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
Mean-adjusted spreads	2.24%	2.84%	1.38%*	2.10%	3.14%*
Market model	3.46%*	3.80%*	2.88%	3.06%*	4.94%*
CDS factor model	2.82%	2.32%	1.44%*	2.28%	3.26%*
Portfolios (rating)	2.24%	2.84%	1.38%*	2.10%	4.14%*

Upper tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
Mean-adjusted spreads	2.18%	1.92%*	3.02%*	2.42%	1.60%*
Market model	2.42%	0.98%*	1.64%*	1.80%*	1.14%*
CDS factor model	2.64%	2.16%	2.50%	2.14%	1.88%*
Portfolios (rating)	2.18%	1.92%*	3.02%*	2.42%	1.60%*

Notes: This table reports the day zero rejection rates of the null hypothesis of no abnormal performance for different test statistics ($\alpha = 2.5\%$, one-tailed). The results are based on 5,000 replications of 200 randomly drawn CDS contract-event day combinations. For the normal approximation of the 5,000 binomial trials the average rejection rate should be between 2.07% and 2.93% (at the 95% confidence interval).

The result in both panels of Table 4.6 show that only the non-parametric rank test is well specified for most models. All other test statistics show rejection rates that are strongly asymmetric and lie outside of the expected range.

In the lower tail, rejection rates are generally too low. On the other hand, most test statistics reject the null hypothesis too frequently in the upper tail. With the exception of the test of BMP (1991), however, rejection rates in the lower and upper tails frequently sum to 5%. The test of BMP (1991) also reveals an asymmetric distribution but leads to rejection rates that are often too low in both tails. As mentioned above, this is not directly a problem for the specification of this test statistic but it may point to a lower power. These findings are robust across regions and credit quality.

With respect to the normal return models, we find that the market model performs worst in both subsamples, regardless of the test statistic used. In the lower tail, all test statistics show significant deviations from the theoretical significance level. The matched portfolio approach performs slightly better. Together with the CDS factor model and the mean-adjusted model, it shows rejection rates that are consistently within the expected range for the non-parametric rank test. Again, our results do not show qualitative differences across the regional subsamples and do not seem to be affected by credit quality (not reported here but available upon request).

Summarizing the results of the size tests, we find that the non-parametric rank test is the only test statistic that is well specified across all models and should hence be used for event studies based on CDS data. The test of BMP (1991) also does not lead to excessive Type I errors, but we expect low power due to very low rejection rates. In terms of normal return models, researchers should either use a CDS factor model or, alternatively, when factor data are not available, the simple mean-adjusted model.

4.4.3 Power of Tests

In this section, we examine the performance of the different models and test statistics with regard to potential Type II errors (not rejecting the null of no abnormal CDS spread change when it is false). By introducing positive and negative relative spread change shocks on day zero of +0.5% and -0.5%, respectively, we observe how frequently the null hypothesis of no abnormal CDS spread change is correctly rejected. We think that a spread change of 0.5% is reasonable as, among others, Shivakumar et al. (2011) find average abnormal spread changes between 0.2% and 1.5% in the context of managerial forecast announcements.[12]

Based on the results displayed in Table 4.7, we document the same empirical pattern across different regions. Surprisingly and in contrast to the findings of Bessembinder et al. (2009) in the context of abnormal bond returns, our simulations show that the matched portfolio approach used to calculate abnormal spread changes is the least powerful of all models. Hence, using this model in CDS event studies may lead to biased inferences. Independent of the use of parametric and non-parametric test statistics, the CDS factor model seems to be the most powerful model since it exhibits the highest detection rates overall. Similar to the findings of Bessembinder et al. (2009) in the context of abnormal bond returns, non-parametric test statistics are more powerful in detecting abnormal spread changes compared to the standard parametric tests. Given shocks of +0.5% and -0.5%, respectively, the non-parametric rank test rejects the null hypothesis in at least 98% of the cases across all models. The power of the generalized sign test is qualitatively similar but performs slightly worse when positive shocks are introduced. The performance of the simple t-statistics is very volatile since rejection rates range between 25% and 70%, depending on the CDS model used. Hence, these are the least powerful test statistics in CDS event studies. The power of the non-parametric statistic of BMP (1991) is in between the simple test statistics and the non-parametric test statistics. Its rejection rates range between 55% and 82%.

In sum, we conclude that relying on the CDS factor model and using the non-parametric generalized sign or rank test is the best method to detect abnormal CDS spread changes that are in fact true. Alternatively, if, for example, the data to construct the CDS factor model are not available, researchers can also rely on the mean-adjusted model since it is also well specified and performs only slightly worse than the CDS factor model if the non-parametric tests are used.

[12] Furthermore we think that introducing a shock of +/- 0.5% is "conservative", since this "artificial" shock accounts for less than one-third of the daily realized unsigned spread changes (the average unsigned relative spread change for our European sample is 1.84% and that for our North American sample is 1.66%).

Table 4.7: Power of Tests (Relative Spread Changes)

Panel A: North America

Relative CDS Spread Changes: +0.50%

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
Mean-adjusted spreads	48.44%	56.84%	75.50%	99.74%	98.88%
Market model	56.82%	63.88%	80.89%	99.70%	98.68%
CDS factor model	59.70%	64.26%	82.53%	99.26%	96.60%
Portfolios (rating)	25.73%	40.88%	55.91%	98.33%	93.66%

Relative CDS Spread Changes: -0.50%

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
Mean-adjusted spreads	43.86%	51.82%	61.08%	99.36%	99.30%
Market model	50.14%	56.40%	68.60%	99.46%	99.60%
CDS factor model	51.01%	58.58%	70.35%	99.53%	99.68%
Portfolios (rating)	26.96%	42.22%	57.76%	98.72%	99.86%

Panel B: Europe

Relative CDS Spread Changes: +0.50%

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
Mean-adjusted spreads	43.92%	50.28%	74.66%	99.58%	98.26%
Market model	59.52%	63.26%	79.98%	99.54%	97.96%
CDS factor model	60.03%	64.89%	81.06%	99.62%	98.82%
Portfolios (rating)	27.32%	40.26%	55.36%	98.18%	93.38%

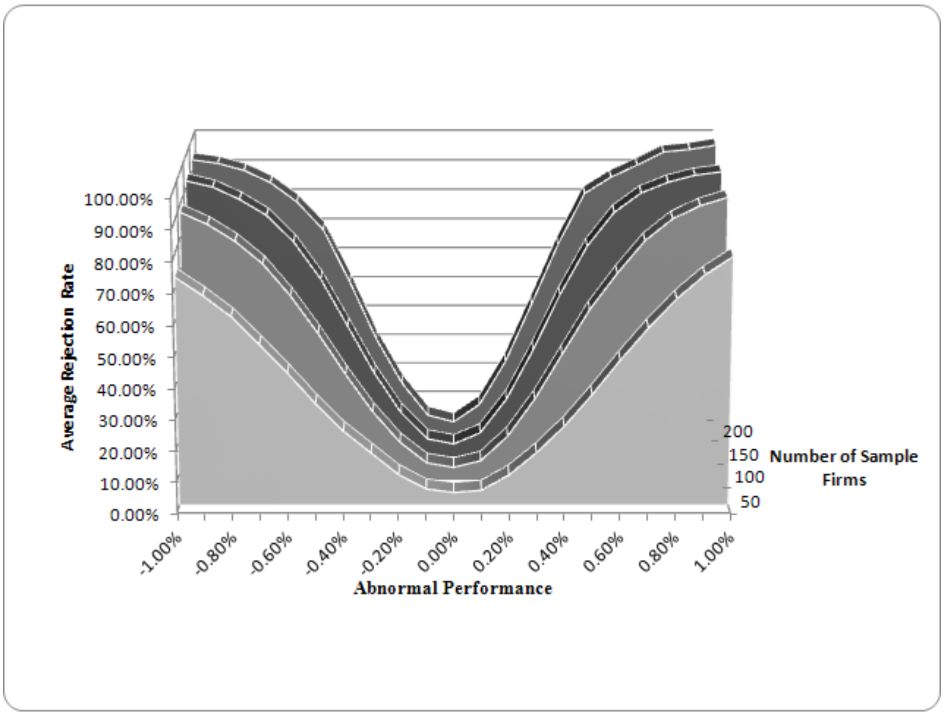
Relative CDS Spread Changes: -0.50%

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
Mean-adjusted spreads	47.20%	52.42%	60.10%	99.56%	99.48%
Market model	69.14%	70.88%	77.84%	99.76%	99.80%
CDS factor model	70.34%	71.84%	78.65%	99.85%	99.78%
Portfolios (rating)	29.90%	44.54%	59.44%	98.34%	99.78%

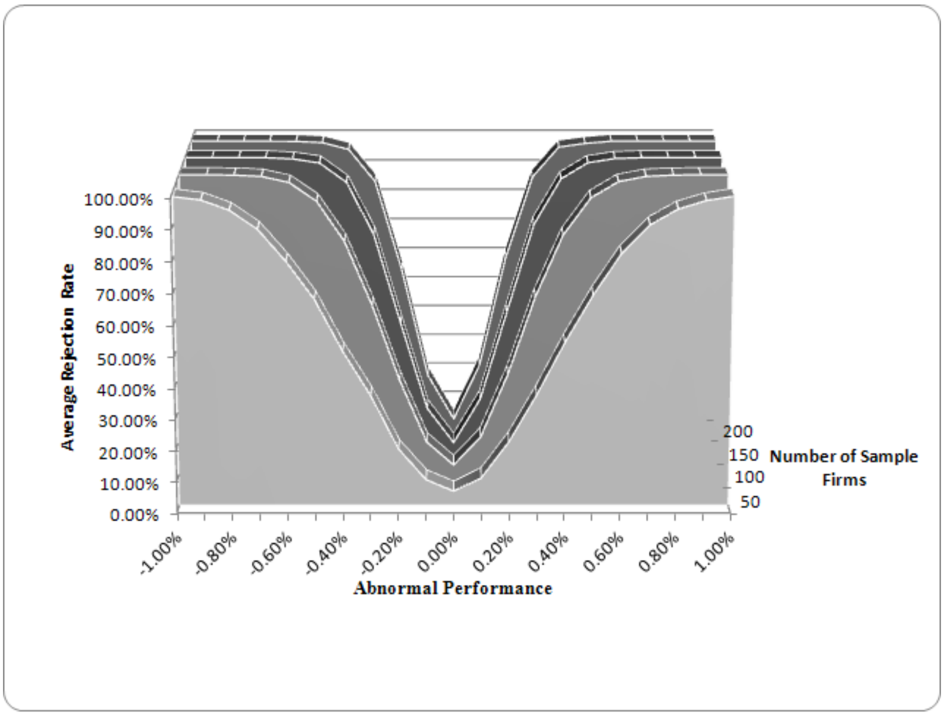
Notes: This table reports the day zero rejection rates of the null hypothesis of no abnormal performance for different test statistics ($\alpha = 5\%$, two tailed). The results are based on 5,000 replications of 200 randomly drawn CDS contract-event day combinations. We add abnormal spread changes (relative) at day zero of 0.5% and -0.5%.

Given the above conclusions, we now take into account the fact that the sample size and level of shocks in CDS event study applications may differ with regard to the specific event under scrutiny. Therefore, similar to Bessembinder et al. (2009), we run simulations where the number of observations varies from 50 to 200 and the abnormal shock varies from -1% to +1%. We apply the CDS factor model, which has been shown to be the best-performing model in the context of CDS event studies in our previous analyses. We further restrict our analysis to the test of BMP (1991) among the parametric tests and the rank test among the non-parametric tests since both tests perform best in their respective groups. Our goal is to evaluate the power of the different test statistics under the changing parameters (number of observations and level of shock). Due to the complexity of the results, they are better presented in graphical form, as seen in parts (a) and (b) of Figure 4.1. The most important finding is that the conclusions we draw on the basis of +/- 0.5% shocks hold true for different sample sizes and different levels of shocks. As Figure 4.1 shows, we observe that the non-parametric rank test performs substantially better than the parametric test of BMP (1991) along all different dimensions.

Figure 4.1: The Power of Tests for Different Sample Sizes.



(a) The BMP (1991) test



(b) The rank test

4.5 Robustness and Extensions

4.5.1 Event-Induced Variance Increases

Higgins and Peterson (1998) and Harrington and Shrider (2007) argue that almost any event will induce an increase in cross-sectional variance. This implies that the variance of abnormal returns is higher in the event window (compared to the estimation window). It has been shown that such an event-induced increase in variance can lead to a severe bias of classic test statistics (e.g. Brown and Warner, 1985; Corrado, 1989; Boehmer et al., 1991). This effect is exacerbated if the estimate of the variance of abnormal returns is based only on estimation window returns. For the null of no abnormal return, the event-induced increase in variance will lead to excess rejection rates. Accordingly, we expect the simple time series t-test in particular to be misspecified in the presence of event-induced variance increases.

On the other hand, the importance of this potential bias is disputed in the literature. First, it is not a priori obvious that all events necessarily increase the cross-sectional variance. Depending on the research setting, the event of interest may even lower uncertainty, thus leading to lower variance. Brown and Warner (1985, p. 22) speak of "*some* types of events" around which returns increase, which also implies that the type of event matters. Second, even in the presence of event-induced volatility, the impact on the validity of conventional test statistics may be very limited. As pointed out by Corrado (2011), the importance of the bias depends critically on whether interest is in the sample per se or an extraneous population of similar events.[13] In many cases, the sample can be very close to or even *be* the population itself (e.g., in the case of historical events). In these cases, where interest is in the mean event-induced return, variance increases are not relevant by definition. Test statistics that account for event-induced variance increases only become important if interest is in the population, that is, when inferences beyond the sample mean to the population mean are required. Corrado (2011, pp. 218-219) concludes,

[13] See Corrado (2011, p.216) for an extensive discussion.

> "A perusal of the finance literature suggests that many event studies limit themselves to statistical inferences about the mean event-induced return within the sample ... without projecting inferences onto the mean return of a parent population. This may suggest a conservative bias; however, this bias is diminished by the inevitable follow-up studies with extended data sets that form ongoing streams of research into interesting and important topics. Nevertheless, projecting inferences onto a population larger than the sample can often be instructive. In forming these inferences, cross-sectional variance adjustment procedures advanced in BMP (1991), Sanders and Robins (1991), or Corrado and Zivney (1992) are aptly recommended."

It is thus up to the researcher to decide whether the issue is important in a specific research setting. As a next step, we therefore model an event-induced variance increase and reexamine the performance of the different test statistics. Following the literature (e.g. Brown and Warner, 1985; Boehmer et al., 1991), we assume that the variance increases proportionally to the variance of abnormal returns in the estimation window. In most applications, the variance estimator of the individual time series is used. BMP (1991), however, additionally consider the average variance across all observations in the estimation window. In our simulation, we apply both methods. A constant shock μ as well as a normally distributed random variable with mean zero and a variance that is proportional to the variance of the estimation window (σ^2) are added to the realized abnormal spread change:

$$\mu + x \quad with \quad x \sim N(0, k\sigma^2) \tag{4.10}$$

with k standing for the proportionality factor. BMP (1991) derive economically plausible values for this factor based on several empirical papers (Charest, 1978; Mikkelson, 1981; Penman, 1982; Rosenstein and Wyatt, 1990) and conclude that values for k should lie in a range between 0.44 und 1.25. Accordingly, we use values of 0, 0.5, 1, and 1.5, with $k = 0$ standing for no increase in event-induced variance.

In line with our expectations, the findings in Table 4.8 indicate that the test of BMP (1991) is the only test statistic that is consistently well specified for a variance increase within an economically plausible range. All other test statistics reject the null of no abnormal return too frequently. As an exception, the generalized sign test and the cross-sectional t-test seem well specified, at least for comparatively low variance increases. The results of the power test in Table 4.9 further show that the power of the different test statistics decreases significantly with increases of the proportionality factor k. Given the comparatively low magnitude of the shock (+/- 0.50%), the power of all tests seems to be very low for values of k that exceed 0.5. Rejection rates for both the test of BMP (1991) and the non-parametric tests only become reliable for shocks larger than +/- 1% (results not tabulated here but available upon request). To summarize the size and power tests in Tables 4.8 and 4.9, the test of BMP (1991) seems to be the only test that produces reliable results in the presence of event-induced variance increases.

Table 4.8: Event-Induced Variance and Size of Tests

Panel A: ***Event-Induced Variance Proportional to Individual Security Estimation Window Variance***

Lower tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
k=0.0	2.76%	1.96%*	1.68%*	2.80%	2.86%
k=0.5	3.88%*	2.96%*	2.86%	4.28%*	2.16%
k=1.0	9.88%*	3.04%*	2.92%	5.94%*	2.19%
k=1.5	19.73%*	3.64%*	3.41%*	7.30%*	2.89%

Upper tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
k=0.0	3.02%*	2.38%	2.82%	2.78%	2.38%
k=0.5	3.75%*	2.30%	2.04%*	3.84%*	2.74%
k=1.0	6.80%*	2.98%*	2.41%	7.30%*	3.35%*
k=1.5	11.00%*	3.64%*	3.95%*	8.53%*	3.61%*

Panel B: ***Event-Induced Variance Proportional to Average Estimation Window Variance***

Lower tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
k=0.0	2.76%	1.96%*	1.68%*	2.80%	2.86%
k=0.5	3.59%*	1.91%*	1.86%*	4.17%*	2.14%
k=1.0	9.13%*	2.90%	2.23%	6.29%*	2.97%*
k=1.5	19.56%*	4.11%*	2.49%	7.61%*	3.04%*

Upper tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
k=0.0	3.02%*	2.38%	2.82%	2.78%	2.38%
k=0.5	4.41%*	2.38%	2.84%	6.03%*	2.67%
k=1.0	7.01%*	2.29%	2.86%	8.06%*	3.33%*
k=1.5	9.12%*	2.85%	2.89%	9.67%*	3.56%*

Notes: This table reports the day zero rejection rates of the null hypothesis of no abnormal performance for different test statistics ($\alpha = 2.5\%$, one-tailed). The results are based on 5,000 replications of 200 randomly drawn CDS contract-event day combinations. For the normal approximation of the 5,000 binomial trials the average rejection rate should be between 2.07% and 2.93% (at the 95% confidence interval).

Table 4.9: Event-Induced Variance and Power of Tests

Panel A: ***Event-Induced Variance Proportional to Individual Security Estimation Window Variance***

Relative CDS Spread Changes: +0.50%

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
k=0.0	59.70%	64.26%	82.53%	99.26%	96.60%
k=0.5	49.72%	48.32%	69.75%	92.42%	80.69%
k=1.0	44.56%	27.28%	50.86%	74.54%	53.14%
k=1.5	40.64%	12.72%	33.54%	58.14%	32.87%

Relative CDS Spread Changes: -0.50%

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
k=0.0	51.01%	58.58%	70.35%	99.53%	99.68%
k=0.5	54.58%	51.00%	72.98%	95.10%	82.86%
k=1.0	48.80%	29.18%	54.38%	78.08%	53.50%
k=1.5	42.68%	15.18%	34.74%	60.90%	35.76%

Panel B: ***Event-Induced Variance Proportional to Average Estimation Window Variance***

Relative CDS Spread Changes: +0.50%

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
k=0.0	59.70%	64.26%	82.53%	99.26%	96.60%
k=0.5	53.07%	46.38%	55.65%	82.65%	58.32%
k=1.0	44.02%	25.09%	26.90%	52.88%	28.30%
k=1.5	41.33%	12.67%	13.70%	35.97%	16.47%

Relative CDS Spread Changes: -0.50%

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
k=0.0	51.01%	58.58%	70.35%	99.53%	99.68%
k=0.5	54.58%	51.00%	72.98%	95.10%	82.86%
k=1.0	48.80%	29.18%	54.38%	78.08%	53.50%
k=1.5	42.68%	15.18%	34.74%	60.90%	35.76%

Notes: This table reports the day zero rejection rates of the null hypothesis of no abnormal performance for different test statistics (α = 5%, two tailed). The results are based on 5,000 replications of 200 randomly drawn CDS contract-event day combinations. We add abnormal spread changes (relative) at day zero of 0.5% and -0.5%.

4.5.2 Bootstrapped p-Values

If we take our simulation of event-induced variance increases into account, no single test statistic seems to be consistently well specified. In addition, parametric tests suffer from low power in detecting abnormal spread changes. This is mainly due to the fact that the empirical distributions of our test statistics are skewed, which contradicts the assumption of the normal distribution. Hull et al. (2004), without providing evidence, suggest a bootstrap approach to control for this bias. In a similar setting, Barber et al. (1999) show that even a skewness-adjusted t-test of abnormal buy-and-hold returns deviates from its theoretical distribution under validity of the null hypothesis. The authors argue that a simple bootstrap method will lead to much improved results. We adopt this approach for the case of CDSs. To do so, all abnormal announcement day spread changes are adjusted by their corresponding means. In the next step, 1,000 random samples of size $n/2$ are generated from the original test statistic.[14] This procedure results in an empirical approximation of the null distribution.

The results in Table 4.10 show that bootstrapped p-values consistently lead to rejection rates that are within the theoretically expected range. This holds across all normal return models. The asymmetry documented in Section 4.2 is no longer present, irrespective of regional subsamples. While bootstrapping apparently leads to large improvements in model specification, the power of the different test statistics is not positively affected. As Table 4.11 shows, the rejection rates remain comparatively low. However, in contrast to the results presented in Section 4.4.3, the rejection rates are roughly similar for positive and negative shocks of the same magnitude. Again, the CDS factor model slightly outperforms other normal return models.

[14] As a robustness test, we use a random sample of size $n/4$. All results are virtually identical.

Table 4.10: Bootstrapping and Size of Tests

Panel A: North America

Lower tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP
Mean-adjusted spreads	2.46%	2.49%	2.42%
Market model	2.62%	2.21%	2.62%
CDS factor model	2.58%	2.36%	2.59%
Portfolios (rating)	2.43%	2.26%	3.16%*

Upper tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP
Mean-adjusted spreads	2.46%	2.51%	2.68%
Market model	2.62%	1.96%*	2.62%
CDS factor model	2.58%	2.13%	2.63%
Portfolios (rating)	2.86%	2.16%	1.73%*

Panel B: Europe

Lower tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP
Mean-adjusted spreads	2.51%	3.08%*	2.41%
Market model	2.54%	2.12%	2.56%
CDS factor model	2.58%	2.76%	2.44%
Portfolios (rating)	2.40%	2.60%	2.16%

Upper tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP
Mean-adjusted spreads	2.50%	2.96%*	2.64%
Market model	2.51%	2.58%	2.61%
CDS factor model	2.54%	2.23%	2.53%
Portfolios (rating)	2.40%	1.96%*	2.60%

Notes: This table reports the day zero rejection rates of the null hypothesis of no abnormal performance for different test statistics (α = 2.5%, one-tailed). The results are based on 5,000 replications of 200 randomly drawn CDS contract-event day combinations. For the normal approximation of the 5,000 binomial trials the average rejection rate should be between 2.07% and 2.93% (at the 95% confidence interval).

Table 4.11: Bootstrapping and Power of Tests

Panel A: North America

Relative CDS Spread Changes: +0.50%

	t-Test (time-series)	t-Test (cross-section)	BMP
Mean-adjusted spreads	47.48%	56.42%	74.64%
Market model	64.57%	61.76%	75.81%
CDS factor model	66.00%	63.67%	76.51%
Portfolios (rating)	24.77%	41.27%	56.29%

Relative CDS Spread Changes: -0.50%

	t-Test (time-series)	t-Test (cross-section)	BMP
Mean-adjusted spreads	43.68%	51.32%	61.36%
Market model	63.19%	60.55%	76.52%
CDS factor model	65.99%	63.86%	75.91%
Portfolios (rating)	27.13%	41.74%	57.70%

Panel B: Europe

Relative CDS Spread Changes: +0.50%

	t-Test (time-series)	t-Test (cross-section)	BMP
Mean-adjusted spreads	50.45%	47.39%	72.01%
Market model	63.28%	67.43%	80.95%
CDS factor model	65.02%	68.47%	81.42%
Portfolios (rating)	27.74%	40.77%	53.98%

Relative CDS Spread Changes: -0.50%

	t-Test (time-series)	t-Test (cross-section)	BMP
Mean-adjusted spreads	54.88%	52.99%	62.14%
Market model	64.72%	68.99%	79.66%
CDS factor model	66.92%	69.38%	81.29%
Portfolios (rating)	30.27%	44.24%	59.86%

Notes: This table reports the day zero rejection rates of the null hypothesis of no abnormal performance for different test statistics (α = 5%, two tailed). The results are based on 5,000 replications of 200 randomly drawn CDS contract-event day combinations. We add abnormal spread changes (relative) at day zero of 0.5% and -0.5%.

4.6 Conclusion

This paper extends the findings of Brown and Warner (1980, 1985) and Bessembinder et al. (2009) by applying their simulation approach in the context of stock and bond returns to CDS spread changes. We provide evidence as to which models and test statistics are best suited for empirical applications that investigate the impact of firm-specific or macroeconomic events on firm credit risk. We measure credit risk by examining the change in value of firm CDSs.

Our main finding is that when employing daily CDS data, the non-parametric rank test is the only test statistic that performs well across all models with regard to the avoidance of excessive Type I and II errors. Some of the classical normal return models such as the market model or the matching portfolio approach are only poorly specified. A CDS four-factor model based on the findings of the previous literature on CDS spreads is generally well specified and performs best in detecting abnormal CDS spreads. Surprisingly, the simple mean-adjusted approach also leads to very reasonable results and is recommended if data on the different factors are not available.

In additional analyses we follow BMP (1991) and reexamine the performance of the different test statistics when introducing an event-induced variance increase. As expected, only the test of BMP (1991) is consistently well specified under these conditions. However, at least for relatively low variance increases, the generalized sign and cross-sectional t-test seem to be well specified.

Since several previous studies (e.g. Hull et al., 2004) use a bootstrap approach, we also present results for simulations accounting for the fact that the results may be biased because of inappropriate assumptions about the empirical distribution of the test statistics employed. Overall, the power of the different test statistics is not affected when using bootstrapped p-values. The previous results are mainly confirmed as the CDS four-factor model outperforms all other normal return models.

A Appendix to Chapter 4

A.1 Absolute Spreads

Table A.1: Size of Tests (Absolute Spread Changes)

Lower tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
Mean-adjusted spreads	4.50%*	0.76%*	0.42%*	1.06%*	0.52%*
Market model	4.68%*	0.78%*	0.72%*	1.98%*	1.44%*
CDS factor model	2.94%*	4.46%*	1.06%*	2.20%	2.50%
Portfolios (rating)	4.64%*	1.36%*	3.68%*	3.94%*	7.90%*

Upper tail (2.5%)

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
Mean-adjusted spreads	7.28%*	1.68%*	6.78%*	4.04%*	7.58%*
Market model	7.46%*	1.72%*	4.08%*	3.30%*	3.46%*
CDS factor model	4.04%*	1.74%*	4.10%*	3.80%*	2.66%
Portfolios (rating)	6.18%*	0.84%*	1.45%*	1.50%*	0.60%*

Notes: This table reports the day zero rejection rates of the null hypothesis of no abnormal performance for different test statistics ($\alpha = 2.5\%$, one-tailed). The results are based on 5,000 replications of 200 randomly drawn CDS contract-event day combinations. For the normal approximation of the 5,000 binomial trials the average rejection rate should be between 2.07% and 2.93% (at the 95% confidence interval).

*** Significant at the 1% level

** Significant at the 5% level

* Significant at the 10% level

Table A.2: Power of Tests (Absolute Spread Changes)

Absolute CDS Spread Changes: +0.5 bps

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
Mean-adjusted spreads	8.28%	5.50%	87.02%	100.00%	100.00%
Market model	8.78%	5.72%	87.00%	100.00%	100.00%
CDS factor model	4.36%	6.34%	88.60%	100.00%	100.00%
Portfolios (rating)	7.04%	4.08%	86.80%	100.00%	99.70%

Absolute CDS Spread Changes: -0.5 bps

	t-Test (time-series)	t-Test (cross-section)	BMP	Rank	Generalized sign
Mean-adjusted spreads	5.38%	2.98%	66.10%	100.00%	99.86%
Market model	5.66%	3.00%	73.40%	100.00%	99.96%
CDS factor model	5.86%	3.12%	74.58%	100.00%	99.98%
Portfolios (rating)	5.46%	6.54%	82.98%	100.00%	100.00%

Notes: This table reports the day zero rejection rates of the null hypothesis of no abnormal performance for different test statistics ($\alpha = 5\%$, two tailed). The results are based on 5,000 replications of 200 randomly drawn CDS contract-event day combinations. We add abnormal performance at day zero of +0.5 and -0.5 bps.

A.2 Test Statistics

Besides the selection of an appropriate spread change measure and a model of normal spread changes, a well-specified test statistic must be defined. The following discusses the adaptation of several parametric as well as non-parametric test statistics used in classical event studies to the case of CDS spread changes. All parametric test statistics are built upon the same null hypothesis. Under the validity of the null hypothesis, the average abnormal spread change on the event day should be equal to zero:

$$H_0 : \Delta AAS_0 = \frac{1}{N}\sum_{i=1}^{N}\Delta AS_{i,0} = 0 \tag{A.1}$$

If the event of interest induces an abnormal spread change that is significantly different from zero, the null hypothesis should be rejected. Since the direction of the event-induced spread change is unknown (ex ante) in most applications, it is common to apply two-tailed tests. A simple t-test can be derived from the assumption that abnormal spread changes are independent and identically normally distributed. The test statistic equals the quotient of the average abnormal spread change and its estimated standard deviation:

$$t = \frac{\Delta AAS_0}{S(\Delta AAS_0)} \tag{A.2}$$

The statistic follows a t-distribution with N - D degrees of freedom, where N denotes the number of events and D the number of parameters of the normal spread change model. The distribution of the test statistic is asymptotically normal in sufficiently large samples. The estimated standard error is based on the estimation window observations.[15]

This simple t-test may lead to biased inferences whenever an event induces a variance increase in the spread changes. An estimation of the standard deviation based on the estimation window observations is most likely downward biased in that case. Thus, a valid null hypothesis is rejected too often. Brown and Warner (1985) propose to estimate the standard deviation from the cross section of the event window. For the

[15] Since the standard deviation is estimated from the estimation window observations, the estimator should be adjusted for forecast errors. The adjustment depends on the model of normal spread changes. We calculate all test statistics with the appropriate forecast error adjustments within our simulation.

case of CDSs, the cross-sectional test is accordingly defined as

$$t_{CS} = \frac{\Delta AAS_0}{S_{CS}(\Delta AAS_0)} \tag{A.3}$$

$$S_{CS}(\Delta AAS_0) = \sqrt{\frac{1}{N-1}\left(\sum_{i=1}^{N} \Delta AS_{i,0} - \Delta AAS_0\right)^2} \tag{A.4}$$

Another parametric test is the standardized residuals test introduced by Patell (1976) . The major difference from the previous test is the standardization of abnormal spread changes before calculating the average value:

$$\Delta AS^*_{i,0} = \frac{\Delta AS_{i,0}}{S(\Delta AS_i)} \tag{A.5}$$

$$t_P = \frac{1}{N}\sum_{i=1}^{N} \Delta AS^*_{i,0}\sqrt{\frac{N(D-4)}{D-2}} \tag{A.6}$$

where $\Delta AS^*_{i,0}$ denotes the standardized abnormal spread changes, N is the number of events, and D is the number of parameters of the normal spread change model. The standardization leads to an equal weight for each event. Since the Patell (1976) test is not well specified for an event-induced variance increase, we apply the test of BMP (1991). This test is a hybrid of the Patell (1976) and cross-sectional tests that is robust to event-induced variance increases. The test statistics can be constructed by applying the cross-sectional test to standardized abnormal spread changes:

$$t_{BMP} = \frac{\Delta AAS^*_0}{S_{CS}(\Delta AAS^*_0)} \tag{A.7}$$

$$S_{CS}(\Delta AAS^*_0) = \sqrt{\frac{1}{N-1}\left(\sum_{i=1}^{N} \Delta AS^*_{i,0} - \Delta AAS^*_0\right)^2} \tag{A.8}$$

where ΔAAS^*_0 denotes the average of the standardized abnormal spread changes on the event day.

All aforementioned tests rely on the assumption that abnormal spread changes are normally distributed. However, this assumption does not seem feasible when a distribution exhibits substantial skewness and/or excess kurtosis. In that case non-parametric test statistics can be used instead. The Corrado (1989) rank test and the generalized sign test proposed by Cowan (1992) are commonly used in event studies.

The rank test is based on the transformation of abnormal spread changes into ranks for each time series:

$$R_{i,\tau} = rank\left[AS_{i,\tau}\right] \tag{A.9}$$

Tied ranks should be treated by the method of midranks according to Corrado (1989). We further correct for missing observations, as proposed by Corrado and Zivney (1992), based on a uniform transformation of ranks:

$$U_{i,\tau} = \frac{R_{i,\tau}}{1 + M_i} \tag{A.10}$$

where M_i denotes the number of non-missing observations for time series i. Under the validity of the null hypothesis, the average of the transformed rank should not deviate significantly from 0.5. Based upon this assumption, the test statistics is defined as

$$t_{Rank} = \frac{\frac{1}{N}\sum_{i=1}^{N} U_{i,\tau} - 0.5}{S(U)} \tag{A.11}$$

$$S(U) = \frac{1}{T}\sum_{\tau}\frac{1}{N^2}\sum_{i=1}^{N}\left[U_{i,\tau} - 0.5\right]^2 \tag{A.12}$$

The generalized sign test is derived from the proportion of positive and negative abnormal spread changes on the event date. Under the validity of the null hypothesis, this proportion should not differ from the proportion of positive and negative abnormal spread changes of the estimation windows:

$$t_{Sign} = \frac{p_0^+ - p_{est}^+}{\sqrt{\frac{1}{N}p_{est}^+(1 - p_{est}^+)}} \tag{A.13}$$

where p_0^+ and p_{est}^+ are the percentages of positive abnormal spread changes on the event date and in the estimation window, respectively.

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