Anomalous market reaction
to bankruptcy filings

Luis Coelho
University of Edinburgh

Richard J. Taffler*
University of Edinburgh

First Draft: June 21, 2007
Draft 3.0: March 3, 2008

*Corresponding Author
Richard J Taffler
Martin Currie Professor of Finance and Investment
Management School and Economics
University of Edinburgh
William Robertson Building,
50 George Square
Edinburgh EH8 9JY, U.K
Telephone: 44 (0) 131 651 1375
Fax: 44 (0) 131 650 8337,
E-mail: Richard.Taffler@ed.ac.uk
Anomalous market reaction
to bankruptcy filings

ABSTRACT

Although there is an extensive literature on the market’s anticipation of, and short-term reaction to, the bankruptcy event, the longer-term reaction to Chapter 11 filing has not been properly examined to date. We investigate the subsequent performance of the 351 non-financial, non-utility firms entering into bankruptcy protection between 1979 and 2005, and which continue trading on a major exchange. Consistent with previous research, we find negative abnormal and raw returns both before, and at the Chapter 11 date. However, more importantly, we find a strong, negative and statistically significant post-bankruptcy abnormal return of at least -28% over the 12-month period after the event date. Such market underreaction is consistent with research in other domains suggesting that the market has difficulty in dealing appropriately with extreme bad news events. We also find that the post-Chapter 11 drift is distinct from other reported market anomalies. Potential explanations for our anomalous results are presented.

Keywords: market-pricing anomaly, bankruptcy, event study, efficient market hypothesis

JEL classification: G14, G30
Anomalous market reaction
to bankruptcy filings

1. Introduction

The primary aim of this paper is to test how efficient the market is in pricing the stocks of firms which enter into Chapter 11. In particular, we test whether the market underreacts to news of bankruptcy filings in the longer-term, i.e., up to 12 months subsequent to the announcement date. We find a strong, negative, and statistically significant post-bankruptcy drift of at least -28% over the following year, an important and original result that is clearly inconsistent with conventional market pricing theory. We also show that this market-pricing anomaly is different from other established phenomena such as the post-earnings announcement drift, the post-going concern modification drift, the momentum effect, the penny stock effect as well as the level of pre-event financial distress and industry.

This study is important for several reasons. First, bankruptcy matters; such filings are important economic events. For example, Altman and Hotchkiss (2005:3) report that in the 3-year period between 2001 and 2003, as many as one hundred so-called “billion-dollar babies”, including Wall Street’s top five picks, filed for protection under the Bankruptcy Code. Additionally, New Generation Research reports that 9 out of the 10 major bankruptcies in the US since 1980 occurred after 2000.¹ The same source also shows this tendency is not slowing down: in 2007, the combined value of the assets of the five top bankruptcies exceeded 61 billion dollars.²

Second, whereas the market’s anticipation of the bankruptcy event, and the stock price reaction to formal filing for Chapter 11 are well explored in the literature (e.g., Clark and Weinstein, 1983; Datta and Iskander-Datta, 1995; Dawkins et al, 2007), as is the market response to firm emergence from Chapter 11 (e.g., Eberhart et al, 1999), there is a dearth of

¹ See http://www.bankruptcydata.com/Research/10_LargestBankruptcies.htm for details.
evidence on what happens to the stock price of firms subsequent to a few days after entering into bankruptcy proceedings (Altman and Hotchkiss, 2005:83; Dawkins et al, 2007). In effect, only Morse and Shaw (1988) appear to have considered a somewhat related issue reporting, in contrast to our results, that the market prices the stock of bankrupt firms efficiently. However, this early study works with a small sample of 56 firms, and does not address the same questions as we do directly.

Our study is also important because it directly adds to the growing body of literature suggesting the market has problems in correctly assimilating the implications of public domain bad news events. For example, Michaely et al (1995) show that firms omitting a dividend payment have negative excess returns from the event day until the end of the third year. Womack (1996) reports that the market requires around 6 months to impound fully analysts’ new sell recommendations. Dichev and Piotroski (2001) examine the stock return pattern following bond downgrades, and find that firms undergoing such an event underperform a size and book-to-market control sample by around 10%, on average, over the following year, and with further falls in subsequent years. Also, Taffler et al (2004) and Kausar et al (2008) investigate how the equity market reacts to the disclosure of a first-time going-concern audit report, and show that the market takes up to a year to completely assimilate the implications of this negative event. Exploring the market’s reaction to bankruptcy is particularly interesting in this context because it is the most extreme and unambiguous bad news event in the corporate domain.

Finally, at a more general level, our study results help us understand better how financial markets work. For example, as Hirshleifer (2001) points out, greater uncertainty and lack of accurate feedback about a firm’s fundamentals leaves more room for market mispricing. As such, errors in pricing should be more pronounced for high-uncertainty firms (Jiang et al, 2005; Zhang, 2006), which is precisely the case of bankrupt companies. In addition, exploring how the market responds to this acute episode also enhances our understanding about the arbitrage mechanism. In fact, the limited amount of information available about bankrupt firms (Espahbodi et al, 2001; Clarke et al, 2006), the likely absence of institutional investors in their
equity structure (Del Guercio, 1996; Gompers and Metrick, 2001), the difficulty in uncovering distressed securities’ fundamental value (e.g., Gilson, 1995; Gilson et al, 2000), and the problems associated with high trading costs and short sales constraints (e.g., D’Avolio, 2002) clearly point to the fact that, in this particular market, limits to arbitrage may be binding. In line with this intuition, we demonstrate that an arbitrage investment strategy involving the stock of bankrupt firms is highly risky and likely unprofitable for arbitrageurs to exploit.

The remainder of the paper is organized as follows. Section two presents our sample and method. Section three details our main results. Section four summarizes some of our robustness tests. Section five discusses our results and contribution, and section six concludes.

2. Data and methods

2.1 Data

Our data consists of the 351 non-finance, non-utility industry firms which file for Chapter 11 between 10.01.1979 and 10.12.2005, and remain listed on the NYSE, AMEX or NASDAQ after their bankruptcy date, and have sufficient data available on both CRSP and COMPUSTAT to conduct our analysis. Table 1 summarizes our sample construction strategy. All phases are sequential. In the first step all firms filing for bankruptcy between 1979 and 2005 are identified. Seven sources of information are used for this purpose: 1) the Bankruptcydata.com database; 2) the SEC’s Electronic Data Gathering, Analysis, and Retrieval system (EDGAR); 3) COMPUSTAT’s industrial file; 4) Professor Lynn Lopucki’s

---

3 Only firms filing for bankruptcy between 10.01.1979 and 10.17.2005 are considered as between these two dates, bankruptcy was governed by the Bankruptcy Reform Act of 1978, which became generally effective on October 1, 1979. This Act was substantially revised by the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 with most provisions becoming effective on October 17, 2005. Accordingly, by focusing on this 26-year period we are able to work within a largely unchanged legal framework under which corporations were able to file for Federal protection.

4 See http://www.bankruptcydata.com/ for more details.

5 Companies filing for bankruptcy are required to report this to the SEC within 15 days using a Form 8-K. Accordingly, in order to find the bankruptcy cases reported on EDGAR, we search and manually analyze all 8-K forms available on EDGAR that mention the keywords “bankruptcy”, “Chapter 11” or “reorganization”. The initial search was conducted with the help of
Bankruptcy Research database, the SDC database; Altman and Hotchkiss (2005:15-20), and a list of bankrupt firms provided by Professor Edward Altman. All firms are combined into a single list and duplicates removed, yielding a total of 3,437 non-overlapping cases.

Firms are next located on the Center for Research in Security Prices (CRSP) database leading to 1,411 firms being eliminated, the main reason being that firms could not be found in CRSP. However, a few other cases are also excluded because the firm’s ordinary common stock (CRSP share code 10 or 11) is not traded on a major US stock exchange (CRSP exchange codes 1, 2 or 3) during this period, or the firm does not have at least 24-months of pre-event returns available on CRSP.

In the next step, the 1,556 firms delisted prior to or at their bankruptcy filing date are deleted. From the 470 surviving cases, the 58 firms for which accounting data is not available on COMPUSTAT for a 2-year period before the bankruptcy announcement year are then removed, together with 11 firms incorporated outside the US (as defined by COMPUSTAT). Penultimately, following prior research, we also remove all 40 financial and utility firms from our final sample. The 10 firms filing for Chapter 7 are then finally excluded in the last step of the screening process.

Our 351 sample firms have 53 different two-digit SIC codes (168 different four-digit codes) indicating no significant degree of industry clustering. Sixty percent of our firms trade on NASDAQ (209), 31% (109) on the NYSE, and the remaining 9% (33) on AMEX.

Table 1 here
2.2 Method

2.2.1 Measuring abnormal returns

We use a buy-and-hold strategy to make inferences about our sample firms’ stock return pattern before, during and after their Chapter 11 filing date. Barber and Lyon (1997) show that the alternative cumulative abnormal return (CAR) approach does not accurately capture the return from investing in an average sample firm relative to an appropriate benchmark over the horizon of interest. They favor the buy-and-hold abnormal return (BHAR) strategy since it “correctly reflects the actual investors’ experience”. Moreover, Barber and Lyon (1997) show that CARs are biased predictors of BHARs, which can lead to incorrect inferences about medium- and long-term stock price performance.

Fama (1998), however, criticizes the use of BHARs and favors CARs because of their desirable statistical properties, which allow cleaner tests of mispricing. However, Barber and Lyon (1997) and Kothari and Warner (1997) show that such statistical problems with BHARs usually arise over the 3- to 5-year time horizon whereas we restrict our analysis to a one-year period. This is for two reasons. First, filing for bankruptcy often leads to firm delisting, and thus extending the period for computing abnormal returns is problematic due to the loss of many sample cases (e.g., Morse and Shaw, 1988). Second, firms usually start emerging from bankruptcy 15 months after their Chapter 11 filing date (Kalay et al., 2007), and thus ending the abnormal return calculation period three months before minimizes the impact of this important event on our results.10

Our buy-and-hold abnormal returns are computed as follows:

\[
BHAR_i(\tau_1, \tau_2) = \prod_{t=\tau_1}^{\tau_2} \left(1 + r_{i,t}\right) - \prod_{t=\tau_1}^{\tau_2} \left[1 + E(r_{i,t})\right]
\]

(1)

where \(BHAR_i(\tau_1, \tau_2)\) is the buy-and-hold abnormal return for firm \(i\) from time \(\tau_1\) to \(\tau_2\), \(r_{i,t}\) is the raw return for firm \(i\) at time \(t\) and \(E(r_{i,t})\) is the expected return for firm \(i\) at time \(t\).

---

10 Our typical sample firm spends an average (median) of 24.4 (18.1) months in bankruptcy. This is consistent with previous research by Altman (1993) and Eberhart et al (1999).
Individual BHARs are averaged cross-sectionally as follows (e.g., Barber and Lyon, 1997; Campbell et al, 1997):

$$BHAR(\tau_1, \tau_2) = \sum_{i=1}^{n} BHAR_i(\tau_1, \tau_2)$$

(2)

where $BHAR_i(\tau_1, \tau_2)$ is defined as above, and $n$ is the number of firms with valid BHAR over time period $\tau_1$ to $\tau_2$. As suggested by equation (2), we use equally weighted rather than value-weighted returns since this is more appropriate in the context under analysis as giving the same weight to all firms in the investment portfolio allows maximum diversification of each firm’s idiosyncratic risk, a critical aspect when dealing with failed firms (e.g., Gilson, 1995; Platt, 1999:110). Additionally, previous research shows that equal weighting captures the extent of underperformance better than value weighting does given the particular nature of our bankrupt firms (Brav et al, 2000; Kadiyala and Rau, 2004). Loughran and Ritter (2000) also argue that value-weighted portfolio returns reduce the power of the tests used to detect any potential behavioral bias.

Unless otherwise stated, daily returns collected from CRSP are employed in the calculation of abnormal returns.11 As argued by Kothari and Warner (2007), the use of daily rather than monthly security returns data permits more precise measurement of abnormal returns, and more informative studies of announcement effects. We define a year as twelve 21-trading day intervals, an approach consistent with previous research (e.g., Michaely et al, 1995; Loughran and Ritter, 1995; Ikenberry and Ramnath, 2002). Event day $t=+1$ is included in the bankruptcy announcement window together with days $t=-1$, and $t=0$, the bankruptcy announcement date, as firms are able to file their bankruptcy petition after the market closes (Dawkins et al, 2007).

Buy-and-hold raw returns (BHRR) are also calculated. These particular returns can be understood as a more stringent test of market mispricing, and are used to highlight

---

11 All data sources mentioned in section 2 provide the bankruptcy date for each firm they cover. The only exception is COMPUSTAT. Factiva is used to determine the bankruptcy date for COMPUSTAT cases.
mismeasurement problems associated with our expected return proxy (Kausar et al, 2008).

Buy-and-hold raw returns are computed as follows:

\[
BHRR_i(\tau_1, \tau_2) = \prod_{t=\tau_1}^{\tau_2} (1 + r_{i,t}) - 1
\]

(3)

where \(BHRR_i(\tau_1, \tau_2)\) is the buy-and-hold-raw return for firm \(i\) from time \(\tau_1\) to \(\tau_2\), and \(r_{i,t}\) is the raw return for firm \(i\) at time \(t\). Buy-and-hold raw returns are averaged cross-sectionally in an equivalent way to that of equation (2).

Some of our sample firms are delisted in the 12-month period subsequent to their Chapter 11 filing date. Drawing on Shumway (1997), and Shumway and Warther (1999), we include the delisting return in the calculation of abnormal returns, a procedure also used by Campbell et al (2007). Barber and Lyon (1997), and Lyon et al (1999) point out that the sample’s mean long-run abnormal return calculated with truncation does not represent the average return an investor could earn from investing in an executable strategy, since his use of the proceeds from the investment in a delisted firm is left unresolved. Kausar et al (2008) emphasize that this is a crucial aspect when dealing with highly distressed firms and show that considering a zero abnormal return in the post-delisting period is a reasonable way to deal with this issue. We draw directly on Kausar’s et al (2008) results and assume that, in the post-delisting period, sample firms earn a zero abnormal return.\(^{13,14}\)

\(^{12}\) Performance issues explain 94% of these delisting cases (CRSP delisting codes 500 to 599).

\(^{13}\) Kausar et al (2008) demonstrate how an inappropriate post-delisting reinvestment strategy in the case of financially distressed firms can lead to seriously misleading results, as with Ogneva and Subramanyam (2007).

\(^{14}\) Re-investing the proceeds from the delisting payment in a portfolio of stocks comprising the same size decile of the delisted firm or in the CRSP value-weighted index for the remainder of the compounding period, however, does not alter our results in any meaningful way.
2.2.2 Benchmark procedure

Following Barber and Lyon (1997), and Ang and Zhang (2004), we use a single control firm approach in our main results. Such an approach eliminates the new listing bias, the rebalancing bias, and the skewness problem, yielding well-specified tests in most of the situations they consider.

We identify a control firm by matching each of our sample firms with the firm with most similar size and book-to-market ratio. This approach is consistent with a number of recent studies exploring the medium-term return pattern of firms facing a high degree of financial distress (e.g., Dichev and Piotroski, 2001; Taffler et al, 2004; Ogneva and Subramanyam, 2007; Kausar et al, 2008). First, for each sample firm, market capitalization is measured one month before the bankruptcy filing date. CRSP is then searched for an initial pool of matching candidates with market capitalization at the end of the bankruptcy filing month of 70% to 130% of the sample firm’s equity value. The matched (control) firm is then identified as that firm within this set with the closest book-to-market ratio. To ensure the numerator is available when market value is derived, we use the book value of equity taken from the last annual accounts reported before the bankruptcy year (Fama and French, 1992), and allow a three-month lag to measure the market value of equity. The match is confirmed if: 1) the matched firm has at least 24 pre-event months of returns available on CRSP; 2) is not in bankruptcy; 3) is incorporated in the US; 4) is not a financial or utility firm, and 5) it has sufficient information on COMPUSTAT to conduct our analysis.

Importantly, if a control firm is delisted before the ending date for its corresponding bankrupt firm period, a second firm is spliced in after its delisting date, that with second closest size and book-to-market to that of the delisted firm in the original ranking. Finally, if a chosen control firm itself subsequently files for bankruptcy, we treat it as if it is delisted on its bankruptcy date. These procedures introduce no survivorship or look-ahead bias and minimize

15 This helps reduce the impact of the event on the leading matching variable. As a robustness check, we measure size for all sample firms two, three, six and twelve months before their bankruptcy date and re-run the analysis. Results remain qualitatively unchanged.

16 The market value of every sample firm is measured before its bankruptcy announcement date. This result is confirmed by manually inspecting all cases.
the number of transactions implicit in the calculations (e.g., Loughran and Ritter, 1995; Spiess and Affleck-Graves, 1995).

For illustrative purposes, and to allow comparisons with prior research on the market’s reaction to bankruptcy announcements, we also report parallel market-adjusted return results using the equally weighted CRSP index including dividends as an alternative proxy for expected returns.

2.2.3 Statistical significance of the abnormal return

Following Barber and Lyon (1997), and Ang and Zhang (2004), we employ a t-test to infer the statistical significance of the different mean BHARs. Importantly, we use the cross-section of the buy-and-hold abnormal returns to form an estimator of their variance, which allows it to change after the event (Boehmer et al, 1991; MacKinlay, 1997). This is appropriate since previous research by Aharony et al (1980), and later confirmed by Johnson (1989), and McEnally and Todd (1993), shows that both the systematic and unsystematic risk of bankrupt firms varies as the bankruptcy date approaches.

Equations (1) and (3) are used for exploring the market’s medium-term reaction to bankruptcy announcements. However, longer-horizon returns tend to exhibit positive skewness (e.g., Fama, 1998; Brav, 2000), which is usually more pronounced in the case of smaller firms (Ball et al, 1995). Drawing on Kraft et al (2006), we report mean BHARs that are winsorized at the 1 and 99 percent levels to reduce the impact of extreme outliers in our analysis, a procedure also implemented in previous research by Ikenberry and Ramnath (2002) and Kausar et al (2008). Importantly, Kausar et al (2008) show that winsorizing the abnormal returns is of crucial importance when dealing with small firms since this method helps reducing the impact of low-price stocks on the skewness of ex-post returns. The same argument is also put forward by Kraft et al (2006, 2007), and is especially important in the context of our research since a relatively large number of our bankrupt companies trade at prices below $1 per share.

---

17 See also Cowan and Sergeant (2001) for a discussion on the impacts of winsorization in long-term abnormal returns.
We also present median returns to check the validity of our parametric results. These returns are unaffected by extreme observations, and present some theoretical advantages over mean BHARs (Ang and Zhang, 2004). Additionally, Kausar et al (2007) demonstrate that it is very important to complement the usual parametric analysis of longer-term abnormal returns of highly distressed firm with the computation of their non-parametric equivalents. Consistent with previous research dealing with bankruptcy announcements, a Wilcoxon signed rank-test is employed to test the statistical significance of our median abnormal returns (Dawkins and Rose-Green, 1998; Rose-Green and Dawkins, 2002; Dawkins et al, 2007). Nonetheless, some caution is warranted here. As Ikenberry and Ramnath (2002) point out, median returns are problematic when considering questions of efficiency because of the inconsistency this statistic poses for ex ante trading strategies. Accordingly, median returns are only used for robustness test purposes.

3. Empirical results

3.1 Data descriptives

Table 2 provides sample and control firm descriptive statistics. Panel A shows that our sample firms are in an advanced state of financial distress one year before filing for Chapter 11. For the typical firm, return on assets is negative (mean=-19%, median=-6%), current ratio is low (mean=169%, median=128%), and leverage is relatively high (mean=45%, median=40%). Not surprisingly, average Altman (1968) z-score is low (mean=1.37, median=1.31), suggesting that these firms are likely to fail in the short-run. Results for the matched firms are somewhat different. For instance, even though matched on size and book-to-market these businesses are in a stronger financial position than the bankrupt sample. Mean and median z-score and current ratio are higher, and leverage is appreciably lower (all differences between groups for these variables are statistically significant at the 1% level). Nonetheless the typical matched firm is also losing money: mean return on assets is -14.8%, with the corresponding median not
significantly different from zero at conventional levels. Panel A also shows the bankrupt and control firms have similar total assets and turnover.

Panel B of table 2 summarizes a number of market variables. Both sample and matched firms are small, with average market capitalization of around $160m (median=$32m) and, not surprisingly, have high book-to-market ratios. Panel B also shows that, despite their small average size, our bankrupt firms trade on average for 230 days (out of 252) in the 12-month period following the bankruptcy announcement month. In the comparable period, control firms trade for an average of 224 days, with difference in means significant at a 10% level. The stock of bankrupt firms is clearly still of interest to at least a group of market investors. Panel B also highlights the very significant impact of the bankruptcy filing on mean stock price which falls from $4.97 before the event to $2.08 in the event month, a reduction of -58%. The equivalent decline in median price is from $3.12 to $0.97. In the case of the control firms, prices remain relatively stable, with a mean value of around $9 (median around $5).

Panel B of table 2 again shows that there is a market for the stock of bankrupt firms. In fact, in the 12-months before the bankruptcy date, the average daily turnover for these firms is 0.51%, implying an annual turnover rate of 129%. This rate spikes to 290% in the bankruptcy-announcement month, which shows the importance of the event under analysis. After the dissipation of this initial effect, mean bankruptcy firm daily turnover stabilizes at 0.57%. The data also shows that the reported pattern is specific to the event firms; in the case of the control sample, daily turnover does not exhibit any obvious variation, with a mean value of around 0.43% over the entire period.

Finally, panel C shows that only 25% of the sample firms have positive earnings, and around the same percentage pay dividends. In line with panel A, panel C again presents evidence that the matched firms are financially stronger than the sample firms. In fact, almost 50% have positive earnings, and around 40% pay dividends. Panel C shows that both sample and control firms are usually audited by one of the Big 8 auditing firms.\textsuperscript{18} Around a quarter of

\textsuperscript{18} The following companies are considered as part of the Big 8 group: 1) Arthur Andersen; 2) Arthur Young; 3) Coopers & Lybrand; 4) Ernst & Young; 5) Deloitte; 6) Peat, Marwick and Main; 7) Price Waterhouse; 8) Touche Ross. A number of these accounting firms merged during our observation period.
the bankrupt firms have a first time going-concern audit opinion in their accounts for the year preceding Chapter 11. Only two percent of the matched firms are in the same situation.

3.2 Main results

We now turn to the analysis of our main results. Consistent with previous research, we find that the equity market anticipates the formal announcement of bankruptcy (e.g., Clark and Weinstein, 1983; Datta and Iskandar-Datta, 1995, Dawkins et al, 2007). In fact, panel A of table 3 show that mean (median) one-year pre-event abnormal return is -49% (-43%). We uncover a similar story when considering raw returns (mean=-63%, median= -69%). All values are highly statistically significant (p<0.01). These findings indicate that important information about the forthcoming bankruptcy event is available to the market well before the formal Chapter 11 filing date (e.g., Clark and Weinstein, 1983; Dawkins and Rose-Green, 1998).

Panel B of table 3 shows a strong, negative reaction to the bankruptcy event. In fact, regardless of benchmark, mean (median) abnormal return measured for the (-1,+1) window is around -26% (-27%), and highly significant (p<0.01). This finding is in line with previous research on this topic and reinforce the idea that the bankruptcy event itself is highly value-relevant (e.g., Datta and Iskandar-Datta, 1995; Rose-Green and Dawkins, 2002; Dawkins et al, 2007).

The key results of panel C on table 3, however, point to a strongly negative and statistically significant post-bankruptcy drift lasting at least one full year after the event date. Mean (median) BHAR for the (+2,+252) period = -28% (-27%), both significant at p<0.01. Raw
returns further corroborate this conclusion with mean (median) BHRR for this period of -14% (p<0.01) (-39%, p<0.01).19

The 6-month post-event period represented by the (+2,+126) compounding window provides further evidence in favor of the incomplete market reaction to bankruptcy announcement argument. Mean (median) BHAR = -16% (p<0.01) (-16%, p<0.01). Unadjusted returns provide a similar picture (mean=-15%, p<0.05; median=-19%, p<0.01).20

Of special interest in the context under analysis is the four-month post-event period portrayed by the (+2,+84) compounding window. The Bankruptcy Reform Act of 1978 allows the incumbent management of firms filing for Chapter 11 an exclusivity period of 120 days to develop its reorganization plan.21 Accordingly, this is the period where asymmetry of information between bankrupt firm management and the market is most acute. Panel C of table 3 shows that mean (median) BHAR is now -13% (p<0.01) (-15%; p<0.01). BHRRs further corroborate this pattern.22

Three key ideas summarize this section’s results. The first is that the market is able to anticipate bankruptcy. This pattern has already been documented in previous work and is usually explained by information relating to the forthcoming bankruptcy being released to the market before the event date. The second is that, despite the market’s anticipation of bankruptcy, the event in itself is still very important from an information perspective, with the short-term reaction to its announcement negative and very significant. This is, of course, not surprising, especially if one considers that filing for bankruptcy is surely the worst-case scenario in the corporate domain.

However, our most interesting and original finding comes from analysis of the stock return pattern after the bankruptcy event. Our results clearly suggest that the equity market does not fully and quickly incorporate the impact of bankruptcy on the stock price of affected firms. In

19 The point estimate for the mean unwinsorized size and book-to-market risk-adjusted abnormal return for the (+2,+252) window is -28.1% (p<0.01).
20 The point estimate for the mean unwinsorized size and book-to-market risk-adjusted abnormal return for the (+2,+126) window is -19.3%, (p<0.01).
21 Under some circumstances, the Bankruptcy Court may concede an extension of this deadline (e.g., Gilson, 1995). Of course, not presenting a reorganization plan is also an important news event for all investors involved in the bankruptcy proceedings.
22 The point estimate for the mean unwinsorized size and book-to-market risk-adjusted abnormal return for the (+2,+84) window is -16.0% (p<0.01).
particular, we find a strong, negative and statistically significant post-event drift that lasts for at least one full year after the Chapter 11 filing date with mean (median) BHAR of -28% (-27%). Accordingly, our findings seem to be inconsistent with market efficiency and appear to support the argument that markets are unable to digest bad news events on an unbiased and timely basis (e.g., Bernard and Thomas, 1989, 1990; Michaely et al, 1995; Womack, 1996; Dichev and Piotroski, 2001; Chan, 2003; Taffler et al, 2004; Kausar et al, 2008).

4. Additional tests

Some caution should be exercised, however, in interpreting the above results; there is still much debate surrounding the appropriate measurement of longer-term abnormal returns (e.g., Brown and Warner, 1980, Kothari and Warner, 1997, Lyon et al, 1999). A casual examination of the contemporaneous literature on market pricing anomalies suggests that the best approach to check the soundness of a given result when dealing with longer-term event studies is testing its robustness using a combination of alternative methods (e.g., Boehme and Sorescu, 2002; Hertzel et al, 2002; Ikenberry and Ramnath, 2002; Byun and Rozeff, 2003).

In this section, we first test for a range of competing explanations for our anomalous results, specifically the post-earnings announcement drift, the post-going concern modification drift, the momentum effect, distress risk, the penny stock effect, and industry. We also implement a calendar-time portfolio approach to control for potential problems related to cross-section dependence in BHARs.
4.1 Testing for a post-earnings announcement drift (PEAD) explanation

A voluminous literature shows that earnings surprises are followed by an incomplete market reaction, which is usually more pronounced when the surprise is negative (e.g., Ball and Brown, 1968; Foster et al, 1984; Bernard and Thomas, 1989, 1990). As such, we investigate whether our results are, in fact, driven by the post-earnings announcement drift.

We seek to distinguish between the two drifts (post-bankruptcy announcement, and PEAD) in two ways. In the first case, we use a control firm approach similar to section 2.2.2 above, but now match first by size then by closest earnings surprise to determine our benchmark firms. In this way we can separate out post-bankruptcy drift from any earnings surprise effect, since the benchmark firms have essentially the same earnings surprise in terms of sign and magnitude but do not file for bankruptcy during the test period.

The second test is based on Dichev and Piotroski (2001) who divide their sample according to the sign of the quarterly earnings surprise. The rationale is as follows. If the abnormal return following the bankruptcy announcement is mostly due to the effects of a correlated earnings surprise, then the post-event underperformance should be more acute for negative earnings surprise firms. To test this preposition, we split our sample into two groups conditional on the sign of their pre-bankruptcy earnings surprise and explore for a significant difference in performance between these two earnings surprise portfolios.

A measure of earnings surprise needs to be specified in order to implement both tests described above. Drawing on Foster et al (1984), we define this variable as follows:

\[
\Delta Q_{i,q} = \frac{Q_{i,q} - E(Q_{i,q})}{|Q_{i,q}|}
\]

(4)

where \(\Delta Q_{i,q}\) is the earnings surprise for firm \(i\) for quarter \(q\), \(Q_{i,q}\) is the current quarterly earnings figure for firm \(i\), \(E(Q_{i,q})\) is the expected earnings figure for firm \(i\) in the current quarter, and \(|Q_{i,q}|\) is the absolute value of firm \(i\)'s current quarter earnings. The current quarter is defined as the most recent quarter preceding the bankruptcy announcement date. Our naïve
model assumes that the expected earnings figure for firm $i$ in the current quarter is simply the realized quarterly earnings for the same quarter in the previous year.\textsuperscript{23, 24}

Table 4 summarizes our results. Panel A shows that our sample firms exhibit a strong post-bankruptcy drift even after controlling for earnings surprise. In fact, all mean and median BHARs are negative, with most statistically significant at $p<0.01$. Similar results apply in the case of panel B of table 4, which tests for difference depending on sign of earnings surprise, where both negative and positive earnings surprise portfolios exhibiting post-bankruptcy event drift. For instance, the point estimate for the one-year mean (median) BHAR for the negative earnings surprise portfolio is -32\% (-30\%) ($p<0.01$), while its equivalent for the positive earnings surprise portfolio is -18\% (-16\%) ($p<0.05$). The t-test (Wilcoxon-Mann-Whitney test) for difference in means (medians), however, is significant at the 5\% level (not-significant), providing some weak evidence that firms suffering pre-bankruptcy negative earnings surprise exhibit a more pronounced post-event drift, although this difference does not appear to hold in the case of other compounding windows. Based on these results we conclude that our findings are robust to any potential post-earnings announcement drift explanation.

\begin{table}[h]
\centering
\caption{Table 4 here}
\end{table}

\textsuperscript{23} The literature on the post-earnings announcement drift offers a number of different alternatives for determining the value of expected earnings for a given quarter. The definition used here closely relates to model one in Foster et al (1984).
\textsuperscript{24} All data for calculating equation (4) are collected from COMPUSTAT’s quarterly industrial files (COMPUSTAT item 8).
4.2 Controlling for the post-going concern modification drift, momentum, distress risk, industry and low priced stocks

In this section we attempt to explain our main results in terms of post-going concern modification drift, momentum, distress risk, penny stocks and industry.

Taffler et al (2004) investigate the stock price reaction to UK first-time going-concern audit report disclosures in the calendar year following publication. The authors report that, depending on the adopted benchmark, their firm population underperforms by between 24% and 31% over this period. In a subsequent paper, Kausar et al (2008) find that the US equity market also underreacts to the same event, documenting a downward drift around 14% over the one-year period after the announcement date. These are important results for our research since panel C of table 2 shows that around a quarter of our sample firms receive a first-time going-concern audit report modification in their last accounts prior to filing for Chapter 11. As such, it could be that the post-bankruptcy drift is simply a manifestation of post-going concern underperformance as already documented in the literature.

We explore this issue by dividing our sample into two groups. The GC portfolio refers to those firms receiving a first-time GC audit report in their last published annual accounts before entering into bankruptcy proceeding. All other firms are allocated to the non-GC portfolio. We then compare subsequent 12-month bankrupt and control firm returns across the two sets of firms.25

We find that 12-month (6-month) BHAR for the non-GM portfolio = -21% (-18%) with median BHAR = -24% (-22%), all significant at the 1% (1%) level. The equivalent results for the GC portfolio are mean 12-month BHAR = -28% (-12%), and median BHAR = -21% (-9%), with the one year results significant at the 1% (1%) level, and the 6-month results at the 5% (5%) level. On this basis our anomalous findings are not driven by a post-GC

---

25 The results presented below also control for the GC status of the benchmark firms. In particular, 7 companies had to be replaced for this test because they received a first-time GCM report when were being used as control firms.
underperformance effect, and, in fact, difference in portfolio returns is not significant on a 6- or 12-month time horizon basis.

Panel A of table 3 clearly shows that stock prices fall steeply in the pre-bankruptcy period, and it could be possible that our findings are no more than a continuation of such negative returns as with Jegadeesh and Titman (1993; 2001). To test whether stock momentum is, in fact, driving our results we match each of our bankrupt firms with a new control firm as follows. First, we identify all non-bankrupt, non-finance, non-utility firms with a market capitalization between 70% and 130% of that of each our sample firm’s market capitalization. Second, from this set, we choose the firm with prior 12-month raw returns closest to that of the sample firm.26 We then compare subsequent 12-month bankrupt and control firm returns.

We find that our main results are unaffected. Mean 12-month (6-month) BHARs are -25% (-16%), and median 12-month (6-month) BHARs are -32% (-17%), all significant at better than the 1% (1%) level. As such, we cannot explain our results in terms of prior return continuation.

Panel A of table 2 shows that mean (median) Altman’s (1968) z-score for our sample companies is 1.37 (1.31), where z-score < 1.81 indicates firms which “clearly fall into the bankruptcy category”. On this basis, the majority of our sample firms are financially distressed when filing for Chapter 11. Dichev (1998) suggests that firms with higher distress risk significantly underperform in the following year and, a similar finding is reported by Griffin and Lemmon (2002). As such, we need to distinguish between a financial distress explanation and a bankruptcy-based explanation for our anomalous results. To do this, we adopt the same approach as for the momentum test and now match our bankrupt firms with control firms based on size and z-score.

26 In particular, we compute momentum for both sample and control firms as:

\[ \text{Mom}_i = \frac{1}{12} \sum_{t=-12}^{0} R_i, \]

where \( \text{Mom}_i \) is the momentum for firm \( i \) and \( R_i \) is the raw monthly return of firm \( i \) in month \( t \), with \( t = 0 \) being the bankruptcy announcement month. All data for computing momentum are taken from CRSP’s monthly stock return file.
Our main results are unaffected. The post-bankruptcy abnormal returns are still strong and negative, with mean 12-month (6-month) BHARs -34% (-15%) and median 12-month (6-month) BHARs -35% (-18%), all significant at better than the 1% (1%) level. Such findings suggest our results are not driven by financial distress risk.

Industry clustering arises when events are concentrated in a few particular industries. This is problematic because it reduces the power of statistical tests used to verify the significance of abnormal returns (e.g., Dyckman et al, 1984; Mackinlay, 1997). This issue is important in the context of our research since there is a potential contagion/competitive industry effect when a firm files for bankruptcy (e.g., Lang and Stulz, 1992; Akhigbe et al, 2005). Accordingly, and despite our descriptive analysis indicating that our sample is not affected by a significant degree of industry clustering, we still test for the possibility that our results are driven by an industry clustering explanation.

To control for an industry-specific explanation we match each of our bankrupt firms with control firms on industry, size and book-to-market in that order. First, industry is matched using COMPUSTAT’s two-digit SIC code. The second step is to identify, for each bankrupt firm, all potential control firms that belong to the same industry class and that lie within the sample firm’s size decile.27 Finally, the firm with closest book-to-market ratio to that of the sample firm is chosen as the control firm.

After controlling for industry, we find mean 12-month (6-month) BHARs of -32% (-16%) and median 12-month (6-month) BHARs of -32% (-17%), all significant at better than the 1% (1%) level. These results indicate that our original findings cannot be explained by an industry-specific explanation.

Finally, panel B of table 2 shows that our sample companies are characterized by low average stock price of $2.1 during the bankruptcy month (median = 92 cents). This is a concern.

27 We use a size-decile approach here because the alternative criterion of choosing a benchmark firm with a market capitalization within 70% and 130% of that of the sample firm results in a significant number of event firms not having a suitable control firm.
since previous research suggests that apparent long-term market overreaction may be driven by computational problems associated with the returns of low-priced stocks (e.g., Ball et al, 1995). In addition, incomplete market reaction to bad news is more likely to occur in the case of low-priced firms (e.g., Dichev and Piotroski, 2001; Chan, 2003). To test for a low-price stock explanation for our results, we divide our sample into two portfolios according to their closing stock price as at the end of the second day after bankruptcy (Kausar et al, 2008). In particular, firms with closing price lower than that of the sample median closing stock price are labeled as micro-penny stocks; all others are labeled as penny stocks. We then investigate if there is a difference in performance between these two penny-stock portfolios using the size and book-to-market control firm benchmark approach as in our main results.

We find that 12-month (6-month) BHAR for the micro-penny stock portfolio = -28% (-25%) with median BHAR = -27% (-24%), all significant at the 1% (1%) level. The equivalent results for the penny-stock portfolio are mean 12-month BHAR = -20% (-9%), and median BHAR = -19% (-8%), with the one year results significant at the 1% (1%) level, and the 6-month results at the 5% (5%) level. On this basis our anomalous findings are not driven by a penny stock effect, and, in fact, difference in portfolio returns is not significant on a 12-month time horizon basis.28

4.3 Calendar-time portfolios

Fama (1998) criticizes the used of buy-and-hold abnormal returns arguing that any method that ignores the cross-section dependence of event-firm abnormal returns that are overlapping in calendar time is likely to produce overstated test statistics; a point also raised by Brav (2000). Mitchell and Stafford (2000) claim that major corporate events are usually not random; they cluster through time by industry. Consequently, in most cases, event samples are unlikely to consist of independent observations as assumed in the BHAR approach, and such lack of independence will lead to positive cross-correlation of abnormal returns generating BHAR test

---

28 However, 6-month mean (median) BHARs do differ significantly at the 1% (1%) level.
statistics that are severely overstated. As a result, these authors favor the calendar-time portfolio method introduced by Jaffe (1974), and Mandelker (1974) since it accounts for individual event-firm cross-correlations in the portfolio variance at each point in calendar time. As a test of the robustness of our statistical method, we also implement this approach here.

We use monthly returns to conduct this test (e.g., Boehme and Sorescu, 2002; Ikenberry and Ramnath, 2002; Byun and Rozef, 2003). Sample firms are added to a portfolio at the end of the month following their Chapter 11 filing date, and are held there for 6 or 12 months. The portfolio is rebalanced monthly to drop all firms that reach the end of their 6- or 12-month holding period, and to add all firms filing for bankruptcy in the previous calendar month. Importantly, given the high degree of skewness affecting the distribution of bankrupt firm market capitalization, we employ equally weighted portfolio rebalancing strategies (Ikenberry and Ramnath, 2002; Loughran and Ritter, 2000).

Calendar-portfolio abnormal performance is assessed using Carhart’s (1997) four-factor model. After regressing portfolio excess monthly returns on the independent model variables, we use the intercept $\alpha$ as a measure of abnormal return. If the predictions of the EMH hold, this intercept should not differ significantly from zero at conventional levels. Conversely, if the intercept is statistically significant to zero, this signals incomplete market reaction to the event under scrutiny (Mitchell and Stafford, 2000).

We employ both ordinary least squares (OLS) and weighted least squares (WLS) to estimate our equation parameters. With OLS, results explicitly account for the cross-section dependency that affects the BHAR approach, although at the cost that the technique may produce inefficient estimates. The use of WLS reduces the heteroskedasticity-related problems and accounts well for the fact that our event companies are not evenly spread across the sample period, i.e., the “hot versus cold markets” problem described in Loughran and Ritter (2000). However, it does not deal appropriately with the cross-section dependency problem, which is the reason for using a calendar-time approach in the first place. We also drop from the analysis all months where the calendar portfolio holds fewer than 10 firms (e.g., Mitchell and Stafford, 2000;

---

29 Skewness of the size variable for the set of sample firms is 9.87.
Ikenberry and Ramnath, 2002). The heteroskedastic-consistent t-statistic proposed by White (1980) is used to test the null hypothesis of no abnormal performance.

4.3.1 Measuring abnormal performance

4.3.1.1 Unadjusted intercepts

The model proposed by Carhart (1997) assumes that a stock’s expected return is a linear function of the co-variability with the return on the market and three hedge portfolios related with size, book-to-market, and momentum. The model takes the form:

\[
\alpha_p + \beta_p (r_m - rf) + s_p SMB_t + h_p HML_t + u_p UMD_t + \epsilon_{pt}
\]

where \( r_p \) is the return of portfolio \( p \), \( rf \) is the risk-free rate, \( r_m - rf \), \( SMB_t \), \( HML_t \) and \( UMD_t \) are, respectively, the premia on a broad market portfolio, the difference between the return on a portfolio of small stocks and the return on a portfolio of large stocks; the difference between the return on a portfolio of high book-to-market stocks and the return on a portfolio of low book-to-market stocks; and the difference between the return on the two high prior return portfolios and the return on the two low prior-return portfolios. Parameters \( b_p, s_p, h_p \), and \( u_p \) measure portfolio \( p \)’s sensitivity to each of the four factors considered in the model. Finally, \( \epsilon_{pt} \) is a disturbance term, assumed to be white noise.

4.3.1.2 Adjusted intercepts

Mitchell and Stafford (2000) argue that existing factor models cannot completely explain the cross-section of expected returns, which, in the worse case scenario, may result in biased estimates of the intercept under the null hypothesis of no abnormal performance. Drawing on Boehme and Sorescu (2002), and Ikenberry and Ramnath (2002), we seek to overcome this potential problem by estimating adjusted intercepts derived using an arbitrage portfolio that is long in the stock of bankrupt firms, and short in that of the matched control firms. Specifically, the following regression is also implemented:
\begin{equation}
    r_{p,t} - r_{control,t} = \hat{\alpha}_p + \beta_p (r_{m,t} - rf_t) + \gamma_p SMB_t + \delta_p HML_t + \epsilon_p UMD_t + \epsilon_{p,t}
\end{equation}

where the parameters and variables of equation (7) have the same meaning as in equation (6). The main difference in this second estimation procedure is that the excess returns of the calendar-time portfolio are now calculated using the returns of a carefully selected matched sample, $r_{control,t}$, and not the risk-free rate. For further robustness test purposes we use the returns of matched samples based both on size and book-to-market, and size and momentum to estimate the values of the adjusted intercepts ($\hat{\alpha}_p$).

### 4.3.2 Results\(^{30}\)

Table 5 reports what happens when the unadjusted factor models are used as benchmarks. Irrespective of the estimation procedure and holding period, all intercepts are negative and statistically significant at conventional levels. This is consistent with our earlier BHAR evidence, and indicates that a post-bankruptcy announcement drift equally occurs on a calendar-time basis. For the one-year horizon, abnormal performance estimated using OLS is -2.7% per month, and -2.6% per month using the alternative WLS method. The equivalent annualized figures of -32% and -31% are both higher than the mean BHAR estimate of -28% in table 3, panel C. However, as Ikenberry and Ramnath (2002) point out, as the BHAR and calendar-time portfolio approaches differ in several ways, differences in results are to be expected, quite apart from potential misspecification problems.

Employing the adjusted intercept technique, model intercepts are again consistently negative and statistically significant at conventional levels across estimation methods and holding periods, confirming the post-Chapter 11 filing drift.\(^ {31}\) However, although the size and book-to-market-adjusted results are virtually identical to their unadjusted counterparts in table 5, the size and momentum-adjusted results are weaker with one-year post-event abnormal performance estimated using the OLS (WLS) approach of -22% (-21%). This suggests that

---

\(^{30}\) Using the Fama and French (1993) three-factor model does not alter the qualitative nature of our results.

\(^{31}\) We do not report this results here to save space. They are available upon request from the first author.
failure to control fully for the momentum effect may result in poorer estimates of the market’s reaction to bankruptcy announcements.

Nonetheless, overall, the results of the calendar-time method are very consistent with those obtained with the BHAR approach. Both clearly suggest that the market is unable to deal appropriately with the bankruptcy event in an unbiased way, leading to at least a 12-month post-Chapter 11 drift that is highly negative and statistically significant.

5. Discussion

According to the semi-strong form of the EMH, the market should accurately reflect all value-relevant public information shortly after it becomes publicly known (Fama, 1970). On a face value basis our results are inconsistent with this prediction. If the equity market were efficient in its reaction to bankruptcy announcements, the existence of a statistically significant post-bankruptcy announcement downward drift would not occur. Accordingly, we believe our evidence is more in line with previous research that suggests the market has problems dealing with bad news events (e.g., Bernard and Thomas, 1989, 1990; Michaely et al, 1995; Womack, 1996; Dichev and Piotroski, 2001; Chan, 2003; Taffler et al, 2004; Kausar et al, 2008).

Although our findings could, in principle, be explained by the use of an inappropriate asset-pricing model leading to the mismeasurement of the relative risk of sample and benchmark firms, the magnitude and robustness of our results argue otherwise, a point also raised in previous research (e.g., Spiess and Affleck-Graves, 1995, 1999; Dichev and Piotroski, 2001; Taffler et al, 2004). In this sense, and although we cannot rule out risk-based (i.e., rational) explanations, we believe that other explanations for our findings are more plausible, including the existence of limits to arbitrage (e.g., Shleifer and Vishny, 1997). In effect, one can only conceive the existence of market pricing anomalies when sophisticated agents are unable or unwilling to exploit (correct) such a situation. Several reasons justify why arbitrage should be
limited in the case of bankrupt firms. For instance, it is likely that strategy implementation costs are sufficiently high to deter arbitrage-based investment strategies. In untabulated results, we investigate this issue using the LDV model of Lesmond et al (1999). This model allows us to estimate the all-in (explicit and implicit) roundtrip transaction cost for each sample and matched firm included in our analysis. For our bankrupt firms, mean (median) estimated roundtrip cost is 11.2% (9.0%) in the pre-bankruptcy period, increasing to 14.5% (12.3%) in the post-event period. For control firms, equivalent estimated values are 8.9% (6.9%) and 10.4% (8.3%). As such, implementing an arbitrage strategy involving the stock of bankrupt companies is costly and could yield negative average returns. A similar pattern is documented by Barber et al (2001), Lesmond et al (2004), Taffler et al (2004), and Kausar et al (2008) when exploring the profitability of trading strategies based on analysts’ stock recommendations, stock momentum, and going-concern audit modifications, respectively.

Restrictions affecting the ability to short the stock of bankrupt firms is another factor that explains why arbitrage may be difficult to implement in our particular context. D’Avolio (2002) reports that over half of the stocks with prices below five dollars present in the CRSP database are hard to short. A significant percentage of our bankrupt firms clearly falls within this category. Moreover, given their legal status, we would also argue that bankrupt firms are even more special in this respect.

Finally, as pointed out by Shleifer (2000:14), the arbitrageur always faces the risk that any mispricing may worsen before disappearing. This could result from the actions of noise traders, and is important because the sophisticated investor only profits from his trading strategy once the price converges back to its fundamental value (e.g., De Long et al, 1990; Shleifer and Summers, 1990; Shleifer and Vishny, 1997). Increased risk posed by noise traders limits the amount that the sophisticated investor is willing to invest, which results in reduced ability for the market to correct potential market-pricing anomalies. Using data collected from the

---

32 Transaction costs are only one type of cost affecting arbitrage strategies. However, as noted by Lesmond et al (2004), capturing all the components of the implementation costs associated with an arbitrage strategy is very challenging empirically, and thus we restrict our analysis to transaction costs alone. Nevertheless, including other types of cost would only reinforce our results since these would simply increase the total arbitrage costs in the context we address.
Thomson Financial Network CDA/Spectrum Institutional Holdings file, we also investigate the role of noise traders in the pricing of our bankrupt firms. We find that, in the four post-event quarters, institutional investors own, on average, only 12% of these firms’ stock, or, put differently, retail investors control the market for bankrupt firm stock. This is important since it indicates that noise traders are likely to be responsible for setting our sample firms’ stock prices.

The argument presented in the previous paragraphs suggests that the issue of limits to arbitrage is crucial when analyzing the pricing of bankrupt firm stock. In this context, arbitrage seems to be simply too risky and costly. As a result, the prices of bankrupt firms appear to drift for a long period without traditional market forces being able to correct this situation. However, this does not explain why the mispricing occurs in the first place. Behavioral finance also offers guidance on this issue. The most common explanation for the return pattern documented above is that the market underreacts to bad news because it requires time to impound its implications (e.g., Lakonishok and Vermaelen, 1990; Ikenberry and Lakonishok, 1993; Ikenberry et al, 1995; Spiess and Affleck-Graves, 1999; Hong et al, 2000; Dichev and Piotroski, 2001; Ikenberry and Ramnath, 2002). Along the same lines, Taffler et al (2004) argue that a possible explanation for this phenomenon is that the market is “in denial”, i.e., that investors chose to ignore negative public signals to preserve their self-esteem and ameliorate their anxiety. Within this framework, after bad news, market prices fall but not by as much as implied by the information event. Therefore, prices are “sticky” in that they converge slowly to their new fundamental value over time as the market begins to realize the full implications of the negative disclosure that initially prompted the price-adjustment process. The Barberis et al (1998) and Hong and Stein (1999) models provide a theoretical framework compatible with this particular price-adjustment process. In effect, it seems plausible for the market to take time to realize the full impact of a Chapter 11 announcement, and then eventually separate out which firms will recover from this catastrophic event from those that will not. In the meantime, the market price slowly converges to its new fundamental value leading to the post-bankruptcy downward drift documented in this paper. The fact that most of these firms are not followed by
analysts (e.g., Espahbodi et al, 2001; Clarke et al, 2006), that valuation is difficult (Gilson, 1995; Gilson et al, 2000), and that institutional investors are largely absent from this market seem to provide evidence consistent with this argument.

Daniel et al (1998) offer a different explanation for the post-bankruptcy announcement drift. According to these authors, market participants overreact to their own private information, and underreact to public news. The evidence presented in table 3, Panel C of downward drift in price following Chapter 11 filing is consistent with Daniel et al’s (1998) argument. Overconfidence and biased self-attribution may be two important cognitive biases affecting the investment decisions of the (largely unsophisticated) market participants trading the stock of bankrupt firms.

Disentangling these alternative explanations for the post-bankruptcy announcement drift is empirically challenging and we save this for further research. Nevertheless, our results already allow us to make two important contributions to the literature. First, we present original evidence on what the stock return pattern of bankrupt firms looks like after they file for Federal protection. In this respect, our study adds to existing knowledge, which until now has been restricted to the market’s reaction before and around the filing date and after emergence from Chapter 11. Second, more generally, we add to the strand of the literature suggesting that markets are unable to deal properly with acute bad news events thus violating the semi-strong form of the EMH. However, we suggest that since it is likely difficult for arbitrageurs to exploit this post-bankruptcy announcement drift anomaly in practice, markets are still minimally rational in the Rubinstein (2001) sense.
6. Conclusion

A number of academic papers explore how stock prices behave both before and around the bankruptcy filing date. Specifically, prices fall before the bankruptcy announcement, and the market’s immediate reaction to the event itself is strong and negative. We add to this body of literature by exploring what happens to stock prices after Chapter 11 filing. Our results are clear and powerful. We find that the market fails to react appropriately, and in an unbiased way, to this unambiguous bad news event. In particular, we find a strong, negative, and statistically significant post-bankruptcy announcement buy-and-hold abnormal return of -28% over the 12-month period following the filing date. This result is consistent with other papers that appear to find markets have difficulty in processing bad news. Robustness tests confirm that our main result is not a mere statistical artifact, and we also demonstrate it is distinct from other established stockmarket anomalies. However, our findings support the traditional understanding that investing in the stock of bankrupt firms may not be a very interesting strategy due to limits-to-arbitrage reasons. Profit earning opportunities from exploiting this newly identified market anomaly are more illusory than real.
References


Table 1

Defining the sample

This table summarizes the steps undertaken to identify this study’s sample. The first stage is combining seven different data sources to identify an initial set of non-overlapping firms that filed for bankruptcy in the US between 01.10.1979 and 17.10.2005. In order to be included in the final sample a given company must comply with the following criteria: 1) have enough data on CRSP and COMPUSTAT to conduct the analysis, 2) be listed and remain listed after the bankruptcy announcement date, trading common stock and 3) be a domestic company, filing for Chapter 11. Additionally, firms that are financial or utility companies are not considered in the final sample.

<table>
<thead>
<tr>
<th>Description</th>
<th>Nº</th>
</tr>
</thead>
<tbody>
<tr>
<td>Non-overlapping firm-year observations identified from the different data sources</td>
<td>3,437</td>
</tr>
<tr>
<td>Firm-year observations not found or with insufficient data on CRSP</td>
<td>1,411</td>
</tr>
<tr>
<td>Firm-year observations delisted before or at the bankruptcy filing month</td>
<td>1,556</td>
</tr>
<tr>
<td>Firm-year observations with insufficient data on COMPUSTAT</td>
<td>58</td>
</tr>
<tr>
<td>Firm-year observations classified as foreign</td>
<td>11</td>
</tr>
<tr>
<td>Utilities and financial firms</td>
<td>40</td>
</tr>
<tr>
<td>Firms filing Chapter 7</td>
<td>10</td>
</tr>
<tr>
<td>Final sample size</td>
<td>351</td>
</tr>
</tbody>
</table>

- 36 -
Table 2  
Summary statistics

This table presents summary statistics relating to our population of 351 non-finance, non-utility industry firms, fully listed on the NYSE, AMEX or NASDAQ filing for Chapter 11 between 01.10.1979 and 17.10.2005 and that remained listed on a major US stock exchange after their bankruptcy date. The table also presents summary statistics for a matched sample based on size and book-to-market. Specifically, for each sample company, we identify all CRPS firms with a market capitalization between 70 and 130 percent of its equity market value. The respective control firm is then selected as that firm with book-to-market closest to that of the sample firm. Panel A reports fundamental accounting information. Panel B summarizes market related variables. Panel C presents other relevant firm characteristics. The p-value column of panels A and B shows the significance of a two-tailed t-test (Wilcoxon-Mann-Whitney test) for difference in means (medians).

Panel A: Accounting variables

<table>
<thead>
<tr>
<th>Sample firms (A)</th>
<th>Matched firms (B)</th>
<th>Difference (A-B)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Median</td>
</tr>
<tr>
<td>Sales</td>
<td>596.4</td>
<td>116.9</td>
</tr>
<tr>
<td>TA</td>
<td>646.6</td>
<td>89.7</td>
</tr>
<tr>
<td>ROA</td>
<td>-19%</td>
<td>-6%</td>
</tr>
<tr>
<td>Z-Score</td>
<td>1.37</td>
<td>1.31</td>
</tr>
<tr>
<td>CUR</td>
<td>169%</td>
<td>128%</td>
</tr>
<tr>
<td>LEV</td>
<td>45%</td>
<td>40%</td>
</tr>
</tbody>
</table>

Sales: sales in million of dollars. TA: total assets in millions of dollars. ROA: return on assets (net income/total assets). Z-Score: bankruptcy-risk proxy (Altman, 1968). CUR: current ratio (current assets/current liabilities). LEV: leverage proxy (total debt/total assets). All variables are computed with data taken from the last annual accounts reported before the bankruptcy year.
### Table 2 (cont.): Summary statistics

#### Panel B: Market related variables

<table>
<thead>
<tr>
<th></th>
<th>Sample firms (A)</th>
<th>Matched firms (B)</th>
<th>Difference (A-B)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Median</td>
<td>Mean</td>
</tr>
<tr>
<td>Size</td>
<td>160.0</td>
<td>32.3</td>
<td>159.6</td>
</tr>
<tr>
<td>Book/Market</td>
<td>4.2</td>
<td>2.3</td>
<td>3.8</td>
</tr>
<tr>
<td>Pre price</td>
<td>4.97</td>
<td>3.12</td>
<td>9.80</td>
</tr>
<tr>
<td>Event Price</td>
<td>2.08</td>
<td>0.97</td>
<td>8.67</td>
</tr>
<tr>
<td>Pos Price</td>
<td>2.98</td>
<td>0.71</td>
<td>8.84</td>
</tr>
<tr>
<td>Pre Volume</td>
<td>0.51%</td>
<td>0.34%</td>
<td>0.44%</td>
</tr>
<tr>
<td>Event Volume</td>
<td>1.15%</td>
<td>0.61%</td>
<td>0.42%</td>
</tr>
<tr>
<td>Pos Volume</td>
<td>0.57%</td>
<td>0.30%</td>
<td>0.43%</td>
</tr>
<tr>
<td>Pre Tdays</td>
<td>250</td>
<td>252</td>
<td>227</td>
</tr>
<tr>
<td>Pos Tdays</td>
<td>230</td>
<td>246</td>
<td>224</td>
</tr>
</tbody>
</table>

**Size**: market capitalization (price times shares outstanding), in millions of dollars.

**Book/Market**: book-to-market ratio. **Pre Price**: daily average stock price measured for the 12-month period preceding the bankruptcy filing month (in dollars). **Event price**: same as Pre Price, but for the 30-calendar day period centred on the bankruptcy announcement date. **Pos Price**: same as Pre Price, but for the 12-month period after the bankruptcy announcement month. **Pre Volume**: average daily trading volume (volume/shares outstanding) measured for the 12-month period preceding the bankruptcy announcement month. **Event Volume**: same as Pre Volume but for the 30-calendar day period centred on the bankruptcy announcement date. **Pos Volume**: same as Pre Volume but for the 12-month period after the bankruptcy announcement month. **Pre Tdays**: number of days on which trading takes place in the calendar year preceding the bankruptcy announcement month. **Pos Tdays**: same as Pre Tdays but for the calendar year following the bankruptcy announcement month.
Table 2 (cont.): Summary statistics

Panel C: Other Characteristics

<table>
<thead>
<tr>
<th></th>
<th>Sample firms</th>
<th>Matched firms</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Positive cases</td>
<td>% of sample</td>
</tr>
<tr>
<td>EPS</td>
<td>88</td>
<td>25.1</td>
</tr>
<tr>
<td>Divid</td>
<td>91</td>
<td>25.9</td>
</tr>
<tr>
<td>Big8</td>
<td>287</td>
<td>81.8</td>
</tr>
<tr>
<td>Opinion</td>
<td>263</td>
<td>0.75</td>
</tr>
<tr>
<td>Delist</td>
<td>195</td>
<td>55.6</td>
</tr>
</tbody>
</table>

EPS: earnings per share dummy (1 if positive, 0 otherwise). Divid: dividend paid dummy (1 if dividend paid, 0 otherwise). Big8: auditor quality proxy dummy (1 if Big eight, 0 otherwise). Opinion: auditor opinion dummy (1 if clean – defined as per Kausar et al (2008), 0 otherwise). Delist: delist dummy (1 if company is delisted within one-calendar year of the bankruptcy date, 0 otherwise). All accounting variables (as well as Big8) are taken from the last annual accounts reported before the bankruptcy year.
Table 3

Market reaction to Chapter 11

This table presents buy-and-hold abnormal returns for our population of 351 non-finance, non-
utility firms listed on the NYSE, AMEX or NASDAQ that filed for Chapter 11 between 01.10.1979 and 17.10.2005 and that remained listed on a major US stock exchange after their bankruptcy date. All compounding periods are in trading days, where day zero is the Chapter 11 date. Raw abnormal returns are reported in the first two columns. Market adjusted (using CRSP equally weighted index as benchmark) are reported in the two mid columns. The two last columns report the results using a control firm approach where firms are matched according to size and book-to-market. Specifically, for each sample company, we identify all CRPS firms with a market capitalization between 70 and 130 percent of its equity market value. The respective control firm is then selected as that firm with book-to-market closest to that of the sample firm. The two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median).

Panel A: Pre-event returns

<table>
<thead>
<tr>
<th>Raw Returns</th>
<th>Market Adjusted Returns</th>
<th>Control Firm Benchmark</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Median</td>
</tr>
<tr>
<td>(-252,-2)</td>
<td>-0.63</td>
<td>-0.69</td>
</tr>
<tr>
<td></td>
<td>&lt;0.0001</td>
<td>&lt;0.0001</td>
</tr>
<tr>
<td>(-126,-2)</td>
<td>-0.50</td>
<td>-0.55</td>
</tr>
<tr>
<td></td>
<td>&lt;0.0001</td>
<td>&lt;0.0001</td>
</tr>
</tbody>
</table>

Panel B: Short-term market reaction

<table>
<thead>
<tr>
<th>Raw Returns</th>
<th>Market Adjusted Returns</th>
<th>Control Firm Benchmark</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Median</td>
</tr>
<tr>
<td>(-1,+1)</td>
<td>-0.26</td>
<td>-0.27</td>
</tr>
<tr>
<td></td>
<td>&lt;0.0001</td>
<td>&lt;0.0001</td>
</tr>
</tbody>
</table>
**Table 3 (cont.): Market reaction to Chapter 11**

Panel C: Medium-term market reaction

<table>
<thead>
<tr>
<th>Time Window</th>
<th>Raw Returns Mean</th>
<th>Raw Returns Median</th>
<th>Market Adjusted Returns Mean</th>
<th>Market Adjusted Returns Median</th>
<th>Control Firm Benchmark Mean</th>
<th>Control Firm Benchmark Median</th>
</tr>
</thead>
<tbody>
<tr>
<td>(+2,+84)</td>
<td>-0.13</td>
<td>-0.18</td>
<td>-0.14</td>
<td>-0.24</td>
<td>-0.13</td>
<td>-0.15</td>
</tr>
<tr>
<td></td>
<td>&lt;0.0001</td>
<td>0.0065</td>
<td>&lt;0.0001</td>
<td>&lt;0.0001</td>
<td>0.0014</td>
<td>&lt;0.0001</td>
</tr>
<tr>
<td>(+2,+126)</td>
<td>-0.15</td>
<td>-0.19</td>
<td>-0.20</td>
<td>-0.33</td>
<td>-0.16</td>
<td>-0.16</td>
</tr>
<tr>
<td></td>
<td>&lt;0.0001</td>
<td>0.0002</td>
<td>&lt;0.0001</td>
<td>&lt;0.0001</td>
<td>0.0005</td>
<td>0.0001</td>
</tr>
<tr>
<td>(+2,+252)</td>
<td>-0.14</td>
<td>-0.39</td>
<td>-0.48</td>
<td>-0.67</td>
<td>-0.28</td>
<td>-0.27</td>
</tr>
<tr>
<td></td>
<td>0.0063</td>
<td>&lt;0.0001</td>
<td>&lt;0.0001</td>
<td>&lt;0.0001</td>
<td>&lt;0.0001</td>
<td>&lt;0.0001</td>
</tr>
</tbody>
</table>
Panel A presents buy-and-hold abnormal returns for our population of 351 non-finance, non-utility firms listed on the NYSE, AMEX or NASDAQ that filed for Chapter 11 between 01.10.1979 and 17.10.2005 and that remained listed on a major US stock exchange after their bankruptcy date. All compounding periods are defined in trading days, where day zero is the Chapter 11 date. A control firm approach is used to estimate the abnormal returns. Firms are matched according to size and earnings surprise. Specifically, for each sample company, we identify all CRPS firms with a market capitalization between 70 and 130 percent of its equity market value. The respective control firm is then selected as that firm with earnings surprise value closest to that of the sample firm. The two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median).

Panel A: Controlling for size and earnings surprise - adjusted returns

<table>
<thead>
<tr>
<th>Control Firm Benchmark</th>
<th>Mean</th>
<th>Median</th>
</tr>
</thead>
<tbody>
<tr>
<td>(+2,+84)</td>
<td>-0.09</td>
<td>-0.11</td>
</tr>
<tr>
<td></td>
<td>0.0267</td>
<td>0.0011</td>
</tr>
<tr>
<td>(+2,+126)</td>
<td>-0.15</td>
<td>-0.16</td>
</tr>
<tr>
<td></td>
<td>0.0008</td>
<td>&lt;0.0001</td>
</tr>
<tr>
<td>(+2,+252)</td>
<td>-0.32</td>
<td>-0.33</td>
</tr>
<tr>
<td></td>
<td>&lt;0.0001</td>
<td>&lt;0.0001</td>
</tr>
</tbody>
</table>

Panel B presents buy-and-hold abnormal returns for our population of 351 non-finance, non-utility firms listed on the NYSE, AMEX or NASDAQ that filed for Chapter 11 between 01.10.1979 and 17.10.2005 and that remained listed on a major US stock exchange after their bankruptcy date, conditional on the sign of the quarterly earnings change. Firms with a negative pre-event earnings surprise are allocated to the negative earnings portfolio; all others are classified as the positive earnings surprise portfolio. All compounding periods are defined in trading days, where day zero is the Chapter 11 date. A control firm approach based on size and book-to-market is used to estimate the abnormal returns. Specifically, for each sample company, we identify all CRPS firms with a market capitalization between 70 and 130 percent of its equity market value. The control firm is that firm with book-to-market closest to that of the sample firm. For the Negative and Positive earnings columns, the two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median). In the two last columns, the two-tailed significance level from t-statistics or a Wilcoxon-Mann-Whitney test are reported below the corresponding mean or median difference.
Table 4 (cont.): Controlling for earnings surprise

Panel B: Controlling for earnings surprise – earnings surprise sign

<table>
<thead>
<tr>
<th></th>
<th>Negative Earnings (n=263)</th>
<th>Positive Earnings (n=88)</th>
<th>Difference (Neg - Pos)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Median</td>
<td>Mean</td>
</tr>
<tr>
<td>(+2,+84)</td>
<td>-0.15</td>
<td>-0.18</td>
<td>-0.06</td>
</tr>
<tr>
<td></td>
<td>0.0011</td>
<td>0.0001</td>
<td>0.0486</td>
</tr>
<tr>
<td>(+2,+126)</td>
<td>-0.18</td>
<td>-0.18</td>
<td>-0.10</td>
</tr>
<tr>
<td></td>
<td>0.0002</td>
<td>&lt;0.0001</td>
<td>0.0541</td>
</tr>
<tr>
<td>(+2,+252)</td>
<td>-0.32</td>
<td>-0.30</td>
<td>-0.18</td>
</tr>
<tr>
<td></td>
<td>&lt;0.0001</td>
<td>&lt;0.0001</td>
<td>0.0397</td>
</tr>
</tbody>
</table>
Table 5

Calendar-time approach

This table reports abnormal stock returns for calendar-time portfolios formed using a sample of 351 non-finance, non-utility firms listed on the NYSE, AMEX or NASDAQ that filed for Chapter 11 between 01.10.1979 and 17.10.2005 and that remained listed on a major US stock exchange after their bankruptcy date. Firms are added to the portfolio at the end of the month following the Chapter 11 announcement and are held for 6 or 12 months. Portfolio returns are computed assuming an equally weighted investment strategy. Months where the portfolio holds less than ten stocks are deleted. The abnormal returns are determined using the Carhart (1997) factor model. The parameters are estimated using both OLS and WLS. Monthly returns in the WLS model are weighted by the square root of the number of firms contained in the calendar-time portfolio in that month. The regression intercept provides an estimate of monthly abnormal performance. Heteroskedasticity robust t-statistics are reported.

<table>
<thead>
<tr>
<th></th>
<th>WLS 6 months</th>
<th>WLS 12 months</th>
<th>OLS 6 months</th>
<th>OLS 12 months</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept</td>
<td>-0.0473</td>
<td>-0.0264</td>
<td>-0.0531</td>
<td>-0.0269</td>
</tr>
<tr>
<td></td>
<td>-3.88***</td>
<td>-2.99**</td>
<td>-4.08***</td>
<td>-2.70*</td>
</tr>
<tr>
<td>(b)</td>
<td>1.1364</td>
<td>1.0008</td>
<td>0.9422</td>
<td>0.9716</td>
</tr>
<tr>
<td></td>
<td>3.75***</td>
<td>4.56***</td>
<td>3.53***</td>
<td>4.75***</td>
</tr>
<tr>
<td>(s)</td>
<td>3.5050</td>
<td>2.9909</td>
<td>2.4418</td>
<td>2.0126</td>
</tr>
<tr>
<td></td>
<td>8.34***</td>
<td>9.90***</td>
<td>3.84***</td>
<td>4.16***</td>
</tr>
<tr>
<td>(h)</td>
<td>2.2097</td>
<td>1.8031</td>
<td>0.9712</td>
<td>0.9200</td>
</tr>
<tr>
<td></td>
<td>4.44***</td>
<td>4.92***</td>
<td>1.5</td>
<td>1.87</td>
</tr>
<tr>
<td>(u)</td>
<td>-0.8609</td>
<td>-0.6232</td>
<td>-0.8979</td>
<td>-0.7035</td>
</tr>
<tr>
<td></td>
<td>-2.61*</td>
<td>-2.62*</td>
<td>-1.95</td>
<td>-1.71</td>
</tr>
<tr>
<td>(Adj R^2)</td>
<td>0.2631</td>
<td>0.3111</td>
<td>0.1672</td>
<td>0.2156</td>
</tr>
</tbody>
</table>

*, **, *** indicate significance at the 10%, 5%, 1%, and 0.1% levels respectively.