Calls of convertible debt securities: no bad news at all

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Abstract In this paper, I examine the impact of in-the-money convertible bond calls on stock prices, employing a sample of US convertible bond calls over the period 1994–2011. In contrast to previous literature, I find that conversion-forcing convertible bond calls do not significantly influence stock prices. I posit that the discrepancy between my results and those in the literature is caused by amplified screening criteria, especially strong news cleaning. Companies tend to announce calls as side notes to other major corporate news, resulting in an event-study bias. Further, convertible bond design, moneyness of the conversion option at the announcement date, and convertible-arbitrage strategies cast doubt on the negative abnormal returns reported by previous literature.

Keywords Conversion-forcing convertible bond calls · Event study

JEL Classification G14 · G32

1 Introduction

In 2009, convertible bonds accounted for more than 5 % of all fixed income securities issued, or more than US\$23 billion in absolute amounts.¹ The literature reports that more than 50 % of all issued convertible bonds are called and that the announcement

¹ Mergent FISD database, US headquartered companies in non-regulated industries.

Part of this research was conducted while I was a visiting scholar at the University of British Columbia, Vancouver.

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generates abnormal returns, depending on the moneyness of the conversion option.² If a rational investor decides on equity instead of accepting the company's cash offer, the call announcement is called in-the-money or a forced conversion. In contrast to out-of-the-money calls, announcements of forced conversions decrease the company's market value. [Bechmann \(2004\)](#) and [Brick et al. \(2007\)](#), among others, find significant negative impacts of about 1 % on the announcement date. However, the extant empirical literature does not conclusively explain what is driving the abnormal returns of forced conversions; several theories have been proposed, but none of them prove to be robust.

[Mikkelsen \(1981\)](#) was the first to report that forced conversions have a negative impact on share prices. He argues that the company's decreased value is due to loss of a tax shield. According to US GAAP accounting standards, despite their equity feature, convertible bonds are allocated to debt. Since the bonds are exchanged for new shares, not only does debt vanish but, also, equity increases, which has a significant impact on the company's capital structure. However, later studies (e.g., [Byrd and Moore 1996](#)) cast doubt on this explanation. The preferred dividends provide no tax shield and the same effect is found in convertible preferred stock calls.

In a theoretical asymmetric information model, [Harris and Raviv \(1985\)](#) argue that managers with negative information call the bonds to preserve cash flows. Managers with positive information wait until the news is priced and bondholders elect to convert voluntarily. To assess the information content, [Ofer and Natarajan \(1987\)](#) examine the long-term evolution of share prices after the call. They find abnormal negative returns after the call, supporting the asymmetric information theory. However, [Cowan et al. \(1990\)](#) and others link the abnormal returns to a bias in the estimation procedure.³ To overcome the drawbacks of long-term event studies, [Byrd and Moore \(1996\)](#) investigate earnings forecasts after the call event. Hypothetically, earnings forecasts should be revised downward after a call, but the authors find the opposite; earnings forecasts rise after a forced-conversion call. Analogously, [Ederington and Goh \(2001\)](#) report that insiders buy shares before a call, rather than selling, and that analysts revise their forecasts upward instead of downward. The authors thus reject the hypothesis of negative information.

[Mazzeo and Moore \(1992\)](#) report a 44 % increase in trading volume for their sample during the week of the call announcement compared to weeks before the announcement. Together with a recovery of the share prices during the conversion period, the authors suggest the effect may be caused by market micro-structure effects of liquidity. Following [Mazzeo and Moore \(1992\)](#), [Bechmann \(2004\)](#) argues that the immediate drop and then recovery of stock prices can be attributed to short selling by convertible arbitrageurs hedging their equity risk during the conversion period. Using monthly short-selling data, the authors conclude that hedging-induced price pressure at least partly drives the announcement returns. [Brick et al. \(2007\)](#) test the hypothesis with different proxies for liquidity to circumvent short-selling data and conclude that liquidity does not drive abnormal returns, nor do liquidity effects in combination with dividend

² Moneyness of the conversion option is defined as the ratio of the equity value received by conversion (conversion value) divided by the amount of cash obtained from immediate redemption (nominal plus accrued interest) minus one.

³ For a brief discussion, see Sect. 4.

dilution. Shareholders might expect their dividends to be diluted by the newly issued shares. Also, the recovery period is not related to the abnormal returns and neither are proxies for asymmetric information or tax shields.

To summarize, the question of what causes the abnormal returns remains unresolved. The objective of this paper is to employ an event study to assess the pure impact of in-the-money convertible bond calls on share prices. Event studies are designed to estimate the impact of new information on a company by relating the real return to a "should be" return without the new information. However, for an event study to be accurate, the released news has to contain information new to market participants and no other news can be released. I argue that an event study on the call of convertible bonds violates these two assumptions.

The news content of forced conversions is doubtful, for two reasons. First, due to market restrictions and financing problems, some convertible bonds are issued with the intention of being converted into equity in the following years (Lewis et al. 1999). Their covenants underline this backdoor equity purpose. The second reason stems from the moneyness of the conversion option at the official call. Empirical literature on call policies shows that bonds are called extremely deep in-the-money, with a strong out-performance of the company before the call. On average, stock prices exceed the breakeven point, meaning that an investor is indifferent about cash and stock, by about 50 % (e.g., Bechmann 2004). Any informed market participant would expect the bonds to already be called in an environment of optimal call policies.

Multiple corporate news releases during the event period can distort the effect to be investigated. Indeed, information technology now generates near constant news coverage of companies. It is often assumed in the literature that the effects of other contemporaneous corporate events will wash out in the whole sample. This can be problematic and lead to a bias when the contemporaneous effects are expected to have a larger impact or they all force a similar market reaction. King and Mauer (2014) report that about 31 % of their convertible sample drops due to contemporaneous news. Indeed, several papers (e.g., Garcia-Feijóo et al. 2010) find evidence for Mayers (1998) sequential financing hypothesis: companies issue callable bonds to overcome agency conflicts of debt and call their bonds to exercise investment options. The announcement of investment options can be simultaneous with the call event.

Despite a potentially low information content, hedging pressure due to arbitrage strategies can introduce a liquidity-dependent share price reaction. However, reasonable doubts as to market micro-structure effects can be found in the convertible arbitrage literature. For example, Brown et al. (2012) argue that the largest share of a convertible bond issue is sold to hedge funds. Hedge fund arbitrage strategies can be replicated with buy-and-hold portfolios (Agarwal et al. 2011), leaving only minor "free float" that might be hedged at the announcement day.

In this paper, I employ extensive and comprehensive news cleaning because convertible calls are often side notes to major corporate announcements that might overshadow the call effect. I also control for the rolling of convertible bonds. Companies tend to ensure their financing by issuing a new bond before calling the outstanding bond. As a consequence of this cleaning, I find no abnormal stock returns when forced conversions are announced. In contrast to the cleaned sample, a sample with less restrictive selection criteria, comparable to those in previous research, reveals negative abnor-

mal returns and differs significantly from the cleaned sample. I thus conclude that conversion-forcing calls have no impact on stock prices, a finding in sharp contrast to existing literature.

The rest of the paper is organized as follows. Section 2 sets out my hypothesis in detail. Section 3 describes my sample and compares it to that used by others. Based on the sample, Sect. 4 reports abnormal returns around the announcement date and the impact of explanatory variables on these. Section 5 concludes.

2 Hypothesis Development

This section sets out my hypothesis regarding the announcement effect of in-the-money calls of convertible bonds.

During the trading days prior to the call announcement, companies tend to strongly outperform the market. [Bechmann \(2004\)](#) reports 8 % out-performance in the period 50 days before the announcement and [Cowan et al. \(1990\)](#) find significant alphas of 5 % in the year before the announcement. Due to this out-performance in a relatively short time that occurs simultaneously with the call, the average conversion option is extremely deep in the money. [Brick et al. \(2007\)](#), [Bajo and Barbi \(2012\)](#), and [Bechmann \(2004\)](#) report moneyness of 64, 88, and 47 %, respectively. In contrast, the theoretical models of [Ingersoll \(1977\)](#) and [Brennan and Schwartz \(1977\)](#) indicate that it is optimal to call a bond as soon as the conversion value exceeds the value of immediate redemption. In an environment of optimal call policies, market participants should have expected the convertibles to be called within a reasonable time frame. This expectation should already be reflected in the stock price and thus not lead to abnormal returns at the announcement date.⁴

Convertible bonds are highly flexible financing instruments and there is an extensive literature on their design. Firms can utilize convertible bonds to issue new shares indirectly by emphasizing the equity feature of the bond ([Stein 1992](#)). Following [Stein \(1992\)](#), [Lewis et al. \(1999\)](#) find that companies design their new convertible issuances to be either more debt-like or more equity-like, depending on their specific financing problems. Testing this hypothesis, [King and Mauer \(2014\)](#) utilize the delta of the conversion option, the probability that the bond will be converted to equity at maturity in their study. They find that the probability of called bonds to be converted is significantly higher than that of those not called at the date of issuance. Moreover, in the sample of [Bechmann \(2001\)](#), in-the-money calls are called significantly earlier than out-of-the-money calls. Hence, an indication for a seasoned equity offering through the back door can be obtained as data on moneyness or the delta of the implied conversion option are publicly available.

[Mazzeo and Moore \(1992\)](#) discover a 44 % increase in trading volume in the week around the announcement relative to an average week before the announcement. Based on this finding, [Bechmann \(2004\)](#) attributes the abnormal returns to convertible-arbitrage hedge funds that hedge the equity risk during the conversion

⁴ According to [Fama \(1970\)](#), all three market efficiencies imply that all past information is reflected in the current stock price.

period by shorting shares at the time of announcement. However, there is considerable evidence that convertible-arbitrage hedge funds already hold most of the convertible bonds during maturity and have hedged their equity risk: [Brown et al. \(2012\)](#) report that between 2000 and 2008 about 73 % of all privately placed convertibles were sold to convertible-arbitrage hedge funds at issuance, with a maximum of 80 % in 2008. [Mitchell et al. \(2007\)](#) find that convertible arbitrage funds account for 75 % of the convertible market. Most convertible-arbitrage strategies involve short selling the underlying stock. In line with this evidence, a direct hedging demand by the hedge funds is the negative announcement effect on the issuance day ([de Jong et al. 2012](#)). [Agarwal et al. \(2011\)](#) find that a simple buy(short)-and-hold strategy explains most of the hedge fund variation. Consequently, it can be assumed that hedge funds do not trade their convertibles and their equity risk is hedged at convertible issuance.

Based on this literature review I formulate two, potentially interacting, hypotheses that will be tested in this paper. Since the hedging demand of convertible-arbitrage hedge funds is low and a call has negligible information content due to the moneyness of the conversion option and bond design, I posit that in-the-money convertible calls *do not lead to abnormal trading volume* in equity shares and in-the-money convertible calls *do not generate abnormal stock returns*.

3 Data

3.1 Sources and screening criteria

I start by collecting convertible calls from the Bloomberg database. Bloomberg has coverage similar to that of Datastream, but is slightly more reliable. The sample covers the period 1994–2011, is restricted to the US market, and limited to convertible bonds denoted in local currency and exchangeable for common stock. Also, I include call events only when the conversion option is in-the-money, meaning that a rational bondholder would decide to convert the bond into new shares. I employ nine other screening criteria, detailed below.

I merge the sample with the Mergent FISD database to obtain call and issuance characteristics of the bonds and (1) require each bond to be covered by the database. Equity related data including SIC codes are taken from the CRSP tapes. In the case of (2) missing coverage or missing returns, the event is dropped. Redemptions resulting from other corporate decisions (delisting, bankruptcy) are excluded (3) and I exclude calls if the company announced that it would issue new convertibles 1 month before the call (4). A call event is excluded if (5) the underlying company is in the regulated financial or utility industries.⁵

I follow [Garcia-Feijóo et al. \(2010\)](#) and [King and Mauer \(2014\)](#) and drop (6) potentially disturbing covenants such as variable coupon bonds and liquid yield option notes, because the latter are putable for cash, which can give rise to different motives for calling the bond. Issuers sometimes clean up their convertibles after most of the holders

⁵ SIC codes 6000–6999 and 4800–4999. If the codes assigned in the Bloomberg and CRSP database do not match and one code falls in the regulated industries, I exclude the event.

have already converted so as to avoid small outstanding amounts. I thus use (7) only calls where the call amount is larger than one-half of the offering amount. Contemporaneous events can bias the results. I use the Dow Jones FACTIVE database to check for public, company-related information released ± 2 days after the announcement. An event is (8) removed if there was another major corporate event during this period that might disturb stock market reactions to convertible calls. Using publicly available call announcements, I double check all call features and announcement dates. If I (9) could not find an official announcement or not all call features could be discerned by looking at all the databases, I drop the event.

Returns of the value-weighted industry indices and the Fama–French risk factors are publicly available through Kenneth French’s database.⁶

3.2 Sampling and sample size

The initial sample from the Bloomberg database comprises 522 call announcements. After controlling for regulated industries and official announcements in the FACTIVE database, the sample reduces to 320. In the same database I find that 114 of the 320 events are free of contemporaneous news, including merger activity.⁷

To assess the representativeness of my sample, I create and employ a *Dirty* sample, based on selection criteria (1), (2), (5), (6), and (9), in addition to the *Clean* sample, which meets all the criteria specified above. The Dirty sample is intended to reveal differences that arise due to omitted screening criteria. The Clean sample consists of 47 calls; there are 150 in the Dirty sample.⁸ My sample is smaller than those used in other convertible call literature. For example, Brick et al. (2007) employ 392 calls made over 19 years (1985–2002), an extension of Ederington and Goh (2001) sample. Garcia-Feijóo et al. (2010) employ 165 non-financial calls between 1986 and 2001. In contrast to this literature, King and Mauer (2014) use a sample of non-financial bonds issued between 1980 and 2002. They find 175 calls between 1994 and 2010 before news filtering, and Bajo and Barbi (2012) find 92 calls for 2000 to 2009, a subset of my time period. The general pattern in these other samples is consistent with an observed decrease in the issuance of convertible bonds and thus goes some way to explain the size of my sample. Additionally, my screening criteria are considerably stricter than those employed in earlier literature, but are necessary to avoid distorted abnormal returns.

Research on convertible bonds is challenging, since the flexibility in contracting yields a strongly heterogeneous set of convertible bonds. Certain covenants can generate special incentives to call the bond. I follow Garcia-Feijóo et al. (2010) and exclude puttable and variable coupon bonds. The opportunity to call a convertible bond generates financial flexibility; for example, it allows responding to cheaper refinancing

⁶ <http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/>.

⁷ Note that the order of the nine screening criteria is irrelevant and, depending on the order, the decrease of events in every criteria is very volatile. I find only the news filter to have a significantly higher impact on the sample size than the other criteria. If all criteria are fulfilled except (4), (7) and (8), I find that 9 events drop out due to new convertibles, 17 call amounts are too low and 51 incorporate other news.

⁸ In the CAPM-like event study, the sample diminishes due to the estimation period.

possibilities due to decreased credit risk. Many issue announcements of convertibles explicitly state that the bonds to be sold will replace existing convertible bonds. Even if this is not explicitly stated, depending on the characteristics of the new bonds, market participants might expect the outstanding bonds to be exchanged. Thus, an informed market participant foresees the call of the outstanding bonds. Also, there is considerable evidence that firms perform poorly around the offering (see [Dann and Mikkelsen 1984](#)).⁹

Another issue is mergers and acquisitions. Market participants might expect the bonds to be called due to contractual fixing of the underlying stock (target) or covenants (acquiror) after a merger announcement: [Maquieira et al. \(1998\)](#) find large and significant increases in convertible bond prices at announcements of stock-for-stock mergers. In addition, the largest share of influential events in [King and Mauer \(2014\)](#) involves merger activity. Lastly, the sample is substantially reduced due to contemporaneous news. In an earlier study, [King and Mauer \(2014\)](#) report that about 31 % of their sample comprises influential events, but their focus is on call policies. The strong news cleaning conducted in this paper results in about *one-half* the sample being dropped. Companies tend to release call announcements as a side note to other information on the same day. The extension to a 5-day period thus does not overly restrict the sample. Examples of such corporate events include CEO changes, earnings or dividend announcements, major law-suits, and announcements of new products or joint ventures.

Although the remaining Clean sample is small, 47 call events should yield enough evidence, since previous work finds abnormal returns to be large (about 1 %) and highly significant.

3.3 Descriptive statistics

Figure 1 reports the distribution of the calls across the years. The differences in time-series pattern between the Clean and Dirty sample are negligible. Interestingly, in the years following a recession, such as the burst of the dot-com bubble in 2000/2001 and the financial crisis in 2008/2009, very few convertibles are called. A possible explanation for this is that managers push forward a call originally planned for later to exploit the potential over valuation during the boom period.

[Ederington and Goh \(2001\)](#) report an industry clustering of convertible calls in SIC codes 35 (Industrial and Commercial Machinery and Computer Equipment) and 36 [Electronic and Other Electrical Equipment and Components (Except Computer)]. Literature on convertible issues also finds higher weights in these industries: the sample of convertible debt offerings employed by [Lewis et al. \(2001, 2002\)](#) also has a large share in SIC codes 35 and 36, but cannot be compared directly since those authors use a different time period and exclude convertibles when another convertible had been issued within the last 5 years. In comparison to the CRSP average, my sample confirms the higher weights in certain industries (Table 1). Since industry clustering can distort

⁹ I refer to this effect in the hypothesis section.

Fig. 1 Yearly distribution of the call announcements

Table 1 Descriptive statistics regarding the industry clustering/dispersion of the companies at the call announcement

2-digit SIC code (%)	13	28	35	36	73	N
Clean sample	2.1	12.8	17.0	12.8	14.9	47
Dirty sample	7.3	17.3	10.7	12.7	10.0	150
CRSP average	4.9	8.7	6.9	10.2	12.7	

The underlying companies are assigned to the corresponding two-digit SIC code. Percentages are reported if they exceed 5 % for either the Clean sample or the Dirty sample. The CRSP Average reports the average yearly representation of the corresponding two-digit SIC code in the whole CRSP database with screening (5) for 1990 to 2011. N denotes the number of events

the results of an event study, I use Fama–French 12-industry indices in addition to a broad equity market index.

Table 2 summarizes the characteristics of the calls for both the samples. The studied companies have a medium market capitalization compared to those listed on the NYSE. I assess the relative size of the firms by means of the Fama–French 20-market equity breakpoints, which are readily obtainable from Kenneth French’s homepage. This is necessary since the sample covers a long time period in which the distribution of market capitalizations varies greatly and the size relative to the market is of interest. Although news coverage of larger companies is supposed to be greater, I find only a very small size difference between both samples. The outstanding amount called back is smaller for the Dirty sample since the Clean sample does not include clean-up calls and both distributions are highly negatively skewed. Influenced by the call amount, the Clean sample reports a higher mean dilution of about 13 %. Convertible bonds are called back in-the-money after they have existed for about 60 % of their maturity. Most calls are deeply in-the-money, a finding that conflicts with the optimal call policies derived by [Ingersoll \(1977\)](#) and [Brennan and Schwartz \(1977\)](#).¹⁰

¹⁰ See [Bajo and Barbi \(2012\)](#) for an excellent review of optimal call policies.

Table 2 Call characteristics of the convertible call events

	Clean sample					Dirty sample				
	Mean	Median	SD	Min	Max	Mean	Median	SD	Min	Max
Size	12.0	12	4.6	3	20	12.3	12	4.7	1	20
Call amount	221	115	285	1	1,380	163	110	211	0	1,380
Dilution (%)	13.1	12.1	7.8	1.9	34	9.1	7.1	8.2	0.1	53.0
Maturity	0.61	0.62	0.19	0.09	0.98	0.59	0.61	0.20	0.05	0.98
Moneyiness at call	0.80	0.49	0.87	0.02	4.75	0.69	0.42	0.79	0.00	4.75
N	47	47	47	47	47	150	150	150	150	150

Size denotes the market capitalization twentieth (20 highest, 1 lowest) of the calling companies at the announcement date. The breakpoints are obtained from Kenneth French's homepage and include companies listed on the NYSE. *Call Amount* is denoted in million US Dollar, *Dilution* reports the number of new shares if all bondholders convert divided by shares outstanding at the call announcement. *Maturity* denotes the remaining years to maturity divided by contractual maturity. *Moneyiness* presents the conversion premium divided by call premium (nominal plus accrued interest) minus one. N denotes the number of events

My sample period is unique in the literature on convertible calls, because it includes more recent years and stems from a different data provider. To rule out potential biases induced by the period or screening criteria, I relate the call characteristics, presented in Table 2, to other literature. Since dilution is calculated relative to shares outstanding, it can measure the call's impact on the company's financing structure. My results from my sample are similar to those found in the literature: [Brick et al. \(2007\)](#) reports a slightly lower (8.9 %) and [Bechmann \(2004\)](#) a similar, mean dilution (12.3 %). Most research on convertible bonds does not provide direct information on the market capitalization of the calling or issuing companies. Converting the equity breakpoints to market capitalization, the average size of the companies in my sample is in between that sized in [King and Mauer \(2014\)](#) and [Brick et al. \(2007\)](#).

One important aspect of this paper is the moneyiness of the conversion option. For the period 1963–1995, the moneyiness in [Bechmann \(2004\)](#) sample is slightly lower (0.47) compared to [Brick et al. \(2007\)](#) sample (0.64). [Bajo and Barbi \(2012\)](#) employ a large sample with a similar time frame and find their in-the-money calls also exercised far from at-the-money (0.37). Although my sample is slightly deeper in-the-money, all bonds are far from at-the-money. The maximum moneyiness of 4.75 is extreme in the sense that the company's stock price must have gained about 500 % in the last years. However, the maximum is still lower than the maximum reported by [Brick et al. \(2007\)](#) (10.0) and similar to that reported by [Bechmann \(2004\)](#) (4.1). The Clean sample does not incorporate clean-up calls and still exhibits a high moneyiness. I thus suspect that bondholders remain optimistic about the company's future and postpone conversion, since the bond value is supposed to move equity-like.

During the trading days prior to the call announcement, companies tend to strongly outperform the market. Figure 2 shows that the average abnormal return of my sample over the run-up period of 100 trading days is higher than 18 %. The size of the abnormal return is comparable to that found by [Bechmann \(2004\)](#). He reports 8 % in the period

Fig. 2 Indicative plot of the average cumulative abnormal equity return in the run-up period for both samples (y axis). $t = 0$ denotes the announcement day (x axis). The abnormal returns are based on a market model with parameters estimated in $t \in (60, 160)$ for the CRSP value-weighted equity market index

50 days before the announcement; my sample exhibits an abnormal return of around 10 %.

Following [King and Mauer \(2014\)](#), the called convertibles in my sample should be issued more equity-like than all convertibles. Managers wanting to issue equity through the backdoor set the conversion price close to the actual stock price. Indeed, the mean issuance moneyness of the Clean sample (-0.11) and the Dirty sample (-0.13) are similar and close to the -0.12 reported in [Bajo and Barbi \(2012\)](#). Both are significantly bigger than the mean moneyness of all convertible issuances in the whole Mergent FISD database (-0.22).¹¹

To summarize, my sample is representative with regard to all publicly called convertible bonds and to most parameters and characteristics found in the literature on convertible calls. The next section presents the results of the event studies.

4 Event study

4.1 Trading volume

In this section, I assess the validity of the hypothesis that in-the-money calls do not lead to abnormal trading volume. [Mazzeo and Moore \(1992\)](#) discover an increase in trading volume of 44 % in the week around the announcement relative to an average week before the announcement. On this basis, [Bechmann \(2004\)](#) ascribes the abnormal returns to convertible-arbitrage hedge funds who hedge the equity risk during the conversion period by shorting shares at the time of announcement. Table 3 reports mean normalized abnormal trading volume (NATV) of this paper's samples according to [Bajo \(2010\)](#):

¹¹ Screening criteria (1), (5) and (6) apply.

Table 3 Mean normalized abnormal trading volume around the call announcement ($t = 0$)

	(-3, -12)		(-3, -68)	
	Clean sample	Dirty sample	Clean sample	Dirty sample
(-1)	0.29* (0.10)	0.33*** (0.01)	0.48*** (0.00)	0.38*** (0.00)
(0)	0.41* (0.09)	0.55*** (0.00)	0.44*** (0.00)	0.52*** (0.00)
(1)	0.04 (0.84)	0.32** (0.01)	0.19 (0.22)	0.33*** (0.00)
<i>N</i>	47	150	47	150

μ and σ are calculated during 2 trading weeks or 66 trading days before the announcement. *P* values in parentheses are calculated with a 2000-repetition bootstrap routine. *, **, *** denote significance at the 10, 5, 1 percent level, respectively. *N* denotes the number of observations

$$\text{NATV} = \frac{\text{TV} - \mu}{\sigma}, \quad (1)$$

where TV is the natural logarithm of 1 plus traded shares on each event day, μ the mean of a pre-event period, and σ the standard deviation. I exclude post-event data to omit a bias that may arise due to bondholders selling their converted securities.

Compared to the trading volume during the 66 days prior to the event day, in both samples market participants trade heavily during all three event days. In contrast, the Clean sample exhibits only borderline significant abnormal trading volume when volume measured two weeks before the call event is considered representative. For this estimation period, the shares of the Dirty sample companies are still comparably substantially traded. It can be inferred from Table 3 that forced conversions of convertible bonds occur during high trading volume periods and that cleaning for contemporaneous news diminishes most abnormal trading volume. Indeed, the results are in line with [Bechmann \(2004\)](#), whose Figure 3 also reveals high trading volume in the weeks before the announcement. A counter-intuitive finding is that trading volume is higher on the day before the announcement date than it is on the day after the announcement. If calls have information content and there are announcements after the stock markets have closed, day $t = 1$ should witness abnormal trading volume. I attribute this result to either previous notice to bondholders or spillover effects from other news. This underlines the importance of the extended news cleaning period. Thus, price pressure has only small influence on the share price of an in-the-money-calling company, which supports my first hypothesis.

4.2 Stock returns

As a first step, I use a standard event-study approach to assess the validity of my second hypothesis. Several authors document that an event study on convertible calls is sensitive to estimation parameters. I address this issue by employing various event-study designs.

There is no consensus in the literature as to the beginning of the event period and I later show that the results differ across them. Let $t = 0$ be the news wire announcement date of the call. The most frequently used event period is $(0, 1)$. The day after is included because information might be released after the stock market has closed. Additionally, I use $(-1, 1)$, first because there might be leakage of information and, second, sometimes bondholders receive notice of the call (by mail or other means) prior to the official announcement. Indeed, some official announcements state explicitly that a notice has been mailed to bondholders. Due to the high abnormal trading volume on the day before the announcement, I also include $(-1, 0)$ as an event window.

I estimate a standard market model by regressing company on index returns.¹²

The value-weighted CRSP (NYSE/NASDAQ/AMEX) index serves as the equity market index. To capture potential industry effects, I substitute the company-specific Fama–French 12-industry indices for the market index. Because of the average market capitalization, the industry indices remain large enough not to be influenced by potential abnormal returns of the calling firms.

Cowan et al. (1990) mention that bias can arise from the choice of a pre-event estimation period. The conversion option implied in convertible bonds is mostly issued far out-of-the-money (Dann and Mikkelsen 1984; Bajo and Barbi 2012). Thus, companies forcing conversions experience largely (abnormal) positive returns over the pre-event period. This can bias the results. Indeed, my sample reinforces this finding [indicative plot, Fig. (2)]. For robustness reasons, I use two estimation windows suggested in the literature for the market model (e.g., Brick et al. 2007): after the announcement and the conversion period, $t \in (60, 160)$, and 1 year after the announcement $t \in (251, 506)$. The selection criterion (2) from Sect. 3 already covers the availability of CRSP return data.¹³

Reported average cumulative abnormal returns (ACAR) are the cross-sectional mean of all cumulative abnormal returns over the announcement days for every company/event for the whole sample. Since I have a small sample, I choose a bootstrap routine (see, e.g., Davison and Hinkley 1997) to assess the significance of the abnormal returns. Bootstrapping the abnormal returns can overcome the strict normality assumption, which can bias the significances (McWilliams and Siegel 1997) and is especially crucial in the case of few events. I choose 2,000 repetitions, which is a conservative number that suits the methodology and reduces errors (Andrews and Buchinsky 2001). Due to the post-announcement regressions for normal performance, late events drop out of the sample, particularly those occurring in the year 2011. To address this issue, I use an alternative event-study methodology based on Biktimirov et al. (2004). The methodology trades the advantage of a circumvented estimation period for missing event-specific abnormal returns. Since the methodology does not need an estimation period, recent events can be included. Rather than time-series data, a cross-sectional Fama and French (1993) factor estimation for every event day across all companies is used. For event periods longer than 1 day, I pool the data and create a panel. Again, I use

¹² I obtain similar results for market-adjusted returns.

¹³ For the later estimation window, I include an event if the company exists in $t \in (251, 351)$, a subset of 100 days of the full estimation period $t \in (251, 506)$. However, the results do not change when only full periods are used.

a 2,000-repetition bootstrap for the significances. Furthermore, the procedure accounts for size and growth effects that might play a role in my sample: Nash et al. (2003) find that convertibles have a high share in firms with greater growth opportunities.

Table 4 reports the abnormal returns for both samples, different event windows, and for varied estimation periods using the CRSP value-weighted index and the Fama–French 12-industry classification.

The mean abnormal returns of the Dirty sample (Panel A of Table 4) are consistent with the literature. For example, Brick et al. (2007) report mean abnormal returns of -0.75% , Bechmann (2004) of -1.75% , and Garcia-Feijóo et al. (2010) of -1.07% . The results for the median and the winsorized sample reveal no large outliers as well as a symmetric distribution of the abnormal returns. The 3-day event period yields the largest and, despite the high trading volume, $(-1, 0)$ the lowest abnormal returns. While most medians remain significantly negative, the industry indices increase the variance of the abnormal returns and lead to borderline significant means. The significances are lower than those found in the literature. All three papers discussed above find their mean abnormal returns significant at the 1% level. I attribute the lower significances of the Dirty sample to different contemporaneous news announcements. As I point out in Sect. 3, many call announcements are side notes to other important company news, often earnings reports. As my sample covers a different period than does previous literature, the news is also different. For example, earnings reports contemporaneously announced with the calls could be more positive or announced dividends smaller.

The main contribution of this paper can be found in Panel B of Table 4, the abnormal returns of the Clean sample. Pure, non-disturbed call announcements of in-the-money convertible bonds do not lead to significant abnormal returns. Mean, median, and winsorized samples are far from any common significance level. Although my sample size of 45 calls is small,¹⁴ it should reveal at least a tendency toward negative abnormal returns if the results in previous literature are accurate. However, some mean abnormal returns are actually positive and two specifications reveal more positive than negative abnormal returns. Again, the industry indices increase most p values even more. The Clean sample further increases the number of positive abnormal returns for almost every event-study specification, especially for the most common $(0,1)$ event window. The Dirty sample yields only slightly higher percentages of positive abnormal returns than those reported in literature; for example, Brick et al. (2007) find 37% positive abnormal returns in the $(0,1)$ event window.

In line with previous literature, I find that the abnormal returns vary with estimation specification. Estimation period, event period, and index all have an influence on the abnormal returns. Unreported estimations with non-bootstrapped standard errors supply the same results.

The cross-sectional event study results set out in Table 5 support the previous findings. For the Clean sample, all alphas are far from any common significance level. The fit of the regressions, measured by the adjusted R^2 , is negative for $t = 0$ compared to 20% in $t = -1$ and $t = 1$. This could mean that the call announcement slightly

¹⁴ Note that for this regression approach, two events drop out of the sample due to a too short estimation period, but will be included in the cross-sectional regressions.

Table 4 Cumulative abnormal returns for the Dirty sample (Panel A) and the Clean sample (Panel B) according to the market model and with the CRSP value-weighted index or the Fama–French 12-industry classification as independent variables

(60,160)					(251,506)			
	Mean	Winsor	Median	Pos. (%)	Mean	Winsor	Median	Pos. (%)
Panel A: dirty sample								
CRSP								
(−1,0)	−0.4 (0.20)	−0.2 (0.32)	−0.9 (0.34)	46	−0.3 (0.37)	−0.2 (0.57)	−0.3 (0.29)	44
(0,1)	−0.6* (0.10)	−0.6** (0.04)	−0.7*** (0.01)	41	−0.6 (0.15)	−0.6* (0.05)	−0.8** (0.02)	42
(−1,1)	−0.7* (0.09)	−0.6* (0.06)	−0.6** (0.03)	38	−0.7 (0.14)	−0.6 (0.12)	−0.8** (0.04)	39
FF12								
(−1,0)	−0.3 (0.35)	−0.2 (0.50)	0.0 (0.53)	50	−0.2 (0.56)	0.0 (0.93)	0.0 (0.91)	51
(0,1)	−0.6 (0.11)	−0.6** (0.04)	−0.4** (0.02)	43	−0.6* (0.10)	−0.6** (0.04)	−0.6** (0.02)	41
(−1,1)	−0.7 (0.12)	−0.6* (0.08)	−0.6** (0.08)	42	−0.7 (0.15)	−0.5 (0.18)	−0.7 (0.18)	44
<i>N</i>	133	133	133	133	133	133	133	133
Panel B: clean sample								
CRSP								
(−1,0)	0.0 (0.99)	0.0 (0.98)	−0.2 (0.55)	44	0.1 (0.89)	0.1 (0.84)	0.0 (0.84)	51
(0,1)	−0.3 (0.61)	−0.2 (0.61)	−0.2 (0.94)	46	0.0 (0.99)	0.1 (0.88)	−0.1 (0.95)	49
(−1,1)	−0.6 (0.32)	−0.6 (0.25)	−0.7 (0.20)	40	−0.3 (0.68)	−0.2 (0.66)	−0.2 (0.57)	44
FF12								
(−1,0)	0.3 (0.53)	0.2 (0.69)	0.3 (0.72)	51	0.4 (0.49)	0.3 (0.57)	0.2 (0.55)	56
(0,1)	0.0 (0.97)	−0.1 (0.85)	0.1 (0.84)	51	0.2 (0.74)	0.1 (0.84)	−0.1 (0.91)	49
(−1,1)	−0.2 (0.77)	−0.3 (0.60)	−0.3 (0.72)	49	0.0 (0.98)	0.1 (0.82)	0.7 (0.83)	53
<i>N</i>	45	45	45	45	45	45	45	45

The results are reported in %, for different estimation periods (columns) and different event periods (rows). Winsor reports ACARs for a winsorized (5 % top/bottom) sample and the sixth and tenth column the percentage of positive CARs. *, **, *** denote significance at the 10, 5, and 1 percent level obtained from a 2000-repetition bootstrap procedure according to the *p* values in parentheses. The *p* values for the median are generated

Table 5 This table reports regression results based on the cross-sectional regression for the Dirty sample (Panel A) and the Clean sample (Panel B)

	(-1)	(0)	(1)	(-1,0)	(0,1)	(-1,1)
Panel A: dirty sample						
α (%)	-0.3 (0.44)	-0.5* (0.06)	-0.1 (0.59)	-0.3* (0.10)	-0.3* (0.08)	-0.3* (0.09)
Market-RF	1.79*** (0.00)	0.90*** (0.00)	1.11*** (0.00)	1.29*** (0.00)	0.99*** (0.00)	1.16*** (0.00)
SMB	1.00** (0.02)	0.25 (0.72)	0.85 (0.13)	0.60 (0.16)	0.47 (0.31)	0.58** (0.08)
HML	0.68 (0.19)	0.40 (0.50)	-1.01** (0.02)	0.42 (0.27)	-0.44 (0.25)	-0.21 (0.50)
adj. R^2	0.20	0.06	0.30	0.13	0.16	0.17
N	150	150	150	300	300	450
Panel B: clean sample						
α (%)	-0.2 (0.67)	0.4 (0.53)	-0.4 (0.20)	0.1 (0.80)	-0.1 (0.74)	-0.1 (0.71)
Market-RF	1.83*** (0.00)	0.94* (0.06)	0.79** (0.02)	1.32*** (0.00)	0.82*** (0.00)	1.03*** (0.00)
SMB	0.40 (0.54)	-0.91 (0.54)	0.52 (0.37)	-0.21 (0.74)	-0.08 (0.90)	0.01 (0.99)
HML	0.07 (0.67)	0.32 (0.78)	-0.46 (0.49)	-0.22 (0.75)	-0.24 (0.66)	-0.45 (0.32)
Adj. R^2	0.21	-0.01	0.20	0.11	0.07	0.13
N	47	47	47	94	94	141

Results report the factor loadings and p values. The columns denote the different days or periods around the announcement $t = 0$. Column 5 and 6 report results for pooled data. p values in parentheses are derived through a 2000-repetition bootstrap routine. *, **, *** denote significance at the 10, 5, and 1 percent level, respectively. N denotes the number of data points in the regression

influences the share price, but these influences are positive instead of negative and far from any common significance level. The Dirty sample shows negative alphas not only for the pooled regressions, but also for the announcement day regression. Except for the market factor, all Fama–French factors are insignificant and the signs of the loadings change. [Garcia-Feijóo et al. \(2010\)](#) employ a similar methodology and find significantly negative abnormal returns.

Combining the results of the different event studies, I conclude that my hypothesis is confirmed for this data set; there are no significant abnormal returns for forced-conversion convertible calls. In the next section, I test whether the abnormal returns are statistically different and comment on the screening criteria.

4.3 Explanatory regressions

I assess the statistical difference between the samples by a regression of abnormal returns (CAR) that includes several literature-based explanatory variables:

$$\text{CAR} = \alpha + \beta_1 \text{Dirty} + \beta_2 \text{Size} + \beta_3 \text{Liquidity} + \beta_4 \text{Overvaluation} + \epsilon \quad (2)$$

The dependent variable is the Dirty sample, which includes the Clean sample as a subset. *Dirty* is a dummy variable that takes the value 1 if the event is not in the Clean sample. If the abnormal returns differ between the Clean and Dirty samples, I expect this variable to be significantly negative. I follow [Brick et al. \(2007\)](#) and assess the *Size* of the company by the logarithmic market capitalization at the announcement date $t = 0$. However, the results are similar when call amount divided by market capitalization is used instead. Size is intended to capture analyst coverage; thus abnormal returns for larger companies are expected to be closer to zero. To assess the potential market micro-structure effects, I employ the liquidity measure of [Amihud \(2002\)](#):

$$\text{Liquidity} = 10^6 \cdot \sum_{t=-2}^{-251} \frac{|\text{Return}_t|}{\text{Closing price}_t \cdot \text{Trading volume}_t}. \quad (3)$$

If the price pressure hypothesis is correct, I expect the variable *Liquidity* to have a negative impact on the abnormal returns. If the asymmetric information hypothesis holds true, firms call their convertible bonds when managers consider the stock overvalued. *Overvaluation* is the only significant and partly robust independent variable in [Brick et al. \(2007\)](#). I thus take my lead from those authors and estimate cumulative abnormal returns between $t \in (-60, -2)$ and the Fama–French 12-industry indices estimated between $t \in (60, 160)$. I expect a negative sign.

Since nearly all calls are made deeply in-the-money, I drop the moneyiness variable. [Nash et al. \(2003\)](#) report that firms with high investment opportunities are more likely to issue convertible bonds. If firms are engaged in valuable investment projects, they are less likely to pay shareholder dividends. Indeed, very few companies paid a dividend during the 1 year before the call announcement. For this reason, dividend dilution cannot be of concern to current shareholders, and thus this variable is omitted from the regressions.

Table 6 reports results for the industry indices-based CARs; the results are similar for CRSP index-based CARs. The only significant variable is the Dirty sample dummy, yielding a difference between the samples due to amplified screening criteria at the announcement day. This seems to drive the multi-day event periods *Dirty* variable. All other independent variables are either small or change sign and are far from any conventional significance level, which is consistent with earlier literature. Also, the poor fit of the regressions is in line with literature containing explanatory regressions of cumulative abnormal returns.

The question now is: Which screening criteria are responsible for the difference between the samples?¹⁵ Since I find the 30 highest negative abnormal returns to be robust to estimation event window, I evaluate the specific screening criteria that resulted in them being put into one or the other of the samples. If the event is screened, it is

¹⁵ Another regression setup would be to introduce a dummy variable for every contemporary news and screening criteria. However, this would substantially diminish the degrees of freedom since the set of news is very heterogeneous.

Table 6 Regression results based on Eq. (2) for the Clean sample (Panel A) and the Dirty sample (Panel B)

	(-1)	(0)	(1)	(-1,0)	(0,1)	(-1,1)
Panel A: FF12, (60,160)						
α	-0.010 (0.76)	0.014 (0.70)	-0.021 (0.70)	0.004 (0.93)	-0.007 (0.92)	-0.017 (0.83)
Dirty	0.001 (0.87)	-0.012** (0.04)	0.003 (0.23)	-0.011 (0.15)	-0.009 (0.27)	-0.008 (0.40)
Liquidity	0.004 (0.99)	-0.067 (0.84)	0.015 (0.99)	-0.063 (0.92)	-0.052 (0.97)	-0.048 (0.97)
Size	0.000 (0.77)	0.000 (0.76)	0.001 (0.77)	0.000 (0.97)	0.000 (0.94)	0.001 (0.86)
Overvaluation	-0.006 (0.50)	0.017 (0.24)	0.002 (0.83)	0.012 (0.43)	0.019 (0.31)	0.013 (0.48)
Adj. R^2	-0.03	0.03	-0.03	-0.01	-0.00	-0.02
N	133	133	133	133	133	133
Panel B: FF12, (251,506)						
α	-0.001 (0.98)	-0.003 (0.94)	0.003 (0.95)	-0.004 (0.95)	0.000 (0.99)	-0.001 (0.99)
Dirty	0.002 (0.78)	-0.011* (0.07)	0.000 (0.94)	-0.009 (0.22)	-0.011 (0.14)	-0.009 (0.28)
Liquidity	-0.002 (0.99)	-0.000 (0.86)	0.024 (0.98)	-0.049 (0.95)	-0.024 (0.98)	-0.025 (0.98)
Size	0.000 (0.99)	0.000 (0.76)	0.000 (0.89)	0.000 (0.90)	0.000 (0.94)	0.000 (0.99)
Overvaluation	-0.008 (0.40)	0.013 (0.36)	-0.004 (0.52)	0.005 (0.73)	0.010 (0.57)	0.002 (0.93)
Adj. R^2	-0.03	0.01	-0.03	-0.02	-0.01	-0.02
N	133	133	133	133	133	133

The results report the factor loadings and p values. The columns denote the different days or periods around the announcement $t = 0$. Independent variables are a dummy for the Dirty sample (*Dirty*), a liquidity measure (*Liquidity*), logarithmic market capitalization (*Size*), and a proxy for *Overvaluation*. p values in parentheses are derived through a 2000-repetition bootstrap routine. *, **, *** denote significance at the 10, 5, and 1 percent level, respectively. N denotes the number of data points in the regression

either due to delisting/bankruptcy, exchanged bonds, clean-up calls, or other news (criteria 3, 4, 7, and 8). Of these 30 abnormal returns, 10 can be attributed to other news, such as CEO changes or joint ventures and five are contemporaneous to earnings announcements. Five bonds are exchanged, seven events are clean-up calls, and two calls are merger related.¹⁶ I conclude that it is not one specific event that is driving the differences between the samples, but the complete set of contemporaneous events.

¹⁶ An event can be in more than one category. Merger-related calls do not count as other news.

5 Conclusion

This paper sheds new light on the announcement effects of forced conversions of convertible bonds. In stark contrast with previous work, I find no significant impact from in-the-money calls on stock prices, a finding that is robust to different event study techniques and parameters. Previous literature provides information- and liquidity-based explanations for negative abnormal returns. The results of this paper disagree. First, convertible bonds that undergo a forced conversion are mostly issued more equity-like than debt-like. A conversion is in line with convertible bond's backdoor-equity purpose. Second, the average conversion option at the announcement date is deeply in-the-money. Market participants expect the bonds to be converted and thus all relevant information is already priced. Third, hedging demand at announcement date should be low due to hedge funds' buy-and-hold arbitrage strategies. These funds buy and hedge large shares of the bonds at issuance. I find only low abnormal trading volume in a screened sample compared to the volume found for a sample with less restrictive selection criteria.

The sample with less restrictive screening criteria has (partly) significantly negative abnormal returns on the announcement day, whereas the abnormal returns of the cleaned sample are not distinguishable from zero. The difference between the samples is partly significant. In assessing what is behind the differences in the two samples, I find the occurrence of corporate news at the same time as the call announcement to be very relevant. Companies tend to announce a call of a convertible and also other firm-specific news on the same day. Such news often involves earnings results or bond exchanges. These are high-impact and well-cited events and overshadow any potential convertible call effect. One possible explanation for contemporaneous news is that managers are fully aware of the typically negative impact of an in-the-money call and "disguise" the call in other news.

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