

Market reaction to bad news:
The case of bankruptcy filings

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To my parents, Isabel e José

Abstract

Finance scholars disagree on how real world financial markets work. On the one hand, efficient market hypothesis (EMH) advocates claim that arbitrage ensures that market prices do not systematically deviate from their fundamental value even when some market participants are less than fully rational. Hence, in the EMH world, securities' prices always reflect all available information. On the other hand, behavioural finance theorists argue that investors suffer important cognitive biases and that arbitrage is both risky and costly. In this alternative setting, prices may not reflect all available information and can systematically deviate from their fundamental value for long periods of time.

My thesis contributes to this ongoing debate by exploring how the US equity market reacts to bankruptcy announcements. Using a set of 351 non-financial, non-utility firms filing for Chapter 11 between 1979 and 2005 that remain listed on a main exchange, I first find a strong, negative and statistically significant mean post-bankruptcy announcement drift. This ranges from -24 to -44 percent over the following 12 months depending on the benchmark adopted to measure abnormal returns. A number of robustness tests confirm that this result is not a mere statistical artefact. In fact, the post-bankruptcy drift is not subsumed by known confounding factors like the post-earnings announcement drift, the post-first-time going concern drift, the momentum effect, the book-to-market effect, industry clustering or the level of financial distress. In addition, I show that my main result is robust to different methods for conducting longer-term event studies. My empirical findings are consistent with the previous behavioural finance literature that claims that the market is unable to deal appropriately with acute bad news events.

In the second part of this thesis, I investigate how limits to arbitrage impact the stock price of firms undergoing a Chapter 11 reorganization. I find that, despite the apparent large negative abnormal returns, the post-bankruptcy announcement drift offers only an illusory profit opportunity. Moreover, I show that noise trader risk is critical for the pricing of these firms' stock. Taken together, my results suggest that limits to arbitrage issues can explain the persistence of the market-pricing anomaly I uncover. As such, the market for firms in Chapter 11 appears to be "*minimally rational*" (Rubinstein, 2001). My work additionally explores whether behavioural finance theory can help clarify why the post-bankruptcy announcement drift occurs in the first place. I find that the Barberis, Shleifer and Vishny (1998) and the Hong and Stein (1999) models do not account well for the typical return pattern associated with the announcement of Chapter 11. My results call into question the reliability of existing theoretical models based on behavioural concepts in explaining how real world financial markets really work.

In the last part of this thesis, I show that the different motivations for filing for Chapter 11 Court protection affect the market's reaction to this extreme event. Solvent firms addressing the Bankruptcy Court not as a last resort but as a planned business strategy characterize a strategic bankruptcy; companies on the verge of imminent failure typify a non-strategic bankruptcy. I find that for non-strategic bankruptcies, there is a negative and statistically significant post-event drift lasting at least twelve months. Conversely, I show that, although the initial market reaction to bankruptcy filing is similar in the case of strategic bankruptcies in terms of viewing all bankruptcies as homogeneous, there is a subsequent reversal in the stock return pattern for these peculiar firms. In effect, abnormal returns become strongly positive and significant suggesting that, over time, the market to recognise strategic bankruptcies as good news events.

Overall, the results of my PhD allow me to make some important contributions to finance theory and the finance literature, in particular in the bad news disclosure and market pricing domains.

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Declaration

I certify that this thesis does not incorporate any material previously submitted for a degree or diploma in any University; and that to the best of my knowledge and belief it does not contain any material previously published or written by another person where due reference is not made in the text. I also certify that the thesis has been composed by myself and that all the work is my own.

Luis Miguel Serra Coelho, March 2008

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Chapter 1

Introduction

Academic researchers in finance are in the middle of a debate about the way people make decisions and how this should be modelled. In general, people make observations, process data and make judgements and decisions. Such actions have implications for the composition of individual portfolios, the range of securities offered in the market, the character of earnings forecasts, the way in which securities are priced and so forth. Assumptions have to be made about investors' decision-making processes when building models to study financial markets. It is precisely here where scholars tend to disagree. The efficient market hypothesis (EMH) advocates argue that decision-makers possess Von Neumann-Morgenstern preferences and use Bayesian techniques to make appropriate statistical judgements (Thaler, 1999). However, recent behavioural literature has produced evidence that we do not behave as rational optimizers (Ritter, 2003). Instead, people seem to use rules of thumb to reduce the amount of time and effort required by the decision-making process (e.g., Hirshleifer, 2001). Relying on such heuristics may result in sub-optimal decisions that incorporate irrelevant information and/or ignore other value-relevant data. Which of these competing views explains better the workings of real world financial markets? This is still a question open to discussion.

Studying the equity market's reaction to bankruptcy announcements provides a privileged opportunity for contributing to this ongoing debate. Today bankruptcy is a reality affecting even the biggest firms of the world's most developed economy. According to the 2001 Bankruptcy Yearbook and Almanac, in 2000, a record of 176 publicly traded companies filed for bankruptcy in the United States (US). The combined value of these bankruptcies' assets exceeded 94 billion dollars, representing a 61 percent increase over the previous year. Altman and Hotchkiss (2005, p. 3) update this figure documenting that, in the US and for 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, a leading institution in collecting and analysing bankruptcy-related information, reports that, from 1980 onwards, nine out of the ten major bankruptcies in the US 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 in the US exceeded 61 billion dollars.²

Not surprisingly, corporate bankruptcy has captured the attention of the general public in the last years (Altman and Hotchkiss, 2005, p. 3). This phenomenon, certainly motivated by the recent failure of massive companies like Enron and WorldCom, continues to grow as new problems keep hitting both the US and international financial markets. The most recent example is the subprime mortgage financial crisis that started in mid-2006 in the US. At the heart of this crisis, which has now grown to international proportions, is the rising interest rate that dramatically increased the monthly payments on the newly popular adjustable-rate mortgages. As a result, many homeowners became unable to meet their financial commitments and lenders without a means to recoup their losses. A severe credit crunch soon emerged, threatening the solvency of a number of well-established financial institutions. New Century Financial Corporation is perhaps the most renowned victim of this situation. As of January 1, 2007, the company had approximately 7,200 full-time employees, a market capitalization of 1.75 billion dollars and a fiscal-year net income of 417 million dollars in 2005. On March 14, 2007, the market value of its stock was less than 55 million dollars. Fifteen days later, New Century Financial Corporation and its related entities filed voluntary petitions for relief under Chapter 11 of the US Bankruptcy Code, throwing 3,200 people into the unemployment lines.³ Clearly, the traditional view that bankruptcy only concerns small, privately held companies is now outdated (Shrader and Hickman, 1993 and Altman, 1999, p. 3). In today's world, bankruptcy matters because it can affect virtually all existing companies.

¹ See http://www.bankruptcydata.com/Research/10_LargestBankruptcies.htm for details.

² See http://www.bankruptcydata.com/Research/Ch11_2007.htm for details.

³ Julie Creswell and Vikas Bajaj, writing for the *New York Times*, on April 3, 2007.

Interestingly, despite the number of academic studies exploring several dimensions relating to bankruptcy, we still lack a thorough understanding on how the equity market deals with this extreme bad news event, especially in the longer-run (Altman and Hotchkiss, 2005, p. 83; Dawkins, Bhattacharya and Bamber, 2007). This happens because the major US exchanges would typically delist the stock of firms filing for bankruptcy shortly after the event date, something that only changed after the mid-1980s (Dawkins, Bhattacharya and Bamber, 2007). In parallel, the evidence in favour of behavioural finance is mounting when one considers how the market reacts to bad news events, with a number of empirical papers demonstrating that the market has problems in assimilating adverse public disclosures on a timely and unbiased manner. Classical work here is that by Bernard and Thomas (1989, 1990), Michaely, Thaler and Womack (1995), Womack (1996), Dichev and Piotroski (2001), Chan (2003) and Taffler, Lu and Kausar (2004).

Investigating the market's reaction to corporate bankruptcy announcements offers a unique context within which to expand previous knowledge on this area. In effect, the early literature sometimes focuses on events that can only vaguely be defined as negative events. For instance, Michaely, Thaler and Womack (1995) suggest that dividend omissions are bad news but fail to justify such a claim. Shefrin and Statman (1984) provide some support to Michaely, Thaler and Womack's (1995) argument when stating that individuals like dividends because they follow the rule of "*spend the dividend, not the principal*". Yet, dividends are taxed at a higher rate than capital gains in the US and thus individuals with higher marginal rates may actually regard a dividend omission as good news. In this respect, bankruptcy is a superior event since it is clearly the most extreme case of bad news in the corporate domain and thus no ambiguity exists on the qualitative nature of the signal transmitted to the market. Additionally, and perhaps more importantly, bankrupt companies are likely candidates to be mispriced by the market due to the important issue of limits to arbitrage affecting this particular market. In effect, the typical bankrupt firm is small and highly financially distressed (e.g., Campbell, Hilscher and Szilayi, 2007; Kalay, Singhal and Tashjian, 2007). Information is scarce because analysts' coverage is usually very low (e.g., Espahbodi, Dugar and Tehranian, 2001;

Clarke et al, 2006). Institutional investors are likely to be absent from this market both for legal (Del Guercio, 1996) and idiosyncratic (e.g., Gompers and Metrick, 2001) reasons. Not surprisingly, fundamental valuation is difficult while bankruptcy is underway (Gilson, 1995; Gilson, Hotchkiss and Ruback, 2000). Finally, trading costs and short-sale constraints are probably binding in this context (e.g., D'Avolio, 2002).

Does the US equity market quickly and accurately react to bankruptcy announcements? If not, why does this happen? These are the key questions I address in this thesis. My main findings can be summarized around four ideas. Firstly, consistent with the predictions of behavioural finance, there is evidence that the market is unable to deal appropriately with corporate bankruptcy announcements. In particular, I find a strong, negative and statistically significant post-bankruptcy drift lasting at least one full year after the event date. Such drift ranges from -24 to -44 percent on average, depending on the benchmark adopted to measure the abnormal returns. Importantly, a number of robustness tests show that this result is not a mere statistical artefact. In fact, the post-bankruptcy drift does not disappear after controlling for known confounding problems like the post-earnings announcement drift, the post-first-time going-concern drift, the momentum effect, the book-to-market effect, industry clustering or the level of financial distress. Crucially, my main result does not change even after considering several alternative methodologies for conducting longer-term event studies.

Secondly, I find that implementation costs prevent sophisticated investors from acting because they render arbitrage unprofitable. In addition, there is evidence that noise trader risk plays a fundamental role in the pricing of bankrupt firms' stock. In effect, in the typical case, individual investors own an average of 90 percent of the firm's equity while bankruptcy is underway. Taken together, these results suggest that the existence of limits to arbitrage explains why the post-bankruptcy stock price drift is not corrected by traditional market forces even in the longer-term. As such, it seems that the market for bankrupt firms is "*minimally rational*" (Rubinstein, 2001).

Thirdly, I find that the behavioural models of the Barberis, Shleifer and Vishny (1998) and Hong and Stein (1999) do not capture the return pattern associated with the announcement of Chapter 11 bankruptcy. This result is in line with Fama's (1998, p. 285) argument that behavioural-driven models perform very poorly outside the setting for which they were initially developed for. It also calls into question the validity of existing theoretical models built around behavioural concepts in explaining the workings of real world financial markets.

In the last part of this thesis, I investigate the market's reaction to apparently similar bad news events with completely distinct underlying motivations. Solvent firms addressing the Bankruptcy Court not as a last resort but as a planned business strategy characterize the first type of bankruptcy; in contrast, companies on the verge of imminent financial collapse typify a non-strategic bankruptcy. I find that firms filing both strategic and non-strategic Chapter 11s experience very similar negative risk-adjusted returns of over 50 percent during the 12-month pre-event period, and fall a further 25 percent around the bankruptcy announcement date. These results suggest that the market is unable to differentiate between these two qualitatively different bad news events prior to and around the bankruptcy event date. However, in contrast, I document an asymmetric longer-term market reaction to bankruptcy announcements conditional on type of event. In the case of non-strategic bankruptcies, I find a negative and statistically significant post-event drift of -29 percent lasting for at least one full year after the Chapter 11 announcement date. Conversely, in the case of strategic bankruptcies, there is a reversal in the subsequent stock return pattern. In effect, for these firms, I find statistically significant risk-adjusted abnormal returns of +29 percent over the 6-month period following the Chapter 11 announcement date. My results thus suggest that, over time, the context surrounding the disclosure of firm-specific bad news affects the way the market perceives such events.

The rest of this thesis is organized as follows. Chapter 2 discusses the implications of the EMH and behavioural finance for empirical research on corporate bankruptcy. Chapter 3 draws on the conclusions of chapter 2 and tests the US equity market's reaction to bankruptcy

announcements. In chapter 4, I conduct a number of robustness tests to check the soundness of chapter 3's findings. Chapter 5 investigates the role of limits to arbitrage in the pricing of bankrupt firms' stock. In Chapter 6, I examine if behavioural finance theory explains the medium-term post-bankruptcy performance detailed in the previous chapters. Chapter 7 extends my study by exploring to what extent the market's reaction to bankruptcy is conditional on the underlying motivation for the filing. Chapter 8 summarizes the main conclusions of this study, discusses its limitations and suggests possible lines of research for further work.

Chapter 2

Literature Review

2.0 Introduction

One of the dominant themes in the finance academic literature since the 1960s has been the concept of an efficient capital market. Fama (1970) presents the basic form of the EMH, claiming that *"a market in which prices always "fully reflect" all available information is called "efficient"*. This hypothesis is based on the assumption that investors *"have access both to the correct specification of the "true" economic model and to unbiased estimators of its coefficients"* (Friedman, 1979, p. 38). After the publication of Fama's (1970) seminal paper, a number of studies provided empirical and theoretical support for the EMH, which led Jensen in 1978 to write, *"I believe there is no proposition in economics which has more solid empirical evidence supporting it than the Efficient Market Hypothesis"*.

For the last forty years, the EMH has been the cornerstone of modern finance (Shleifer, 2000, p. 1). Recently, a new generation of financial economists has developed a different theoretical approach to explain how financial markets work (Thaler, 1999). This new approach, behavioural finance, claims that under certain conditions, market prices may systematically deviate from fundamental values for long periods (e.g., Barberis and Thaler, 2005, p. 6). Two main reasons justify this result. First, cognitive biases affect the ability of the average investor in processing accurately all available information (e.g., Hirshleifer, 2001). Second, limits to arbitrage impede rational investors in forcing prices back to fundamentals (e.g., Shleifer and Vishny, 1997).

Studying the market's reaction to bankruptcy announcements provides a particularly interesting arena in which to test out EMH and behavioural finance predictions. In fact, we do not have a thorough understanding on how the equity market deals with this extreme bad news event, especially in the longer-run (Altman and Hotchkiss, 2005, p. 83; Dawkins, Bhattacharya, and Bamber, 2007). Anecdotal evidence, however, suggests that the market of bankrupt firms' stock is highly inefficient. For instance, mainstream American newspapers usually conjecture that

stock prices *may not* reflect their fundamental value while bankruptcy is underway, employing terms such as "*foolish*", "*naïve*" and "*irrational*" to describe investors trading on this type of security (Russel, Branch and Torbey, 1999). The Securities and Exchange Commission (SEC), which is the US agency having primary responsibility for enforcing the Federal security laws and regulating the stock market, also has extensive information available on its website cautioning investors to act prudently when dealing with the stock of bankrupt firms.⁴ Academic literature, nonetheless, largely suggests that distressed securities are efficiently priced (e.g., Warner, 1977a; Friedson and Cherry, 1990; Blume, Keim and Patel, 1991; Cornell and Green, 1991; Buell, 1992; Eberhart and Sweeney, 1992; Altman and Eberhart, 1994).

Does the US equity market quickly and accurately react to bankruptcy announcements? If not, why does this happen? These are the main issues I address in this thesis. I begin by reviewing key literature on the EMH and behavioural finance, which helps putting these questions into context. A brief overview of the main empirical results already available is also provided. I present background information about the bankruptcy procedures in the US and related literature in the last part of this chapter.

2.1 Efficient market hypothesis: theoretical foundations

Before the 1950s, the practitioner community in the US claimed that simple investment analysis could be used to outperform the market (Fama, 1965). However, in the early 1950s and 1960s, a set of finance papers challenged this idea by showing that changes in security prices follow a random pattern (e.g., Kendall, 1953; Cowles, 1960; Alexander, 1961; Mandelbrot, 1963; Fama, 1965; Samuelson, 1965; Fama and Blume, 1966). These studies provided the initial intellectual capital for the development of the EMH. Under this hypothesis, market prices are "right", in that agents who understand Bayes' Law and have sensible preferences set them (Thaler, 1999).⁵ The basic case of the EMH rests on three arguments that rely on progressively weaker

⁴ See <http://www.sec.gov/investor/pubs/bankrupt.htm> for details.

⁵ Reverend Thomas Bayes (1702-1761) developed Bayes' Theorem, originally published after his death in 1763 and published again in 1958. Bayes' Theorem provides a way of revising conditional probabilities by using available information. It also offers a procedure for determining how probability statements should be adjusted given additional information. See Wonnacott and Wonnacott (1990) and Newbold, Carlson and Thorne (2003) for details.

assumptions about the degree of market participants' rationality. These assumptions and their implications are discussed in more detail below.

2.1.1 Rationality and the efficient market hypothesis

In its most radical form, the EMH posits that all market participants are fully rational (Thaler, 1999). Rationality means two things. First, when new information becomes available, market participants update their beliefs correctly, particularly in the manner described by Bayes Law (Barberis and Thaler, 2005, p. 1). Second, they make choices that are normatively accepted, in the sense that they are consistent with Savage's notion of Subjective Expected Utility (Barberis and Thaler, 2005, p. 1).⁶ Accordingly, rational investors are always able to value each security at its fundamental value⁷ and to respond quickly and correctly to new value-relevant information (Fama, 1970). If all investors behave as the rational optimizer described here markets will be, by definition, informationally efficient.⁸

2.1.2 Noise traders and the efficient market hypothesis

Kyle (1985) and Black (1986) are among the first to argue that at least some market participants trade on noise, not on information. Noise traders or irrational investors are simply those who act on a signal that ultimately proves to be value-irrelevant (Lee, 2001). The existence of such market participants disputes the EMH key assumption presented in the previous paragraph. Friedman (1953), however, argues that noise traders are irrelevant in the

⁶ The theory of Subjective Expected Utility (SEU) is the central element of the neoclassical theory of rational economic behaviour. SEU basic assumptions are that choices are made among a fixed and given set of alternatives, with a subjectively known probability distribution of outcomes for each possible alternative and in such a way as to maximize the expected value of a given utility function. See Savage (1972), Mas-Colell, Whinston and Green (1995) and Jehle and Reny (2001) for details.

⁷ A security's fundamental value is given by the net present value of its future cash-flows, discounted at the appropriate risk rate (e.g., Pliska, 1997, p. 2). In the particular case of stocks, it is usually argued that the fundamental value represents the present value of expected future dividends (Lee, 2001). Mathematically:

$$V_t \equiv \sum_{i=1}^{\infty} \frac{E_t(D_{t+i})}{(1+r)^i}$$

where V_t is the stock's fundamental value at time t , $E_t(D_{t+i})$ is the expected future dividend for period $t+i$ based on information available at time t and r is the appropriate risk-adjusted discount rate for the expected dividend stream.

⁸ A precondition for this form of the EMH to hold is that the cost of obtaining and trading on value-relevant information is zero (Grossman and Stiglitz, 1980). A weaker and perhaps more sensible version of the EMH says that prices should reflect value-relevant information to the point where the marginal benefit of acting on that information equals its marginal cost (Jensen, 1978).

price formation process because rational investors always stand ready to exploit their pricing errors. This results in a systematic adjustment process that ultimately drives noise traders out of the market and prices back to fundamentals. Moreover, the EMH proponents also suggest that noise traders follow non-correlated investment strategies (e.g., Bagehot, 1972). In this case, no price bias should exist because, in equilibrium, opposite positions cancel out (Shleifer and Summers, 1990). These two arguments explain why the EMH should hold even when some market participants are not fully rational.

2.1.3 Smart money and the efficient market hypothesis

Recent research shows that the above-mentioned arguments against the importance of noise traders in the price formation process are flawed. For instance, De Long et al (1990b), Shleifer and Summers (1990), De Long et al (1991) and Shleifer and Vishny (1997) demonstrate that noise traders do matter as long as rational investors are risk-averse and/or have limited investment horizons. Furthermore, extant psychological evidence shows that people do not deviate randomly from rationality but rather that most deviate in the same way (e.g., Hirshleifer, 2001). Consequently, noise traders are expected to follow correlated investment strategies since they tend to form their demand for securities based on common beliefs, fads or sentiment (e.g., Shiller, 1984). Recent empirical research by Jackson (2003), Barber, Odean and Zhu (2006a, 2006b), Hvidkjaer (2006a, 2006b) and Barber and Odean (forthcoming) provides evidence in favour of this proposition.

Interestingly, EMH advocates argue that these findings are not important. Arbitrageurs' actions and the arbitrage mechanism facilitate this outcome (e.g., Lee, 2001). Arbitrageurs are usually assumed to be professional, highly specialized investors who combine their knowledge with resources from outside market participants. They thoroughly collect and analyse relevant information in order to check for any possible market *mispicing*, i.e., a misalignment between a security's market price and its fundamental value (Shleifer and Summers, 1990 and Shleifer and Vishny, 1997). As Garman and Ohlson (1981) explain, due to the existence of implementation costs, market prices are likely to wander around their fundamental value and within an

arbitrage band. As soon as the price drifts outside this band, an arbitrage opportunity exists and it becomes profitable for the arbitrageur to act.⁹ For instance, if the market price falls below the lower limit of the arbitrage band, it becomes advantageous for the arbitrageur to buy large quantities of the security because it is now undervalued by the aggregate market. However, he will only profit from this strategy when the market corrects itself. This happens when the market price increases to a point where it starts to wander inside the arbitrage band once again.¹⁰ By systematically exploiting the arbitrage opportunity, the arbitrageur aligns the security price once again with its fundamental value.

It is worth noticing three important characteristics of this process. Firstly, the arbitrageur does not have to invest any money to earn a sure profit. In fact, in the classical setting, he always hedges his position with an asset that is similar in terms of risk to that being mispriced by the market. This is crucial since it makes the arbitrage *riskless* for the arbitrageur. Secondly, by buying and selling large quantities of these similar assets, the arbitrageur modifies their market demand. This process affects the assets' market price and only terminates when both are priced at their fundamental value. Finally, according to the EMH advocates, competition among arbitrageurs for superior returns ensures that this adjustment mechanism works almost instantaneously (Shleifer, 2000, p. 4). Hence, it takes just one arbitrageur to justify why the EMH should hold even when noise traders exist and use correlated investment strategies to operate in financial markets.

2.2 Behavioural finance: an alternative approach

As Barberis and Thaler (2005, p. 1) indicate, the classical paradigm is appealingly simple and would be very satisfying if its predictions were confirmed in practice. However, recent research shows that basic facts about the behaviour of the aggregate stock market, the cross-section of average returns and individual trading activities cannot be fully understood within the EMH. Behavioural finance offers a new vision about how financial markets work. According to Thaler

⁹ Strictly speaking, there is an arbitrage opportunity when an investor has a positive probability of achieving a positive return with no risk of loss (e.g., Ingersoll, 1987, pp. 52-53; Pliska, 1997, p. 5).

¹⁰ The converse strategy could be applied when the market price rises above the upper limit of the arbitrage band.

(1993, p. 17), this alternative is simply "*open minded finance*". It is based on two pillars: the existence of limits to arbitrage and investor psychology (Shleifer and Summers, 1990). The following paragraphs detail the main theoretical features of behavioural finance.

2.2.1 Limits to arbitrage

The EMH posits that, if mispricing occurs, rational investors will immediately take a position to exploit it for a profit using a riskless and costless arbitrage strategy. Behavioural finance argues that this is *not true* since such strategies are, in fact, very risky and costly (e.g., Shleifer and Vishny, 1997; Lee, 2001). Three factors explain this result:

i. Fundamental risk: the basic idea of an arbitrage strategy is that the arbitrageur is able to hedge his position. To do this, he needs to find a security that has the same or essentially the same payoff profile in every state of the world as the one being mispriced by the market. However, substitute securities are usually highly imperfect (if existing at all), making it impossible to remove all the fundamental risk (Shleifer, 2000, p. 14; Barberis and Thaler, 2005, p. 5). Accordingly, this risk will always be a significant deterrent to arbitrage.

ii. Noise trader risk: noise traders' actions decisively affect the risk borne by arbitrageurs when engaging in an arbitrage strategy (e.g., De Long et al, 1990b; Shleifer and Summers, 1990; De Long et al, 1991 and Shleifer and Vishny, 1997). To see why, assume that pessimistic noise traders push the market price of a security below its fundamental value. Recognizing this pricing anomaly, an arbitrageur may intervene by buying the security and shorting a substitute one. According to the EMH advocates, the arbitrageur's position is hedged since the short sale removes the fundamental risk. Even if we assume this is true, the fact is that the arbitrageur still has to cope with the possibility that noise traders remain pessimistic for a long period or even that they trade in a way that lowers even further the security's market price.¹¹ Hence, the arbitrageur has a potential gain if the price converges to its fundamental value. Yet, in the short-run, he faces an effective financial loss. As De Long et

¹¹ This assumes that psychological biases and sentiment cause noise traders to trade systematically. Otherwise, their trades would cancel out in equilibrium.

al (1990b) and Shleifer and Summers (1990) explain, this problem is particularly important when arbitrageurs are risk averse and/or have finite investment horizons. Shleifer and Vishny (1997) point out that a sufficient condition for this result to hold is that *"brains and resources are separated by an agency relationship"*. Put simply, as long as arbitrageurs do not own the resources they require for conducting their activities, noise trader risk will limit their ability to correct market-pricing anomalies.

iii. Implementation costs: an arbitrageur willing to exploit a mispricing must deal with the cost of implementing his trading strategy (e.g., Jensen, 1978). In fact, it is widely accepted that even the most experienced and influential arbitrageur has to pay transaction costs like the bid-ask spread and commissions that reduce the profitability of his trades (e.g., Pontiff, 1996; Lesmond, Schill and Zhou, 2004). Other costs should also be considered here. For instance, Pontiff (2006) emphasizes that holding costs, i.e., those that are incurred every period a given position remains open, also play a fundamental role in the way arbitrageurs act on the market. Merton (1987) argues that the cost of finding a mispricing as well as the cost of the resources needed to exploit it may further reduce the interest of the arbitrageur in correcting a potential market-pricing anomaly. Moreover, Nagel (2005) points out that short-selling constraints are crucial in explaining the inability of arbitrageurs to correct prices when the market overprices a given asset. The fees charged for borrowing securities are the simplest example of such constraints (D'Avolio, 2002). Additionally, in several occasions, arbitrageurs simply cannot find securities to borrow at any price (Shleifer, 2000, p. 13). To sum up, implementation costs matter because they may render arbitrage unprofitable (Barberis and Thaler, 2005, p. 6).

The above paragraphs shows that, in contrast with the arbitrage mechanism suggested by the EMH proponents, real world arbitrage entails both costs and risk, which can significantly limit the ability of arbitrageurs to exploit an eventual market mispricing (e.g., Shleifer, 2000, p. 15; Barberis and Thaler, 2005, p. 6). As such, persistent price deviations from their fundamental value can continue to exist for long periods without traditional market forces being able to correct such situation.

2.2.2 Investor psychology

One of the most important tenets of behavioural finance is that the average investor is not fully rational (Barberis and Thaler, 2005, p. 12). However, behavioural theorists do more than simply stating that investors deviate from the maxims of economic rationality (e.g., Ritter, 2003). Drawing on extensive evidence compiled by cognitive psychologists, behavioural finance uses concepts such as overconfidence, representativeness, conservatism, anchoring, framing, mental accounting, pride and regret and availability biases to explain how investors are likely to form their beliefs and thus why there is no evident reason for expecting them to act rationally.¹² In practice, this approach simply recognizes that the average investor is unable to collect all the relevant information and process it rigorously as required by classical finance. Instead, behavioural finance suggests that investors often use rules of thumb when dealing with the deluge of information available to them (Hirshleifer, 2001).

Overall, the literature on cognitive psychology provides a promising framework for analysing investors' behaviour in the stock market. The breakthrough here is that the stringent assumption of rationality used in conventional finance can finally be relaxed (Ritter, 2003). This leads to a new context in which several anomalous findings can be better understood and some of the most interesting puzzles in finance be finally explained (Olsen, 1998).

2.3 Market efficiency and anomalies

Anomalies are empirical results that seem to be inconsistent with maintained theories of asset-pricing behaviour (Schwert, 2003) and are the most important challenge to the traditional view that securities are rationally priced (Hawawini and Keim, 1995). In the following paragraphs, I outline some of the most salient findings of this body of literature. It is not my intention to review all existing contributions; this would be outside the scope of this thesis. Additionally,

¹² This is not an all-inclusive list of the cognitive biases already documented by psychologists. In fact, the literature on this issue is voluminous. Hirshleifer (2001) offers a good summary of some of the psychological effects that are potentially interesting for securities markets. Barberis and Thaler (2005) also survey a number of psychological biases that are relevant for finance applications. Nofsinger (2005) offers a basic introduction to this theme.

several recent papers already provide a thorough overview of the theme. For instance, Fama (1998), Rubinstein (2001), Malkiel (2003) and Schwert (2003) look at the evidence from the perspective of classical finance theory. Conversely, the survey papers of Shiller (2003), Barberis and Thaler (2005) and Baker, Ruback and Wurgler (2007) and the books by Shleifer (2000) and Shefrin (2002) do the same but from a behavioural point of view. Dimson and Mussavian (2000) offer a more impartial summary of this area of the literature.

2.3.1 Test for return predictability

In his original paper, Fama (1970) defines the weak-form efficient market hypothesis as the impossibility of earning superior returns based on the knowledge of past returns and prices. In the 1991 sequel, Fama changes this definition and presents the alternative *test for return predictability*. This definition includes the original weak-form EMH and the more general area of forecasting returns with variables like dividend yields and interest rates. I start by discussing two flagship anomalies that fall into this category, the overreaction hypothesis of De Bondt and Thaler (1985) and the momentum effect of Jegadeesh and Titman (1993). The first paper provides evidence of long-term reversals that are predictable using only past return information. De Bondt and Thaler (1985) start by dividing firms into two groups, loser and winner portfolios, based on their previous 3-year stock return. They find that losers outperform winners in the 5-year post-formation period. De Bondt and Thaler (1985) attribute their results to investors' overreaction to past information. In 1987, the same authors revisit this issue accounting for the possibility that their earlier results could be driven by overreaction to earnings news. De Bondt and Thaler (1987) report that after losers (winners) experience earnings declines (increases) during the portfolio formation period, earnings move in the opposite direction in the subsequent test period. This evidence is consistent with a failure of stock prices to reflect the fact that annual earnings do not strictly follow a random walk, but show some mean reversion in the distribution tails (Brooks and Buckmaster, 1976).

The second anomaly is the momentum effect. In their seminal paper, Jegadeesh and Titman (1993) show evidence of short-term trends in returns. In particular, they find that strategies

that buy stocks with high returns over the 3- to 12-months and sell stocks with poor returns over the same period earn profits of about one percent per month over the following year. They attribute their results to delayed price reaction to firm specific information. Jegadeesh and Titman (2001) review the early evidence and find additional support in favour of a momentum effect. The main result presented in these papers has also been replicated outside the US. Most notably among this line of research is the work of Rouwenhorst (1998), who finds short-term momentum in returns in twelve European countries.

A number of papers document that returns are predictable using only firm-specific information. Banz (1981) is one of the first contributions in this domain. He shows that small (large) firms' average return is too high (low) given these companies' market betas, the well-known size effect. Fama and French (1992) update Banz's (1981) findings. They classify stocks into deciles based on market capitalization and measure the average return of the firms in each decile over the first year after formation. Consistent with previous evidence, the authors report that smaller firms outperform larger ones by 0.74 percent on average per month over the period 1963 to 1990. Fundamentals scaled by price are another type of publicly available information that seems to predict future returns. An example of this situation is the book-to-market ratio. Both US and international studies show that value stocks¹³ generate higher returns than growth stocks.¹⁴ For instance, Fama and French (1992, 1996) classify stocks into deciles based on their book-to-market ratio and calculate the average return for the firms in each decile in the year after formation. They find that the average monthly return of value stocks is 1.53 percent higher than the average monthly return of growth stocks. This difference in average returns is usually termed as the value premium and cannot be explained by the market beta. In a complementary paper, Lakonishok, Shleifer and Vishny (1994) divide stocks into nine portfolios based on their past 5-years sales growth and cash flow-to-price ratio. The authors report that the value portfolio has an average annual return of 10.7 percent and outperforms its growth counterpart. Contrary to Fama and French (1992, 1996), Lakonishok, Shleifer and Vishny

¹³ Companies with a high book-to-market ratio.

¹⁴ Companies with a low book-to-market ratio.

(1994) posit that value strategies yield higher returns since they exploit the sub-optimal behaviour of the typical investor and not because they are fundamentally riskier.

2.3.2 Event studies

In 1991, Fama uses the expression "*event studies*" to designate what in 1970 he terms the semi-strong form of the EMH. This version of the EMH states that investors cannot earn superior risk-adjusted returns by using publicly available information. Event studies provide one of the best ways to test this postulation since they are designed to capture the speed and accuracy with which market prices adjust to new, widely accessible information (Fama, 1991). In particular, event studies documenting nonzero abnormal security returns that persist after a particular event are inconsistent with the semi-strong form of market efficiency (Brown and Warner, 1980). Kothari and Warner (2007) point out that there are more than 500 published event studies and that this literature continues to grow. Covering all of them here is thus infeasible. As a result, below I review only some of the event studies that more closely relate to my own research.

The market's reaction to initial public offerings (IPOs) is one of the most thoroughly explored themes in this area. Ritter (1991), who is the first to address this particular problem, looks at 1,526 IPOs occurring from 1975 to 1984 in the US and finds that these issues *underperform* relative to a group of matched firms listed on the American Stock Exchange (AMEX) and on the New York Stock Exchange (NYSE). He interprets his result as evidence that investors become too optimistic about IPO firms, inflating the initial IPO return (from the IPO price to the secondary market trading price) and lowering subsequent returns. Additional research tends to confirm Ritter's (1991) finding (e.g., Loughran and Ritter, 1995 and 2000; Brav and Gompers, 1997; Brav, Geczy and Gompers, 2000). In fact, Ritter and Welch (2002) review the empirical evidence and conclude that most studies provide evidence in favour of an incomplete market reaction to IPOs. Ritter and Welch (2002) also discuss potential reasons that can account for this anomaly and posit that behavioural finance offers the most promising way of explaining the IPO underperformance phenomenon.

Research on the market's reaction to seasoned equity offerings (SEOs) also provides insight into how the market deals with corporate offerings. One of the first references in this area is that by Spiess and Affleck-Graves (1995). The authors study long-run stock returns following SEOs to determine whether managers' ability to exploit overvaluation opportunities is a broad market fact or a phenomenon specific to the IPO market. They report that the median return in the 5-year period following the SEO is 10.0 percent, which is comparable to a median 5-year holding return of 42.3 percent for similar size, non-issuing firms in corresponding industries. Spiess and Affleck-Graves (1995) conclude that managers are able to determine when the market is overpaying for their firm's stock and take advantage of this situation by issuing new equity. Crucially, this argument implies that the market is, in the short-run, unable to understand the managers' move. In effect, according to Spiess and Affleck-Graves (1995), the long-term underperformance of SEOs is explained by the fact that the market requires time to fully understand managements' signal when issuing new equity.

In another study, Spiess and Affleck-Graves (1999) document significant long-run stock price underperformance following both straight and convertible debt offerings. However, their results are limited to smaller, younger and North American Securities Dealers Automated Quotation System (NASDAQ) listed firms and among issues that are not investment grade. In addition, the underperformance is also limited to offerings that occur in high volume periods. Despite these limitations, Spiess and Affleck-Graves (1999) conclude their evidence is consistent with the interpretation that debt offerings signal that the firm is overvalued. The authors add that their results suggest the market underreacts to negative information conveyed at the time of the issue announcement.

In a similar vein, Ikenberry, Lakonishok and Vermaelen (1995) posit that the market treats share repurchases with scepticism, leading prices to adjust slowly over time. The authors examine the long-run performance of stock returns following open market repurchases announcements over the 1980-1990 period. As predicted, Ikenberry, Lakonishok and Vermaelen

(1995) find that, on average, the market does underreact to the event under scrutiny. Lasfer (2000) finds similar evidence when using a sample of UK companies and Ikenberry, Lakonishok and Vermaelen (2000) report essentially the same results for the Canadian stock market. This suggests that underreaction to share repurchases announcements is a robust phenomenon.

Research in the area of mergers and acquisitions also presents evidence consistent with long-term underreaction. Agrawal and Jaffe (2000) review the literature on this theme and conclude that long-run performance is negative following mergers but non-negative after tender offers. In effect, in the case of mergers, most of the studies they review report statistically significant results, with the opposite occurring for papers analysing tender offers. Agrawal and Jaffe (2000) also summarize a few contributions providing insight into why the market underperforms after mergers announcements. According to the authors, explanations based on the method of paying and performance extrapolation fit well the extant empirical evidence in this area.

A number of studies also examine how the market deals with stock splits. This event is particularly interesting because it is innocuous, i.e., it does not directly affect the splitting firm's future cash-flows. Nevertheless, at least two recent papers find that stock splits generate significant abnormal returns in the long-run (Ikenberry, Rankine and Stice, 1996 and Ikenberry and Ramnath, 2002). These studies also document that the phenomenon is more pronounced for smaller firms, with low book-to-market and companies splitting to low share prices. The authors claim that stock splits generate a market reaction that is consistent with the notion of slow incorporation of new information into stock prices. Cooper, Dimitrov and Rau (2001) also present evidence that the market reacts to events that do not directly affect the risk-return characteristics of the event firm. In particular, they show that, during the internet hype, companies changing their name to a dotcom-related designation enjoyed positive announcement returns of around 74 percent. Importantly, Cooper, Dimitrov and Rau (2001) show this increase in stock price occurs even for firms that have no-internet related business, which suggest that the market does, in fact, react to non-fundamental information.

I now turn to studies that explore the market's reaction to bad news events. I start with the seminal paper of Ball and Brown (1968). The authors document that, subsequent to the announcement of earnings, abnormal returns continue to drift down (up) for companies announcing a surprisingly negative (positive) variation in earnings. This phenomenon seems to continue from an initial reaction on day one through to day 180 and, in some cases, even longer than that. The post-earnings announcement drift suggests that the market takes time to fully incorporate the information of the earnings disclosure into the stock price of announcing companies. Consequently, the result presented in Ball and Brown (1968) constitutes an important violation of the semi-strong form of the EMH. Not surprisingly, this study deserved close scrutiny by many scholars over the years. In particular, shortly after its publication, a number of authors reviewed Ball and Brown's (1968) contribution only to find essentially the same result (e.g., Jones and Litzenberger, 1970; Foster, Olsen and Shevlin, 1984). These early studies, however, suffered from a range of limitations, which impair the soundness of their conclusions (e.g., Bernard and Thomas, 1989). Bernard and Thomas (1989, 1990), Freeman and Tse (1989) and Mendenhall (1991) are subsequent contributions to this area, all of which address more appropriately the shortcomings of the early literature. In general, these papers confirm Ball and Brown's (1968) original findings, suggesting that investors do react with delay to the information contained in earnings announcements.

Michael, Thaler and Womack (1995) examine how the stock market deals with changes in firms' dividend policy. As the authors explain, companies that omit a dividend disclose a negative signal to the market while companies initiating a dividend payment signal the converse. Results show that omitting (initiating) firms continue to generate negative (positive) excess returns from the event day until the end of the third year. Importantly, Michael, Thaler and Womack's (1995) findings are more robust for the omission sample than for its initiation counterpart. In fact, for the former, all tests are statistically significant regardless of the adopted benchmark and particular compounding period. The drift for the initiation sample, however, is only significant for particular benchmarks and time intervals. Michael, Thaler and

Womack (1995) conclude that their study provides evidence in favour of an incomplete market reaction to corporate announcements that signal value-relevant information to the market, a phenomenon that is especially strong in the case of bad news.

Dichev and Piotroski (2001) examine the long-run stock return pattern arising after a bond-rating change. They find a statistically significant underperformance following downgrades but no reliable excess returns are documented in the case of upgrades. Most of the underperformance of downgrades occurs in the first year after the announcement, with abnormal returns of around 10 percent a year being reported for this period. There is also some evidence, albeit weaker, that the underperformance following downgrades continues for at least years two and three after the event date. Dichev and Piotroski (2001) also test the robustness of their findings conditional on firm size, credit quality and preceding earnings surprise. Their results are quite stable. Nevertheless, Dichev and Piotroski's (2001) findings are clearer when considering smaller firms with non-investment grade debt. This suggests that the anomaly documented in the paper is more severe for the most extreme cases, i.e., those where the firm has lower credit quality and/or higher financial distress risk. In the last part of the paper, Dichev and Piotroski (2001) discuss three explanations for their findings: 1) systematic risk, 2) problems of asymmetry of information and 3) a behavioural-driven explanation. After carefully analysing their results, the authors favour the conclusion that behavioural biases are likely to account well for the empirical evidence they uncover.

In a recent contribution to this area of the literature, Chan (2003) examines the market's reaction following public news. The author finds that stocks experiencing negative returns concurrent with the incidence of a news story continue to underperform their size, book-to-market and event return-matched peers. Importantly, Chan (2003) reports a significantly weaker drift for stocks experiencing good news. Additionally, there is evidence that extreme return stocks that had no news headlines for a given month experienced reversal in the subsequent month and little abnormal performance after that. A number of robustness tests indicate that the main results of Chan (2003) are not a mere statistical artefact. However, the

author does acknowledge that the abnormal returns tend to be concentrated on smaller, low-price stocks.

Two additional contributions deserve special attention here. The first is that by Taffler, Lu and Kausar (2004) and the second is that by Kausar, Taffler and Tan (2008). These studies are particularly interesting because they deal with a clear and unambiguous bad news event. In effect, both papers investigate how the equity market reacts to the disclosure of a first-time going-concern audit report (GCM). The difference between them is that the first looks at the UK equity market, while the second focuses on the US market. Their results are, however, very similar: both find a negative and statistically significant risk-adjusted post-event drift lasting one year after the disclosure date of the GCM audit report. In particular, Taffler, Lu and Kausar (2004) report that, depending on the adopted benchmark, their sample firms underperform by between 24 and 31 percent over the period of interest. Kausar, Taffler and Tan (2008) find that the US equity market also underreacts, resulting in a subsequent downward drift between 9 and 19 percent over the one-year period subsequent to announcement date, conditional on the adopted benchmark for computing the abnormal returns. In both cases, the authors emphasize that their findings fit well with existing behavioural explanations. As Taffler, Lu and Kausar (2004, p. 293) put it "*(...) our analysis does not allow us to reject the behavioral (sic.) proposition that investors are, in fact, biased in their ability to process the bad news conveyed by a going-concern audit opinion appropriately leading to market underreaction. The relatively high levels of trading activity in the stocks of our small losing firms and the fact that both institutional investors and firm insiders do not sell their holdings post-GCM are consistent with the idea of "denial" of the implications of a GCM audit report in stock valuation judgments.*".

Another strand of the literature, involving financial intermediaries, also presents evidence on how the market reacts to bad news. Womack (1996) analyses what happens to market prices when security analysts at major US brokerage firms change their stock recommendations. He finds that the post-recommendation drift associated with a new buy is significant but short-lived. In particular, the author reports that the mean size-adjusted return for the first post-

event month beginning two days after the recorded date of the new buy recommendation is +2.4 percent. After that period, abnormal returns are not statistically different from zero at meaningful levels. New sell recommendations, however, are associated with a strong and negative drift lasting 6-months after the event date. Womack (1996) points out that his results indicate a failure of information to flow fully into stock prices, which is consistent with previous behavioural research. Ryan and Taffler (2006) re-examine the post-recommendation drift issue using UK data. The authors report the existence of a significant drift lasting at least 6-months after new sell recommendations but no significant excess returns subsequent to new buy recommendations. As a result, this out-of-the sample test also suggests that the market is unable to properly deal with bad news events.

2.3.3 Market efficiency and anomalies: a note

The previous paragraphs summarize some of the empirical evidence against the EMH. Advocates of the classical position, however, remain suspicious of these results and argue that they are more apparent than real (Schwert, 2003). For instance, Fama (1998) claims that anomalies are the mere work of chance and questions the soundness of behavioural empirical work on methodological grounds. Mitchell and Stafford (2000) and Kothari (2001) are also among those who argue that the empirical evidence against the EMH is "*weak*" due to the numerous problems affecting the reliability of long-term event studies. Schwert (2003), on the other hand, points out that most of the alleged market-pricing errors disappear once they are documented. Other scholars dispute the anomalies literature arguing that its results cannot be translated into an exploitable trading strategy (e.g., Rubinstein, 2001; Malkiel, 2003, 2005; Ross, 2005, p. 67). Nonetheless, even such critics are forced to admit that the anomalous market reaction to earnings announcements and the momentum effect are both robust phenomena. Fama (1998) recognizes that these anomalies are "*above suspicion*" and Kothari (2001, p. 196) commenting on the earnings announcement drift says, "*the survival of the anomaly thirty years after it was first discovered leads me to believe that there is a rational explanation for it, but evidence consistent with rationality remains elusive*".

I argue that extant empirical results on how the market deals with bad news events also offer a strong case against the EMH. In fact, there is extensive evidence suggesting that the aggregate market requires too much time to fully incorporate into security prices the complete impact of negative disclosures, something that is at odds with the main prediction of the semi-strong form of the EMH. In the next section, I explain why investigating in detail how the market reacts to the announcement of corporate bankruptcy may help shed further light on this issue.

2.4 The corporate bankruptcy event

This section is divided in two parts. The first presents background information about corporate bankruptcy in the US. The second discusses the implications of the EMH and behavioural finance for the research on corporate bankruptcy. A brief summary of extant empirical evidence on this subject is also provided.

2.4.1 Bankruptcy and bankruptcy law

A central tenet in economics is that competition drives markets towards a state of long-run equilibrium in which the surviving companies produce at minimum marginal cost (Jehle and Reny, 2001, pp. 153-158). In the transition to this equilibrium, ineffective firms are forced out of the market. Bankruptcy assumes a primary role in this process since it is the legal mechanism that allows eliminating such inefficient companies (White, 1989). The recent collapse of Enron and WorldCom, two of the biggest companies in the world, has fuelled the interest of the public on corporate bankruptcy (Healy and Palepu, 2003; Akhigbe, Martin and Whyte, 2005). However, these cases are only the tip of the iceberg. In the last 27 years, nine out of the ten major bankruptcies in the US occurred after 2000.¹⁵ More importantly, there is evidence that the bankruptcy phenomenon is here to stay: in 2007, the combined value of the assets of the ten top bankruptcies in the US exceeded 65 billion dollars.¹⁶

Things have not always been like this. In medieval Italy, a businessman would usually see his trading bench destroyed if failing to repay his debts. From the Italian for broken bench, "*banca rotta*", comes the term bankruptcy. In fact, the Romans were the first to use this concept. Under Roman law, dating back to 118 B.C., the entire estate of a debtor was sold in one lump sale to a single buyer, who would then pay creditors a percentage of what was owed. The debtor continued to be responsible for any remaining debt and failure to repay it in a fairly quick order would dictate one of four possible fates: imprisonment, enslavement, exile or even death (Delaney, 1998, p. 12). Following the spirit of the Roman law, rules and practices

¹⁵ See http://www.bankruptcydata.com/Research/10_LargestBankruptcies.htm for details.

¹⁶ See http://www.bankruptcydata.com/Research/Ch11_2007.htm for details.

concerning bankruptcy before the 20th century generally favoured the creditor and were very harsh toward the bankrupt. At that time, the focus was on recovering the investments of the creditors and not conceding debtors a second chance.

In the US, early federal laws concerning bankruptcy were temporary responses to bad economic conditions. In effect, the first official bankruptcy law in this country was enacted in 1800 in response to land speculation, being repealed three years later. Similarly, the second bankruptcy law was passed in 1841 as a response to the panic of 1837 and lasted only until 1843. The economic turmoil of the American Civil War caused Congress to pass another bankruptcy law in 1867, which was revoked in 1878. These initial experiences shared a common characteristic: to some extent, they allowed the discharge of unpaid debts, an innovation when compared to the ancient Roman practices on this domain.

Modern bankruptcy rules in the US emphasize exactly the notion that debtors should be granted the possibility to reorganize. The Bankruptcy Act of 1898 pioneered the idea of providing companies in distress with the option of protecting themselves from creditors by introducing the concept of "*equity receivership*". During the thorny years of the 1930s, the reorganization provision was made much more formal and extensive. In fact, the extremely negative economic and social effects of the Great Depression prompted the production of a massive amount of bankruptcy legislation. In only two years, two major contributions were made in this domain: the Bankruptcy Act of 1933 and the Bankruptcy Act of 1934. This legislation culminated with the Chandler Act of 1938, which included substantial provisions for business' reorganization. The advent of World War II cooled the interest in corporate bankruptcy, a situation that remained largely unchanged until the 1970s. During this period, bankruptcy was not a hot topic in the news.

The Bankruptcy Reform Act of 1978 was passed in 1978 and took effect on October 1, 1979. This act substantially renovated bankruptcy practices, making it easier for both businesses and individuals to file for bankruptcy and reorganize. Under the new law, Chapters X, XI and XII

were consolidated into a single Chapter 11 and a new chapter, Chapter 7, was introduced.^{17, 18} Under Chapter 7, a trustee is appointed to oversee the orderly liquidation of the firm's assets. Claimants are then paid according to the Absolute Priority Rule (APR), which simply states that no lower claimant shall receive any value until all higher claimants are paid in full. At the very bottom of the pecking order are the equity interests, in the order of preferred stockholders, common stockholders and warrant holders.

Most large companies, however, usually choose to file for Chapter 11. The major difference between filing a Chapter 11 or a Chapter 7 is that a firm filing for Chapter 11 aims at reorganizing its business in order to become profitable again. More precisely, the bankrupt firm still operates as a going concern under current management while creditors, shareholders and management negotiate a reorganization plan to restructure the company. To this end, liability claimholders with similar seniorities are assigned to the same class and the corresponding official (or unofficial) creditor committees are established. The law guarantees that the management of the debtor has 120 days of exclusivity to submit a reorganization plan. This right can and usually is extended by the Court for a specific period, after which every creditor group can submit its own plan. The reorganization plan may call for substantial consolidation of pre-petition liabilities, cancellation of old equity, conversion of old debt into equity or infusion of new equity. In order to approve the plan, each creditor must vote on the plan by class and each class of creditors must adopt it by a majority in number of claims and two-thirds by value. This procedure is called "*unanimous consent procedure*" or UCP. If a reorganization plan cannot be approved under UCP, a second procedure, "*cram down*", is available. Under this alternative, as long as at least one class of creditors has voted in favour, the bankruptcy court can confirm the plan or a modified version of it. The only requirement here is that each dissenting class is treated "*fairly and equitably*". In practice, this means that all unsecured creditors either receive full payment of the face value of their claims over the period of the plan or else that lower ranking classes receive nothing.

¹⁷ According to the legal convention, Roman numerals should be used for chapters under the 1898 Bankruptcy Act and Arabic numerals for chapters under the 1978 Bankruptcy Reform Act (Delaney, 1998, p. 27).

¹⁸ This Act also introduced a personal bankruptcy code, Chapter 13, which replaced the old Chapter XIII of the 1898 Act.

The last major modification in US bankruptcy law was the introduction of the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005. As Altman and Hotchkiss (2005) clarify, the main innovations of the 2005 Act concern the governance of consumer bankruptcy. In effect, the legal dispositions relating to corporate bankruptcy remain largely unaffected by the new code. Nevertheless, the authors do stress the idea that the new law is more creditor friendly than its predecessor, which may impact the ability of firms to reorganize successfully.¹⁹

2.4.2 Bankruptcy, the efficient market hypothesis and behavioural finance

Over the years, corporate bankruptcy has been a popular theme in both finance and accounting journals. With regard to this, Altman (1993) writes "*the interest of academics in corporate bankruptcy is as old as bankruptcy itself*". One of the most thoroughly explored areas in this context is how stock prices behave around the bankruptcy filing date (e.g., Clark and Weinstein, 1983; Datta and Iskander-Datta, 1995; Dawkins, Bhattacharya and Bamber, 2007). Investigating this issue is important because it allows us to understand how markets deal with extreme bad news events. In fact, a bankruptcy announcement conveys important information concerning the cash flow prospects of the firm that should lead to a reassessment of its fundamental value (e.g., Clarke and Weinstein, 1983; Datta and Iskandar-Datta, 1995; Russel and Branch, 2001; Dawkins, Bhattacharya and Bamber, 2007). To be precise, even in the most optimistic scenario, a bankrupt firm has to pay the legal and other costs associated with bankruptcy administration (e.g., Warner, 1977b; Ang, Chua and McConnell, 1982; Altman, 1984; Branch, 2002; LoPucki and Doherty, 2004; Bris, Welch and Zhu, 2006). Furthermore, these companies usually have to bear indirect costs such as the diversion of scarce management time, additional lost sales during and after bankruptcy, constraints on capital investment and R&D spending and the loss of key employees, which also affect negatively the firms' fundamental value (e.g., Altman, 1984; Opler and Titman, 1994; Maksimovic and Phillips, 1998; Pulvino, 1999; Branch, 2002).

¹⁹ See Altman and Hotchkiss (2005) pages 47 to 55 for further details.

Interestingly, we lack a thorough understanding of how the equity market reacts to bankruptcy announcements in the longer-run (Altman and Hotchkiss, 2005, p. 83; Dawkins, Bhattacharya and Bamber, 2007). This happens because major US exchanges would typically delist the stock of firms filing for bankruptcy shortly after the event date, something that only changed after the mid-1980s (Dawkins, Bhattacharya and Bamber, 2007). As a result, researchers were forced to focus their attention on the pre-event period or, at best, the few days surrounding the formal announcement of bankruptcy. Extant research, nonetheless, does offer some insight about the behaviour of the market in the face of corporate failure. The next paragraphs provide a brief review some of the contributions in this area.

2.4.2.1 Why invest in the stock of bankrupt firms?

A by-product of the bankruptcy boom of the last years has been the development of a thriving market for the equity shares of Chapter 11 debtors (Platt, 1999, p. 106). At a first glance, this is a puzzling phenomenon since investing in these firms is a peculiar idea. Yet, there is evidence that this has become a widely accepted investment strategy (Platt, 1999, p. 106). Academic literature and the popular US press have put forward two main arguments to explain this trend.

According to the first line of reasoning, investors act rationally when buying the stock of bankrupt firms. For instance, Merton (1974) looks at a firm's equity as a call option that shareholders should exercise by continuing to service the debt as long as the company's assets exceed its debt claims. In this setting, the equity of bankrupt firms is simply a deep out-of-the money option. As argued by Russel, Branch and Torbey (1999), this option has positive value, which reflects the premium for time, i.e., the possibility that assets will exceed debt before its maturity date. Additionally, the positive value of the option also provides a dollar measure of the probability that the firm will eventually pay-off something to its original shareholders once bankruptcy is resolved. Literature suggesting that the APR is often violated in bankruptcy proceedings provides evidence in favour of this argument (e.g., Franks and Torous, 1989; Eberhart, Moore and Roenfeldt, 1990; Weiss, 1990). Tax-reasons are another example of why one may rationally decide to purchase the stock of bankrupt firms. In fact, investors wanting to

offset their capital gains can do so by using the value they eventually lose with this risky investment strategy.²⁰ Finally, Lhabitant (2006, pp. 230-231) explains that institutional investors, namely hedge funds, may have an incentive to invest in the equity of bankrupt firms. According to the author, this decision is normally prompted by the desire to control the target company, which can be achieved by buying a sufficient number of voting shares. This *active approach* ensures that the hedge fund is in command of the reorganization process thus improving the odds of maximizing the payoff of its investment in the failed company.

The second line of reasoning posits that people invest in the stock of bankrupt firms because they are irrational. Two major stories fall within this category. The first is that people buy this type of security because they wrongfully perceive a considerable potential for price appreciation in this market. This position, put forward by Russel and Branch (2001), is motivated by the fact that bankrupt firms' stock usually trades at very low prices. The second story, mentioned in several news articles, can be referred to as the "*finding a bigger fool*" hypothesis. This explanation states that people buying the stock of bankrupt firms believe that they will be able to sell it to some other "*fool*" for a higher price. In this case, investors' behaviour is irrational because it is divorced from any analysis of the firm's fundamentals, drawing simply on sentiment.

Clearly, different motivations for investing in the market of bankrupt firms will lead to different pricing patterns. I review some of the evidence uncovered by the finance literature on this matter below.

²⁰ See <http://www.sec.gov/investor/pubs/bankrupt.htm> for further details.

2.4.2.2 The market is efficient when dealing with corporate bankruptcy

Clark and Weinstein (1983) examine the market's anticipation and very short-run reaction to bankruptcy announcements. They report that shareholders sustain substantial losses over long periods before the event date. Clark and Weinstein (1983) also emphasize that these investors lose large amounts of money during the bankruptcy month and that such losses are especially concentrated in the three trading-day interval surrounding the bankruptcy date. They conclude that bankruptcy conveys important unanticipated information to the market.

In a later study, Datta and Iskandar-Datta (1995) investigate the effect of bankruptcy on four types of security holders: shareholders, secured debt holders, unsecured debt holders and convertible debt holders. Consistent with previous research, Datta and Iskandar-Datta (1995) report an adverse stock price reaction at the event date. The authors also find that different classes of debt holders react differently to bankruptcy announcements. In particular, secured debt holders seem to be unaffected whereas the unsecured and convertible debt classes exhibit significant adverse price reaction to the event. According to Datta and Iskandar-Datta (1995), their results are explained by the fact that secured creditors are given privileged treatment while bankruptcy is underway, something that does not happen with unsecured debt holders.

Beneish and Press (1995) explore the relationship between technical default, debt service default and bankruptcy using a sample of 134 firms traded on the NYSE. They show that these events are interrelated. In particular, their results demonstrate that: 1) firms in technical default are more likely to suffer serious future distress (like debt service default or bankruptcy) than non-defaulters and 2) bankruptcy is more probable after a debt service default. Beneish and Press (1995) also conclude that the valuation effects of technical default provide an important and timely warning to investors, because they partially anticipate losses from subsequent distress announcements. In fact, the effects of debt service default announcements are attenuated if preceded by disclosure of technical default in the previous year. Moreover, they show that firms filing for Chapter 11 suffer a less severe price reduction at the event date when bankruptcy is preceded by a debt service default.

Burgstahler, Jiambalvo and Noreen (1989) explore the association between bankruptcy prediction models and the market's reaction to bankruptcy announcements. The authors posit that due to the direct and indirect costs of bankruptcy, there are *a priori* grounds for expecting changes in the probability of bankruptcy to have cash flow implications and hence incremental informational content. Results indicate that unexpected changes in the probability of bankruptcy are useful in explaining security returns, a conclusion that holds after controlling for the effect of unexpected earnings. According to Burgstahler, Jiambalvo and Noreen (1989), this shows the importance of accounting in determining the value of the firm, pointing out that the market fully incorporates into the stock price the information provided by public accounting documents. In a related study, Dawkins and Rose-Green (1998) examine the relationship between prior *Wall Street Journal* announcements of a possible bankruptcy and the price reaction to subsequent Chapter 11 filings. They find that firms with a prior announcement in the *Journal* experience a smaller price reaction at the event date when compared to other firms that did not receive a similar disclosure in that *Journal*, which is consistent with the semi-strong form of the EMH.

Rose-Green and Dawkins (2000) investigate if, at the time of bankruptcy filing, the market differentiates between firms that are subsequently liquidated from those that are reorganized. They find that the former group has a significantly larger negative price reaction at the event date than the latter. Rose-Green and Dawkins (2000) conclude that the US equity market is very efficient since it has a high degree of insight into subsequent bankruptcy resolution.

In a very interesting study, Morse and Shaw (1988) empirically test the impact of the Bankruptcy Reform Act of 1978 on the risk and return characteristics of bankrupt companies' stock. They show that firms going into bankruptcy prior to 1978 experience the same degree of financial distress as companies entering this situation after that date. More importantly for the development of this thesis, Morse and Shaw (1988) find that the 3-year post-bankruptcy average abnormal returns are large but not statistically significant. In other words, just as

predicted by the EMH, the authors document a complete market reaction to bankruptcy announcements.

In a related study, Russel, Branch and Torbey (1999) use an option-pricing model to produce theoretical estimates for the equity value of a sample of 154 firms that filed for bankruptcy between 1984 and 1993. The authors show that the estimated values are aligned with the observable market capitalization of the companies in the post-event period. Similarly to Morse and Shaw (1988), Russel, Branch and Torbey (1999) conclude that their study provides evidence in favour of the idea that, on average, the market efficiently prices the stock of bankrupt companies.

Overall, the studies above suggest that the market deals efficiently with corporate bankruptcy announcements. In effect, some report that the market anticipates and reacts to firm failure on a timely fashion while others reveal that it is able to impound into the stock price prior events signalling that bankruptcy is likely. Morse and Shaw (1988) and Russel, Branch and Torbey (1999) are particularly interesting contributions, in that they suggest the market correctly prices the stock of bankrupt firms in the post-event period. This is a puzzling finding, given extant results from behavioural finance on how the market reacts to bad news events. I argue, however, that several caveats impair the reliability of these papers' conclusions. For instance, Morse and Shaw (1988) use only 56 companies, most of which are delisted in the post-bankruptcy period. Importantly, the authors rely solely on mean buy-and-hold returns (BHARs) to make inferences about the magnitude and statistical significance of the post-event drift. Additionally, a value-weighted index is used to control for risk over a period that spans 36 months after the bankruptcy date. We know now that the combination of market-adjusted BHARs with such a long compounding period is problematic, especially when a value-weighted index is employed to calibrate event studies that focus on small companies (Ball, Kothari and Shanken, 1995; Kothari and Warner, 1997). Clearly, these methodological problems alone call into question the robustness of the paper's conclusion regarding the post-bankruptcy drift.

The shortcomings of the Russel, Branch and Torbey (1999) study are even more striking. For instance, the Black and Scholes (1973) option-pricing model employed by the authors may simply be inadequate to value bankrupt firms' equity (Altman and Hotchkiss, 2005, pp. 103-120). Additionally, Russel, Branch and Torbey (1999) cover only a small sample of 39 firms, for which they have very incomplete data. The authors acknowledge this fact pointing out that *"Crudeness of data and lack of information on bankrupt firms compound the valuation problem"*. More importantly, Russel, Branch and Torbey (1999) admit that changing the assumptions when computing the theoretical values for the bankrupt firms' equity dramatically changes their results: *"When assets are valued at about 40 percent of liabilities, estimates tend to be less than the observed market values and would imply that investors in these bankrupt firms are overpaying."* They thus conclude that *"Future research into this topic will need to overcome the limitations of data availability for these firms in order to settle this question"*. By the authors own admission, the limitations affecting their study cast reasonable doubt on its conclusions.

2.4.2.3 The market is not that efficient when dealing with corporate bankruptcy

There is also evidence suggesting that the market does not deal appropriately with bankruptcy announcements. Hubbard and Stephenson (1997) is one of the clearest contributions in this domain. The authors investigate to what extent the stock price of bankrupt firms reflects the provisions of their reorganization plans and find that investors grossly overestimate the value of what they might receive upon emergence from Chapter 11. To be precise, Hubbard and Stephenson (1997) show that a quarter of their sample firms are traded for nontrivial positive prices despite the fact of having filed a reorganization plan that granted nothing to their shareholders. Hubbard and Stephenson (1997) conclude that *"investors do not appear to understand the bankruptcy process and/or are not aware of the terms of publicly available reorganization plans"*.

Schatzberg and Reiber (1992) use an independent dataset to replicate the early study of Clark and Weinstein (1983) and, in sharp contrast with the previous evidence, find that post-

bankruptcy price movements are consistent with the hypothesis of short-term overreaction and subsequent price rebound. The authors explore three possible explanations for their finding: 1) estimation error; 2) missing data during the test period and 3) the transaction cost hypothesis. However, none of these alternatives fits their results. Accordingly, Schatzberg and Reiber (1992) conclude that the market for bankrupt companies does not react efficiently to bankruptcy announcements even in the short-run.

Seyhun and Bradley (1997) analyse the trading behaviour of corporate insiders around the announcement of bankruptcy using a large sample of firms that filed for bankruptcy between 1990 and 1991. Their results show that these market participants engage in significant sales of their firm's stock in the months and even years preceding a bankruptcy filing, thereby avoiding significant capital losses. In particular, Seyhun and Bradley (1997) document that insider selling begins five years before the filing date and builds to a crescendo up to the announcement month. Clearly, this study questions the strong-form of the EMH, which posits that current securities prices instantly and fully reflect all information, both public *and private*. In a more recent study, Ma (2001) also finds evidence of abnormal behaviour by insiders of bankrupt firms.

Eberhart, Altman and Aggarwal (1999) directly test the efficiency of the market of firms emerging from Chapter 11. They find weak evidence of positive excess returns in the short-term and strong evidence of positive abnormal returns in the long-run. To be precise, the authors report that, in the first 200 days after emergence, mean excess returns vary from 24.6 to 138.8 percent conditional on the particular benchmark they use. Eberhart, Altman and Aggarwal (1999) also demonstrate that their results are not driven by poor adjustment for risk nor by transaction costs. Additionally, there is evidence that the post-emergence drift is different from the post-earnings anomaly originally documented by Ball and Brown (1968). Eberhart, Altman and Aggarwal (1999) conclude that "*our results cast doubt on the informational efficiency of this market*".

A final comment relates to the recent work of Dawkins, Bhattacharya and Bamber (2007), who thoroughly examine the short-term return pattern arising after the announcement of Chapter 11. They find weak evidence of a price reversal occurring after the bankruptcy announcement date. Contrary to *a priori* expectations, the authors show that this effect is concentrated on larger and more heavily traded firms and that it is not attributable to microstructure problems like the bid-ask bounce. Interestingly, Dawkins, Bhattacharya and Bamber (2007) also report that the activities of large traders are likely to be responsible for the price reversal phenomenon they document. This paper additionally shows that the post-event return pattern is conditional on the overall phase of the market. In particular, there is only evidence of price reversal for the bullish period of 1993 to 1999. Conversely, there is no evidence of an anomalous market reaction to bankruptcy for the complementary bearish period of 2000 to 2003 that is also covered in the study. As Dawkins, Bhattacharya and Bamber (2007) emphasize, their findings suggest that investors' reaction to information events is more irrational when occurring in bullish than in bearish markets.

In general, and contrary to the empirical evidence presented in section 2.4.2.2, the studies above suggest that the market is unable to deal efficiently with corporate bankruptcy announcements. Interestingly, none of these papers specifically investigates the longer-term returns conditional on the bankruptcy event. For instance, Hubbard and Stephenson (1997) examine the question of efficiency by comparing market prices with the prices defined in the companies' reorganization plan. On the other hand, Seyhun and Bradley (1997) and Ma (2001) show that the market of bankrupt firms cannot be strong-form efficient because corporate insiders actively use their privileged information to trade. Finally, Schatzberg and Reiber (1992) and Dawkins, Bhattacharya and Bamber (2007) restrict their analysis to a few days after the event while Eberhart, Altman and Aggarwal (1999) focus their attention on the complementary issue of the market's reaction to emergence from Chapter 11. Noticeably, more research is needed to finally set the question of how the market reacts to bankruptcy announcements in the longer-run.

2.5 Summary of the chapter

Few propositions in economics are held with more fervour than the view that financial markets are efficient. The logic of this assertion is simple and compelling. If securities prices could be predicted, knowledgeable investors would buy cheap and sell dear. Soon, the forces of competition and rational arbitrage would guarantee that prices adjust to their fundamental value, only to move again, randomly, in response to unanticipated value-relevant information (De Bondt and Thaler, 1989). In the last two decades, however, a new school of thought has emerged and challenged the position of the EMH as the primordial paradigm in finance. This new approach, behavioural finance, claims that, under certain conditions, market prices may systematically deviate from their fundamental value for long periods (Barberis and Thaler, 2005, p. 6). The combination of limits to arbitrage and cognitive biases justifies this result.

Interestingly, to date, it is still not possible to ascertain which of these competing theories does a better job at describing the workings of real world financial markets. However, the empirical evidence against the EMH is conspicuous, especially when one considers how the market deals with bad news. Classical work here is that by Bernard and Thomas (1989, 1999), Michaely, Thaler and Womack (1995), Womack (1996), Dichev and Piotroski (2001), Chan (2003), and Taffler, Lu and Kausar (2004). All these studies share a common conclusion: the market has problems assimilating adverse public disclosures in a timely and unbiased manner, which is clearly inconsistent with the main prediction of the semi-strong form of the EMH.

Bankruptcy is the most extreme bad news event in the corporate domain. Surprisingly, despite the interest of several academics on the subject, we still know very little about what happens to stock prices once bankruptcy is declared (Altman and Hotchkiss, 2005, p. 83; Dawkins, Bhattacharya and Bamber, 2007). Some studies suggest that the market deals efficiently with this event (e.g., Clark and Weinstein, 1983; Morse and Shaw, 1988; Datta and Iskandar-Datta, 1995; Russel, Branch and Torbey, 1999) while others imply just the opposite (e.g., Schatzberg and Reiber, 1992; Hubbard and Stephenson, 1997; Seyhun and Bradley, 1997; Ma, 2001; Dawkins, Bhattacharya and Bamber, 2007). However, with the exception of Morse and Shaw

(1988), none of these papers specifically investigates the longer-term abnormal returns conditional on the announcement of bankruptcy. Nevertheless, several limitations cast reasonable doubt on Morse and Shaw's (1988) conclusions.

Exploring this gap in the literature should enable me to contribute to the ongoing debate between the EMH advocates and behavioural finance theorists. As Hirshleifer (2001) points out, greater uncertainty and lack of accurate feedback about a firm's fundamentals leaves more room for psychological bias. As such, misvaluation effects should be stronger for high-uncertainty firms (Jiang, Lee and Zhang, 2005; Zhang, 2006), which is precisely the case of bankrupt companies. Moreover, a number of *a priori* reasons help understand why limits to arbitrage should impede arbitrageurs from correcting a potential mispricing. For instance, the typical firm filing for protection of a Federal Bankruptcy Court is usually small and highly distressed (e.g., Campbell, Hilscher and Szilayi, 2007; Kalay, Singhal and Tashjian, 2007), which renders fundamental valuation difficult (Gilson, 1995; Gilson, Hotchkiss and Ruback, 2000). Analyst coverage is also very low for this type of firm (e.g., Espahbodi, Dugar and Tehranian, 2001; Clarke et al, 2006). Institutional investors are also likely to be absent from this market for both legal (Del Guercio, 1996) and idiosyncratic (e.g., Gompers and Metrick, 2001) reasons. Finally, trading costs and short-sale restrictions are probably binding in this context (e.g., D'Avolio, 2002).

Is this line of reasoning correct? I investigate this issue in detail in the following chapters.

Chapter 3

The Market's Reaction to Bankruptcy Announcements

3.0 Introduction

The previous chapter suggests that investigating how the US equity market reacts to bankruptcy announcements is an interesting theme for two reasons: 1) we do not know much about what happens to stock prices once bankruptcy is declared, and 2) exploring this gap in the literature should allow me to shed light on the debate between the EMH advocates and behavioural finance theorists. In this first empirical chapter, I start addressing the issue by means of a simple research question: does the US equity market quickly and accurately react to bankruptcy announcements?

Anecdotal evidence suggests that the market may have problems in dealing efficiently with public news associated with Chapter 11 bankruptcy proceedings. For instance, Hubbard and Stephenson (1997) report that, on January 20, 1993, the management of LTV Steel filed a reorganization plan to bring the company out of bankruptcy, which gave only warrants to existing shareholders valued by the firm at 3.22 cents per share. Surprisingly, the price of the firm's common stock rose steadily in the two following trading sessions, reaching a high of 2 dollars per share. LTV's stock was actively traded for four months before its reorganization plan was confirmed by the Bankruptcy Court, closing at 0.25 dollars per share, i.e., *eight times* the stated value in the plan. Hubbard and Stephenson (1997) also describe the case of Ames Department Stores. In 1992, this company traded for eight months after management filed a reorganization plan eliminating the interest of its common and preferred shareholders. Nevertheless, the company's stock traded for prices as high as 75 cents per share in this period. A more recent example is that of Enron Corp. On July 11, 2003, this firm filed a reorganization plan that gave nothing to existing shareholders. Remarkably, the company's stock price rose by 70 percent and its trading volume increased by a factor of eight around the date of the confirmation of Enron's reorganization plan. A final example is that of Kmart Corp. On April 22,

2003, the firm announced that the bankruptcy court had confirmed its reorganization plan, which cancelled the firm's old equity. However, within a week, the company's share price had doubled.²¹ The cases above clearly suggest that the market may fail to properly understand the implications of Chapter 11 in shareholders' value even at late stages of the bankruptcy proceedings, when the ultimate fate of extant equity holders has become clear.

Other examples, however, suggest that the market is efficient when dealing with news relating to bankrupt firms. For instance, when UAL Corp. stated in a June 2003 regulatory filing that it was "*highly likely*" that it would cancel its old equity shares, the stock price of the company quickly declined by 58 percent.²² Similarly, when Air Canada announced in June 2003 that its restructuring would make its current stock worthless, share prices fell by 36 percent.²³

3.1 Empirical implications

The paragraphs above emphasise chapter 2's main idea that more research is needed to formally address the question regarding the efficiency of the market for bankrupt firms. As such, I propose to test the following null hypothesis:

H₁: Risk-adjusted abnormal returns following the announcement of Chapter 11 bankruptcy are zero.

This hypothesis follows the tradition in finance research, implying that the market reacts efficiently to the event under investigation. In fact, the semi-strong form of the EMH suggests that, after the announcement of bankruptcy, the firm's stock price should converge immediately to its new fundamental value. As a result, no significant abnormal returns should exist shortly *after* the bankruptcy's public disclosure date.

²¹ Bennett, M., "I Pity the Greater Fool: The Market for Debtors' Old Equity Shares", *American Bankruptcy Institute*, November 1, 2003.

²² Reuters news story, published on June 10, 2003.

²³ Reuters news story, published on June 12, 2003.

This chapter continues as follows. Sections 3.2 and 3.3 provide details about the sample and methodology employed in exploring the research hypothesis presented above. Results are discussed in section 3.4. Section 3.5 concludes.

3.2 Sample selection

I start by compiling a list of firms filing for bankruptcy in the US between 01.10.1979 and 17.10.2005. Within this period, bankruptcy in the US was governed by the Bankruptcy Reform Act of 1978, which was enacted on 1978 and became generally effective on October 1, 1979. In 2005, this Act was substantially revised by the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005. Most of the provisions of this new Act became effective on October 17, 2005. Accordingly, by focusing on the period between 01.10.1979 and 17.10.2005, I am able to explore a phase where the legal framework within which US corporations could file for Federal protection remained largely unchanged.

I use seven sources to identify this study's sample firms: 1) Bankruptcydata.com database;²⁴ 2) the SEC's Electronic Data Gathering, Analysis, and Retrieval system (EDGAR);²⁵ 3) COMPUSTAT's industrial file; 4) Professor Lynn Lopucki's Bankruptcy Research database;²⁶ 5) SDC database; 6) Altman and Hotchkiss (2005), pages 15 to 20, and 7) a list of bankrupt firms provided by Professor Edward Altman. All firms are combined into a single list and duplicates eliminated, which yields a total of 3,437 unique cases. I then use the screening process described below to identify my final sample.

The firms are first located on the *Center for Research in Security Prices* (CRSP) database leading to 1,411 being eliminated, the main reason being that firms could not be found in CRSP. Moreover, a few other cases are also excluded because the firm's ordinary common stock

²⁴ See <http://www.bankruptcydata.com/> for more details.

²⁵ Publicly traded companies filing for bankruptcy are required to report it to the SEC within 15 days using a Form 8-K. In order to find the bankruptcy cases reported to the SEC, I search and manually analyse 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 10kwizard, a software designed to facilitate the keyword search on EDGAR. See <http://www.10kwizard.com/main.php?spage> for details.

²⁶ See <http://lopucki.law.ucla.edu/> for details.

(CRSP share code 10 or 11) is not traded, the firm does not have at least 24-months of pre-event returns available on CRSP or does not trade on a major US stock exchange (CRSP exchange codes 1, 2 or 3) during that period.

My primary objective is investigating the post-bankruptcy stock price performance. Consequently, in step two, I delete 1,556 firms because they are delisted prior to or at their bankruptcy filing date. 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 removed. In step three, the 11 companies incorporated outside the US (as defined by COMPUSTAT) are also excluded in order to ensure consistency in the legal framework governing all bankruptcy cases analysed in this study. Penultimately, following prior research, I also remove all 40 financial and utility companies from my final sample.²⁷ Utility companies are generally heavily regulated by the government, which results in bankruptcy having a different meaning for these firms. Financial companies are not considered because they are handled differently while in Chapter 11. The 10 companies filing for Chapter 7 are excluded in the last step of the screening process. The considerable legal differences and immediate consequences between filing for Chapter 11 or for Chapter 7 under the Bankruptcy Reform Act of 1978 justify this action.²⁸

Table 3.1 summarizes my screening process. As can be seen, in the end, I identify 351 non-finance, non-utility industry firms that file for Chapter 11 between 01.10.1979 and 17.10.2005 and remain listed on a major US stock exchange after their bankruptcy date. All companies have sufficient data available on both CRSP and COMPUSTAT to conduct my analysis. These firms have 53 different two-digit SIC codes (168 different four-digit codes) indicating no significant degree of industry clustering. The maximum number of firms in any single four-digit SIC code is 16 (or 4.6 percent).²⁹ Most of the sample firms trade on the NASDAQ (209 of 351,

²⁷ Financial and utility companies are defined as in the 49 industry portfolios available at Professor Kenneth French's website. See http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/Data_Library/det_49_ind_port.html for details.

²⁸ According to the Act, a company filing a Chapter 11 aims at reorganizing its business with the objective of becoming profitable again. Conversely, under Chapter 7, the company stops all operations and goes completely out of business. See section 2.4.1 for more details.

²⁹ The SIC code is 1311, for firms in the Crude Petroleum and Natural Gas industry.

or 59.6 percent). Another group of 109 companies (31 percent) trades on the NYSE and the remaining 33 (9.4 percent) on the AMEX.

Table 3.1
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 unique 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 any 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. Financial and utility companies are not considered in the final sample.

	N
Unique bankruptcies identified from the different data sources	3,437
Bankruptcies not found or with insufficient data on CRSP	1,411
Bankruptcies delisted before or at the bankruptcy filing month	1,556
Bankruptcies with insufficient data on COMPUSTAT	58
Bankruptcies classified as foreign	11
Utilities and financial firms	40
Firms filing Chapter 7	10
Final sample size	351

3.3 Data and methodology

3.3.1 Measuring abnormal returns

I use a buy-and-hold abnormal return (BHAR) strategy to make inferences about my sample firms' stock return pattern before, during and after their Chapter 11 date. Barber and Lyon (1997) show that the alternative cumulative abnormal returns (CARs) do not accurately capture the magnitude of investing in an average sample firm relative to an appropriate benchmark over the horizon of interest, which is precisely the objective of long-run event studies of stock returns. Accordingly, the authors favour the 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 an incorrect inference about medium- and long-term stock price performance.

Fama (1998), however, criticizes the use of BHARs and favours CARs because of their desirable statistical properties, which allow cleaner tests of mispricing. Fortunately, Barber and Lyon (1997) and Kothari and Warner (1997) show that the statistical problems uncovered by Fama (1998) with the use of BHARs usually arise over the 3- to 5-year time horizon whereas I restrict my 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). Secondly, my typical sample firm spends an average (median) of 24.4 (18.1) months in bankruptcy.³⁰ Ending the abnormal return calculation period twelve months before minimizes the impact of this important event on my results. Buy-and-hold abnormal returns are computed as follows:

$$BHAR_i(\tau_1, \tau_2) = \prod_{t=\tau_1}^{\tau_2} (1 + r_{i,t}) - \prod_{t=\tau_1}^{\tau_2} [1 + E(r_{i,t})] \quad (3.1)$$

where $BHAR_i(\tau_1, \tau_2)$ is the buy-and-hold return for firm i from time τ_1 to τ_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 .³¹ In order to produce meaningful results, individual BHARs are averaged cross-sectionally as follows (e.g., Barber and Lyon, 1997; Campbell, Lo and MacKinlay, 1997):

$$\overline{BHAR}(\tau_1, \tau_2) = \frac{1}{n} \sum_{i=1}^n BHAR_i(\tau_1, \tau_2) \quad (3.2)$$

where $BHAR_i(\tau_1, \tau_2)$ is defined as above and n is the number of firms with a valid BHAR for time τ_1 to τ_2 . As suggested by equation (3.2), I use equally weighted rather than value-weighted returns since this is more appropriate in the context I address. In fact, strategies that give the same weight to all firms in the investment portfolio allow maximum diversification of each company's idiosyncratic risk, a critical aspect when dealing with failed firms (e.g., Gilson,

³⁰ Altman (1993) and Eberhart, Altman and Aggarwal (1999) report similar statistics for the average/median time spent in Chapter 11 bankruptcy reorganizations.

³¹ CRSP reports simple returns (both on its daily and monthly file). For more information see the data description section on WRDS about variable RET.

1995; Platt, 1999, p. 110).³² Additionally, previous research has shown that equally weighting captures the extent of underperformance better than value-weighting (Brav, Geczy and Gompers, 2000; Kadiyala and Rau, 2004). Loughran and Ritter (2000) also argue that value-weighted portfolio returns reduce the power of the tests to detect any potential behavioural bias.

Unless otherwise stated, daily returns collected from CRSP are employed in the calculation of abnormal returns, where $t=0$ is the bankruptcy announcement date.³³ As argued by Kothari and Warner (2007, p. 8), the use of daily rather than monthly security return data permits a more precise measurement of abnormal returns and more informative studies of announcement effects. I define a year as twelve 21-trading day intervals, an approach consistent with previous research (e.g, Michaely, Thaler and Womack, 1995; Loughran and Ritter 1995; Ikenberry and Ramnath, 2002). Importantly, in all tests based on daily data, event day +1 is included in the bankruptcy announcement window. Dawkins, Bhattacharya and Bamber (2007) point out that US stock markets close at 4:00 p.m. Eastern Standard Time (EST) while US Courts do not close until 5:00 p.m. local time, making it possible for firms to file their bankruptcy petition after the market closes on event day zero (i.e., after 4:00 p.m. EST). In such cases, investors cannot trade on the information disclosed at the event date until the next trading day.

Some of my sample firms are delisted in the 12-month period subsequent to their Chapter 11 date.³⁴ Drawing on Shumway (1997) and Shumway and Warther (1999), I include the delisting return in the calculation of the abnormal returns, a procedure also used by Campbell, Hilscher and Szilayi (2007). CRSP provides delisting returns for 165 cases, with an average value of -19.21 percent. Following Ogneva and Subramanyam (2007), for the remaining 30 cases with no data available on CRSP, I substitute the missing delisting return with the average delisting

³² In a recent paper, Klein, Rosenfeld and Tucker (2006) examine the long-term stock performance of firms following reverse stock splits. Similarly to my own case, this paper deals with event firms that are small and trade at very low prices. In order to deal with this issue when analysing the market reaction to the announcement of reverse stock splits, Klein, Rosenfeld and Tucker (2006) also compute equally weighted returns in lieu of value-weighted returns.

³³ All data sources mentioned in section 3.1 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.

³⁴ Performance issues explain 94 percent of these delistings (CRSP delisting codes 500 to 599).

return in the entire CRSP database for the similar type of delisting (as identified by CRSP 3-digit delisting code). The average delisting return after considering this correction for all 195 cases is -20.16 percent.³⁵

In line with Barber and Lyon (1997) and Lyon, Barber and Tsai (1999), when a company is delisted, I assume proceeds from the delisting payment are re-invested in a portfolio of stocks comprising the same size decile of the delisted firm for the remaining of the compounding period. As the authors explain, the sample's mean long-run abnormal returns 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.

3.3.2 Benchmarking

The appropriate expected return measure has been widely discussed in the literature (e.g., Barber and Lyon, 1997; Kothari and Warner, 1997; Lyon, Barber and Tsai, 1999, Ang and Zhang, 2004). Drawing on Barber and Lyon (1997) and Ang and Zhang (2004), I use a single control firm approach in my main results. Barber and Lyon (1997) point out that such methodology eliminates the new listing bias, the rebalancing bias and the skewness problem, yielding well-specified tests in most of the situations they consider. Ang and Zhang (2004) also show that the single control firm approach resolves the problem of the event firm not being near the centroid of the respective matched portfolio when the alternative reference portfolio method is employed to estimate the abnormal performance of sample firms. Crucially, Ang and Zhang (2004) demonstrate that such a problem is more acute with small firms, which is precisely the case of my sample companies. In addition, a very recent paper by Klein, Rosenfeld and Tucker (2006, pp. 3-4) emphasizes the idea that the single-matched control firm approach allows the research to produce more accurate abnormal return estimates when the event firms

³⁵ As an additional robustness check, I also substitute the missing delisting returns with the appropriate value identified in Shumway (1997) and Shumway and Warner (1999). This alternative does not affect my results in any meaningful way.

are from the extreme tails of the distribution, such as stock price and firm size, which is the case of the bankrupt firms analysed here.

I identify a control firm by matching each of my sample firms with the company with most similar size and book-to-market ratio. This approach is consistent with a number of recent studies exploring the medium-term stock return performance of gravely financially distressed firms (e.g., Dichev and Piotroski, 2001, Taffler, Lu and Kausar, 2004; Ogneva and Subramanyam, 2007; Kausar, Taffler and Tan, 2008). A two-step method is used to conduct the match. First, for each sample firm, I screen CRSP looking for an initial pool of matching candidates based on size. For the match candidates, size is defined as market capitalization (shares outstanding times price) at the end of the bankruptcy month. For sample firms, size is also defined as market capitalization but it is measured one month before the bankruptcy date (Liu, Szewczyk and Zantout, 2008).³⁶ For each sample firm i , the initial pool of candidates consists of all CRSP firms with a market capitalization between 70 and 130 percent of that of firm i 's equity market value.

In the second step, a control firm is found for each of my sample companies by choosing from its initial pool of candidates the firm with the closest book-to-market ratio. Calculating such ratios requires combining accounting and market data. Fama and French (1992, 1993) argue that it is important to guarantee that accounting variables are known before the market variables they are paired with. In order to ensure this result, I calculate the book-to-market ratio as follows. For both sample and control firms, I use COMPUSTAT item 60 taken from the last annual accounts reported before the bankruptcy year as the book value of equity (Fama and French, 1992), and allow a 3-month lag to measure the market value of equity.³⁷

³⁶ This helps reducing the impact of the event on the leading matching variable. As a robustness check, I 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. Measuring sample firms' size after the event date also does not change my results in any meaningful way.

³⁷ The market value of every sample firm is measured before its bankruptcy announcement date. This result is confirmed by manually inspecting all cases.

The match is confirmed if: 1) the control firm has at least 24 pre-event months of returns available on CRSP; 2) it is not in bankruptcy and is fully listed on a major US stock exchange on the 24-month pre-event period, trading ordinary common stock; 3) it is incorporated in the US; 4) it is not a financial or utility company and 5) it has sufficient information on COMPUSTAT to conduct my analysis.

In the case of multiple matches, I randomly choose one of the matching firms (Ogneva and Subramanyam, 2007). Additionally, if a control firm is delisted before the ending date for its corresponding bankrupt firm, a second company is spliced in after the delisting date of the first matching firm. The replacement firm is the second non-event firm with most similar size and book-to-market to that of the delisted firm on the original ranking. Furthermore, if a chosen matching firm subsequently files for bankruptcy, I treat it as if it is delisted on its bankruptcy date. These procedures introduce no survivorship or look-ahead bias and minimize the number of transactions implicit in the calculations (Loughran and Ritter, 1995; Spiess and Affleck-Graves, 1995).

For illustrative purposes and to allow comparisons with prior research dealing with the market's reaction to bankruptcy announcements, I also use the equally weighted CRSP index including dividends as an alternative proxy for expected returns. As pointed out by Dawkins, Bhattacharya and Bamber (2007), using the equally weighted CRSP index is the correct choice because bankrupt firms are usually smaller than the median CRSP firm is.

3.3.3 Testing the statistical significance of the abnormal returns

Following Barber and Lyon (1997) and Ang and Zhang (2004), I employ a t-test to infer about the statistical significance of the different mean BHARs. I 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 date (Boehmer, Musumeci and Poulsen, 1991; MacKinlay, 1997). This is appropriate since previous research by Aharony, Jones and Swary (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.

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, Kothari and Shanken, 1995). Drawing on Kraft, Leone and Wasley (2006), I report mean BHARs that are winsorized at the 1 and 99 percent levels to reduce the impact of extreme outliers in my analysis, a procedure also implemented in previous research by Ikenberry and Ramnath (2002) and Kausar, Taffler and Tan (2008).³⁸ Importantly, Kausar, Taffler and Tan (2008) show that winsorizing 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, Leone and Wasley (2006, 2007) and is especially important in the context of my research since a relatively large number of my bankrupt companies trade at prices below 1.00 dollar per share.

I also present median abnormal returns since they are unaffected by extreme observations and present some theoretical advantages over mean BHARs (Ang and Zhang, 2004). Consistent with previous research dealing with bankruptcy announcements, a Wilcoxon signed rank-test is employed to test the statistical significance of the median abnormal returns (Dawkins and Rose-Green, 1998; Rose-Green and Dawkins, 2002; Dawkins, Bhattacharya and Bamber, 2007).

3.4 Results

3.4.1 Descriptive statistics

Table 3.2 provides sample and control firm descriptive statistics. Panel A shows that my 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 percent, median=-6 percent), current ratio is low (mean=169 percent, median=128 percent) and leverage is relatively high (mean=45 percent, median=40 percent). Not surprisingly, the average Altman (1968) z-score is low

³⁸ See also Cowan and Sergeant (2001) for a discussion on the impacts of winsorization in long-term abnormal returns.

(mean=1.37, median=1.31), suggesting that these firms are likely to fail in the short-run.³⁹ Results for the control firms are somewhat different. For instance, there is clear evidence that these companies 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 mean and median differences between groups for these variables are statistically significant at the one percent level). Nonetheless, the data also shows that the typical control firm is losing money: the mean return on assets is -15 percent and the corresponding median is 1 percent (not significant at the 10 percent level). Panel A also shows that sample and matched firms are of similar size, at least when total assets or sales are used as proxies for this variable, as demonstrated by the t-test and the Wilcoxon-Mann-Whitney test for these two variables.

Panel B of table 3.2 summarizes a number of market variables. Both sample and matched firms are small, with an average market capitalization of around 160 million dollars (median=32 million dollars).⁴⁰ Additionally, panel B shows that both sets of firms have high book-to-market ratios, which is an expected result. For sample firms this reflects the likely negative expectation of the market in respect to their future prospects. For control firms, this result is obtained by construction due to the matching procedure employed in this study.

Panel B shows that, despite their small average size, sample firms trade on average 230 days (out of 252) in the 12-month period following the bankruptcy announcement month. In the comparable period, control firms trade, on average, 224 days, with difference in means significant at a ten percent level. This result indicates that bankrupt firm's stock is of interest for at least a group of investors. Panel B also highlights the very significant impact of the bankruptcy filing on the stock price. For sample firms, the average price falls from 4.97 dollars before the event to 2.08 in the event month, which represents a loss of -58 percent in value. The equivalent decline in median price is from 3.12 dollars to 0.97 dollars. In contrast, the

³⁹ In his seminal paper, Altman (1968) clarifies that firms with a z-score lower than 1.81 face a high probability of filing for bankruptcy within a year.

⁴⁰ According to the official information available on the NYSE Euronext website on 2 March 2008, the median market capitalization of the NYSE composite index companies is 1.9 billions of dollars. For more up-to-date information go to http://www.NYSE.com/marketinfo/indexes/nyse_characteristics.shtml.

market price of the benchmark firms remains relatively stable, with a mean value of around 8 dollars (median = 4.50 dollars).

Panel B of table 3.2 again shows that there is a market for the stock of bankrupt companies. In fact, in the 12-months before the bankruptcy date, the average daily turnover for these firms is 0.51 percent, implying an annual turnover of 129 percent. This rate spikes to 290 percent in the bankruptcy-announcement month, which shows the importance of the event under analysis. After the dissipation of this initial effect, the average daily turnover of the bankrupt firms stabilizes at 0.57 percent. Taken together, these figures reveal an increase in the demand for these companies' stock from the pre- to the post-event period and corroborate my early conclusion that at least some investors are keen on trading the stock of bankrupt companies, a phenomenon also documented by Hubbard and Stephenson (1997). The data also shows that this pattern is specific to my event firms. In fact, in the case of the control sample, the daily turnover does not exhibit any obvious variation, with a mean estimated value of around 0.43 percent for the entire period.

Finally, panel C of table 3.2 shows that only 25 percent of the sample firms have positive earnings and around the same percentage are paying dividends to their shareholders. In line with panel A, panel C again suggests that the benchmark companies are financially stronger than my sample firms. In fact, almost 50 percent have positive earnings and around 40 percent are paying cash dividends. Panel C shows that both sample and matched firms are usually audited by one of the Big 8 auditing firms.⁴¹ Around a quarter of the bankrupt firms receive a first-time going-concern audit opinion in their accounts on the year preceding their Chapter 11 filing.⁴² The proportion of matched firms in the same situation is much lower: two percent.

⁴¹ 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 & Touche; 6) Peat, Marwick and Main; 7) PriceWaterhouseCoopers and 8) Touche Ross. A number of these accounting firms merged with each other throughout the years (see COMPUSTAT page 27 of the data definitions section for details). All eight companies are used in my auditor quality proxy because my sample period goes back as far as 1979.

⁴² I define the going-concern status of both sample and control firms as Kausar, Taffler and Tan (2008). Accordingly, the statistics now presented refer to companies receiving a first-time going-concern audit report modification in the year preceding their Chapter 11.

Table 3.2

Summary statistics

This table presents summary statistics relating to my 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, I 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

	Sample firms (A)		Matched firms (B)		Difference (A-B)			
	Mean	Median	Mean	Median	Mean	P-value	Median	P-value
Sales	596.4	116.9	634.9	129.5	-38.5	0.7786	-12.6	0.3301
TA	646.6	89.7	754.6	128.1	-108.0	0.5532	-38.4	0.2360
ROA	-19%	-6%	-15%	1%	-4%	0.2592	-7%	<0.0001
Z-Score	1.37	1.31	2.14	2.12	-0.77	0.0040	-0.81	0.0049
CUR	169%	128%	231%	178%	-62%	0.0008	-50%	<0.0001
LEV	45%	40%	36%	33%	9%	0.0006	7%	0.0005

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.

Panel B: Market related variables

	Sample firms (A)		Matched firms (B)		Difference (A-B)			
	Mean	Median	Mean	Median	Mean	P-value	Median	P-value
Size	160.0	32.3	159.6	32.2	0.5	0.9901	0.04	0.7378
Book/Market	4.2	2.3	3.8	2.2	0.4	0.4321	0.1	0.5432
Pre price	4.97	3.12	9.80	5.49	-4.83	<0.0001	-2.37	<0.0001
Event Price	2.08	0.97	8.67	4.38	-6.59	<0.0001	-3.41	<0.0001
Pos Price	2.98	0.71	8.84	4.27	-5.86	<0.0001	-3.56	<0.0001
Pre Volume	0.51%	0.34%	0.44%	0.25%	0.07%	0.0559	0.09%	0.0026
Event Volume	1.15%	0.61%	0.42%	0.23%	0.73%	<0.0001	0.38%	<0.0001
Pos Volume	0.57%	0.30%	0.43%	0.24%	0.14%	0.1887	0.06%	0.0275
Pre Tdays	250	252	227	249	23	<0.0001	3	<0.0001
Pos Tdays	230	246	224	248	6	0.0951	-2	0.0058

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 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 3.2 (cont.): Summary statistics

Panel C: Other Characteristics

	Sample firms		Matched firms	
	Positive cases	% of sample	Positive cases	% of sample
EPS	88	25.1	172	49.0
Divid	91	25.9	134	38.2
Big8	287	81.8	294	83.8
Opinion	263	0.75	344	0.98
Delist	195	55.6	-	-

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, Taffler and Tan (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.

3.4.2 Main results

I now turn to the analysis of my main results. Consistent with previous research, I find that the equity market anticipates the formal announcement of bankruptcy (e.g., Clark and Weinstein, 1983; Datta and Iskandar-Datta, 1995, Dawkins, Bhattacharya and Bamber, 2007). In fact, panel A of table 3.3 shows that, depending on the adopted benchmark, the mean (median) one-year pre-event risk-adjusted abnormal returns range from -89 to -49 percent (-91 to -43 percent). All values are highly statistically significant ($p < 0.0001$). These findings indicate that important information about the forthcoming bankruptcy event is leaked into the market well before the Chapter 11 date (e.g., Clark and Weinstein, 1983; Dawkins and Rose-Green, 1998). Studies focusing on the trading pattern of corporate insiders around the bankruptcy date explain how such information may be relayed to the market (e.g., Seyhun and Bradley, 1997 and Ma, 2001). The actions of the media are also an important determinant in this context (Dawkins and Rose-Green, 1998).

Panel B of table 3.3 shows a strong and negative market reaction to the announcement of bankruptcy. In fact, regardless of the adopted benchmark, the mean (median) risk-adjusted abnormal return measured for the (-1,+1) window is around -26 percent (-27 percent) and

highly significant ($p < 0.0001$). My results are in line with previous research on this topic and reinforce the idea that the bankruptcy event is highly value-relevant (e.g., Datta and Iskandar-Datta, 1995; Rose-Green and Dawkins, 2002; Dawkins, Bhattacharya and Bamber, 2007).

Panel C of table 3.3 presents this chapter's key result. In fact, I find a negative and statistically significant post-bankruptcy drift, lasting at least one full year after the event date. Depending on the benchmark, the mean (median) risk-adjusted abnormal return for the (+2,+252) period ranges from -44 to -24 percent (-61 to -24 percent). Both the parametric and non-parametric tests show that these values are highly statistically significant ($p < 0.0001$).⁴³ The 6-month post-event period represented by the (+2,+126) compounding window provides further evidence in favour of an incomplete market reaction to bankruptcy announcements. The mean risk-adjusted return here varies between -17 percent ($p = 0.0002$) and -14 percent ($p < 0.0001$). The corresponding median ranges from -22 to -18 percent ($p < 0.0001$ in both cases).⁴⁴

Of special interest in the context under analysis is the 4-month post-event period portrayed by the (+2,+84) compounding window. In fact, according to the Bankruptcy Reform Act of 1978, the incumbent management of firms filing for Chapter 11 has an exclusivity period of 120 days to develop a reorganization plan.⁴⁵ Hence, this is the period where the asymmetry of information between the bankrupt firms' management and the market is more acute. Panel C of table 3.3 shows that the point estimate for the size and book-to-market mean adjusted abnormal return is now of -12 percent ($p = 0.0014$). The corresponding median is -14 percent, which is also highly significant ($p < 0.0001$). Market-adjusted results provide additional evidence supporting the previous results (mean = -14 percent, $p < 0.0001$; median = -22 percent, $p < 0.0001$).⁴⁶

⁴³ The point estimate for the mean un-winsorized size and book-to-market risk-adjusted abnormal return for the (+2,+252) window is -24.1 percent, with a p-value of 0.0013.

⁴⁴ The point estimate for the mean un-winsorized size and book-to-market risk-adjusted abnormal return for the (+2,+126) window is -20.6 percent, with a p-value of 0.0032.

⁴⁵ 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 interested in the stock of bankrupt companies.

⁴⁶ The point estimate for the mean un-winsorized size and book-to-market risk-adjusted abnormal return for the (+2,+84) window is -16.2 percent, with a p-value of 0.0088.

Table 3.3
Market reaction to Chapter 11

This table presents buy-and-hold abnormal returns for my 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. Market adjusted (using CRSP equally weighted index as benchmark) are reported in the two first 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, I 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: Standard event study - pre-event returns

	Market Adjusted Returns		Control Firm Benchmark	
	Mean	Median	Mean	Median
(-252,-2)	-0.89 <0.0001	-0.91 <0.0001	-0.49 <0.0001	-0.43 <0.0001
(-126,-2)	-0.62 <0.0001	-0.64 <0.0001	-0.42 <0.0001	-0.42 <0.0001

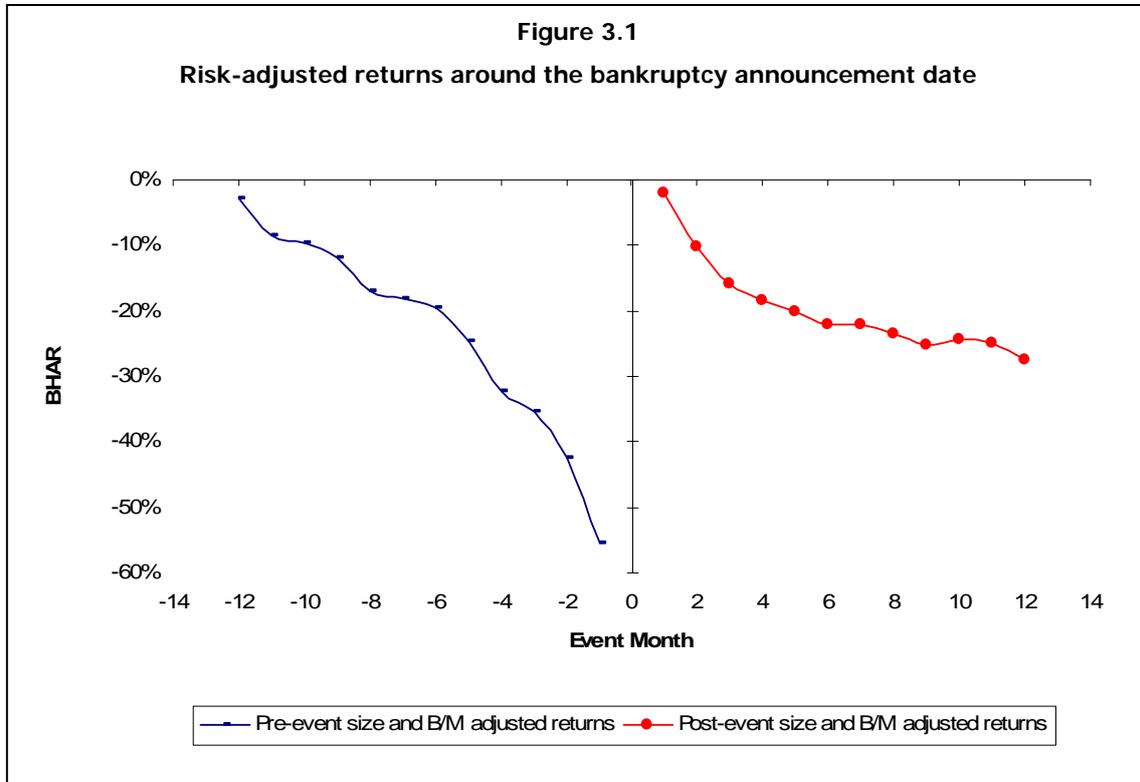
Panel B: Standard event study: market reaction around the Chapter 11 announcement date

	Market Adjusted Returns		Control Firm Benchmark	
	Mean	Median	Mean	Median
(-1,+1)	-0.27 <0.0001	-0.28 <0.0001	-0.26 <0.0001	-0.27 <0.0001
(-2,+2)	-0.28 <0.0001	-0.31 <0.0001	-0.27 <0.0001	-0.31 <0.0001

Panel C: Longer-horizon post-event abnormal returns

	Market Adjusted Returns		Control Firm Benchmark	
	Mean	Median	Mean	Median
(+2,+84)	-0.14 <0.0001	-0.22 <0.0001	-0.12 0.0014	-0.14 0.0001
(+2,+126)	-0.14 <0.0001	-0.22 <0.0001	-0.17 0.0002	-0.18 0.0001
(+2,+252)	-0.44 <0.0001	-0.61 <0.0001	-0.24 <0.0001	-0.24 <0.0001

For illustrative purposes, figure 3.1 graphs the mean size and book-to-market risk adjusted BHARs over a period of 25 months centred on the bankruptcy announcement month.⁴⁷



In line with table 3.3, figure 3.1 shows a sharp decline in the bankrupt firms' risk-adjusted returns in the pre-event period. More importantly, the same figure highlights the incomplete market reaction to the bad news conveyed by the bankruptcy announcement. The negative drift emerging after event-month zero portrays this phenomenon.

⁴⁷ Monthly returns are calculated following Kausar, Taffler and Tan (2008). To be precise, returns for 25 months centred on the bankruptcy announcement month are collected from CRPS monthly stock return file for both sample and control firms. The bankruptcy month is termed as the event month and excluded from the analysis. Equations (3.1) and (3.2) are then used to compute the abnormal returns presented above. The monthly post-event abnormal returns of interest are as follows: $BHAR_{(+1, +4)} = -0.18$ (p-value=0.0010), $BHAR_{(+1, +6)} = -0.22$ (p-value=0.0002) and $BHAR_{(+1, +12)} = -0.27$ (p-value=0.0023).

3.5 Summary and limitations

This chapter explores how the market deals with the announcement of corporate bankruptcy. I find that the mean size and book-to-market risk adjusted returns for my sample firms over the one-year pre-event period is -49 percent, significant at the one percent level, which suggests that the equity market is able to anticipate bankruptcy. I also find that the mean size and book-to-market risk adjusted returns over the 3-day interval centred on the Chapter 11 date is -26 percent, significant at the one percent level. My findings clearly suggest that the announcement of bankruptcy is a key event from an information perspective. This is not a surprising result, especially if one considers that filing for bankruptcy is surely the worst-case scenario in the corporate domain.

The most interesting finding comes from the analysis of the stock return pattern emerging after the event date. The evidence is not consistent with this chapter's null hypothesis, i.e., that the US equity market fully and quickly incorporates the impact of bankruptcy in the share price of the affected companies. Instead, I find a strong, negative and statistically significant post-event drift that lasts at least one full year after the Chapter 11 date. Such drift varies between -24 and -44 percent on average, depending on the benchmark adopted to measure the abnormal returns. My findings are clearly inconsistent with the semi-strong form of the EMH and provide evidence in favour of the behavioural argument that the market is unable to deal correctly with bad news events (e.g., Bernard and Thomas, 1989, 1990; Michaely, Thaler and Womack, 1995; Womack, 1996; Dichev and Piotroski, 2001; Chan, 2003; Taffler, Lu and Kausar, 2004 and Kausar, Taffler and Tan, 2008).

Some caution should be exerted when reading the results above. In fact, there is still much debate surrounding the measurement of longer-term abnormal returns. Extant research shows that inferences for long-horizons tests "*require extreme caution*" (Kothari and Warner, 1997, p. 301) and even using the best available methods "*the analysis of long-run abnormal returns is treacherous*" (Lyon, Barber and Tsai, 1999). These contributions emphasize the earlier warnings about the reliability of long-horizon methods (e.g., Brown and Warner, 1980, p. 225). A casual

examination of the contemporaneous literature on market-pricing anomalies suggests that the best approach when dealing with longer-term event studies is exploring a combination of alternative methodologies in order to check the soundness of a given result (e.g., Boehme and Sorescu, 2002; Hertz et al, 2002; Ikenberry, Ramnath, 2002; Byun and Rozeff, 2003). The next chapter pursues exactly this avenue in an attempt to show that my main result is not a mere statistical artefact.

Chapter 4

Re-examining the Market's Reaction to Bankruptcy Announcements

4.0 Introduction

Beginning with the seminal papers of Brown and Warner (1980, 1985), a wide body of finance literature has carefully scrutinized the technology commonly used to conduct long-run event studies. Classical work here is that by Barber and Lyon (1997), Kothari and Warner (1997), Fama (1998), Lyon, Barber and Tsai (1999), Brav (2000), Mitchell and Stafford (2000) and Ang and Zang (2004), just to mention a few key contributions to this ongoing discussion. In a recent paper, Kothari and Warner (2007) survey most of these studies and summarize the state of the art as follows (p. 8): "*although long-horizon methods have improved, serious limitations of the long-horizon methods have been brought to light and still remain*". In a similar spirit, Lyon, Barber and Tsai (1999) argue that a critical point in assessing the accuracy of long-horizon event studies is to conduct a number of robustness tests designed to overcome known problems with particular methodologies. Fama (1998) additionally points out that one can only be more confident about his findings when the results are not particularly sensitive to reasonable methodological changes.

This chapter aims to achieve this objective in the context of my research and I focus my attention on the post-bankruptcy period. The chapter is divided in four parts. The first borrows heavily from the work of Kausar, Taffler and Tan (2008), Lyon, Barber and Tsai (1999) and Ang and Zang (2004) and reports what happens when I use different methods to compute\test the post-bankruptcy abnormal returns. The second shows that the anomaly documented in the previous chapter persist even after a number of alternative explanations are examined. The third section presents a final robustness test that employs the calendar-time portfolio method suggested by Fama (1998) and Mitchell and Strafford (2000) to further check the validity of my initial findings. The last part concludes.

4.1 Revisiting the computation and significance of the post-bankruptcy abnormal returns

4.1.1 Small firm reinvestment bias

One critical issue when computing buy-and-hold abnormal returns of small, low-price and highly distressed companies is dealing effectively with event firms that are delisted during the compounding window. Kausar, Taffler and Tan (2008) claim that, in this particular setting, reinvesting the proceeds from these companies' delisting in each firm's size decile is inappropriate because such a procedure introduces an upwards bias in the computation of abnormal returns and argue that a more reasonable approach is to assume zero abnormal returns in the post-delisting period. Put simply, the authors suggest that, once a firm is delisted, any proceeds should be reinvested in a neutral market portfolio so that the same market portfolio also represents the relevant benchmark post-delisting. Crucially, Kausar, Taffler and Tan (2008) demonstrate that this alternative method does minimize the reinvestment bias, allowing better estimates of abnormal returns for their sample of small, low-price and highly distressed GCM-stocks.

In order to investigate to what extent my results are influenced by the small firm reinvestment bias documented by Kausar, Taffler and Tan (2008), I re-run my initial event study considering that delisted firms earn zero post-delisting abnormal returns. To be precise, I use the method described in section 3.3 to compute the post-bankruptcy abnormal returns of my event companies but assume that the delisting proceeds of sample firm i are reinvested in its control firm based on size and book-to-market after its delisting date.

Moreover, drawing on Kausar, Taffler and Tan (2008) I also conduct a robustness test that assumes that the proceeds of delisted bankrupt firms are reinvested in the CRSP value-weighted market index.⁴⁸

⁴⁸ Kausar, Taffler and Tan (2008) also use the S&P 600 Small Cap. Index as an additional source of reinvestment for the proceeds of their delisted GCM firms. Using this alternative is not feasible in my context because such an index starts in 31 December, 1993.

Table 4.1 summarizes the results. In panel A, delisting proceeds are reinvested in control firms sharing similar size and book-to-market. I find that this alternative method does not change the qualitative nature of the results reported in chapter 3: all mean and median post-bankruptcy BHARs computed under this new setup are still negative and highly significant at normal levels.⁴⁹

Panel B reports what happens when I assume that the delisting proceeds are reinvested in the CRSP value-weighted index. Again, results do not change significantly since all computed BHARs are still negative and statistically significant at normal levels.⁵⁰ In face of this evidence, I conclude that my initial results are not influenced by the small firm reinvestment bias documented by Kausar, Taffler and Tan (2008).

⁴⁹ Un-winsorized results are very similar to those reported here.

⁵⁰ Un-winsorized results are very similar to those reported here.

Table 4.1
Controlling for the small firm reinvestment bias

Panel A presents buy-and-hold abnormal returns for my 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, assuming that the delisting proceeds of bankrupt firms are reinvested in their control firms based on size and book-to-market. All compounding periods are in trading days, where day zero is the Chapter 11 date. Market adjusted (using CRSP equally weighted index as benchmark) are reported in the two first 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, I 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: Delisting proceeds are reinvested in a matched firm sharing similar size and book to market

	Market Adjusted Returns		Control Firm Benchmark	
	Mean	Median	Mean	Median
(+2,+84)	-0.14 <0.0001	-0.24 <0.0001	-0.13 0.0014	-0.15 <0.0001
(+2,+126)	-0.20 <0.0001	-0.33 <0.0001	-0.16 0.0005	-0.16 0.0001
(+2,+252)	-0.48 <0.0001	-0.67 <0.0001	-0.28 <0.0001	-0.27 <0.0001

Panel B presents buy-and-hold abnormal returns for my 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, assuming that the delisting proceeds of bankrupt firms are reinvested in the CRSP value-weighted index including dividends. All compounding periods are in trading days, where day zero is the Chapter 11 date. Market adjusted (using CRSP equally weighted index as benchmark) are reported in the two first 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, I 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 B: Delisting proceeds are reinvested in CRSP value-weighted index

	Market Adjusted Returns		Control Firm Benchmark	
	Mean	Median	Mean	Median
(+2,+84)	-0.14 <0.0001	-0.22 <0.0001	-0.13 0.0017	-0.15 0.0001
(+2,+126)	-0.21 <0.0001	-0.32 <0.0001	-0.17 0.0002	-0.18 0.0002
(+2,+252)	-0.43 <0.0001	-0.62 <0.0001	-0.24 0.0004	-0.26 <0.0001

4.1.2 Testing the statistical significance of longer-term abnormal returns

One of the most elusive issues when dealing with event studies is determining the statistical significance of the abnormal returns. Lyon, Barber and Tsai (1999) carefully examine this question and conclude that, in particular situations, the simple t-test may yield biased results. The authors recommend using a bootstrapped skewness-adjusted t-statistic to overcome this problem.⁵¹ The corrected t-statistic, t_{sa} , is given by:

$$t_{sa} = \sqrt{n} \left(S + \frac{1}{3} \hat{\gamma} S^2 + \frac{1}{6n} \hat{\gamma} \right) \quad (4.1)$$

where $\hat{\gamma}$ is an estimate of the coefficient of skewness, $S\sqrt{n}$ is the value of the traditional (not-corrected) t-statistic and n is the total number of firms with available information. Lyon, Barber and Tsai (1999) suggest computing $\hat{\gamma}$ and S as follows:

$$\hat{\gamma} = \frac{\sum_{i=1}^n (Ar_{i,\tau} - \overline{Ar}_{\tau})^3}{n \sigma(Ar_{\tau})^3} \quad (4.2)$$

$$S = \frac{\overline{Ar}_{\tau}}{\sigma(Ar_{\tau})} \quad (4.3)$$

where $Ar_{i,\tau}$ is the buy-and-hold return for firm i calculated as in equation (3.1), \overline{Ar}_{τ} is the mean sample BHAR computed as in equation (3.2) and $\sigma(Ar_{\tau})$ is the cross-sectional standard deviation of abnormal returns of the n firms with available information.

The bootstrap procedure is implemented here as in Lyon, Barber and Tsai (1999). I draw 1,000 bootstrapped resamples from my original sample, each containing 88 firms.⁵² For each resample, I calculate the statistic:

⁵¹ Another possibility is to use an empirically generated distribution of mean long-run abnormal stock returns from pseudoportfolios as in Ikenberry, Lakonishok and Vermaelen (1995).

⁵² This number of firms per sub-sample complies with Lyon, Barber and Tsai's (1999) recommendation of using one fourth of the total sample firms in each bootstrapped sort (p. 174).

$$t_{b,sa} = \sqrt{n_b} \left(S_b + \frac{1}{3} \hat{\gamma}_b S_b^2 + \frac{1}{6n_b} \hat{\gamma}_b \right) \quad (4.4)$$

where $\hat{\gamma}_b$ and S_b are computed as above. It follows that $t_{b,sa}$, $\hat{\gamma}_b$ and S_b are the bootstrapped analogues of t_{sa} , $\hat{\gamma}$ and S from my original sample for the $b = 1, 2, \dots, 999, 1,000$ resamples. Following Lyon, Barber and Tsai (1999), I reject the null hypothesis that the mean medium-term abnormal return is zero if: $t_{sa} < x_l^*$ or $t_{sa} > x_u^*$. These two critical values, x_l^* and x_u^* , are computed from the 1,000 resamples by solving:

$$\Pr[t_{b,sa} \leq x_l^*] = \Pr[t_{b,sa} \geq x_u^*] = \frac{\alpha}{2} \quad (4.5)$$

where α is the significance level. In practice, in my application, α is the p-value, i.e., the lowest value of significance for which the null hypothesis of no abnormal returns would be rejected.

Recently, Ang and Zhang (2004) re-examine the issue of testing the statistical significance of long-run abnormal returns. The authors report that a combination of the non-parametric sign test and a single firm benchmark has more power than any other alternative they consider in their simulations. More importantly, the authors find that this is the only testing procedure that performs well for samples of small firms, a key contribution for my own research. As such, drawing on Ang and Zhang (2004), I use the sign test to re-assess the significance of the non-parametric results of chapter 3. In this context, the null hypothesis is that the median abnormal return is zero. The test statistic is as follows:

$$M = (n^+ - n^-) / 2 \quad (4.6)$$

where n^+ (n^-) is the number of BHARs that are greater (lower) than zero. Observations are discarded when the BHAR for firm i is zero.

The test statistic is compared to the observed statistic, given by:

$$M_{obs} = 0.5^{(n_t-1)} \sum_{j=0}^{\min(n^+, n^-)} \binom{n_t}{j} \quad (4.7)$$

where $n_t = n^+ + n^-$ is the number of non-null BHARs and j is the number of firms with a valid BHAR.

Table 4.2 shows the results.⁵³ The evidence is clearly in line with that of chapter 3. Despite the change in methodology, all mean and median BHARs are still highly statistically significant at normal levels. In other words, the post-bankruptcy drift seems robust to alternative ways of assessing the statistical significance of medium-term abnormal returns.

Table 4.2
Revisiting the significance of the post-bankruptcy abnormal returns

This table presents buy-and-hold abnormal returns for my 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. Market adjusted (using CRSP equally weighted index as benchmark) are reported in the first two columns. The last two columns report the results using a control firm approach. Firms are matched according to size and book-to-market. Specifically, for each sample company, I 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. All p-values associated with mean results are obtained using the skewness-adjusted bootstrapping method of Lyon, Barber and Tsai (1999), with 1,000 replications. The two-tailed significance level from a sign test is reported below the corresponding median.

	Market Adjusted Returns		Control Firm Benchmark	
	Mean	Median	Mean	Median
(+2,+84)	-0.12 0.0040	-0.22 0.0020	-0.16 0.0040	-0.14 0.0002
(+2,+126)	-0.20 0.0020	-0.22 <0.0001	-0.21 0.0020	-0.18 0.0001
(+2,+252)	-0.42 0.0040	-0.61 <0.0001	-0.24 0.0060	-0.24 0.0015

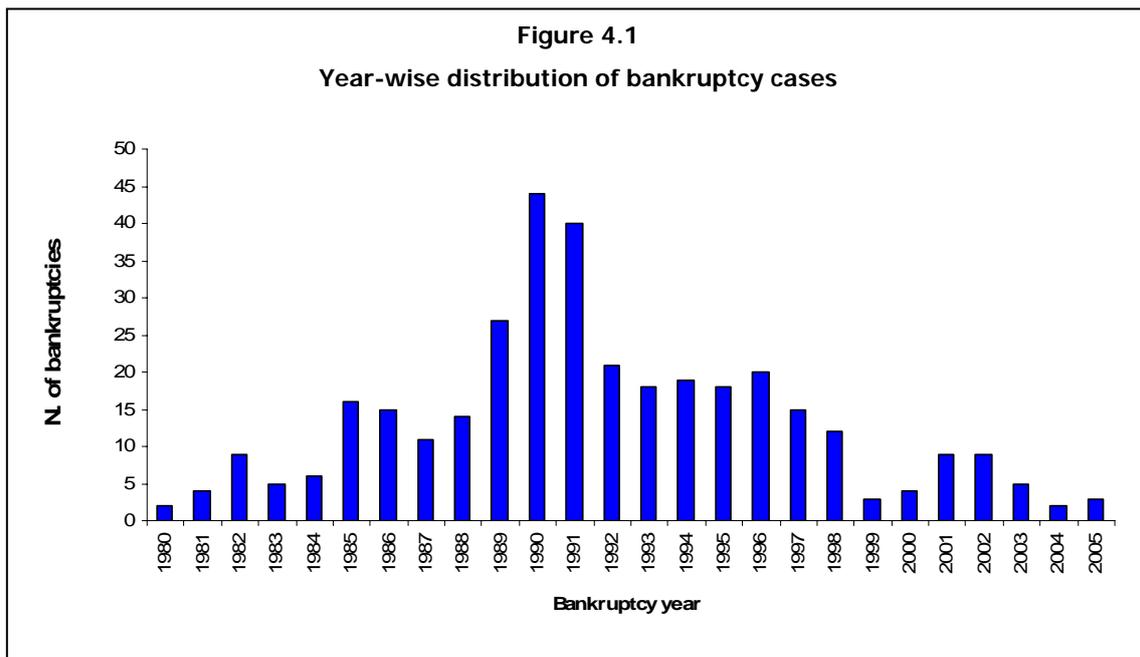
⁵³ Table 4.2 reports unwisORIZED mean BHARs for all compounding periods.

4.2 Consistency

This section examines the consistency of the post-bankruptcy drift uncovered in the previous chapter. Following extant research, I use the results from the control firm approach based on size and book-to-market to implement all consistency tests summarized below (e.g., Ikenberry, Lakonishok and Vermaelen, 1995; Michaely, Thaler and Womack, 1995; Spiess and Affleck-Graves, 1995; Dichev and Piotroski, 2001; Ikenberry and Ramnath, 2002).

4.2.1 Consistency by year

In this section I examine whether the negative price drift following a Chapter 11 bankruptcy announcement is confined to specific years. It is possible that the mispricing of bankrupt firms may have existed in the earlier periods of my sample and then was recognized by investors and has since disappeared (Liu, Szewczyk and Zantout, 2008). Additionally, it is possible that increased transparency, wider diffusion of information and improved regulation and oversight have led to an increase in the efficiency of capital markets, in which case the mispricing of bankrupt firms should be concentrated on the initial years of my sample (Liu, Szewczyk and Zantout, 2008). Figure 4.1 shows the year-wise distribution of my bankruptcy cases:



As can be seen, my sample is not concentrated in any particular period, although an important number of bankruptcies occur during 1990 (44 cases) and 1991 (40 cases). As such, it is important to test if my results are similar across different sub-periods within my sample period and not driven not solely by a few atypical years. Following Michaely, Thaler and Womack (1995), I investigate this possibility by splitting the sample in two periods so that there is approximately the same number of cases in each sub-period. A t-test and a Wilcoxon-Mann-Whitney test are then used to investigate if there is a difference in performance between the firms included in these complementary periods.

Panel A in table 4.3 shows the results. There is some evidence that the post-bankruptcy drift is more pronounced in the earlier years of my sample, especially when the longer-term reaction of the market is considered. In effect, for the 1980-1991 period, the mean BHAR for the one-year window is -28 percent, whereas its counterpart for the 1992-2005 period is only -18 percent. A similar story is found when non-parametric results are analysed (median abnormal return is -28 percent for the 1980-1991 period and -16 percent for the 1992-2005 period). However, panel A of table 4.3 also shows that the mean and median differences now reported are not statistically significant even at a ten percent level. Thus, I can only conclude that the anomaly uncovered in chapter 3 is present throughout my entire sample period, not having significantly decayed over time.⁵⁴

4.2.2 Consistency by size

Existing research demonstrates that abnormal returns for small and large firms differ (e.g., Banz, 1981; Fama and French, 1992). Moreover, previous research reports that an incomplete market reaction to bad news is more likely to occur in the case of smaller firms (e.g., Dichev and Piotroski, 2001; Chan, 2003). As such, investigating the impact of size in my initial results is a very important consistency test. In order to do so, I split my sample into two groups

⁵⁴ Splitting the sample in shorter intervals is problematic due to my small sample size. However, in unreported results, I divide the sample in three and four periods following the same rule of having roughly the same number of observations in each sub-period. Results are consistent with those reported here, i.e., there is some evidence that the anomaly is somewhat more pronounced in the initial years of the sample.

conditional on firm size.⁵⁵ Firms with a market capitalization lower than that of the total sample's median market capitalization are allocated to the smallest firm portfolio, while the remaining ones form the small firm portfolio. A t-test and a Wilcoxon-Mann-Whitney test are employed to investigate if there is a difference in performance between these two size portfolios.

Panel B of table 4.3 summarizes the results. I find that the post-bankruptcy drift is present in both sets. In fact, all mean and median BHARs are negative and statistically significant for both portfolios. The evidence also suggests that the anomaly is somewhat more pronounced for the smallest firms. However, both the t-test and the Wilcoxon-Mann-Whitney test demonstrate this result is only significant for the first two compounding periods. Overall, I would argue that the medium-term post-bankruptcy announcement drift is not driven by a size effect. Nevertheless, this variable does seem to play an important role on the magnitude of the anomaly for the shorter post-event periods.

4.2.3 Consistency by book-to-market

Previous research highlights the importance of the book-to-market ratio in explaining the cross-section of expected returns (e.g., Fama and French, 1992; Lakonishok, Shleifer and Vishny, 1994). I use a similar approach to that described above in order to check the impact of this variable in my initial results. The key difference is that the sample is now divided into two groups conditional on the companies' book-to-market ratio.⁵⁶ To be precise, firms with a book-to-market ratio lower than that of the total sample's median book-to-market ratio are allocated to a low B/M portfolio; all others are classified as the high B/M portfolio.

Results are presented on panel C of table 4.3. I find that the post-bankruptcy drift is common to both book-to-market portfolios. In effect, irrespective of the particular compounding window, mean and median BHARs are always negative and statistically significant at normal levels for

⁵⁵ Size is measured as in section 3.3.2.

⁵⁶ The book-to-market ratio is measured as in section 3.3.2.

the two groups. Furthermore, the two last columns on panel C show that the mean and median performance of these portfolios is not statistically different in any of the compounding periods analysed here.

Table 4.3
Post-bankruptcy abnormal returns by year, size and book-to-market

Panel A presents buy-and-hold abnormal returns for my 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 year that the companies file for bankruptcy. 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, I 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 mean (median). In the last two columns, the two-tailed significance level from t-statistics or a Wilcoxon-Mann-Whitney test are reported below the corresponding mean or median difference.

Panel A: consistency by year

	(1) 1980-1991 (n=193)		(2) 1992-2005 (n=158)		Difference (1 - 2)	
	Mean	Median	Mean	Median	Mean	Median
(+2,+84)	-0.07 0.0911	-0.11 0.0811	-0.17 0.0021	-0.16 0.0004	0.10 0.2046	0.05 0.2256
(+2,+126)	-0.16 0.0088	-0.16 0.0161	-0.19 0.0083	-0.21 0.0028	0.03 0.6721	0.05 0.4808
(+2,+252)	-0.28 0.0006	-0.28 <0.0001	-0.18 0.0409	-0.16 0.0177	-0.10 0.5501	-0.12 0.3311

Panel B presents buy-and-hold abnormal returns for my 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 firms' size. Event firms with a market capitalization lower than that of the total sample's median market capitalization are allocated to the smallest firm portfolio; the remaining companies form the small firm 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, I 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. For the smallest and small firm columns, the two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the mean (median). In the last two 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 4.3 (cont.): Post-bankruptcy abnormal returns by year, size and book-to-market

Panel B: consistency by size

	Smallest firms (n=176)		Small Firms (n=175)		Difference (Smallest - Small)	
	Mean	Median	Mean	Median	Mean	Median
(+2,+84)	-0.20 <0.0001	-0.21 <0.0001	-0.06 0.0691	-0.05 0.0881	-0.14 0.0014	-0.16 0.0025
(+2,+126)	-0.24 <0.0001	-0.27 <0.0001	-0.11 0.0321	-0.09 0.0023	-0.13 0.0014	-0.18 0.0045
(+2,+252)	-0.27 0.0019	-0.28 <0.0001	-0.22 0.0427	-0.21 0.0052	-0.05 0.3207	-0.07 0.2577

Panel C presents buy-and-hold abnormal returns for my 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 firms' book-to-market ratio. Firms with a book-to-market ratio lower than that of the total sample's median book-to-market ratio are allocated to a low B/M portfolio; the remaining companies form the high B/M portfolio. All compounding periods are defined in trading days, where day zero is the formal Chapter 11 filing date. A control firm approach based on size and book-to-market is used to estimate the abnormal returns. For each sample company, I 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. For the low B/M and high B/M columns, the two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median). In the last two columns, the two-tailed significance level from t-statistics or a Wilcoxon-Mann-Whitney test are reported below the corresponding mean or median difference.

Panel C: consistency by book-to-market

	Low B/M (n=176)		High B/M (n=175)		Difference (Low - High)	
	Mean	Median	Mean	Median	Mean	Median
(+2,+84)	-0.10 0.0069	-0.15 0.0047	-0.15 0.0023	-0.13 0.0005	0.05 0.2509	-0.02 0.2711
(+2,+126)	-0.13 0.0074	-0.14 0.0053	-0.22 0.0001	-0.22 <0.0001	0.09 0.1415	0.08 0.1076
(+2,+252)	-0.20 0.0294	-0.22 0.0566	-0.29 <0.0001	-0.27 <0.0001	0.09 0.1617	0.05 0.2195

4.3 Robustness

In this section, I conduct a number of tests to verify if the post-bankruptcy drift is due to alternative explanations already documented in the literature. In particular, I investigate the impact of earnings surprises, the post-going-concern modification drift, momentum, the industry effect, distress risk and the penny stock effect on my results. With these tests, I additionally control for possible shortcomings of my initial matching procedure and consider complementary sources of risk that may systematically affect the return pattern of bankrupt companies (Lyon, Barber and Tsai, 1999; Dichev and Piotroski, 2001).

4.3.1 Earnings announcements

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, Olsen and Shevlin, 1984; Bernard and Thomas, 1989, 1990). It is possible that a similar effect contaminates the results presented in the previous chapter, especially because the majority of my sample companies are severely financially distressed (see section 3.4).

Two tests are employed to investigate how the post-earnings announcement drift affects my initial findings. For the first, a new matched sample is defined specifically to control for this problem. The procedure is equivalent to that of section 3.3.2, but with the difference that, in this alternative context, earnings surprise and not the book-to-market ratio determines the benchmark firms (Kausar, Taffler and Tan, 2008). In particular, every control company is now the size-matched candidate with closest earnings surprise value to that of the sample firm. This technique allows me to separate out the post-bankruptcy drift from the 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 the negative earnings surprise firms. To test this preposition, I split my sample into two groups conditional on the sign of their pre-bankruptcy earnings surprise. I then use a t-test and a Wilcoxon-Mann-Whitney test to verify if there is a 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, Olsen and Shevlin (1984), I define this variable as follows:

$$\Delta Q_{i,q} = \frac{Q_{i,q} - E(Q_{i,q})}{|Q_{i,q}|} \quad (4.8)$$

where $\Delta Q_{i,q}$ is the earnings surprise for firm i for quarter q , $Q_{i,q}$ are the current quarterly earnings figure for firm i , $E(Q_{i,q})$ are the expected earnings figure for firm i in the current quarter and $|Q_{i,q}|$ is the absolute value of firm i 's current quarterly earnings. Following Dichev and Piotroski (2001), I define current quarter as the most recent quarter preceding the bankruptcy announcement date. Additionally, I define $E(Q_{i,q})$ as $E(Q_{i,q}) = Q_{i,q-4}$. This naïve model assumes that the expected earnings figure for firm i in the current quarter is simply the realised quarterly earnings for the same quarter in the previous year.⁵⁷ All data for calculating equation (4.8) are collected from COMPUSTAT's quarterly industrial files (COMPUSTAT item 8).

Table 4.4 summarizes my results. Panel A shows that my sample firms exhibit a strong post-bankruptcy drift even after controlling for earnings surprise. In fact, all mean and median BHARs are negative and most of them are statistically significant at the one percent level.

On the other hand, the parametric results of panel B suggest that the anomaly is more pronounced for those companies suffering from a negative pre-event earnings surprise. In particular, the point estimate for the one-year mean BHAR for the negative earnings surprise

⁵⁷ 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, Olsen and Shevlin (1984).

portfolio is -28 percent ($p < 0.0001$), while its equivalent for the positive earnings surprise portfolio is -13 percent ($p = 0.0374$). Furthermore, the t-test for differences in means for the (+2,+252) period is significant at the ten percent level. This result indicates that firms suffering a pre-bankruptcy negative earnings surprise have, on average, a more pronounced post-event drift.

Some caution is warranted here since there are only 88 sample firms with a positive earnings surprise before their Chapter 11 date. In fact, the analysis of the median BHARs shows a somewhat different story. In contrast with the parametric results, the Wilcoxon-Man-Whitney test reveals that the difference between the median BHARs of the negative and positive earnings surprise portfolios *is not* statistically significant, a conclusion that holds independently of the compounding window under scrutiny.

Overall, in face of this evidence, I would argue that my initial results are not severely contaminated by a potential post-earnings announcement drift. Nevertheless, there is some evidence, albeit weak, that the post-bankruptcy drift is somewhat more acute when the event is preceded by a negative earnings surprise.

Table 4.4
Controlling for earnings surprises

Panel A presents buy-and-hold abnormal returns for my 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, I 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

	<u>Control Firm Benchmark</u>	
	<u>Mean</u>	<u>Median</u>
(+2,+84)	-0.09 0.0267	-0.11 0.0011
(+2,+126)	-0.15 0.0008	-0.16 <0.0001
(+2,+252)	-0.32 <0.0001	-0.33 <0.0001

Panel B presents buy-and-hold abnormal returns for my 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, I 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.

Panel B: Controlling for earnings surprise – earnings surprise sign

	<u>Negative Earnings (n=263)</u>		<u>Positive Earnings (n=88)</u>		<u>Difference (Neg - Pos)</u>	
	<u>Mean</u>	<u>Median</u>	<u>Mean</u>	<u>Median</u>	<u>Mean</u>	<u>Median</u>
(+2,+84)	-0.15 0.0011	-0.16 0.0001	-0.06 0.0165	-0.11 0.0479	-0.09 0.3337	-0.05 0.4578
(+2,+126)	-0.19 0.0003	-0.15 0.0003	-0.10 0.0419	-0.22 0.0343	-0.09 0.4204	0.07 0.5716
(+2,+252)	-0.28 <0.0001	-0.25 <0.0001	-0.13 0.0374	-0.19 0.0218	-0.15 0.0546	-0.06 0.1532

4.3.2 First-time going-concern opinions

Taffler, Lu and Kausar (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 percent over this period. In a subsequent paper, Kausar, Taffler and Tan (2008) find that the US equity market also underreacts to the same event, documenting a downward drift of around 14 percent over the one-year period after the announcement date. These are important results for my own research. In fact, panel C of table 3.2 shows that around a quarter of my sample firms receive a first-time going-concern audit report modification in their last accounts prior to filing for Chapter 11. Accordingly, it could be argued that the post-bankruptcy drift is simply a manifestation of the post-going concern underperformance already documented in the literature.

I explore this issue by dividing my sample into two groups. The GCM portfolio refers to those firms receiving a first-time going concern audit report in their last published annual accounts before entering into bankruptcy proceedings. All other companies are allocated to the non-GCM portfolio. I then use a t-test and a Wilcoxon-Mann-Whitney test to investigate if there is a difference in performance between these two GCM groups. As pointed out by Kausar, Taffler and Tan (2008), separating between these two types of firms is not a straightforward task. In order to overcome this issue and catalogue each of my sample companies as either a GCM or a non-GCM case, I use a list of firms receiving a first-time going concern audit report in the US provided by Dr Asad Kausar.⁵⁸

Results are presented in table 4.5. I find that after explicitly controlling for the impact the variable under scrutiny, all mean and median BHARs are still negative and significant. Accordingly, I conclude that my initial results are not driven by a post-going concern underperformance effect.

⁵⁸ The results presented below also control for the going concern status of the benchmark firms. In particular, 7 companies had to be replaced for this test.

Table 4.5
Controlling for the post-GCM drift

This table presents buy-and-hold abnormal returns for my 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 GCM status of the firm. Firms receiving a first-time GCM audit report in the last annual accounts reported before the Chapter 11 year are allocated to the GCM portfolio; the remaining firms are classified in the non-GCM portfolio. All compounding periods are defined in trading days, where day zero is the Chapter 11 date. A control firm approach based on size, book-to-market and GCM status is used to estimate the abnormal returns. Specifically, for each sample company, I identify all CRPS firms with a market capitalization between 70 and 130 percent of its equity market value and that have not received a first-time audit report in the year they are matched with the event firm. The respective control firm is then selected as that firm with book-to-market closest to that of the sample firm. For the Non-GCM and GCM columns, the two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median). In the last two columns, the two-tailed significance level from t-statistics or a Wilcoxon-Mann-Whitney test are reported below the corresponding mean or median difference.

	Non-GCM (n=263)		GCM (n=88)		Difference (Non-GCM - GCM)	
	Mean	Median	Mean	Median	Mean	Median
(+2,+84)	-0.14 0.0011	-0.16 <0.0001	-0.11 0.0345	-0.11 0.0341	-0.03 0.6011	-0.05 0.2761
(+2,+126)	-0.18 0.0087	-0.22 <0.0001	-0.12 0.0387	-0.09 0.0411	-0.06 0.5412	-0.13 0.2562
(+2,+252)	-0.21 <0.0001	-0.24 <0.0001	-0.28 0.0043	-0.21 0.0001	0.07 0.5413	-0.03 0.5291

4.3.3 Momentum

In their seminal paper, Jegadeesh and Titman (1993) report that movements in the stock price over the period of 6- to 12-months tend to predict future movements in the same direction. Their results were subsequently documented in multiple settings and, nowadays, momentum is one of the most widely accepted violations of the EMH (e.g., Fama, 1998; Hong, Lim and Stein, 2000; Jegadeesh and Titman, 2001; Kothari, 2001). It is quite possible that my initial findings are simply a manifestation of the momentum anomaly since the previous chapter clearly shows that stock prices fall steeply in the pre-bankruptcy period (see panel A of table 3.3).

To investigate the impact of momentum, I adopt the same methodology as in section 4.3.1 and conduct two separate tests. For the first, a new matched sample is constructed as follows (Kausar, Taffler and Tan 2008). I start by identifying all firms with a market value of equity

between 70 and 130 percent of that of my sample companies' market capitalization. From this set, I choose the firm with momentum closest to that of the sample company if it complies with all the requirements mentioned in section 3.3.2. In the second test, I split my sample according to the firms' past momentum. Positive momentum companies are assigned to the positive momentum portfolio; the remaining ones are allocated to the negative momentum portfolio. I then use the results obtained with my matched sample based on size and book-to-market and a t-test and a Wilcoxon-Mann-Whitney test to investigate if there is a difference in performance between these two momentum groups. I compute momentum for both sample and control firms as follows:

$$Mom_i = \frac{1}{12} \sum_{t=-12}^{-1} R_{i,t} \quad (4.9)$$

where Mom_i is the momentum for firm i and $R_{i,t}$ is the raw monthly return of firm i in month t , with $t=0$ being the bankruptcy announcement month. All data for computing momentum for both sample and benchmark companies are taken from CRSP's monthly stock return file.

Table 4.6 summarizes the results. The main conclusion from panel A is that the momentum effect is not driving my initial results. In fact, after controlling for the impact of this variable, the post-bankruptcy abnormal returns are still negative and statistically significant (all mean and median BHARs are negative and most of them are significant at the one percent level). In addition, panel B of table 4.6 shows that the difference between the performance of the positive and negative momentum portfolios *is not* statistically significant given that none of the t and Wilcoxon-Mann-Whitney tests are statistically different from zero at normal levels. As such, it is only fair to conclude that my initial results cannot be explained in terms of prior return continuation.

Table 4.6
Controlling for momentum

Panel A presents buy-and-hold abnormal returns for my 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 momentum. Specifically, for each sample company, I 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 momentum 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 momentum - adjusted returns

	Control Firm Benchmark	
	Mean	Median
(+2,+84)	-0.11 0.0114	-0.15 0.0028
(+2,+126)	-0.16 0.0006	-0.17 0.0002
(+2,+252)	-0.25 <0.0001	-0.32 <0.0001

Panel B presents buy-and-hold abnormal returns for my 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 their pre-event momentum. Firms with positive pre-event momentum are allocated to the positive momentum portfolio; all others are classified in the negative momentum portfolio. All compounding periods are defined in trading days, where day zero is the formal Chapter 11 filing date. A control firm approach based on size and book-to-market is used to estimate the abnormal returns. Specifically, for each sample company, I 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. For the Negative and Positive momentum columns, the two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median). In the last two columns, the two-tailed significance level from t-statistics or a Wilcoxon-Mann-Whitney test are reported below the corresponding mean or median difference.

Panel B: Controlling for momentum – momentum sign

	Negative mom. (n=294)		Positive mom. (n=57)		Difference (Neg - Pos)	
	Mean	Median	Mean	Median	Mean	Median
(+2,+84)	-0.11 0.0059	-0.14 0.0012	-0.17 0.0563	-0.17 0.0224	0.06 0.5894	0.03 0.7201
(+2,+126)	-0.14 0.0028	-0.16 0.0021	-0.28 0.0122	-0.25 0.0101	0.14 0.2578	0.09 0.4481
(+2,+252)	-0.22 0.0011	-0.23 <0.0001	-0.30 0.0925	-0.31 0.0285	0.08 0.6685	0.08 0.6623

4.3.4 Distress risk

Panel A on table 3.2 shows that the mean (median) Altman (1968) z-score for my sample companies is 1.37 (1.31), where a z-score inferior to 1.81 "*clearly fall into the bankruptcy category*". On this basis, the majority of my sample firms are financially distressed when filing for Chapter 11. Dichev (1998) suggest that firms with higher distress risk significantly underperform in the following year. A similar result is reported by Griffin and Lemmon (2002), who show that firms in an advance state of finance distress and with a low book-to-market ratio earn comparably low subsequent returns. It is therefore important to verify if the post-bankruptcy drift is not only a manifestation of the pre-event distress affecting my sample companies.

To explore the hypothesis that a financial distress factor is responsible for my results, I adopt the same methodology as in section 4.3.1 and conduct two separate tests. For the first, a new matched sample is constructed as follows. I start by identifying all firms with a market value of equity between 70 and 130 percent of my sample companies' market capitalization. From this set, I choose the firm with z-score closest to that of the sample company if it complies with all the requirements mentioned in section 3.3.2.^{59, 60} For the second test, I divide my sample according to the firms' distress profile and generate two sub-sets: the high-distress group, where z-score is ≤ 1.81 and the low-distress group, where z-score is > 1.81 . I then use the results obtained with my matched sample based on size and book-to-market and a t-test and a Wilcoxon-Mann-Whitney test to investigate if there is a difference in performance between these two distress portfolios.

Table 4.7 summarizes my results. Panel A shows that a financial distress factor is not responsible for the results found in the previous chapter. In fact, after controlling for the impact of this new variable, the post-bankruptcy abnormal returns are still negative and statistically significant (all mean and median BHARs are negative and significant at the one percent level).

⁵⁹ In unreported results, I also consider a matched sample based on industry and z-score and a second on industry, size and z-score. Results are very similar to those reported here.

⁶⁰ Z-scores for both sample and matched companies are determined using data from the fiscal year ending one year before the bankruptcy announcement year.

Moreover, panel B shows that there is no statistically significant difference between the medium-term performance of my high and low financial distress-risk portfolios. To be precise, none of the t and Wilcoxon-Mann-Whitney tests is statistically different from zero even at the ten percent level. As such, I conclude that my initial result cannot be explained in terms of my sample firms' pre-event financial distress profile.

Table 4.7
Controlling for distress risk

Panel A presents buy-and-hold abnormal returns for my population of 351 non-finance, non-utility firms listed on the NYSE, AMEX, 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 a size and distress risk. Specifically, for each sample company, I 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 Alman's (1968) z-score closest to that of the sample firm. The two-tailed significance level from t-statistics (Wilcoxon signed rank-test) are reported below the corresponding mean (median).

Panel A: Controlling for size and distress risk - adjusted returns

	Control Firm Benchmark	
	Mean	Median
(+2,+84)	-0.11 0.0033	-0.15 0.0001
(+2,+126)	-0.15 0.0003	-0.18 <0.0001
(+2,+252)	-0.34 <0.0001	-0.35 <0.0001

Panel B presents buy-and-hold abnormal returns for my population of 351 non-finance, non-utility firms listed on the NYSE, AMEX, 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 firms' distress risk pre-event profile. Companies with a z-score higher than 1.81 in the year before their bankruptcy year are allocated to the low-distress risk portfolio; all others are classified in the high-distress risk 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, I 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. For Low- and High-distress risk 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.

Panel B: Controlling for distress risk – distress risk sign

	High-dist. risk (n=249)		Low-dist. risk (n=102)		Difference (High - Low)	
	Mean	Median	Mean	Median	Mean	Median
(+2,+84)	-0.13 0.0025	-0.14 0.0040	-0.12 0.0793	-0.20 0.0183	-0.01 0.9967	0.06 0.9423
(+2,+126)	-0.17 0.0007	-0.18 0.0004	-0.18 0.0851	-0.16 0.0111	0.01 0.9646	-0.02 0.8717
(+2,+252)	-0.24 0.0041	-0.23 <0.0001	-0.29 0.0217	-0.31 0.0087	0.05 0.3236	0.08 0.3616

4.3.5 Industry

Industry clustering occurs when events are concentrated in a few particular industries. This is problematic because it reduces the power of the statistical tests used to verify the significance of abnormal returns (e.g., Dyckman, Philbrick and Stephan, 1984; Mackinlay, 1997). This issue is important in the context of my research since a number of papers report the existence of a contagion/competitive industry effect whenever a company files for bankruptcy (e.g., Lang and Stulz, 1992; Akhigbe, Martin and Whyte, 2005). Accordingly, and despite my descriptive analysis indicating that my sample is not affected by a significant degree of industry clustering, I still test for the possibility that my initial findings are driven by an industry effect.

To test for the impact of this variable, I follow the matching procedure used by Eberhart, Altman and Aggarwal (1999), and define a new matched sample based on three main characteristics: industry, size and book-to-market. The first step here is identifying all match candidates for each of my sample companies based on industry, defined according to the two-digit SIC code provided by COMPUSTAT. For sample companies, such code is appraised on the bankruptcy filing month. For benchmark firms, I check the SIC code for years -1 and 0, where 0 is the bankruptcy year. Only firms that did not change their two-digit SIC code in this period and have the same two-digit SIC code as the sample company are considered in the second step of the matching process. This second step consists in choosing the firms with similar size to that of the sample company.⁶¹ This is done in two stages. Firstly, for each of my sample companies, all potential industry-matched candidates are used to define a decile-based distribution of the size variable. Secondly, based on its size, the sample company is allocated to one of these size deciles. Only potential candidates that lie in the same size decile as the sample company are considered in the last phase of the matching procedure.⁶² This last step consists in choosing the company that has the closest book-to-market ratio to that of the sample firm.⁶³ The match is only confirmed if the candidate firm complies with all the

⁶¹ For match and sample companies, size is measured as in section 3.3.2.

⁶² I use a size-decile approach here because the alternative criterion of choosing a benchmark firm with a market capitalization within 70 and 130 percent of that of the sample firm results in a significant number of event firms not having a suitable control firm.

⁶³ For match and sample companies, book-to-market is measured as in section 3.3.2.

requirements mentioned in section 3.3.2.⁶⁴ After finding the new control sample based on this algorithm, I re-compute the post-bankruptcy medium-term BHARs and check their statistical significance as in chapter 3.

Table 4.8 summarizes my results. This table shows that, even after controlling for the industry effect, all mean and median BHARs are negative and statistically significant. Accordingly, I conclude that the post-bankruptcy drift is not driven by an industry effect.

Table 4.8
Controlling for industry

This table presents buy-and-hold abnormal returns for my 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 an industry, size and book-to-market criteria. Specifically, the benchmark company is defined as the firm with the same COMPUSTAT's two-digit SIC code, that lies on the same size decile as the sample firm and has the closest book-to-market ratio to that of the event company. The two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median).

	Control Firm Benchmark	
	Mean	Median
(+2,+84)	-0.09 0.0129	-0.12 0.0009
(+2,+126)	-0.16 0.0002	-0.17 <0.0001
(+2,+252)	-0.32 <0.0001	-0.32 <0.0001

⁶⁴ In untabulated results, I also consider a control sample based only on industry and size. Results remain substantially unchanged.

4.3.6 Low-price stocks

A striking characteristic of my sample companies is their low average stock price (see panel B on table 3.2). This is a concern since previous research suggests that apparent long-term market overreaction may be driven by computational problems associated with the returns of low-price stocks (e.g., Ball, Kothari and Shanken, 1995). Moreover, previous research reports that an incomplete market reaction to bad news is more likely to occur in the case of low-price firms (e.g., Dichev and Piotroski, 2001; Chan, 2003; Klein, Rosenfeld, Tucker, 2006).

To test the impact of this variable, I define a new matched sample based on two main characteristics: industry and stock price.⁶⁵ I use the procedure described in section 4.3.5 to control for the industry characteristic. After determining all suitable candidates based on industry, each control firm is then defined as the company with the closest stock price to that of the sample firm. For event firms, the stock price is measured two days before the event day; for benchmark firms, the stock price is measured two days after the bankruptcy date.⁶⁶ Once again, the match is only confirmed if the candidate firm complies with all the requirements mentioned in section 3.3.2.⁶⁷

As an additional test, I divide my sample into two portfolios according to their closing stock price at the end of the second day after bankruptcy (Kausar, Taffler and Tan, 2008). In particular, firms with closing price lower than that of the total sample's median closing stock price are labelled as micro-penny stocks; all others are labelled as penny stocks. I then use the results obtained with my matched sample based on size and book-to-market and a t-test and a Wilcoxon-Mann-Whitney test to investigate if there is a difference in performance between these two penny stock portfolios.

⁶⁵ I do not explicitly control for size here because size and price are highly correlated (the Spearman correlation coefficient is 0.70, with $p < 0.0001$ and the Pearson correlation coefficient of 0.68, with $p < 0.0001$).

⁶⁶ This helps reducing the impact of the event on the matching variable. As a robustness check, I measure the stock price for all sample firms two, five and ten days before their bankruptcy date and re-run the analysis. Results remain qualitatively unchanged. Measuring sample firms' stock price in the post-event period also does not change my results in any meaningful way.

⁶⁷ In untabulated results, I also consider a control sample based only on the stock price. Results remain substantially unchanged.

Table 4.9 presents the results. Panel A shows that, after explicitly controlling for the penny stock effect, all mean and median BHARs are still negative and significant. Panel B, however, points to a different conclusion. In effect, the t-test (Wilcoxon-Mann-Whitney) for differences in means (medians) is statistically significant at the one percent (one percent) level for the (+2,+84) window and is statistically significant at the one percent (one percent) level for the (+2,+126) window. For the one-year post-event period, however, the t-test (Wilcoxon-Mann-Whitney test) for difference in means (medians) is not statistically significant (not statistically significant) at normal levels. As such, the evidence in panel B of table 4.9 suggests that my initial results are concentrated on the more low-price stocks, a phenomenon that is particularly clear for the shorter post-event periods.

Table 4.9
Controlling for low-price stocks

Panel A presents buy-and-hold abnormal returns for my 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 industry and stock price. Specifically, the benchmark company is defined as the firm with the same COMPUSTAT's two-digit SIC code of the bankrupt company and that has the closest closing stock price to that of the sample company. The two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median).

Panel A: Controlling for industry and low-price stocks – adjusted returns

	Control Firm Benchmark	
	Mean	Median
(+2,+84)	-0.13 0.0022	-0.14 <0.0001
(+2,+126)	-0.19 <0.0001	-0.20 <0.0001
(+2,+252)	-0.42 <0.0001	-0.34 <0.0001

Panel B presents buy-and-hold abnormal returns for my 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 closing price at the end of the second day after the bankruptcy filing date. Firms with closing price at the end of the second day after bankruptcy lower than that of the total sample's median closing stock price are labelled as micro-penny stocks; all others are labelled as penny stocks. 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, I 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. For the Micro Penny and Penny columns, the two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median). In the last two columns, the two-tailed significance level from t-statistics or a Wilcoxon-Mann-Whitney test are reported below the corresponding mean or median difference.

Panel B: Controlling for low-price stocks – post-bankruptcy stock price

	Micro Penny (n=176)		Penny (n=175)		Dif. (Micro Penny - Penny)	
	Mean	Median	Mean	Median	Mean	Median
(+2,+84)	-0.19 0.0002	-0.22 <0.0001	-0.05 0.7783	-0.07 0.1515	-0.14 0.0036	-0.15 0.0095
(+2,+126)	-0.25 <0.0001	-0.24 <0.0001	-0.09 0.0426	-0.08 0.0239	-0.16 0.0036	-0.16 0.0065
(+2,+252)	-0.28 0.0006	-0.27 <0.0001	-0.20 0.0048	-0.19 0.0006	-0.08 0.3982	-0.08 0.1784

4.3.7 Robustness tests using different control samples – a note

Lyon, Barber and Tsai (1999, p. 198) claim that using a control firm approach based on size and book-to-market may not be sufficient to overcome the limitations of the traditional capital asset pricing model. The authors elaborate and explain that long-term event studies relying solely on this risk-adjustment technique can produce biased estimates of the true long-term abnormal returns. Lyon, Barber and Tsai (1999) recommend researchers to look at other variables that may help explain the cross-section of stock returns and to use them in their long-term event studies as robustness tests.

The last sections implement this idea in the context of my research. A natural concern, however, arises from the fact that finding a truly independent set of benchmark samples is simply a difficult task. For instance, in the particular case of this study, most of the control samples comply with the same size requirement, something that is of crucial importance to ensure some methodological consistency across the different tests. Yet, it may also create a situation of serious cross-contamination between benchmark samples. This would occur if event firm i is always paired with the same control firm j across all (or the majority of) the benchmark samples, a possibility that is not explicitly accounted for in my matching procedures. Although it is feasible to introduce a restriction to overcome this problem, such action is not advisable. In fact, the all point of using a control sample based on firm-specific characteristics is to identify *the* non-event company that is most similar to the event firm across the attributes of interest. Restricting the matching procedure to ensure that the same company is not paired with a given event firm in complementary matches clearly defeats this purpose.

It follows that an important question that needs to be answered here is to what extent the different control samples used in this study are truly independent. In order to investigate this issue, I list and compare each pair of event and non-event firms across all the different benchmark samples employed in this study. After carefully analysing the results, I find that, in the worst possible situation, there are 26 cases where the same pair of event and non-event firms is used in two different control samples (this occurs for the *distress risk* and *industry, size*

and book-to-market benchmarks). Additionally, I find that my main size and book-to-market benchmark is almost completely unaffected by the problem at hand. To be precise, in the worst scenario, there are seven cases where the same pair of event and non-event firms is used in both my size and book-to-market and another control sample (*industry, size and book-to-market*). As such, in face of this evidence, I would argue that my results are not severely biased due to a problem of cross-contamination between control samples.

4.4 More robustness tests and a new estimation technique

So far, the evidence suggests that the US equity market does not react efficiently to the announcement of corporate bankruptcy. Am I confident about this result? Not quite. The reason is that, until now, all my findings are based on the BHAR method, which has been severely criticized in the recent years. Fama (1998) strongly argues against it because the systematic errors that arise with imperfect expected returns proxies are compounded with long-horizon returns. This is what he terms as the "*bad model problem*". More importantly, Fama (1998) also claims that any methodology that ignores the cross-sectional dependence of event firms' abnormal returns that are overlapping in calendar time is likely to produce overstated test statistics, a point also raised by Brav (2000).

Enthusiasts of the BHARs method argue that a carefully selected control sample mitigates the bad model problem. They additionally suggest the use of a bootstrapped procedure for conducting statistical inferences about long-term abnormal returns. The rationale is that this non-parametric alternative reduces the concerns related to the skewness of individual firms' long-horizon abnormal returns, which thwart the reliability of the parametric methods (e.g., Kothari and Warner, 1997; Lyon, Barber and Tsai, 1999; Loughran and Ritter, 2000). However, as Mitchell and Stafford (2000) emphasize, even this enhanced BHAR method does not account for the dependence problem highlighted by Fama (1998). In fact, major corporate events are usually not random: they cluster through time by industry (Mitchell and Stafford, 2000). Consequently, in most cases, event samples are unlikely to consist of independent observations as assumed in the BHAR approach. Mitchell and Strafford (2000) show that such lack of

independence leads to positive cross-correlation of abnormal returns, which generates BHAR test statistics that are severely overstated.

In a nutshell, both Fama (1998) and Mitchell and Stafford (2000) provide clear arguments against using BHARs to analyse long-term stock abnormal performance. Instead, they favour the calendar-time portfolio method introduced by Jaffe (1974) and Mandelker (1974). This alternative should produce better results since it accounts for the cross-sectional correlations of the individual event firms in the portfolio variance at each point in calendar time. As a final robustness test, I also implement this technique here.

4.4.1 Measuring long-term abnormal returns - a calendar-time portfolio approach

Following prior research, I use monthly returns to conduct this test (e.g., Boehme and Sorescu, 2002; Ikenberry and Ramnath, 2002; Byun and Rozeff, 2003; Liu, Szewczyk and Zantout, 2008). Sample firms are added to a portfolio of bankrupt stocks at the end of the month following their Chapter 11 date and are held there for 6 or 12 months. The portfolio is monthly rebalanced to drop all companies that reach the end of their 6- or 12-month holding period and add all firms that have just filed for bankruptcy in the previous calendar month.

Debate exists on how to best conduct the monthly rebalancing of the calendar-time portfolio. Two main alternatives have been put forward by the literature: equally or value-weighted rebalancing strategies. Given the high degree of skewness affecting the size variable of my event companies, I focus my attention on the former rather than on the latter.⁶⁸ Accordingly, the rebalancing of the portfolio is done assuming an equally weighted investment strategy where, in every month, all firms receive the same weight. As pointed out by Ikenberry and Ramnath (2002), this approach does not ensure that each firm has the same impact on the results. Yet, it allows for a higher degree of diversification, lowering the impact of idiosyncratic noise in my results, a critical aspect when dealing with failed firms (e.g., Gilson, 1995; Platt,

⁶⁸ Skewness of the size variable for the set of sample firms is 9.87.

1999, p. 110). Loughran and Ritter (2000) also argue that equal weighting is better because it does not obscure an eventual mispricing that is more likely to occur with smaller firms.

The abnormal performance of the calendar-time portfolio is assessed using the factor models of Fama and French (1993) and Carhart (1997). After regressing the portfolio's excess monthly returns on the independent variables of the benchmark models, I use the intercept α as a measure of abnormal return. If the predictions of the EMH hold, the intercept should not be statistically different from zero at normal levels. Conversely, estimates of the intercept that are statistically significant signal an incomplete market reaction to the event under scrutiny (Mitchell and Stafford, 2000).

Following previous research, I use the ordinary least squares (OLS) method to estimate the parameters of the Fama and French (1993) and Carhart (1997) models. However, this technique may yield inefficient estimates due to heteroskedasticity-related problems, which arise due to the monthly rebalancing since the variance is related to the number of firms in the portfolio. In an attempt to reduce the impact of heteroskedasticity on my results, I drop from the analysis all months where the calendar-time portfolio holds fewer than ten firms (e.g., Mitchell and Stafford, 2000 and Ikenberry and Ramnath, 2002). Additionally, the heteroskedastic-consistent t-statistic proposed by White (1980) is used to test the null hypothesis of no abnormal performance.

4.4.2 Measuring the abnormal performance

4.4.2.1 Unadjusted intercepts

The model proposed by Fama and French (1993) assumes that a stock's expected return is a linear function of the co-variability with the return on the market and two hedge portfolios related with size and book-to-market. Equation (4.10) describes the conceptual framework of this model:

$$E(r_p) - rf = b_p (E(r_m) - rf) + s_p E(SMB) + h_p E(HML) \quad (4.10)$$

where $E(r_p)$ is the expected return of portfolio p , rf is the risk-free rate, $E(r_m) - rf$, $E(SMB)$ and $E(HML)$ are, respectively, the expected 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 and 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. The parameters b_p , s_p and h_p measure portfolio's p sensibility to each of the three factors considered in the model.

In order to use Fama and French's (1993) model, appropriate values for b_p , s_p and h_p must be estimated. Equation (4.11) presents the time-series regression usually implemented to achieve this objective:

$$r_{p,t} - rf_t = \alpha_p + b_p (rm_t - rf_t) + s_p SMB_t + h_p HML_t + \varepsilon_{p,t} \quad (4.11)$$

where $\varepsilon_{p,t}$ is the disturbance term, assumed to be white noise, and all the remaining variables and parameters are defined as in equation (4.10). All data for performing this test are collected from Professor French's website.⁶⁹

⁶⁹ Go to <http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/> for details.

The model proposed by Carhart (1997) is very similar to that of Fama and French (1993) but it adds an extra factor to equation (4.10): the momentum factor. The revised model is given by:

$$E(r_p) - rf = b_p (E(r_m) - rf) + s_p E(SMB) + h_p E(HML) + u_p E(UMD) \quad (4.12)$$

where $E(UMD)$ is the average return on the two high prior return portfolios minus the average return on the two low prior return portfolios and u_p is the parameter that measures portfolio's p sensibility to the momentum factor. All other variables and parameters are defined as above. Again, the parameters of the Carhart (1997) model must be estimated using an auxiliary regression, which is usually the following:

$$r_{p,t} - rf_t = \alpha_p + b_p (rm_t - rf_t) + s_p SMB_t + h_p HML_t + u_p UMD_t + \varepsilon_{p,t} \quad (4.13)$$

where $\varepsilon_{p,t}$ is disturbance term, assumed to be white noise, and all the remaining variables and parameters have similar meanings as in equation (4.12). All data for performing this test are collected from Professor French's website.⁷⁰

4.4.2.2 Adjusted intercepts

Mitchell and Stafford (2000) claim that adjusting for risk using the Fama and French (1993) and the Carhart (1997) models may not be satisfactory. The argument is that these factor models cannot completely explain the cross-section of expected returns. For instance, Fama and French (1993) point out that three of the 25 portfolios formed based on size and book-to-market are associated with abnormal return estimates that are significantly different from zero. These portfolios are comprised of low book-to-market and small size firms, which is precisely the case of my event firms. In addition, Fama and French (1996) document a momentum bias for the three-factor model of equation (4.11). This evidence suggests that, in the worse case scenario, the estimate of the intercept under the null hypothesis of no abnormal performance may be biased when risk is adjusted by using the two factor models mentioned above.

⁷⁰ Go to <http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/> for details.

Drawing on Boehme and Sorescu (2002) and Ikenberry and Ramnath (2002), I try to overcome this problem by estimating adjusted intercepts that are derived using an arbitrage portfolio that is long in the stock of bankrupt companies, and short in that of the control firms used earlier in this chapter. To be precise, I implement the following regressions:

$$r_{p,t} - r_{control,t} = \hat{a}_p + b_p (rm_t - rf_t) + s_p SMB_t + h_p HML_t + \varepsilon_{p,t} \quad (4.14)$$

$$r_{p,t} - r_{control,t} = \hat{a}_p + b_p (rm_t - rf_t) + s_p SMB_t + h_p HML_t + u_p UMD_t + \varepsilon_{p,t} \quad (4.15)$$

where the parameters and variables of equation (4.14) have the same meaning as in equation (4.11), with the same applying to equations (4.15) and (4.13). The comparison between these two paired sets of equations shows that 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 control sample, $r_{control,t}$, and not the risk-free rate. In practice, I use the returns of the control samples based on size and book-to-market and size and momentum to estimate the values of the adjusted intercepts (\hat{a}_p). Such intercepts represent a measure of the medium-term abnormal return performance that specifically accounts for the size and book-to-market (size and momentum) bias inherent to the traditional factor models.

4.4.3 Results⁷¹

Table 4.10 summarizes my results. Panel A reports what happens when the unadjusted Fama and French (1993) and Carhart (1997) models are used as benchmark. I find that irrespective of the holding period, all intercepts are negative and statistical significant at normal levels. This is in line with the BHAR evidence discussed above, indicating that a post-bankruptcy announcement drift *is* in place. For the one-year horizon and depending on the factor model, panel A shows an abnormal performance ranging from -3.37 to -2.69 percent per month. These monthly estimates imply a yearly underperformance between -39.9 and -32.2 percent, which is considerable higher than the point estimate of -24 percent obtained with the size and book-to-market risk-adjusted BHARs for the corresponding period (see panel C, table 3.3). As Ikenberry and Ramnath (2002) point out, these two approaches differ in several ways and differences *are* to be expected. However, as argued in section 4.4.2.2, the acute disparity in results may simply be due to a misspecification problem.

Panels B and C of table 4.10 show the results for the adjusted intercept technique. In this case, the point estimates for the intercepts are again negative and statistically significant at normal levels across the different holding periods. It is important to emphasize that, under this alternative method, the size and momentum adjusted results tend to be weaker than their size and book-to-market counterparts. For instance, the one-year post-event abnormal performance estimated using the Carhart (1997) model and the size and momentum adjustment is -1.71 percent, whereas its size and book-to-market equivalent is -2.52 percent. The former result implies a 12-month underperformance of -20.5 percent, which is significantly lower than the -31.2 percent implied by the later. This suggests that failure to control for the momentum effect may result in incorrect estimates of the market's post-event reaction to bankruptcy announcements.

Panels B and C of table 4.10 also favour the conclusion that the unadjusted intercepts reported on panel A of the same table are likely overestimating the true magnitude of the post-

⁷¹ I do not report the results for the alternative 4-month holding period because they are not reliable. In fact, in this case, I have only 56 months to work with.

bankruptcy drift. In effect, when the adjusted intercept method is employed to compute the calendar-time results, the estimates of the market reaction to the announcement of bankruptcy are much closer to those obtained with the use of BHARs.

Overall, it seems safe to conclude that the post-bankruptcy drift uncovered in chapter 3 is robust to this alternative method for conducting longer-term event studies. Yet, a word of caution is now in order. This section started by emphasizing the idea that BHARs may fail to produce accurate estimates of the long-term abnormal performance, which justified the need to complement my initial analysis with the calendar-time portfolio approach. However, a number of scholars have also pointed out that this alternative is not without its own pitfalls. For instance, Lyon, Barber and Tsai (1999) show that the calendar-time method is generally incorrectly specified in non-random samples. Additionally, Barber and Lyon (1997) demonstrate that the arithmetic summation of returns (as it is done with calendar-time returns) does not precisely measure investors' experience. More importantly, Loughran and Ritter (2000) argue that this approach has low power to detect abnormal performance. All in all, these critiques point to a simple conclusion: although presenting some potential advantages over the traditional BHARs, the calendar-time method does not guarantee the accuracy of results.

In addition, it should be noted that I have a limited number of months to work with when using the calendar-time portfolio technique. To be precise, for the 12-month holding period, I have a total of 204 months; for the 6-month holding period I have only 108 months available. Accordingly, the results presented in this section should be read with this caveat in mind.

Table 4.10
Calendar-time portfolio approach

Panel A - unadjusted intercepts: This panel 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. Abnormal returns are determined using the Fama and French (1993) and the Carhart (1997) factor models, which are estimated using OLS. The regression intercept provides an estimate of monthly abnormal performance. Heteroskedasticity robust t-statistics are reported. N indicates the number of observations (months) included in the OLS estimation procedure.

	6-months holding period		12-months holding period	
	Three-factors	Four-factors	Three-factors	Four-factors
Intercept	-0.0616 -5.37***	-0.0531 -4.08***	-0.0337 -4.21***	-0.0269 -2.70*
b	1.0756 4.10***	0.9422 3.53***	1.0612 5.55***	0.9716 4.75***
s	2.2973 3.69***	2.4418 3.84***	1.9375 3.96***	2.0126 4.16***
h	1.0897 1.70	0.9712 1.50	1.0678 2.27*	0.9200 1.87
u	- -	-0.8979 -1.95	- -	-0.7035 -1.71
N	108	108	204	204
$Adj R^2$	0.1394	0.1672	0.1845	0.2156

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

Table 4.10 (cont.): Calendar-time portfolio approach

Panel B – size and book-to-market adjusted intercepts: This panel 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. Abnormal returns are determined using the Fama and French (1993) and the Carhart (1997) factor models, with the excess returns being adjusted with a control sample based on size and book-to-market. Specifically, for each sample company, I 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 models' parameters are estimated using OLS. The adjusted regression intercept provides an estimate of monthly abnormal performance. Heteroskedasticity robust t-statistics are reported. *N* indicates the number of observations (months) included in the OLS estimation procedure.

	6-months holding period		12-months holding period	
	Three-factors	Four-factors	Three-factors	Four-factors
Intercept	-0.0548 -4.77***	-0.0501 -3.85***	-0.0280 -3.53***	-0.0252 -2.52*
<i>b</i>	0.1632 0.60	0.0910 0.34	-0.0424 -0.22	-0.0798 -0.39
<i>s</i>	1.5190 2.45*	1.5973 2.48*	1.0417 2.35*	1.0730 2.34*
<i>h</i>	0.6331 0.98	0.5689 0.89	0.3831 0.87	0.3215 0.70
<i>u</i>	- -	-0.4864 -1.03	- -	-0.2936 -0.73
<i>N</i>	108	108	204	204
<i>Adj R</i> ²	0.0408	0.0477	0.0335	0.0376

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

Table 4.10 (cont.): Calendar-time portfolio approach

Panel C: size and momentum adjusted intercepts: This panel 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 excluded. Abnormal returns are determined using the Fama and French (1993) and the Carhart (1997) factor models, with the excess returns being adjusted with a control sample based on size and momentum. Specifically, for each sample company, I 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 momentum closest to that of the sample firm. The models' parameters are estimated using OLS. The adjusted regression intercept provides an estimate of monthly abnormal performance. Heteroskedasticity robust t-statistics are reported. *N* indicates the number of observations (months) included in the OLS estimation procedure.

	6-months holding period		12-months holding period	
	Three-factors	Four-factors	Three-factors	Four-factors
Intercept	-0.0388 -3.24**	-0.0343 -2.38*	-0.0171 -2.02\$	-0.0171 -2.61*
<i>b</i>	0.0398 0.14	-0.0219 -0.07	-0.0766 -0.39	-0.0768 -0.37
<i>s</i>	1.3612 2.01\$	1.4265 2.03\$	1.1330 2.22\$	1.1332 2.14\$
<i>h</i>	1.5835 2.47*	1.5313 2.36*	1.0688 2.11\$	1.0685 2.00\$
<i>u</i>	- -	-0.4138 -0.74	- -	-0.0013 0.11
<i>N</i>	108	108	204	204
<i>Adj R</i> ²	0.0478	0.0519	0.0497	0.0469

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

4.5 Summary and limitations

This chapter explores to what extent the post-bankruptcy announcement drift documented in chapter 3 is robust to a number of other potential explanations already documented in the literature. The evidence points to a clear conclusion: the anomaly does not disappear after controlling for known confounding problems like the post-earnings announcement drift, the post-GCM drift, the book-to-market effect, industry clustering or the level of financial distress. Importantly, I find essentially the same results even after considering a range of alternative methodological combinations for undertaking a medium-term event study.

Early literature cautions about the dangers of putting market efficiency to the test. In fact, Fama (1970, 1991) strongly emphasizes that such line of research will always be clouded by the joint-hypothesis problem. In the particular case of my research, apprehension regarding how to calculate and make inferences about medium-term abnormal returns adds to this concern. As Kothari and Warner (2007) point out, the bottom line is that no correct method exists for conducting long-horizon event studies yet.⁷² Hence, the best practice is to use different methodologies and verify the degree of stability across results. This is done here and, albeit some evidence suggesting that a momentum factor is present and that the anomaly is more pronounced for smaller, low-price firms, the overall results are very consistent. Accordingly, and even with the above-mentioned caveats in mind, I argue that there is enough evidence to conclude that the US equity market fails to appropriately react to news contained in Chapter 11 bankruptcy announcements.

⁷² Over the last few years a number of authors have tried to develop new methods that overcome the known econometric problems with long-horizon event studies. See, for example, the paper by Jegadeesh and Karceski (forthcoming).

Chapter 5

Limits to Arbitrage and the Market's Reaction to Bankruptcy Announcements

5.0 Overview

The main result of the previous chapters is clearly at odds with the predictions of the semi-strong form of the EMH. If the equity market were efficient in its reaction to bankruptcy announcements, the existence of a statistically significant post-bankruptcy downward drift would not occur. It is possible that my findings are simply due to the use of an inappropriate asset-pricing model, a situation that would lead to the mismeasurement of the relative risk of sample and benchmark firms. However, the magnitude and robustness of my results suggest otherwise, a point also raised by previous behavioural empirical research (e.g., Spiess and Affleck-Graves, 1995, 1999; Dichev and Piotroski, 2001; Taffler, Lu, Kausar, 2004, Kausar, Taffler and Tan, 2008).

In this chapter, I explore an alternative explanation for the inability of the market to correct this market-pricing anomaly. As section 2.1.3 emphasizes, arbitrage is one of the central tenets of the EMH, enforcing the law of one price and keeping markets efficient. In the classical setting, arbitrage requires no capital and entails no risk (e.g., Ingersoll, 1987, pp. 52-53; Pliska, 1997, p. 5). In effect, by simultaneously selling and purchasing identical securities at favourably different prices, the arbitrageur captures an immediate payoff with no upfront capital. However, as argued in section 2.2.1, pure arbitrage exists only in perfect capital markets. In the real world, imperfect information and market frictions make arbitrage risky and costly (e.g., Shleifer and Vishny, 1997).

Limits to arbitrage can impede arbitrageurs' actions at least in two ways. First, when there is suspicion over the economic nature of an apparent mispricing, sophisticated investors may be reluctant to incur the potentially large fixed costs of exploiting the arbitrage opportunity

(Merton, 1987). In particular, uncertainty about the distribution of arbitrage returns will deter arbitrage activity until potential sophisticated investors have enough information to conclude that the expected payoff is sufficiently large as to cover their costs. As such, opportunities may persist while arbitrageurs learn about the best way to exploit them.

Second, the arbitrageur needs to cope with the unpredictability of the future resale price (Shleifer, 2000, p. 14). In other words, he faces the risk that the mispricing worsens before disappearing. This may be the result of noise traders' actions and is important because the sophisticated investor only profits from his trading strategy once the price converges back to its fundamental value. As shown by De Long et al (1990b), Shleifer and Summers (1990) and Shleifer and Vishny (1997), if the arbitrageur does not have access to additional capital when the mispricing increases, he may be forced to prematurely unwind his position and incur a loss. This limits the amount that the sophisticated investor is willing to invest, which results in a lower ability for correcting potential market-pricing anomalies.

To the best of my knowledge, the role of limits to arbitrage in the pricing of bankrupt firms' stock is yet to be explored by the literature. Hence, drawing directly from the insights provided by behavioural finance theory, in this chapter, I address the following research questions: 1) *Is noise trader risk important for the pricing of bankrupt firms?* 2) *What is the impact of implementation costs in this context?* Answering these queries should help explaining the post-bankruptcy stock return pattern previously uncovered.

This chapter is organised as follows. Section 5.1 explores how noise traders affect the pricing of firms in Chapter 11. Section 5.2 investigates to what extent implementation costs hinder the ability of arbitrageurs to act in this particular market. Section 5.3 concludes.

5.1 Noise traders, institutional investors and corporate bankruptcy

5.1.1 Introduction

Black (1986, p. 531) defines noise traders as follows: "*noise trading is trading on noise as if it were information. People who trade on noise are willing to trade even though from an objective point of view they would be better off not trading. Perhaps they think the noise they are trading on is information. Or perhaps they just like to trade.*"

Individual investors play the role of noise traders in equity markets (Coval, Hirshleifer and Shumway, 2005; Barber, Odean and Zhu, 2006a). Theoretical research suggests that these market participants may exhibit irrational trading behaviour. For instance, Shiller (1984) and De Long et al (1990a) posit that fads and fashion are likely to impact their investment decisions. Similarly, Shleifer and Summers (1990) claim that individuals may herd if they follow the same signals (e.g., brokerage house recommendations, popular market gurus or forecasters) or place greater importance on recent news. Odean (1998b) and Daniel, Hirshleifer and Subrahmanyam (1998, 2001) suggest that these market participants trade too much since they are overconfident about the quality of their information. This results in sub-optimal trading that may lead to securities' under or overpricing. Lakonishok, Shleifer and Vishny (1994) claim that individual investors engage in positive feedback trading because they extrapolate past growth rates too far into the future. In the same vein, Barberis, Shleifer and Vishny (1998) suggest that the representativeness heuristic of Tversky and Kahneman (1974) may lead investors to buy securities with strong recent returns. Alternatively, Shefrin and Statman (1985) argue that individuals suffer from a disposition effect, i.e., they tend to use contrarian investment strategies by selling past winners too soon and holding too long to past losers.

A growing body of empirical literature also explores the trading pattern of individual investors. It is now clear that these market participants follow correlated investment strategies (e.g., Jackson, 2003; Barber, Odean and Zhu, 2006a, 2006b; Hvidkjaer, 2006a, 2006b; Kaniel, Saar and Titman, 2006; Kumar and Lee, 2006; Barber and Odean, forthcoming). Several other studies link trading by individuals to future returns. For instance, Coval, Hirshleifer and

Shumway (2005) find persistence in the trading performance of individual investors and that a small proportion is consistently able to outperform the market. Ivkovich, Sialm and Weisbenner (forthcoming) and Ivkovich and Weisbenner (2005) report similar findings. Grinblatt and Keloharju (2000) and Barber et al (2007) document that the trades of individuals in Finland and Taiwan, respectively, yield significantly lower returns than those by institutions.

There is evidence that individuals tend to hold underdiversified stock portfolios (Lewellen, Schlarbaum and Lease, 1974 and Goetzmann and Kumar, 2005) and retirement accounts (Benartzi, 2001; Benartzi and Thaler, 2001). Consistent with the disposition effect of Shefrin and Statman (1985), Odean (1998a) finds that these market participants have a significant preference for selling winners and holding losers, except in December when tax-motivated selling prevails. Additional evidence in favour of the disposition effect can be found in Barber and Odean (1999), Grinblatt and Keloharju (2001), Shapira and Venezia (2001), Jackson (2003) and Dhar and Zhu (2006).

In another piece of research, Odean (1999) reports that investors with discount brokerage accounts trade excessively in the sense that their returns are, on average, reduced through trading. Barber and Odean (2000, 2001, 2002) emphasize the role of overconfidence in the underperformance of individual investors. Barber and Odean (2000) document that households holding an account at a large discount brokerage firm underperform a value-weighted market index by around 1.1 percent annually. The authors show that this poor performance can be traced to the costs associated with the excessive trading that characterizes this type of account. Drawing on psychological evidence, Barber and Odean (2001) posit that overconfidence is more likely to affect men than women and, as such, predict that men will perform worse than women. Their tests show that men do trade more and thereby reduce their returns more so than do women. Finally, Barber and Odean (2002) analyse the performance of investors who switched from phone-based to online trading. They report that such market participants increased their trading activity and traded more speculatively. As a result, they faced increased cumulative costs of excess trading that led to poor performance.

In the finance literature, institutional investors play the counterpart role of individuals. Institutions include insurance companies, banks, mutual funds, investment advisors and other institutional investors like privately managed pension funds and university endowments (Ke and Ramalingegowda, 2005). Together, they own more than half of the US publicly traded equities and are responsible for more than 50 percent of all trades in the US stock market (Cai and Zheng, 2004). Finance scholars typically argue that institutional investors are more sophisticated and better informed than individuals (e.g., Lakonishok, Shleifer and Vishny, 1992; Nofsinger and Sias, 1999; Cohen, Gompers and Vuolteenaho, 2002; Ke and Ramalingegowda; 2005; Barber and Odean, forthcoming).⁷³ In effect, institutions are exposed to a variety of news reports and analyses, as well as to the guidance of professional money managers, which puts them in a privileged position to evaluate firms' fundamentals. Accordingly, on average, one should expect them to make better investment decisions than the remaining market participants.

Previous research has uncovered interesting institutional preferences for certain types of securities and/or firm characteristics. For instance, Del Guercio (1996) finds that banks tend to tilt their portfolios towards the high-quality, prudent sector of the equity market, while mutual fund managers do not. She concludes that this is the direct result of the variation among different types of institutions in their exposure to liability under prudent-man laws. Falkenstein (1996) reports that open-end mutual funds display a nonlinear preference towards stocks with high volatility. Additionally, he finds that, with few exceptions, mutual funds are averse to low-price stocks and their demand for equity consistently increases with liquidity. Falkenstein (1996) also concludes that managers of these funds dislike firms for which there is not much information available. In a related paper, Gompers and Metrick (2001) study the equity holdings of all institutions having at least 100 million dollars under management. They show that these market participants, as compared with other investors, prefer to buy large, liquid stocks that have low past returns.

⁷³ Some authors claim otherwise. For instance, Dreman (1979) and Friedman (1984) posit that institutions trade based on irrational psychological factors, causing temporary price bubbles. On the other hand, Scharfstein and Stein (1990), Lakonishok et al (1991) and Lakonishok, Shleifer and Vishny (1994) suggest that agency problems may encourage institutional herding.

There is also some empirical evidence on how institutional investors deal with apparent market-pricing anomalies. For instance, drawing on Bushee's (2001) method for separating institutions according to their investment profile, Ke and Ramalingegowda (2005) investigate to what extent these market participants are able to exploit the well-know post-earnings announcement drift. They find that institutions with high portfolio turnover and with highly diversified portfolio-holdings earn a 3-month mean abnormal return of 5.1 percent net of transaction costs by exploring such an anomaly. Kausar, Taffler and Tan (2008) explore how institutional investors deal with the disclosure of a first-time going-concern audit report in the US and find that these market participants steadily reduce their stockholdings in the affected companies well before the event date. The authors conclude that institutions are less prone to behavioural trading biases in the processing of the extreme bad news event they analyse than the remaining market participants.

5.1.2 Empirical implications

The last section suggests a potential association between the type of demand for a firm's stock and its mispricing. In particular, firms owned mainly by institutions are likely to trade near their fundamental value; for them information is abundant and accurate. Conversely, firms owned mainly by individuals may trade at any price; in their case, information is probably scarce and imprecise. Hence, studying the demand pattern for a firm's stock seems to be of fundamental importance for understanding its market price.

Interestingly, for bankrupt companies, evidence on this issue is almost inexistent, with the SEC being perhaps the best source of information in this context. This US agency dedicates a full page of its website to explaining in simple words many of the details that investing in bankrupt firms entails.⁷⁴ Two paragraphs are particularly striking. The first reads as follows: "(...) *Investors should be cautious when buying common stock of companies in Chapter 11 bankruptcy. It is extremely risky and is likely to lead to financial loss. (...) **In most instances, the company's plan of reorganization will cancel the existing equity shares.***" The second is even more remarkable, stating that: "*The bankruptcy court may determine that stockholders don't get anything because the debtor is insolvent. (...) If the company's liabilities are greater than its assets, your stock may be worthless.*"

The 2003 annual report of the SEC is also very interesting. On page 31, the SEC informs that "*Although complaints in most categories significantly declined during 2003, (...) complaints concerning corporate bankruptcy increased by 8%, entering our "top ten" list for the first time.*" Moreover, on page 33, under the title of "*Educational Campaigns*", the SEC explains that "*During 2003, we received numerous complaints from investors who purchased stock in bankrupt companies under the mistaken belief that the stock price would rise when the company emerged from bankruptcy. In each case, however, the company had announced in its plan of reorganization its intention to cancel its existing common stock and to issue new stock. We substantially revised our "Corporate Bankruptcy" brochure and partnered with a national*

⁷⁴ See <http://www.sec.gov/investor/pubs/bankrupt.htm> for details.

quotation service to alert investors about the dangers of investing in bankrupt companies. This campaign received widespread media attention, including articles in mainstream financial magazines and nationally syndicated columns as well as interviews on business television programs and coverage on national nightly news shows."

The information made available by the SEC suggests that individual investors are particularly drawn to the market of bankrupt firms. Additional anecdotal evidence supporting this idea can be found in the US press. At the time of this study, a casual search on Google using the keywords "*corporate bankruptcy*" and "*United States*" yields around 70,400 hits. Many of these results are news articles published in mainstream US newspapers like *The Wall Street Journal*, *The Financial Times* or *The Washington Post*. After randomly reading some of this information, a clear message emerges: stay away from the securities of bankrupt firms. This view is well summarized by Len Boselovic, in an article published by *The Post-Gazette* on December 03, 2001. The reporter writes: "*Anyone inclined to dabble in the shares of bankrupt companies should seek out employees and retirees of these companies and ask them one question: how much more are you buying? More than likely, they've had all they can stomach. That's all the guidance even an unwitting investor should need.*"

The last paragraphs suggest that individuals are the key investors in the market for bankrupt firms. Interestingly, Thaler (1999) highlights that, for the EMH to hold, a rational investor *must* be the market's marginal investor since only this ensures that prices are set by an agent with an unbiased expectation about the assets' fundamental value. According to Thaler (1999), a necessary condition to guarantee this result is that noise traders *do not* largely exceed rational investors in dollar-weighted terms, a point theoretically demonstrated by Hand (1990).⁷⁵ In addition, section 5.1.1 explains why one should expect firms to be mispriced by the aggregate market when individuals own the majority of their stock.

⁷⁵ Hand (1990) posits that the stock price of firm i is determined by a marginal investor. The author then argues that, as of time $t-1$, the probability that such marginal investor at period t is an unsophisticated agent ($P_{i,t}$) is positive but less than one. Hand (1990) then shows that, under this setting, $P_{i,t}$ varies according to the relative proportion of firm i 's stock that is held by unsophisticated investors as a whole. In particular, the author demonstrates that $P_{i,t}$ converges to one as the shareholdings of the unsophisticated agents approach 100 percent.

It follows that studying the relationship between the relative weights of institutional and individual investors stockholdings in bankrupt companies is important to comprehend how the aggregate equity market prices such firms. One way to investigate this issue is to look at the behaviour of institutions since there is no commercial database detailing how individuals trade on US equity markets. In fact, as Nofsinger and Sias (1999) emphasize, the fraction of shares held by institutions is one less the fraction of shares held by individuals. As such, an increase (decrease) in the percentage of shares held by institutions is equivalent to a decrease (increase) in the percentage of shares own by individuals. Drawing on this intuition, I propose to test the following null hypothesis:

H₂: Both in the pre and post-event period, there is no difference in the percentage of shares held by institutions in bankrupt firms and comparable non-event firms.

5.1.3 Data

I gather the information relating to institutional holdings from the Thomson Financial Network CDA/Spectrum Institutional holdings file.⁷⁶ The data covers my entire sample period, beginning in the first quarter of 1980 and ending in the last quarter of 2006. I use the CUSIP number to match the institutional holdings file with my sample firms and find that 342 companies have information available (97.4 percent).⁷⁷ However, not all firms are covered in every quarter. In the pre-event period, an average of 291 firms per quarter (83 percent) is available in the CDA/Spectrum file. In the post-event period this number drops to 203 (58 percent).

I use the same source to collect data for my control sample of firms matched on size and book-to-market.⁷⁸ The Spectrum Institutional holding file has information for 346 (98.5 percent) of these firms. Again, not all of them are covered in each quarter. In this case, the average

⁷⁶ A 1978 amendment to the Securities and Exchange Act of 1934 requires all institutional investors with greater than 100 million dollars of securities under discretionary management to report their holdings to the SEC. Holdings need to be reported 45 days after the close of each quarter on the SEC's form 13F, where all common-stock positions greater than 10,000 shares or 200,000 dollars must be disclosed. See <http://www.sec.gov/answers/form13f.htm> and <http://www.sec.gov/divisions/investment/13ffaq.htm> for more details.

⁷⁷ Details about the sample firms are available on section 3.2.

⁷⁸ Details about the control firms are available on section 3.3.2.

number of firms covered per quarter is 325 (92.7 percent), with no difference being noted before and after the event date.

5.1.4 Methodology

I address my research hypothesis by examining how institutional investors' stockholdings change over time. Institutional ownership is used as a proxy for this variable (e.g., Nofsinger and Sias, 1999; Chen, Jegadeesh and Wermers, 2000; Ke and Ramalingegowda, 2005) and is computed as follows:

$$Inst_{i,t} = \frac{Shares\ held_{i,t}}{Shares\ outstanding_{i,t}} \quad (5.1)$$

where $Shares\ held_{i,t}$ is the number of shares of firm i held by institutional investors at time t and $Shares\ outstanding_{i,t}$ is firm i 's number of outstanding shares at time t . Working with equation (5.1) requires a strict definition for time t . I overcome this issue by identifying what I term here as quarter 0. For firm i , this quarter is simply the first quarter where institutions report their holdings about the firm after its bankruptcy date.⁷⁹ Once quarter 0 is found, it is possible to determine other quarters just like in a standard event-study. I compute equation (5.1) for a total of 17 quarters centred on quarter 0, which is sufficient to understand how institutional investors deal with corporate bankruptcy.

⁷⁹ The same date is used for each pair of sample and control firms.

5.1.5 Results

Table 5.1 summarizes my results. I find that in event-quarter -8 institutions own, on average, 25 percent of my bankrupt firms' shares (median holdings are 20 percent). Four quarters latter, they own, on average, 21 percent of the debtors' shares (median holdings are 16 percent). Once Chapter 11 becomes effective (quarter 0), institutional investors own, on average, only 12 percent of these companies' shares, a pattern that remains largely unchanged for another four post-event quarters. Importantly, institutions' median holdings right after Chapter 11 are 8 percent, decreasing to 6 percent four quarters latter.

I additionally conduct a t-test and a Wilcoxon-Mann-Whitney test to verify if the mean and median percentage of bankrupt firms' shares held by institutional investors is statistically different between quarter 0 and quarters -8, -4, 4 and 8. I am unable to find any statistically significant difference between quarters 0 and 4 (the p-value of the t-test and the Wilcoxon-Mann-Whitney test is 0.9479 and 0.4070, respectively). A completely opposite result emerges when considering quarters -8 and 0 and -4 and 0: in this case, both the parametric and non-parametric tests are significant at the one percent level. For quarters 0 and 8, results are mixed since I find a p-value of 0.0206 (0.1761) for the t-test (Wilcoxon-Mann-Whitney test).

Taken together, these results suggest that institutions change significantly their stockholdings in bankrupt firms twice in the period under scrutiny. The first, occurring around the Chapter 11 date, leads to a massive reduction in the debtors' equity structure. Five quarters latter, this initial reaction is partially reversed and institutions increase, albeit slightly, the number of shares of failed companies in their portfolios.

Previous research shows that institutional investors dislike small firms' stock (see section 5.1.1) and, as such, the patterns described above may not be specific to my sample of bankrupt firms. Table 5.1 suggests otherwise. In event-quarter -8, institutions own on average 24 percent of the shares of my control firms (median holdings are 19 percent), a figure that is consistent with that of the bankrupt companies. Not surprisingly, for this particular quarter, both the t-test and

the Wilcoxon-Mann-Witney test are not significant at normal levels. This changes four quarters later. In event-quarter -4, the mean and median difference between sample and benchmark firms is now around five percent and significant at normal levels. Such difference increases with time and, in quarter 0, becomes very clear. In this quarter, institutions own, on average, 23 percent of control companies' shares (18 percent in median terms) and only 12 percent of the bankrupt firms (8 percent in median terms). The mean and median difference between groups is significant at better than the one percent level, with the same pattern applying to the following eight quarters of available data.⁸⁰ In face of this evidence, I reject the null hypothesis under analysis here (H_2).

⁸⁰ In untabulated results, I rerun the analysis accounting for the fact that some of my sample firms are delisted after filing for bankruptcy. In this alternative test, benchmark firms are deleted when the bankrupt firm they are paired with is delisted. Results are very similar to those reported here.

Table 5.1

Institutional investors' stockholding of bankrupt companies

This table presents institutional stockholdings for my 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. Information about institutional stockholdings for a control sample based on size and book-to-market is also provided. Specifically, for each sample company, I 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. Below, institutional ownership is computed as $Inst_{i,t} = Shares\ held_{i,t} / Shares\ outstanding_{i,t}$, where $Shares\ held_{i,t}$ is the number of shares of firm i held by the institutional investors at the end of event-quarter t and $Shares\ outstanding_{i,t}$ is firm i 's outstanding shares at the end of event-quarter t . Event-quarter 0 is defined as the first quarter where institutions report their holdings about the firm (sample or matched) after the bankruptcy date. The last two columns report the two-tailed significance level from a t -test and a Wilcoxon-Man-Whitney test for the difference in means and medians, respectively. N reports the number of companies with available information to compute $Inst_{i,t}$ in event-quarter t .

Quarter	Sample firms			Control firms			Significance	
	Mean	Median	N	Mean	Median	N	Mean	Median
-8	24.4%	20.1%	263	24.2%	19.4%	323	0.9192	0.5043
-7	24.1%	20.1%	274	23.9%	19.5%	324	0.9348	0.6282
-6	22.5%	17.6%	282	24.4%	20.0%	326	0.2699	0.6156
-5	21.9%	17.1%	288	24.1%	19.9%	330	0.1725	0.4696
-4	20.6%	15.5%	299	25.4%	20.4%	327	0.0042	0.0283
-3	19.6%	14.3%	303	24.1%	19.4%	330	0.0036	0.0267
-2	18.0%	12.7%	306	24.0%	19.7%	326	<0.0001	0.0010
-1	16.1%	10.7%	310	23.4%	19.5%	330	<0.0001	<0.0001
0	11.6%	7.9%	306	23.2%	17.9%	333	<0.0001	<0.0001
1	11.0%	6.7%	264	23.4%	17.5%	333	<0.0001	<0.0001
2	11.1%	6.1%	229	23.8%	17.5%	331	<0.0001	<0.0001
3	11.2%	5.9%	198	22.9%	16.5%	335	<0.0001	<0.0001
4	11.7%	5.7%	189	22.7%	16.4%	335	<0.0001	<0.0001
5	12.8%	6.0%	173	23.6%	18.0%	326	<0.0001	<0.0001
6	13.6%	6.1%	168	24.2%	19.2%	316	<0.0001	<0.0001
7	14.0%	5.6%	160	25.0%	18.8%	308	<0.0001	<0.0001
8	15.8%	6.2%	148	24.9%	18.7%	301	<0.0001	<0.0001

5.1.6 Summary and limitations

This section attempts to explore the role of noise traders in the pricing of bankrupt firms. Unfortunately, data about these market participants' holdings is not readily available, a problem that I overcome by investigating how institutional investors' stockholdings change around the bankruptcy date. Two main ideas emerge from my analysis: 1) institutions steadily sell debtors' stock as Chapter 11 approaches; 2) once bankruptcy is underway, the participation of institutional investors in the market for bankrupt firms is, at best, marginal.

My results are similar to those reported by Kausar, Taffler and Tan (2008) and suggest that institutional investors are less exposed to particular behavioural biases, which allows them to deal more rationally with the catastrophic event at hand. My findings also help complement previous research by Seyhun and Bradley (1997) and Ma (2001), who study the behaviour of bankrupt firms' insiders. Both papers show that these market participants engage in significant sales of their firm's stock in the months and even years preceding the event date. Such strategy allows insiders to avoid significant capital losses and is very similar to that documented now for institutional investors. The research of Seyhun and Bradley (1997) and Ma (2001) thus suggest that insiders, who are by definition individual investors with superior (privileged) information about their companies, also have only a minor participation in the market of bankrupt firms.

Another way to read this section's main result is that noise traders control the market for bankrupt firms' stock. To be precise, in the typical case, individual investors own an average of around 90 percent of the stock while the company is undergoing its Chapter 11 reorganization. As such, they are likely to be responsible for setting the debtors' stock price or, in the words of Hand (1990) and Thaler (1999), noise traders are the marginal investor in this particular market. Accordingly, it seems that the anomaly documented in the previous chapters may be the result of a substantial number of traders making their investment decisions based on sentiment and not on information. In effect, it is commonly accepted that noise traders are particularly vulnerable to psychological biases that impair their ability to make rational investment decisions (e.g., Shiller, 1984; Shefrin and Statman, 1985; Shleifer and Summers,

1990 and Lakonishok, Shleifer and Vishny, 1994). I would argue that this problem is even more crucial in a setting where information is scarce (e.g., Espahbodi, Dugar and Tehranian, 2001; Clarke et al, 2006) and valuation is difficult (Gilson, 1995; Gilson, Hotchkiss and Ruback, 2000). In a nutshell, in line with section 2.4.2.1, my results provide evidence in favour of the story that irrational reasons explain why a large number of individual investors trade on bankrupt firms' stock, which may lead to the incorrect pricing of this security.

There is, however, an important shortcoming affecting the conclusions presented above. In effect, some institutional investors are particularly interested in distressed companies' securities. For instance, vulture funds seem to be predominantly drawn to this market (Rosenberg, 2000; Lhabitant, 2006). These funds are usually financial organizations specialized in buying securities in distressed environments, such as high-yield bonds in or near default or equities that are in or near bankruptcy. I tried to investigate the role of these market participants in the pricing of my sample companies but, sadly, the typology of institutional investors employed by CDA/Spectrum is not well suited for addressing this issue. In an attempt to deal with this problem, I contacted the *Institutional Investor Magazine* to see if they had any information that I could use in my research. Unfortunately, I was not able to obtain any data from them. I also got in touch with Professors Edith Hotchkiss and Robert Mooradian, who in 1997 co-authored a paper analysing the role of vulture investors in the governance and reorganization of a sample of 288 firms that defaulted on their public debt. The idea was to gather information on the activities of potential vulture investors involved in the Chapter 11 proceedings of my sample firms. I did not have much success either. This creates an opportunity that may be explored in further research.

5.2 Arbitrage implementation costs and the mispricing of bankrupt firms

5.2.1 Introduction

In this section, I explore the role of arbitrage implementation costs in the pricing of bankrupt firms' stock. As Barberis and Thaler (2005, p. 6) explain, these costs matter because they hinder arbitrageurs' ability to exploit a mispricing. Additionally, in extreme cases, when it is too costly to learn about the mispricing or the resources required to exploit it are too expensive, arbitrageurs may simply choose not to act on it (Merton, 1987).

A number of empirical studies already explore the impact of implementation costs on the profitability of investment strategies involving market anomalies. One of the first contributions to this area is that by Stoll and Whaley (1983), who re-examine the small-firm effect documented in Banz's (1981) seminal paper. Stoll and Whaley (1983) show that out-of-the-pocket transaction costs can at least partially explain such a market anomaly.

More recent contributions to this area are, among others, those by Pontiff (1996), Choi (2000), Barber et al (2001), Pontiff and Schill (2001), Lesmond, Schill and Zhou (2004), Mendenhall (2004), Taffler, Lu and Kausar (2004), Mashruwala, Rajgopal and Shevlin (2006) and Kausar, Taffler and Tan (2008). These papers focus on different anomalies, ranging from the mispricing of close-end funds, the momentum effect, investing according to analysts' recommendations, the post-earnings announcement drift, the post-going concern drift and the accruals effect. In general, this body of literature reports a similar result: it is unlikely that sophisticated investors can earn positive abnormal returns once arbitrage implementation costs are properly factored into the analysis.

5.2.2 Empirical implications

As mentioned above, there is extensive evidence suggesting that implementation costs impede arbitrageurs in correcting some known market-pricing anomalies. Yet, previous research has not formally investigated to what extent the same phenomenon affects the market for bankrupt firms. In order to address this question, I propose to test the following null hypothesis:

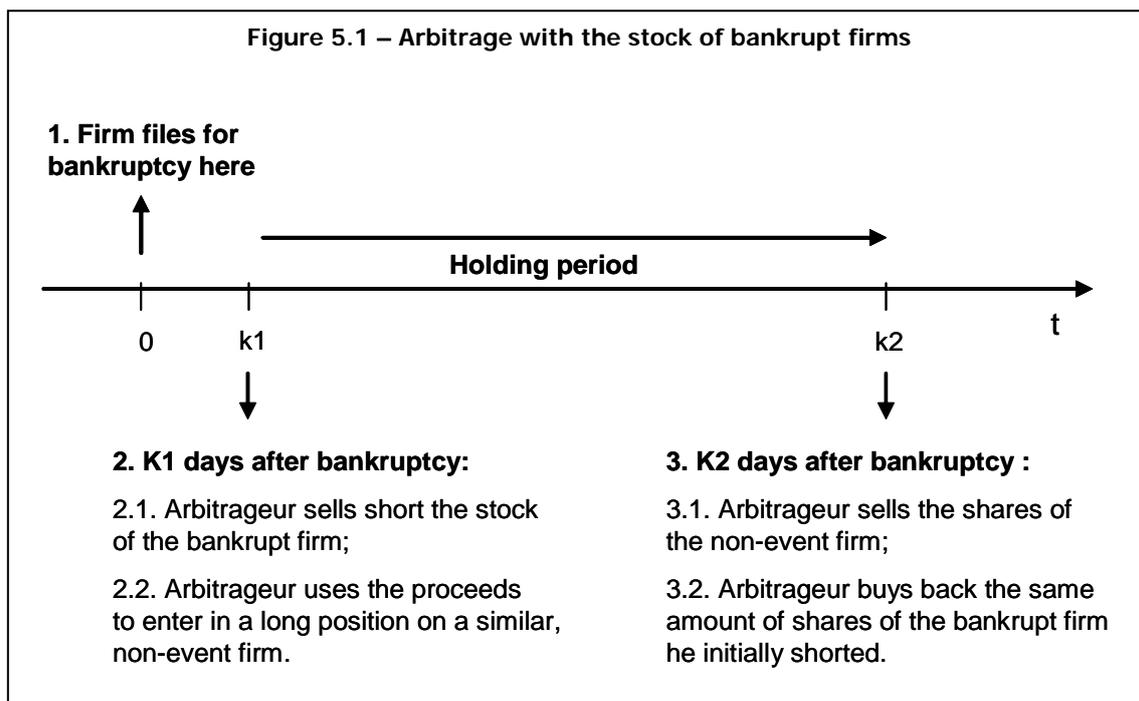
H₃: Expected returns from an arbitrage strategy designed to explore the post-bankruptcy announcement drift are zero.

A clear definition of implementation costs needs to be adopted in order to test this null hypothesis. This is not a trivial issue. For instance, Barberis and Thaler (2005, p. 6) identify a number of these costs like commissions, the bid-ask spread, price impact, shorting fees or the costs incurred by the arbitrageur to find and learn about a mispricing. On the other hand, Pontiff and Schill (2001) and Pontiff (2006) mention other costs like borrowing costs, opportunity costs from not being able to fully invest short-sale proceeds or the risk exposure from imperfectly hedged positions. Unfortunately, as noted by Lesmond, Schill and Zhou (2004), capturing all the components of an arbitrage strategy's implementation costs is very challenging from an empirical point of view. As a result, most of the existing literature focuses on a particular sub-set, usually referred to as *transaction costs*, which occur when a transaction takes place (e.g., Hanna and Ready, 2005; Ng, Rusticus and Verdi, 2008). These are easier to compute and include brokerage and short-sale fees, market impact costs and the bid-ask spread. Drawing on previous research in this area, I also conduct my analysis considering only the impact of transaction costs in the profitability of an arbitrage strategy involving the stock of bankrupt firms. The next paragraphs summarize the methodology used to achieve this objective.

5.2.3 Data and method

5.2.3.1 Zero-investment strategy in event time

A similar approach to that of Taffler, Lu and Kausar (2004) and Kausar, Taffler and Tan (2008) is employed here. The key idea is that an arbitrageur trying to exploit the post-bankruptcy drift needs to accomplish two things. First, he has to short the stock of the bankrupt companies once they file for Chapter 11 and hedge his position by buying shares in other firms sharing a number of fundamental characteristics. Second, after a certain holding period, he needs to reverse these positions. In practice, this involves buying back the amount of shares of the bankrupt firms that he initially shorted and selling all shares of the matched companies that he initially bought. Figure 5.1 puts this strategy into perspective:



In figure 5.1, time is measured in event days, where $t = 0$ is the bankruptcy date. A number of alternative scenarios are used to implement the investment strategy pictured above. In my base scenario, the arbitrageur goes short in a notional value of 25,000 dollars on each of the bankrupt companies and uses the net proceeds to buy shares of matched firms sharing similar

size and book-to-market.^{81, 82} For each pair of bankrupt and benchmark companies, these initial trades occur two trading days after the Chapter 11 date, i.e., $K_1 = 2$. These positions are closed after a holding period of 252 trading days ($K_2 = 252$, roughly one year). Importantly, if a given bankrupt firm is delisted during the holding period, the position on both bankrupt and matched company is prematurely closed at the delisting date. Variations to the base scenario include changing the amount invested in each bankrupt company, using other matched firms (size and momentum, industry and stock-price, size and z-score and industry, size and book-to-market), opening the initial position at different post-event days, considering alternative holding periods and inferring the stock price behaviour after the delisting date, as suggested by Taffler, Lu and Kausar (2004) and Kausar, Taffler and Tan (2008).⁸³

A crucial aspect is how transaction costs are handled here. Following Taffler, Lu and Kausar (2004) and Kausar, Taffler and Tan (2008), I consider three types of transaction costs: 1) stock borrowing costs; 2) trading commissions and 3) the bid-ask spread. The first type affects the zero-investment strategy's profitability because the arbitrageur needs to borrow the bankrupt firms' stock before conducting the required short sale. D'Avolio (2002) reports that nine percent of stocks on the CRSP database have loaning fees in excess of one percent *per annum*. He refers to these as "*special*" stocks, which face an effective mean loaning fee of 4.3 percent *per annum*. Drawing on Kausar, Taffler and Tan (2008), I use a conservative approach and assume a shorting cost of 4.3 percent *per annum* for bankrupt companies below the sample's median market capitalization and one percent *per annum* for all other firms.

Commission costs are also very important because they have to be paid *per transaction* (both for bankrupt and control firms), thus reducing the financial benefit of engaging in any given trade. In this study, I follow Lesmond, Schill and Zhou (2004) and use a four percent commission rate for stocks under one dollar per share and 0.25 percent for all remaining stocks.

⁸¹ As Kausar, Taffler and Tan (2005) explain, 25,000 dollars should be a sufficiently small amount not likely to cause significant price impact on the market.

⁸² Details about the sample firms (control firms) are available on section 3.2 (section 3.3.2).

⁸³ Details about the alternative benchmark firms are available on chapter 4.

The bid-ask spread plays a key role in assessing the transaction costs faced by investors, especially when dealing with small, less liquid stocks (Pontiff, 1996; Lesmond, Schill and Zhou, 2004). This variable's impact is incorporated into the analysis by allowing all trades to be conducted at the respective bid or ask closing price (for both sample and matched firms). Whenever one of these prices is not available, I follow Kausar, Taffler and Tan (2008) and estimate its value. In particular, the missing figure is inferred using the closing price for the relevant trading day and half of the median bid-ask spread across all cases in the sample with available data.

5.2.3.2 Zero-investment strategy in calendar time

Lesmond, Schill and Zhou (2004) use a different method to explore the impact of transaction costs on a zero-investment strategy's profitability. In their setting, the investment decisions are analysed in calendar time.⁸⁴ The arbitrageur has three basic choices: 1) when to short bankrupt firms' stock; 2) which companies to buy in order to hedge his position and 3) the strategy's investment horizon. In my base simulation, each bankrupt firm is shorted and added to an investment portfolio in the month following its Chapter 11 date. Additionally, the net proceeds from this initial trade are invested in a non-event firm, sharing similar size and book-to-market. The stocks are held in the portfolio for a 12-month period or until the bankrupt firm is delisted. At that point, the arbitrageur closes both positions. The portfolio's monthly equally weighted buy-and-hold return represents the expected paper profits from engaging in this strategy.

As Lesmond, Schill and Zhou (2004) highlight, this strategy is not costless because the portfolio has to be rebalanced periodically. To be precise, every month the arbitrageur adds to the portfolio the firms that have filed for bankruptcy in the previous month and drops all companies that have reached their holding period limit. In line with section 5.2.3.1, I conservatively

⁸⁴ Implementing this method requires monthly data for both sample and benchmark firms, which I collect from CRSP's monthly stock return file.

assume that stock borrowing costs, trading commissions and the bid-ask spread are the only costs affecting this strategy's profitability.

Stock borrowing costs and trading commissions are handled as above. Bid-ask spreads, however, require a somewhat different approach. I start by computing the value of the bid-ask spread for each stock (both for bankrupt and matched firms) in two different windows. The first occurs before the stock enters the portfolio; the second occurs before the position on that stock is closed. As suggested by Stoll and Whaley (1983), half of each individual spread is used as the cost associated with the bid-ask spread when a particular company is added to or dropped from the portfolio. Importantly, the median bid-ask spread across all cases in the sample with available data is used if it is not possible to calculate a given firm-specific spread.

The last step is computing the value of a monthly equally weighted transaction cost for this rolling portfolio, which provides an estimate of the cost that an arbitrageur would have to bear in order to earn the paper profits defined above.

5.2.3.3 Estimating the bid-ask spread

An additional complication acknowledged in this study relates to the fact that estimating the bid-ask spread is not a straightforward task. In fact, the literature provides a menu of procedures for consideration. Lesmond, Schill and Zhou (2004) discuss a number of approaches that a researcher may follow when dealing with this problem. Due to the varying strengths and weaknesses of the various methods available in the literature, I use four alternative techniques to determine complementary estimates for the bid-ask spread of both sample and control companies. Details about all methods are presented below.

5.2.3.3.1 Quoted spread estimate

Quoted spread estimates are calculated as in Stoll and Whaley (1983) and Bhardwaj and Brooks (1992). This method requires closing bid and ask prices, which are collected from CRSP's daily database. I consider two estimation periods, which are defined around two key moments: 1)

$t = 0$, the bankruptcy date and 2) $t = \alpha$, which occurs one year after $t = 0$ or at the delisting date of the bankrupt firm, whichever comes first.⁸⁵ The first estimation period begins one year before $t = 0$ and ends two weeks prior to that date. The second starts one week after $t = 0$ and terminates two weeks earlier than $t = \alpha$.⁸⁶ Equation (5.2) for the pre-bankruptcy period and (5.3) for the post-bankruptcy period are then used to calculate the quoted spread estimate for each of my sample and control companies:

$$Spread_{i,t} = \frac{1}{t} \sum_{t=-252}^{-10} \frac{(Ask_{i,t} - Bid_{i,t})}{\frac{1}{2}(Ask_{i,t} + Bid_{i,t})} \quad (5.2)$$

$$Spread_{i,t} = \frac{1}{t} \sum_{t=\alpha-247}^{\alpha-10} \frac{(Ask_{i,t} - Bid_{i,t})}{\frac{1}{2}(Ask_{i,t} + Bid_{i,t})} \quad (5.3)$$

where $Ask_{i,t}$ is the closing ask price for firm i on day t , $Bid_{i,t}$ is the closing bid price for firm i on day t and α is defined as above.

5.2.3.3.2 Direct effective spread estimate

Direct effective spread estimates are calculated as in Lesmond, Schill and Zhou (2004). In addition to closing bid and ask prices, this alternative measure also requires information about the closing price of the security under analysis. As a result, daily closing prices collected from CRSP are matched with the contemporaneous closing bid and ask prices. Once again, two estimation periods are considered for both sample and matched firms, along the lines described above. The bid-ask estimates are now given by equation (5.4) for the pre-bankruptcy period and (5.5) for the post-bankruptcy period:

⁸⁵ For each pair of sample and control firms, $t=0$ and $t = \alpha$ are the same.

⁸⁶ Alternative estimation windows are also considered for robustness purposes. In particular, I use a 2- and a 3-month period before $t=0$ and $t = \alpha$ as an alternative to my original framework as well as ending the estimation period two, five, and ten trading days before these key dates. Results remain qualitatively unchanged.

$$Spread_{i,t} = \frac{1}{t} \sum_{t=-252}^{-10} \frac{\left(P_{i,t} - \frac{1}{2}(Ask_{i,t} + Bid_{i,t}) \right)}{P_{i,t}} \quad (5.4)$$

$$Spread_{i,t} = \frac{1}{t} \sum_{t=\alpha-247}^{\alpha-10} \frac{\left(P_{i,t} - \frac{1}{2}(Ask_{i,t} + Bid_{i,t}) \right)}{P_{i,t}} \quad (5.5)$$

where $Ask_{i,t}$ is the closing ask price for firm i in day t , $Bid_{i,t}$ is the closing bid price for firm i in day t , $P_{i,t}$ is the closing price for firm i in day t and α is defined as above.

5.2.3.3.3 Roll effective spread estimate

Roll (1984) shows that the presence of a bid-ask bounce induces negative serial covariance in price changes. Based on this result, he calculates an implied spread from the observed serial correlation of transaction price changes. In particular, Roll's (1984) estimator of the percentage bid-ask spread is given by:

$$\hat{s}_{i,(t_1,t_2)} = 200 \sqrt{-\hat{c}_{j,(t_1,t_2)}} \quad (5.6)$$

where $\hat{c}_{j,t}$ is the serial covariance of stock i 's returns from time t_1 to time t_2 . Estimating the autocovariance structure of a given firm's returns is the first step in implementing equation (5.6).⁸⁷ Daily returns are collected from CRSP for both sample and control firms to achieve this objective. In order to maintain a coherent approach, the autocovariance structure is calculated for each stock for the two sub-periods identified in section 5.2.3.3.1. Importantly, in line with Shultz (2000) and Lesmond, Schill and Zhou (2004), I omit all estimates produced for firms with positive return autocovariance. As Harris (1990) and Lesmond, Schill and Zhou (2004) explain, these are cases that do not comply with Roll's (1984) theoretical assumptions, resulting in estimated spreads that are invalid and thus cannot be used in my analysis.

⁸⁷ See Hamilton (1994, pp. 45, 61 and 62) for details on how to estimate an autocovariance function.

5.2.3.3.4 The LDV model

The limited dependent variable threshold (LDV) model of Lesmond, Ogden and Trzcinka (1999) is the last method employed to estimate the bid-ask spread. The model's intuition is that investors will only act on information concerning the stock's fundamental value when the return generated by the trade exceeds the costs associated with trading. Otherwise, investors will refrain from trading and the observed return on that stock will be zero. It follows that, in this setting, trading costs can be understood as a threshold that must be exceeded before investors act upon new value-relevant information.

Lesmond, Ogden and Trzcinka (1999) posit that the market model is the true generating process for returns, subject to transaction costs. In particular, the true return on a security, R_i^* , the observed return, R_i , and the market return, R_m , are related as follows:

$$R_{i,t}^* = \beta_i R_{m,t} + e_{i,t} \quad (5.7)$$

where β_i measures the sensitivity of firm i 's returns to general market movements and $e_{i,t}$ is white noise. Moreover, the LDV model obeys the following restrictions:

$$\begin{aligned} R_{i,t} &= R_{i,t}^* - \alpha_{1,i} & \text{if} & & R_{i,t}^* < \alpha_{1,i}, & & \alpha_{1,i} < 0 \\ R_{i,t} &= 0 & \text{if} & & \alpha_{1,i} \leq R_{i,t}^* \leq \alpha_{2,i} \\ R_{i,t} &= R_{i,t}^* - \alpha_{2,i} & \text{if} & & R_{i,t}^* > \alpha_{2,i}, & & \alpha_{2,i} > 0 \end{aligned} \quad (5.8)$$

Equation (5.7) describes the generation process of firm i 's true return. In a frictionless market, prices will immediately reflect contemporaneous market-wide and firm-specific information. However, in the presence of trading costs, observed returns reflect new information only when the value of the information signal exceeds the cost of trading. The model's constraints, given by (5.8), describe the relationship between true and observed return. In the first and last constraints, where the value of the true return exceeds the trading cost threshold, observed

returns are equal to the true returns up to the value of transaction costs. The parameter $\alpha_{1,i}$ is the trading cost threshold that must be exceeded before investors act on negative information for firm i , while $\alpha_{2,i}$ does the same but for positive information. Accordingly, $\alpha_{1,i}$ and $\alpha_{2,i}$ represent the proportional trading cost for selling and buying firm i 's stock, respectively. When the true return does not exceed the transaction cost threshold (i.e., $\alpha_{1,i} \leq R_{i,t}^* \leq \alpha_{2,i}$), the observed return is zero.

Econometrically speaking, the LDV model is a limited dependent variable model, censored in the middle, with two unknown parameters $\alpha_{1,i}$ and $\alpha_{2,i}$. In particular, the resulting likelihood function is given by:

$$L = \prod_{t \in R_1} \frac{1}{\sigma_j} \phi_1(\zeta_t) \prod_{t \in R_2} \frac{1}{\sigma_j} \phi_2(\zeta_t) \prod_{t \in R_0} \Pr(\text{no change})_t \quad (5.9)$$

where R_1 and R_2 denote the regions where the measured return, $R_{i,t}$, is nonzero in negative and positive market return regions, respectively. Additionally, R_0 denotes the zero return region.

The terms ϕ_1 and ϕ_2 refer to the standard normal density function for decreases and increases in the measured return, respectively. These are the standardized residuals evaluated at $\zeta = \varepsilon/\sigma$, where σ^2 is the variance estimated using only the nonzero measured returns.

Finally, $\Pr(\text{no change})_t$ is the probability of a zero return. The model is estimated by maximum likelihood using one year of daily returns, collected from CRSP's daily stock file, for each sample and control firm.⁸⁸ Following Lesmond, Schill and Zhou (2004), I use the CRSP equally weighted market return as the market index since each firm receives an equal weight throughout my analysis.

⁸⁸ I am grateful to Professor David Lesmond for advice on estimating the LDV model and for providing me with his computer codes for implementing the model. Maddala (1983) is also a valuable theoretical reference for understanding the workings of this type of model.

An additional word is required here. The LDV model *does not* provide an estimate of the firm-specific bid-ask spread. In fact, the LDV measure includes both explicit costs, such as the bid-ask spread and commissions but also implicit costs, such as short-sales constraints, taxes and the price impact. It follows that, by calculating the difference between $\alpha_{1,i}$ and $\alpha_{2,i}$, the LDV measure produces an estimate of the all-in (explicit and implicit) roundtrip transaction cost for each firm included in the analysis. Consequently, when the LDV model is used, the methodologies discussed in 5.2.3.1 and 5.2.3.2 are adjusted in the sense that the only relevant cost considered for each firm is given by the LDV measure.

5.2.4 Results

5.2.4.1 Bid-ask estimates

I start by presenting the results obtained for the different bid-ask estimates. Table 5.2 shows that investors face large spreads when trading bankrupt companies' stock. In the pre-event period, the mean estimates vary between 5.83 and 8.27 percent (median values range from 5.16 to 6.85 percent). These values are much larger than the 1.0 or 2.0 percent round-trip costs estimated in previous studies for large capitalization stocks (e.g., Stoll and Whaley, 1983; Kothare and Laux, 1995; Keim and Madhavan, 1998). The analysis of the post-bankruptcy period is even more revealing. Table 5.2 shows that the mean bid-ask spread estimates for that particular period now vary between 8.94 and 12.50 percent (median values range from 6.61 and 10.70 percent).

This sharp increase in the bid-ask spread sheds some light on how market makers react to the bankruptcy event. According to microstructure models, bid-ask spreads compensate dealers for losses due to informed trading and for costs associated with processing orders and carrying inventory (Harris, 2003). The early literature (e.g., Demsetz, 1968; Benston and Hagerman, 1974) emphasizes order-processing and inventory-carrying costs. In these models, spreads increase with price volatility and decrease with price levels, trading volume and competitive pressure. Copeland and Galai (1983) and Glosten and Milgrom (1985) add informed trading risk

to this analysis. These models posit that dealers are confronted with two types of traders: liquidity-motivated traders and informed traders and assume dealers and liquidity-motivated traders possess identical sets of information, while informed traders have unique, non-public information. Informed trading risk arises because informed traders only sell (buy) when their estimates of the true price is below (above) the market makers' bid (ask) quote. Consequently, dealers always lose to informed traders and the only way to recoup their losses is to increase spreads to liquidity-traders. My results provide direct support for these theoretical models. In effect, the information asymmetry affecting bankrupt companies is dramatic, especially right after the Chapter 11 date. In these initial moments, only a few insiders know precisely what is going on with the company and can use their privileged information to earn abnormal returns. In this respect, the results of previous research undertaken by Seyhun and Bradley (1997) and Ma (2001) indicate that corporate insiders of bankrupt firms do use their superior information to trade thereby avoiding the significant capital losses associated with such an extreme negative event. As predicted by Copeland and Galai (1983) and Glosten and Milgrom (1985), when confronted with this situation, dealers respond by widening their bid-ask spreads thus recouping from liquidity-traders what they lose to informed traders.

Table 5.2 also shows different spread estimates for two sets of control firms, one based on size and book-to-market and other on size and momentum. I find that the estimated bid-ask spread for these firms is still relatively high, both in the pre- and post-event period. In effect, for the size and book-to-market benchmark sample (size and momentum), the lowest mean estimate for the pre-event period is 3.79 percent (4.68 percent) and for the post-event period is 3.94 percent (6.12 percent). A possible explanation for this result lies on the fact that all benchmark firms must comply with a certain size requirement, which is defined around the sample firms' rather small market capitalization. Table 5.2 also shows that, irrespective of the period, the two control firms' bid-ask spread is lower than that of the bankrupt companies. Moreover, in sharp contrast with what happens to event firms, bid-ask spread estimates for the benchmark companies do not suffer a dramatic increase from the pre- to the post-event period. This

finding clearly suggests that the bankruptcy announcement is actually driving this effect in the case of my sample firms.

The results obtained with the LDV model are largely consistent with the evidence above. In fact, both in the pre- and post-event period, bankrupt companies exhibit higher round trip transaction costs than the respective control firms. Panel D of table 5.2 also suggests that event and the two types of non-event companies' total transaction costs increase significantly from the pre- to the post-event period. Complementary tests, however, show that the variation in total transaction costs between the pre- and post-event period, as measured by the LDV model, is always higher for sample than for matched firms.

Table 5.2

Bid-ask spread estimates for sample and control firms

This table presents bid-ask spread estimates for my 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. The table also shows the results for size and book-to-market (size and momentum) matched sample. In the case of the size and book-to-market benchmark (size and momentum) sample, for each sample company, I 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 (momentum) closest to that of the sample firm. The quoted spread measure is computed as in Stoll and Whaley (1983). The direct effective spread estimate is computed as in Lesmond, Schill and Zhou (2004). The Roll effective spread estimate is computed as in Roll (1984). The LDV measure is computed as in Lesmond, Ogden and Trzcinka (1999). In panels A, B, C and D, the Pre bank. column refers to the pre-event period bid-ask estimates. All pre-event estimates are computed with daily data collected from CRSP using a period that begins one year before the bankruptcy date of the event firm and ends two weeks before that date. The same bankruptcy date is used for each pair of event and non-event companies. In panels A, B, C and D, the Post bank. column refers to the post-event period bid-ask estimates. All post-event estimates are computed with daily data collected from CRSP using a period that begins one week after the bankruptcy date of the event firm and ends one year after that date or at the delisting date of the event firm, whichever comes first. The same bankruptcy date is used for event and non-event companies. In panels A, B, C and D, *N* reports the number of companies with available information to compute the respective bid-ask estimate.

Panel A: Quoted spread estimate

	Sample Firms		Size and B/M		Size and Momentum	
	Pre bank.	Post bank.	Pre bank.	Post bank.	Pre bank.	Post bank.
Mean	8.27%	12.50%	6.25%	7.18%	7.06%	7.84%
Median	6.85%	10.70%	4.30%	4.38%	4.68%	5.79%
N	205	211	191	197	218	222
St. Dev.	6.26%	7.33%	6.14%	8.89%	7.83%	6.66%

Panel B: Direct effective estimate

	Sample Firms		Size and B/M		Size and Momentum	
	Pre bank.	Post bank.	Pre bank.	Post bank.	Pre bank.	Post bank.
Mean	5.83%	8.94%	3.79%	3.94%	4.68%	6.12%
Median	5.16%	6.61%	2.96%	2.64%	3.58%	4.31%
N	205	211	191	197	218	222
St. Dev.	4.16%	5.09%	3.48%	3.96%	4.20%	6.14%

Panel C: Roll effective estimate

	Sample Firms		Size and B/M		Size and Momentum	
	Pre bank.	Post bank.	Pre bank.	Post bank.	Pre bank.	Post bank.
Mean	7.62%	9.89%	4.66%	5.28%	5.82%	7.47%
Median	6.30%	7.27%	3.75%	3.78%	4.37%	4.84%
N	225	267	223	222	225	235
St. Dev.	6.57%	6.85%	3.83%	4.94%	5.22%	8.10%

Panel D: LDV effective estimate

	Sample Firms		Size and B/M		Size and Momentum	
	Pre bank.	Post bank.	Pre bank.	Post bank.	Pre bank.	Post bank.
Mean	11.22%	14.48%	8.89%	10.44%	9.72%	11.43%
Median	9.03%	12.30%	6.85%	8.34%	7.22%	8.34%
N	351	351	351	351	351	351
St. Dev.	7.73%	5.28%	8.51%	9.42%	7.98%	8.94%

5.2.4.2 Profitability of the zero-investment strategy in event time

I now turn to the analysis of the profitability of the zero-investment strategy in event time presented in section 5.2.3.1. Panel A of table 5.3 summarizes my base scenario's results. I find that, on average, a sophisticated investor engaging in an arbitrage strategy involving bankrupt firms' stock *will not* earn a positive return at either a 6- or 12-month holding horizon. To be precise, with a per firm investment of 25,000 dollars, the best mean result available is a loss of 18.0 (11.2) percent for the 6-month (12-month) holding period, which represents a minimum average dollar loss of 4,500 (2,800). Median returns confirm this result: most of them are negative and significant, with some being positive but not statistically different from zero at normal levels. Moreover, there is clear evidence that the arbitrage strategy under analysis here is very risky for the arbitrageur. The large figures for the standard deviation and inter-quartile range obtained for the arbitrage strategy's return justify such claim.

Panel B of table 5.3 shows what happens when one considers a size and momentum control sample in the simulation. Results are very consistent with those presented above. For a 12-month (6-month) holding period, the best mean result available is a loss of 10.5 (17.6) percent or 2,625 (4,400) dollars. Median returns largely confirm these results. The high standard deviation and inter-quartile range obtained with this alternative simulation suggest once again that a sophisticated investor must be willing to bear a significant degree of risk in order to implement the arbitrage strategy under consideration.

Finally, it should be noted that my results remain qualitative unchanged after considering numerous variations of the base scenario. In particular, considering alternative control samples based on industry, size and book-to-market, size and z-score and industry and stock price, opening the arbitrage strategy initial positions in the third, fifth and tenth post-event day and holding the positions open for four, five and nine months does not affect my results in any meaningful way.⁸⁹ In face of this evidence, I do not reject this section's null hypothesis (H_3).

⁸⁹ Combining several of these changes does not affect my results either.

Table 5.3

Illustrative profits earned with a zero-investment strategy in event time

This table presents the results obtained with an illustrative zero-investment strategy in event time using my 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. The arbitrageur goes short in a notional value of 25,000 dollars on each bankrupt company and uses the net proceeds to buy shares of a matched firm sharing similar characteristics. In panel A, firms are matched according to size and book-to-market. Specifically, for each sample company, I 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. In panel B, firms are matched according to size and momentum. Specifically, for each sample company, I 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 momentum closest to that of the sample firm. The initial trades occur two trading days after the event date and the positions are closed after a period of 252 (126) trading days or at the delisting date of the event firm, whichever comes first. Three types of transaction costs are considered in the computation of the results presented below: 1) stock borrowing costs; 2) trading commissions and 3) the bid-ask spread. A shorting cost of 4.3 percent *per annum* is used for the bankrupt companies below the sample's median market capitalization and a shorting cost of one percent *per annum* is used for all other firms. A four percent commission rate is used for both event and non-event firms with stock prices below one dollar per share; a 0.25 percent commission rate is used in the remaining cases. The impact of the bid-ask spread is incorporated into the analysis by allowing all trades to be conducted at the respective bid or ask closing price (for both sample and matched firms). Whenever one of these prices is not available, I estimate its value. The missing figure is inferred using the closing price for the relevant trading day and half of the median bid-ask spread across all cases in the sample with available data. Four different bid-ask estimates are considered. In panels A and B, the Direct effective spread column refers to the bid-ask spread computed as in Lesmond, Schill and Zhou (2004). In panels A and B, the Quoted spread column refers to the bid-ask spread computed as in Stoll and Whaley (1983). In panels A and B, the Roll effective spread column refers the bid-ask spread computed as in Roll (1984). In panels A and B, the LDV effective spread column refers to the bid-ask spread computed as in Lesmond, Ogden and Trzcinka (1999). In panels A and B, the two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median).

Table 5.3 (cont.): Illustrative profits earned with a zero-investment strategy in event time

Panel A: Base scenario - firms are matched according to size and book to market

	Direct effective spread		Quoted spread		Roll effective spread		LDV effective spread	
	6-months	12-months	6-months	12-months	6-months	12-months	6-months	12-months
Mean	-18.0%	-11.2%	-20.3%	-14.4%	-19.0%	-13.0%	-21.5%	-15.4%
p-value	0.0001	0.0646	<0.0001	0.0266	<0.0001	0.0443	<0.0001	0.0183
Median	-5.1%	1.2%	-5.7%	1.0%	-5.3%	2.1%	-6.1%	-2.1%
p-value	0.0104	0.3421	0.0021	0.1681	0.0059	0.2621	0.0009	0.1159
St.Dev.	89.0%	120.1%	90.2%	121.3%	88.1%	120.6%	79.8%	122.6%
25th percentil	-54.5%	-57.4%	-57.8%	-60.1%	-56.4%	-58.2%	-59.3%	-61.6%
75th percentil	37.5%	48.1%	35.6%	46.4%	36.2%	47.8%	35.0%	44.8%

Panel B: Alternative scenario - firms are matched according to size and momentum

	Direct effective spread		Quoted spread		Roll effective spread		LDV effective spread	
	6-months	12-months	6-months	12-months	6-months	12-months	6-months	12-months
Mean	-17.6%	-10.5%	-19.8%	-12.8%	-18.5%	-11.4%	-22.1%	-15.0%
p-value	0.0080	0.1260	0.0002	0.0622	0.0040	0.0961	<0.0001	0.0288
Median	-7.3%	1.5%	-10.1%	-1.1%	-8.4%	-1.3%	-11.4%	-3.1%
p-value	0.0011	0.3140	0.0002	0.1850	0.0006	0.2612	<0.0001	0.0984
St.Dev.	96.4%	128.4%	97.3%	129.7%	97.5%	129.1%	97.0%	128.9%
25th percentil	-58.2%	-54.3%	-60.4%	-55.1%	-59.6%	-54.6%	-65.2%	-57.3%
75th percentil	33.9%	45.2%	31.9%	43.6%	33.1%	44.6%	29.8%	42.3%

5.2.4.3 Profitability of the zero-investment strategy in calendar time

Table 5.4 summarizes the results when the zero-investment strategy in calendar time is employed. Overall, the new evidence is consistent with that presented above. In fact, irrespective of the control sample used to hedge the short position and the estimator employed for determining the bid-ask spread affecting the trades, all mean and median returns computed with this method are still negative and statistically different from zero at normal levels. For instance, panel A shows what happens when a size and book-to-market benchmark sample is used to generate the results. On average, a sophisticated investor can expect to lose money both at a 6- and 12-month holding period. In fact, the best result available for the 6-month (12-month) window is a mean loss of 8.4 percent (10.3 percent). Importantly, median returns confirm the parametric results. Considering an alternative control sample based on size and momentum does not alter the nature of my findings. Under this scenario, the arbitrageur may expect to lose between 10.2 and 14.3 percent of his investment over a 6-month period and between 11.9 and 15.3 percent over the complementary 12-month window. Once again, median results confirm this story.

As with the previous section, I conducted a number of simulations to confirm the robustness of the reported results. I find a very similar pattern after changing the benchmark companies and calculating the firm-specific bid-ask spreads over different estimation periods. Again, in face of this evidence, I do not reject the null hypothesis under analysis in this section (H_3).

Table 5.4

Illustrative profits earned with a zero-investment strategy in calendar time

This table presents the results obtained with an illustrative zero-investment strategy in calendar time using my 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. Each bankrupt firm is shorted and added to an investment portfolio in the month following its Chapter 11 date. The net proceeds from this initial trade are invested in a non-event firm, sharing similar characteristics. In panel A, firms are matched according to size and book-to-market. Specifically, for each sample company, I 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. In panel B, firms are matched according to size and momentum. Specifically, for each sample company, I 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 momentum closest to that of the sample firm. The stocks are held in the portfolio for a period of 12 (6) months or until the bankrupt firm is delisted, whichever comes first. The portfolio's monthly equally weighted buy-and-hold return represents the expected paper profits from engaging in this strategy. Three types of transaction costs are considered in the computation of the results presented below: 1) stock borrowing costs; 2) trading commissions and 3) the bid-ask spread. A shorting cost of 4.3 percent *per annum* is used for the bankrupt companies below the sample's median market capitalization and a shorting cost of one percent *per annum* is used for all other firms. A four percent commission rate is used for both event and non-event firms with stock prices below one dollar per share; a 0.25 percent commission rate is used in the remaining cases. The impact of the bid-ask spread is estimated using one of four possible alternatives. In panels A and B, the Direct effective spread column refers to the bid-ask spread computed as in Lesmond, Schill and Zhou (2004). In panels A and B, the Quoted spread column refers to the bid-ask spread computed as in Stoll and Whaley (1983). In panels A and B, the Roll effective spread column refers the bid-ask spread computed as in Roll (1984). In panels A and B, the LDV effective spread column refers to the bid-ask spread computed as in Lesmond, Ogden and Trzcinka (1999). The monthly equally weighted transaction cost for this rolling portfolio provides an estimate of the cost that a sophisticated investor would have to bear in order to engage in the arbitrage strategy. In panels A and B, the two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median).

Table 5.4 (cont.): Illustrative profits earned with a zero-investment strategy in calendar time

Panel A: Base scenario - firms are matched according to size and book to market

	Direct effective spread		Quoted spread		Roll effective spread		LDV effective spread	
	6-months	12-months	6-months	12-months	6-months	12-months	6-months	12-months
Mean	-8.4%	-10.3%	-11.3%	-12.9%	-9.8%	-11.6%	-13.0%	-14.4%
p-value	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001
Median	-8.0%	-9.4%	-10.6%	-11.4%	-9.3%	-9.9%	-12.6%	-13.5%
p-value	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001
St.Dev.	16.1%	19.3%	17.9%	19.2%	17.6%	19.2%	17.1%	21.2%
25th percentil	-19.4%	-20.2%	-23.0%	-23.5%	-20.1%	-22.5%	-24.4%	-25.7%
75th percentil	1.9%	1.3%	0.0%	0.0%	1.4%	2.1%	-1.1%	2.2%

Panel B: Alternative scenario - firms are matched according to size and momentum

	Direct effective spread		Quoted spread		Roll effective spread		LDV effective spread	
	6-months	12-months	6-months	12-months	6-months	12-months	6-months	12-months
Mean	-10.2%	-11.9%	-12.8%	-14.2%	-11.7%	-13.1%	-14.3%	-15.3%
p-value	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001
Median	-7.5%	-9.2%	-10.9%	-12.8%	-9.2%	-11.2%	-12.9%	-13.6%
p-value	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001	<0.0001
St.Dev.	18.1%	23.5%	18.2%	23.5%	18.8%	23.8%	19.2%	23.9%
25th percentil	-21.1%	-21.8%	-23.7%	-25.5%	-20.2%	-22.2%	-27.2%	-27.3%
75th percentil	2.0%	1.1%	0.0%	1.3%	0.0%	1.0%	0.0%	3.1%

5.2.5 Summary and limitations

This section explores whether arbitrageurs can profit from the post-bankruptcy announcement drift. I find that, although a market inefficiency exists, it is not easily exploitable even by sophisticated investors due to high transaction costs affecting the arbitrage process. As such, only an "*illusory profit opportunity*" (Lesmond, Schill and Zhou, 2004) seems to exist in this peculiar market, which helps explain why the anomaly uncovered in the previous chapters persists even in the medium-term.

My result is clearly in line with Rubinstein's (2001) theoretical argument that markets are "*minimally rational*". As such, my findings add to a body of literature documenting that, although some events may be informationally inefficient, investors' opportunities to reap abnormal returns from such information are very limited. This is the case of Barberis et al (2001), who analyse the profitability of arbitrage strategies involving analysts' changes in recommendations, Mendenhall (2004) for the post-earnings announcement drift, Klein, Rosenfeld and Tucker (2006) for the post-reverse stock split drift and Taffler, Lu and Kausar (2004) and Kausar, Taffler and Tan (2008) for the post-GCM drift.

My findings are also important for a more fundamental reason. As Fama (1970) highlights, the primary role of the capital market is the allocation of ownership of the economy's capital stock. In an ideal world, market prices provide all the information investors should require in assigning their savings to the most promising investment opportunities given their own risk preferences. However, my results show that, at least in particular situations where limits to arbitrage are likely to exist, market prices are only noisy proxies of the true fundamental value, a point also emphasized by Lee (2001). As such, in these circumstances, investors should not rely solely on market prices to make their investment decisions.

It should be noted that my analysis is conservative in that it fails to account for all possible sources of implementation costs that a sophisticated investor needs to face when engaging in an arbitrage strategy like the one described above. Perhaps the difficulty in shorting the stock

of bankrupt firms is the most relevant example of this situation. D'Avolio (2002) finds that over 50 percent of the stocks with prices below five dollars present in the CRSP database are hard to short. Given their legal status, I would argue that bankrupt firms are even more special in that respect. Additionally, my results do not explicitly consider the impact of other costs like holding costs or idiosyncratic risk, which previous research has shown to play an important role in the profitability of arbitrage strategies and are surely crucial in the context that I address (e.g., Pontiff and Schill, 2002; Pontiff, 2006). However, in practice, these limitations only strengthen the robustness of my results. In fact, factoring these other costs into the analysis would only reduce further the estimated profitability of the arbitrage strategy.

5.3 Summary of the chapter

This chapter investigates to what extent limits to arbitrage explain the mispricing of bankrupt firms' stock. In particular, I address the following research questions: 1) *Is noise trader risk important for the pricing of bankrupt firms?* 2) *What is the impact of implementation costs in this context?* My main results are easy to summarize. First, I find that noise traders dominate the market for bankrupt firms: in the typical case, individual investors own, on average, 90 percent of the stock while the company is undergoing Chapter 11 reorganization. Second, I show that implementation costs are binding for bankrupt firms. In the best case scenario, a sophisticated investor may expect to lose 10.3 percent on average over a 12-month holding period when engaging in an arbitrage strategy involving these firms' stock.

The implication of my findings is straightforward: arbitrage, in the context I address, is simply too risky and costly. Implementation costs prevent sophisticated investors from acting because they render arbitrage unprofitable. Moreover, even if arbitrageurs are able to overcome this problem, they still have to face noise trader risk, which is very high in this market. As a result, the stock price of bankrupt firms drifts for a long period without traditional market forces being able to correct such situation.

Chapter 6

The Market's Reaction to Bankruptcy Announcements: a Behavioural Story?

6.0 Overview

The last chapter suggests that limits to arbitrage justify why the market does not correctly price the stock of bankrupt firms. The current chapter revisits extant behavioural finance literature and tries to explain why this market-pricing anomaly occurs in the first place. One of the problems with this line of research is the wide number of behavioural stories (i.e., non risk-based) that can be used to describe a return pattern that is not predicted by the EMH. This is actually one of the major criticisms of behavioural finance (e.g., Fama, 1998; Rubinstein, 2001). In fact, scholars only began building formal models rooted in behavioural concepts when voluminous empirical evidence conflicting with the predictions of the EMH started to be published in finance and accounting journals (Shleifer, 2000, p. 16). However, the process was initially cumbersome, with several behavioural stories being produced only to fit specific empirical phenomenon (Fama, 1998; Shefrin, 2005, p. 5).

More recently, Barberis, Shleifer and Vishny (1998) and Hong and Stein (1999) have attempted to develop an encompassing behavioural finance theory.⁹⁰ The first model relies on different psychological biases affecting the investment decisions of a representative agent to explain under which circumstances positive and negative autocorrelation in returns should occur. In contrast, Hong and Stein (1999) use the speed with which firm-specific information is disseminated into the market to motivate their results.

⁹⁰ Daniel, Hirshleifer and Subrahmanyam (1998) also develop a theoretical model rooted on psychological concepts (overconfidence and biased self-attribution) to explain how under and overreaction may occur. However, testing empirically the implications of this model is very challenging since it requires the researcher to observe private signals that affect investors' decisions (see Daniel, Hirshleifer and Subrahmanyam, 1998, p. 1841). As such, I do not attempt to analyse the extent to which this alternative model captures the return pattern associated with the announcement of Chapter 11 bankruptcy here.

In this chapter, I test the implications of each of these two competing models in the particular context of the announcement of corporate bankruptcy. Addressing this issue is important for two main reasons. Firstly, it has the potential for clarifying why the post-bankruptcy drift arises in the first place. Secondly, comparing the performance of the two behavioural models in my setting is an important out-of-the sample test to their predictive ability. As Fama (1998) and Barberis and Thaler (2005, pp. 64-65) highlight, this is the only scientific way to check the models' relative merit and, at the same time, enhance our understanding about how financial markets work.

The chapter is divided in three sections. The first two explore to what extent the two models under analysis explain the stock return pattern associated with the announcement of corporate bankruptcy. The last section summarizes my results and presents the main conclusions.

6.1 Corporate bankruptcy and the Barberis, Shleifer and Vishny (1998) model

In the Barberis, Shleifer and Vishny (1998) model, the decisions of a representative investor are biased due to conservatism and representativeness. Conservatism is attributed to Edwards (1968), who argues that people are slow in updating their beliefs in the face of new evidence. On the other hand, the representativeness bias, initially documented by Tversky and Kahneman (1974), suggests that people put too much weight on recent patterns and too little on the properties of the population that is actually generating the data.

The fundamental assumption of Barberis, Shleifer and Vishny (1998) is that earnings follow a random walk but the representative investor falsely perceives that there are two earnings regimes. In regime 1, he thinks earnings are mean reverting. By definition, this is the most likely regime and generates stock price underreaction. In fact, according to the authors, when the representative investor believes that earnings are mean reverting, he is unable to correctly assess new information released by a change in earnings because he incorrectly infers that such change is probably temporary. Clearly, conservatism drives regime 1 of the Barberis, Shleifer and Vishny (1998) model. The opposite situation is predicted for regime 2. When this regime

holds, the representative investor thinks earnings trend. Once the investor is convinced that regime 2 is in place, he incorrectly extrapolates future trends in earnings based on particular realizations of past earnings changes. This behaviour is rooted in the representativeness bias and ultimately results in stock price overreaction to earnings realizations.

Importantly, in the Barberis, Shleifer and Vishny (1998) model, prices only converge to their fundamental value when the initial expectation of the representative investor is not met. In particular, underreaction is resolved when he realises that earnings are not mean reverting after a run of sequential earnings changes in the same direction. Conversely, overreaction is resolved when a set of random earnings changes convinces the representative investor that earnings are no longer trending. In both cases, however, prices *always* return to fundamentals because the true process generating the earnings realizations is a random walk.

6.1.1 Empirical implications

Fama (1998) points out that the Barberis, Shleifer and Vishny (1998) model is designed to deal with phenomena like the post-earnings announcement drift (e.g., Ball and Brown, 1968), the momentum effect (e.g., Jegadeesh and Titman, 1993), long-term reversals in stock prices (De Bondt and Thaler, 1985) and returns to contrarian investment strategies (Lakonishok, Shleifer and Vishny, 1994). In fact, only on pages 331 and 332, Barberis, Shleifer and Vishny (1998) provide some *very general* guidance on how to interpret their model's implications in the context of a value-relevant information event. The basic intuition relates to whether the representative investor perceives the firm to be trending when the public event occurs. When he believes that the firm is *not trending*, the model predicts *underreaction* to every event. This result is based on the idea that the conservatism bias prevents the representative agent from fully incorporating the public event's impact into the stock price of the affected company.

Importantly, the authors *do not* provide clear insight on what to expect when the representative investor believes the firm *is trending*. Barberis, Shleifer and Vishny (1998) simply suggest that both under and overreaction are possible, depending on whether the

representative investor uses the new information to reinforce his believe that the firm is trending. Chan, Frankel and Kothari (2004) overcome this problem by introducing the concepts of confirming and disconfirming signals in their analysis. The former is defined as a signal that has the same direction as the public signal used to catalogue the firm as trending, while the opposite holds for the latter. Chan, Frankel and Kothari (2004) then argue that confirming signals generate further overreaction and the converse is posited when a disconfirming signal occurs.

This is the key feature for testing to what extent the Barberis, Shleifer and Vishny (1998) model explains the post-bankruptcy return pattern documented in the previous chapters. According to such a model, due to the representativeness bias, investors overreact when they perceive that firms are trending. In practice, when firms are trending upwards, abnormal returns are positive because the representative investor extrapolates the positive trend too far into the future. In contrast, when firms are trending downwards, abnormal returns are negative for the opposite reason. Under this setting, when a given firm is perceived to be trending downwards in the pre-event period, bankruptcy is a confirming signal and thus should reinforce prior beliefs, leading to increased overreaction in the post-event period. Conversely, when the firm is perceived to be trending upwards, bankruptcy is a disconfirming signal and thus should trigger a change in sentiment regarding the future prospects of the firm, i.e., we should expect a reversal in the stock return pattern to occur in the post-event period. Drawing on this intuition, I propose to test the following null hypotheses:

H₄: There is no difference between the post-bankruptcy risk-adjusted abnormal returns of firms trending downwards in the pre-event period and the post-bankruptcy risk adjusted abnormal returns of firms not trending downwards in the pre-event period.⁹¹

H₅: Risk-adjusted abnormal returns of firms trending upwards before the event date are zero after the announcement of bankruptcy.⁹²

⁹¹ The alternative hypothesis is that there is a negative difference between the post-bankruptcy risk-adjusted abnormal returns of firms trending downwards in the pre-event period and the post-bankruptcy risk-adjusted abnormal returns of firms not trending downwards in the pre-event period.

⁹² The alternative hypothesis is that risk-adjusted returns of firms trending upwards before the event date are negative after the announcement of bankruptcy. However, it is important to note that the predictions of the Barberis, Shleifer and Vishny (1998) model will only be accurate in the context I address if the pre-event risk-adjusted returns of firms trending upwards before filing for bankruptcy are positive. In effect, only this combined result justifies that filing for

It should be noted that no prediction is made when firms *are not* trending. The reason is that, for these firms, Barberis, Shleifer and Vishny (1998) suggest that the market underreacts to all information events. As such, this general prediction is not helpful in determining if the *full* model presented by Barberis, Shleifer and Vishny (1998) can help explain the return pattern associated with the announcement of corporate bankruptcy.

6.1.2 Data and methodology

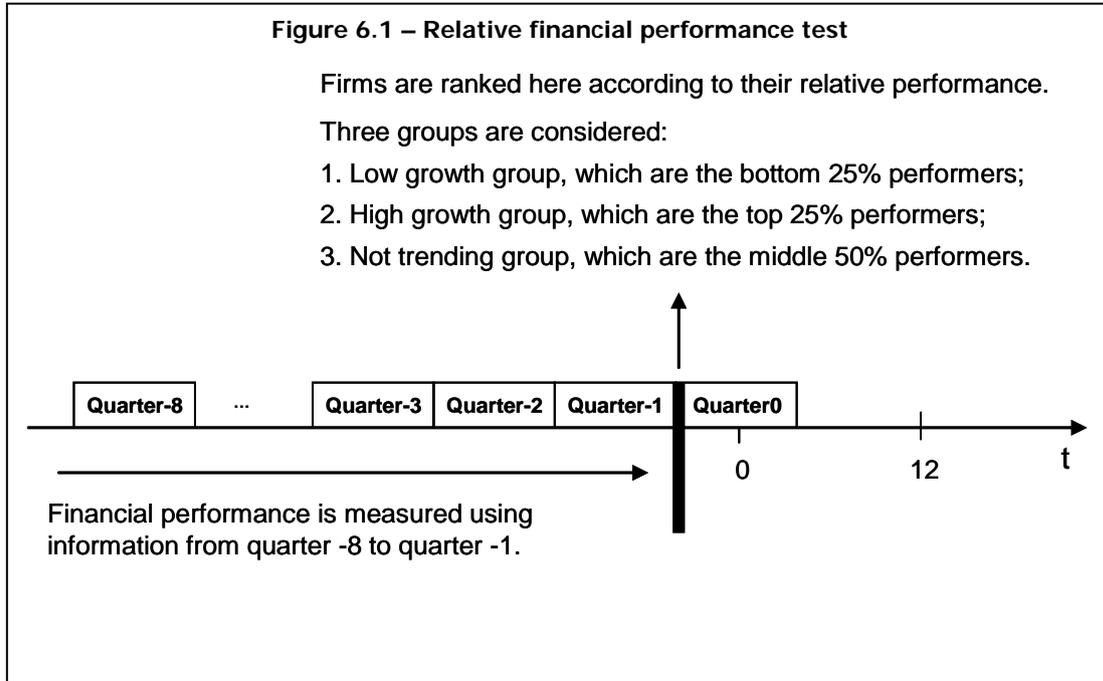
Testing the implications of the Barberis, Shleifer and Vishny (1998) model requires two things: 1) analysing the pre- and post-bankruptcy abnormal returns and 2) defining the mechanism employed by the representative investor to determine whether or not firms are trending. The methodology discussed in section 3.3 is used to overcome the first issue. The second question is resolved by implementing a similar approach to that of Chan, Frankel and Kothari (2004). In line with Chan, Frankel and Kothari (2004), I resort to three accounting variables to establish if a given sample firm is trending (upwards or downwards) before filing for Chapter 11: 1) sales per share, 2) net income per share scaled by total assets per share and 3) earnings per share. All accounting data are collected from COMPUSTAT's quarterly file.⁹³ Drawing on Chan, Frankel and Kothari (2004), I implement two different tests, which are summarized in the next paragraphs.

Chapter 11 bankruptcy, a disconfirming signal for firms trending upwards in the pre-event period, leads to a *reversal* in the stock return pattern of such firms post-event.

⁹³ COMPUSTAT's quarterly data items are as follows: sales is Q2, net income is Q69, total assets is Q44, earnings per share is Q19 and shares outstanding is (Q61*Q17).

6.1.2.1 Relative financial performance

Figure 6.1 presents the main features of the first test.



In figure 6.1, time is measured in months, where $t = 0$ is the Chapter 11 date. The first step is finding the accounts of quarter 0, which are those reported by each sample firm *right after* its bankruptcy date. As suggested by figure 6.1, once quarter 0 is found, it is possible to identify quarters -8 to -1 just as in a standard event-study. Completing this step allows computing the different financial performance measures employed in the test. To be precise, for each sample firm i , the sales per share measure is defined as follows:⁹⁴

$$\left[\left(S_{q-1} + S_{q-2} + S_{q-3} + S_{q-4} \right) - \left(S_{q-5} + S_{q-6} + S_{q-7} + S_{q-8} \right) \right] / \left(S_{q-5} + S_{q-6} + S_{q-7} + S_{q-8} \right) \quad (6.1)$$

where S_{q-t} are the sales per share for quarter t . Similarly, the net income per share for firm i is given by:

⁹⁴ A similar expression is used for earnings per share.

$$\left[\left(NI_{q-1} + NI_{q-2} + NI_{q-3} + NI_{q-4} \right) - \left(NI_{q-5} + NI_{q-6} + NI_{q-7} + NI_{q-8} \right) \right] / A_{q-5} \quad (6.2)$$

where NI_{q-t} is net income per share for quarter t and A_{q-5} is the assets per share in quarter -5.

In the second step, firms are ranked according to their financial performance. To be precise, companies in the bottom quartile by growth are labelled as “low growth” while firms in the top quartile are labelled as “high growth”. All other firms are assigned to the “not trending” group.⁹⁵ This procedure is repeated three times, one for sales per share, other for earnings per share and the last sort is for net income per share. As argued by Chan, Frankel and Kothari (2004), because the assignment of firms to quartiles is based only on the growth over the relevant horizon, the growth-quartile is a measure of trend. Additionally, equations (6.1) and (6.2) help minimizing the impact of seasonality in my results, an issue inherent to this type of analysis.

In the last step, companies in each group are treated as a portfolio and their stock price performance is compared over a 12-month period starting the second event-day after the Chapter 11 date. Moreover, since three different groups are used here, a one-way ANOVA test (Kruskall-Wallis test) is employed to verify if the difference in the mean (median) performance between these portfolios is statistically significant.

6.1.2.2 Absolute financial performance

Chan, Frankel and Kothari (2004) posit that investors are more likely to categorize firms as high or low growth when their past financial performance is consistent. To see why, consider two similar firms (A and B) that, over a one-year period, lose 40 percent of their sales. Based on this accounting measure, both firms are losers. Now, assume that firm A has lost a percentage of its sales every quarter in the last year while firm B has only suffered one big loss in sales in one particular quarter of that year. Clearly, although both firms are losers, firm A is *consistently* a loser while firm B had just one bad quarter that conditioned its yearly performance. According

⁹⁵ Not all of my sample firms have the necessary information for implementing this test. When this happens, the particular firm is dropped from the analysis and I work with a smaller sample.

to Chan, Frankel and Kothari (2004), the representative investor is more likely to catalogue firm A in the “low growth” group than firm B.

In order to capture this idea, I start by identifying quarters -4 to -1 using the methodology discussed above. After that, for each event firm, I compute the variation in the different accounting variables (i.e., sales per share, earnings per share and net income per share) between each consecutive quarter (quarter -3 and quarter -4, quarter -2 and quarter -3 and so forth). Firms are then allocated to the “consistently low growth” (“consistently high growth”) group when all quarterly variations are negative (positive). The remaining companies are assigned to the “not trending” group. Finally, as above, one-way ANOVA and Kruskal-Wallis tests are employed to compare the relative stock price performance of the three groups over a 12-month period starting two event-days after the bankruptcy date.⁹⁶

6.1.3 Results

Table 6.1 summarizes the results of the relative financial performance test. Panel A shows that, when sales per share is used to rank firms, the post-bankruptcy stock return performance of the three portfolios under analysis is *not* statistically different given that both the ANOVA and Kruskal-Wallis tests are not significant at normal levels, a conclusion that holds for both size and book-to-market and size and momentum risk-adjusted returns. My findings are consistent with my first research hypothesis (H_4) but clearly contrasts with the predictions of the Barberis, Shleifer and Vishny (1998) model, which posits that firms receiving a confirming signal at the bankruptcy date (i.e., those allocated to the “low growth” portfolio) should have stronger post-event abnormal returns.

A different pattern emerges when earnings per share is used to rank my sample companies. As shown in panel B of table 6.1, the ANOVA (Kruskal-Wallis) test indicates that the mean (median) performance of the three portfolios considered in the analysis *is* (*is*) statistically different, a conclusion that holds for both size and book-to-market and size and momentum

⁹⁶ See the previous footnote.

risk-adjusted returns. Such a difference seems to be driven by the performance of companies allocated to the “low growth” group, which have lower post-bankruptcy abnormal returns than firms allocated to the two alternative portfolios. In effect, using a set of additional t-tests (Wilcoxon-Mann-Whitney tests), I find that the post-bankruptcy mean (median) size and book-to-market and size and momentum abnormal returns of the “high growth” and “not trending” portfolios are statistically indifferent at normal levels when earnings per share is used to rank sample firms. Moreover, these additional tests reveal that firms allocated to the “low growth” portfolio have, indeed, weaker mean and median post-bankruptcy abnormal returns than firms allocated to the “high growth” and “not trending” portfolios. My findings are clearly in conflict with the predictions of the Barberis, Shleifer and Vishny (1998) model, which envisages exactly the opposite results.

Results are mixed when net income per share is used to rank my sample firms. Panel C of table 6.1 shows that, in this case, the mean performance of the three portfolios is statistically indifferent both for size and book-to-market (p-value of the ANOVA test is 0.1841) and size and momentum (p-value of the ANOVA test is 0.1278) adjusted returns. Yet, this conclusion changes when the Kruskal-Wallis test is employed. In fact, for the size and book-to-market (size and momentum) risk-adjusted returns, this non-parametric test is significant at the ten (five) percent level. Interestingly, panel C of table 6.1 indicates that the size and book-to-market and size and momentum median abnormal returns are more negative for the “high growth” portfolio than for the two alternative sets of firms. Using a range of additional Wilcoxon-Mann-Whitney tests, I find that the post-event median size and book-to-market and size and momentum abnormal returns of the “low growth” and “not trending” portfolios are statistically indifferent at normal levels when net income per share is used to rank my sample firms. Moreover, these tests reveal that firms in the “high growth” portfolio have more negative median post-bankruptcy abnormal returns than firms allocated to both the “low growth” and “not trending” portfolios. Once again, this result is inconsistent with the predictions of the Barberis, Shleifer and Vishny (1998) model.

Testing my second research hypothesis (H_5) requires looking at the return pattern of the three portfolios under analysis in the pre-event period. Panels D, E and F help achieve this objective. I find that, irrespective of the accounting variable and the particular risk adjustment technique, abnormal returns computed for the “high growth” portfolio are always negative and statistically significant *before* the bankruptcy date. Additionally, panels A, B and C show that such a pattern *does not* change in the post-bankruptcy period. Hence, combining the information of panels A, B and C with that provided by panels D, E and F, it is possible to conclude that firms in the “high growth” portfolio *do not* suffer a reversal in their stock return pattern in the post-bankruptcy period as suggested by the Barberis, Shleifer and Vishny (1998) model.

Table 6.1

Testing the Barberis, Shleifer and Vishny (1998) model – relative financial performance

This table presents the results using the relative financial performance measure for testing the empirical implications of the Barberis, Shleifer and Vishny (1998) model with my 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. In the panels below, event firms are matched with firms with similar size and book-to-market or similar size and momentum. When the size and book-to-market benchmark is employed, for every event firm, I start by identifying 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 event firm. When the size and momentum benchmark is employed, for every event firm, I start by identifying 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 momentum closest to that of the event firm. In the panels below, the Low growth, High growth and Not trending columns present the mean and median risk-adjusted returns of the “low growth”, “high growth” and “not-trending” portfolios, respectively. The two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median). In the panels below, the Anova and KW tests column reports the result of a one-way ANOVA test (Kruskall-Wallis test) that checks the significance of the mean (median) difference in performance between the “low growth”, “high growth” and “not-trending” portfolios. For the one-way ANOVA test, the value of the F-test and its significance level are reported. For the Kruskall-Wallis test, the value of the Chi-square test and its significance level are reported. In all panels below, *N* indicates the number of companies included in the “low growth”, “high growth” and “not-trending” portfolio.

Table 6.1 (cont.): Testing the Barberis, Shleifer and Vishny (1998) model – relative financial performance

Panel A: In this panel I use the sales per share measure to infer about the pre-bankruptcy trending regime of each event firm. The sales per share measure is given by:

$$\left[\left(S_{q-1} + S_{q-2} + S_{q-3} + S_{q-4} \right) - \left(S_{q-5} + S_{q-6} + S_{q-7} + S_{q-8} \right) \right] / \left(S_{q-5} + S_{q-6} + S_{q-7} + S_{q-8} \right)$$
, where S_{q-t} is sales per share for quarter t and S_{q-1} is sales per share reported by the company in the last pre-bankruptcy quarterly financial accounts. Firms in the top quartile by sales per share performance are assigned to the “high growth” group. Firms in the bottom quartile by sales per share performance are assigned to the “low growth” group. All other firms are allocated to the “not trending” group. Firms in each group are treated as a portfolio and their stock return performance is compared over a 12-month period using a BHAR strategy starting the second event-day after the Chapter 11 date. Event firms’ raw returns are adjusted using a control firm benchmark based on size or book-to-market or size and momentum. The null hypothesis is that there is no difference between the post-bankruptcy risk-adjusted abnormal returns of firms allocated to the “low growth” portfolio and the post-bankruptcy risk adjusted abnormal returns of firms allocated to the “high growth” or “not trending” portfolios. The alternative hypothesis is that there is a negative difference between the abnormal returns of firms allocated to the “low growth” portfolios and the risk adjusted abnormal returns of firms allocated to the “high growth” or “not trending” portfolios.

	Size and Book-to-market				Size and Momentum			
	Low growth group	High growth group	Not trending group	Anova and KW tests	Low growth group	High growth group	Not trending group	Anova and KW tests
Mean	-0.10	-0.28	-0.24	0.26	-0.22	-0.34	-0.11	0.54
p-value	0.6584	0.0809	0.1121	0.7678	0.2552	0.0067	0.4148	0.5810
Median	-0.18	-0.13	-0.16	0.10	-0.32	-0.28	-0.28	1.14
p-value	0.0240	0.0947	0.0239	0.9513	0.0132	0.0089	0.0615	0.5628
N	61	62	124	-	61	62	124	-

Table 6.1 (cont.): Testing the Barberis, Shleifer and Vishny (1998) model – relative financial performance

Panel B: In this panel I use the earnings per share measure to infer about the pre-bankruptcy trending regime. The earnings per share measure is given by:

$$\left[\left(EPS_{q-1} + EPS_{q-2} + EPS_{q-3} + EPS_{q-4} \right) - \left(EPS_{q-5} + EPS_{q-6} + EPS_{q-7} + EPS_{q-8} \right) \right] / \left(EPS_{q-5} + EPS_{q-6} + EPS_{q-7} + EPS_{q-8} \right)$$
, where EPS_{q-t} is earnings per share for quarter t and EPS_{q-1} is earnings per share reported by the company in the last pre-bankruptcy quarterly financial accounts. Firms in the top quartile by earnings per share performance are assigned to the “high growth” group. Firms in the bottom quartile by earnings per share performance are assigned to the “low growth” group. All other firms are allocated to the “not trending” group. Firms in each group are treated as a portfolio and their stock return performance is compared over a 12-month period using a BHAR strategy starting the second event-day after the Chapter 11 date. Event firms’ raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum. The null hypothesis is that there is no difference between the post-bankruptcy risk-adjusted abnormal returns of firms allocated to the “low growth” portfolio and the post-bankruptcy risk adjusted abnormal returns of firms allocated to the “high growth” or “not trending” portfolios. The alternative hypothesis is that there is a negative difference between the abnormal returns of firms allocated to the “low growth” portfolios and the risk adjusted abnormal returns of firms allocated to the “high growth” or “not trending” portfolios.

	Size and Book-to-market				Size and Momentum			
	Low growth group	High growth group	Not trending group	Anova and KW tests	Low growth group	High growth group	Not trending group	Anova and KW tests
Mean	0.13	-0.31	-0.32	2.38	0.20	-0.32	-0.34	4.11
p-value	0.4941	0.0340	0.0161	0.0948	0.2417	0.0060	0.0073	0.0174
Median	0.05	-0.35	-0.24	5.33	-0.02	-0.37	-0.32	7.44
p-value	0.9411	0.0045	0.0006	0.0694	0.7561	0.0003	0.0005	0.0241
N	69	70	140	-	69	70	140	-

Table 6.1 (cont.): Testing the Barberis, Shleifer and Vishny (1998) model – relative financial performance

Panel C: In this panel I use the net income per share measure to infer about the pre-bankruptcy trending regime. The net income per share measure is given by:

$$\left[\left(NI_{q-1} + NI_{q-2} + NI_{q-3} + NI_{q-4} \right) - \left(NI_{q-5} + NI_{q-6} + NI_{q-7} + NI_{q-8} \right) \right] / A_{q-5}$$
, where NI_{q-t} is net income per share for quarter t , NI_{q-1} is net income per share reported by the company in the last pre-bankruptcy quarterly financial accounts and A_{q-5} is assets per share reported by the company in quarter -5. Firms in the top quartile by net income per share performance are assigned to the “high growth” group. Firms in the bottom quartile by net income per share performance are assigned to the “low growth” group. All other firms are allocated to the “not trending” group. Firms in each group are treated as a portfolio and their stock return performance is compared over a 12-month period using a BHAR strategy starting the second event-day after the Chapter 11 date. Event firms’ raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum. The null hypothesis is that there is no difference between the post-bankruptcy risk-adjusted abnormal returns of firms allocated to the “low growth” portfolio and the post-bankruptcy risk adjusted abnormal returns of firms allocated to the “high growth” or “not trending” portfolios. The alternative hypothesis is that there is a negative difference between the abnormal returns of firms allocated to the “low growth” portfolios and the risk adjusted abnormal returns of firms allocated to the “high growth” or “not trending” portfolios.

	Size and Book-to-market				Size and Momentum			
	Low growth group	High growth group	Not trending group	Anova and KW tests	Low growth group	High growth group	Not trending group	Anova and KW tests
Mean	-0.16	-0.52	-0.08	1.70	0.02	-0.50	-0.19	2.07
p-value	0.3824	0.0424	0.5000	0.1841	0.9170	0.0183	0.1411	0.1278
Median	-0.15	-0.36	-0.07	5.56	-0.15	-0.47	-0.17	7.96
p-value	0.1958	0.0003	0.1201	0.0619	0.2792	<0.0001	0.0396	0.0187
N	62	62	124	-	62	62	124	-

Table 6.1 (cont.): Testing the Barberis, Shleifer and Vishny (1998) model – relative financial performance

Panel D: In this panel I use the sales per share measure to infer about event firms' trending regime. The sales per share measure is given by:

$$\left[\left(S_{q-1} + S_{q-2} + S_{q-3} + S_{q-4} \right) - \left(S_{q-5} + S_{q-6} + S_{q-7} + S_{q-8} \right) \right] / \left(S_{q-5} + S_{q-6} + S_{q-7} + S_{q-8} \right)$$
, where S_{q-t} is sales per share for quarter t and S_{q-1} is sales per share reported by the company in the last pre-bankruptcy quarterly financial accounts. Firms in the top quartile by sales per share performance are assigned to the "high growth" group. Firms in the bottom quartile by sales per share performance are assigned to the "low growth" group. All other firms are allocated to the "not trending" group. Firms in each group are treated as a portfolio and their stock return performance is compared over a 12-month period using a BHAR strategy starting the 252 trading days before the Chapter 11 date. Event firms' raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum.

	Size and Book-to-market				Size and Momentum			
	Low growth group	High growth group	Not trending group	Anova and KW tests	Low growth group	High growth group	Not trending group	Anova and KW tests
Mean	-0.50	-0.85	-0.45	2.07	-0.22	-0.22	-0.16	0.95
p-value	<0.0001	0.0007	<0.0001	0.1279	0.0009	<0.0001	<0.0001	0.3883
Median	-0.37	-0.48	-0.37	5.33	-0.08	-0.14	-0.10	2.88
p-value	<0.0001	<0.0001	<0.0001	0.0696	<0.0001	<0.0001	<0.0001	0.2366
N	61	62	124	-	61	62	124	-

Table 6.1 (cont.): Testing the Barberis, Shleifer and Vishny (1998) model – relative financial performance

Panel E: In this panel I use the earnings per share measure to infer about the event firms' trending regime. The earnings per share measure is given by:

$$\left[\left(EPS_{q-1} + EPS_{q-2} + EPS_{q-3} + EPS_{q-4} \right) - \left(EPS_{q-5} + EPS_{q-6} + EPS_{q-7} + EPS_{q-8} \right) \right] / \left(EPS_{q-5} + EPS_{q-6} + EPS_{q-7} + EPS_{q-8} \right)$$
, where EPS_{q-t} is earnings per share for quarter t and EPS_{q-1} is earnings per share reported by the company in the last pre-bankruptcy quarterly financial accounts. Firms in the top quartile by earnings per share performance are assigned to the “high growth” group. Firms in the bottom quartile by earnings per share performance are assigned to the “low growth” group. All other firms are allocated to the “not trending” group. Firms in each group are treated as a portfolio and their stock return performance is compared over a 12-month period using a BHAR strategy starting the 252 trading days before the Chapter 11 date. Event firms' raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum.

	Size and Book-to-market				Size and Momentum			
	Low growth group	High growth group	Not trending group	Anova and KW tests	Low growth group	High growth group	Not trending group	Anova and KW tests
Mean	-0.46	-0.73	-0.52	0.91	-0.13	-0.24	-0.19	1.53
p-value	<0.0001	0.0014	<0.0001	0.4034	<0.0001	<0.0001	<0.0001	0.2192
Median	-0.41	-0.36	-0.43	0.32	-0.09	-0.13	-0.10	4.62
p-value	<0.0001	<0.0001	<0.0001	0.8535	<0.0001	<0.0001	<0.0001	0.0988
N	69	70	140	-	69	70	140	-

Table 6.1 (cont.): Testing the Barberis, Shleifer and Vishny (1998) model – relative financial performance

Panel F: In this panel I use the net income per share measure to infer about the sample firms' trending regime. The net income per share measure is given by:

$$\left[\left(NI_{q-1} + NI_{q-2} + NI_{q-3} + NI_{q-4} \right) - \left(NI_{q-5} + NI_{q-6} + NI_{q-7} + NI_{q-8} \right) \right] / A_{q-5}$$
 where NI_{q-t} is net income per share for quarter t , NI_{q-1} is net income per share reported by the company in the last pre-bankruptcy quarterly financial accounts and A_{q-5} is assets per share reported by the company in quarter -5. Firms in the top quartile by net income per share performance are assigned to the "high growth" group. Firms in the bottom quartile by net income per share performance are assigned to the "low growth" group. All other firms are allocated to the "not trending" group. Firms in each group are treated as a portfolio and their stock return performance is compared over a 12-month period using a BHAR strategy starting the 252 trading days before the Chapter 11 date. Event firms' raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum.

	Size and Book-to-market				Size and Momentum			
	Low growth group	High growth group	Not trending group	Anova and KW tests	Low growth group	High growth group	Not trending group	Anova and KW tests
Mean	-0.68	-0.50	-0.53	0.36	-0.17	-0.23	-0.18	0.52
p-value	0.0013	0.0179	<0.0001	0.6977	<0.0001	0.0005	<0.0001	0.5923
Median	-0.37	-0.38	-0.44	1.07	-0.10	-0.13	-0.07	3.18
p-value	<0.0001	<0.0001	<0.0001	0.5838	<0.0001	<0.0001	<0.0001	0.2038
N	62	62	124	-	62	62	124	-

Table 6.2 summarizes what happens when firms are ranked according to their absolute financial performance. Consistent with the evidence presented above, all size and book-to-market results suggest that, in the post-event period, there is *no difference* in mean and median performance of the three portfolios under analysis (panels A, B and C). In fact, irrespective of the particular accounting variable used to rank the sample firms, both the ANOVA and the Kruskal-Wallis tests are *not* significant even at a ten percent level.

This conclusion, however, only holds partially when size and momentum is used to control for risk. Under this alternative setting, results from the Kruskal-Wallis test are significant at the ten percent level when earnings per share and net income per share are the key accounting variables. Yet, in both cases, the difference in performance detected by this non-parametric test seems to be driven by the “not-trending” portfolio. In effect, both when earnings per share and net income per share are employed to rank the sample firms, the mean and median abnormal returns computed for the “not-trending” portfolio are always strongly negative and significant, something that does not occur for the other two portfolios. For them, the mean and median abnormal returns are not statistically different from zero even at a ten percent level.

Additionally, Panels D, E and F show that, in the pre-event period, all portfolios have negative abnormal returns, which are usually statistically different from zero at normal levels. This is important because it indicates that bankruptcy *does not* prompt a reversal in the stock return pattern of firms allocated to the “high growth” portfolio as predicted by the Barberis, Shleifer and Vishny (1998) model.

Table 6.2

Testing the Barberis, Shleifer and Vishny (1998) model – absolute financial performance

This table presents the results using the absolute financial performance measure for testing the empirical implications of the Barberis, Shleifer and Vishny (1998) model with my 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. In the panels below, event firms are matched with firms with similar size and book-to-market or similar size and momentum. When the size and book-to-market benchmark is employed, for every event firm, I start by identifying 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 event firm. When the size and momentum benchmark is employed, for every event firm, I start by identifying 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 momentum closest to that of the event firm. In the panels below, the Consistently low growth, Consistently high growth and Not trending columns present the mean and median risk-adjusted returns of the “consistently low growth”, “consistently high growth” and “not-trending” portfolios, respectively. The two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median). In the panels below, the Anova and KW test column reports the result of a one-way ANOVA test (Kruskall-Wallis test) that checks the significance of the mean (median) difference in performance between the “low growth”, “high growth” and “not-trending” portfolios. For the one-way ANOVA test, the value of the F-test and its significance level are reported. For the Kruskall-Wallis test the value of the Chi-square test and its significance level are reported. In all panels below, *N* indicates the number of companies included in the “low growth”, “high growth” and “not-trending” portfolio.

Table 6.2 (cont.): Testing the Barberis, Shleifer and Vishny (1998) model – absolute financial performance

Panel A: In this panel I use the sales per share measure to infer about the pre-bankruptcy trending regime of each event firm. I start by identifying the financial accounts of quarter 0, which are those reported by each sample firm right after its bankruptcy date. After this, I identify the accounts in quarter -1 (-4), which are those reported one quarter (four quarters) before the accounts of quarter 0. I then compute the variation in sales per share between each consecutive quarter (quarter -3 and quarter -4, quarter -2 and quarter -3 and so forth). Firms are allocated to the “consistently low growth” (“consistently high growth”) group when all quarterly variations are negative (positive). The remaining companies are assigned to the “not trending” group. Firms in each group are treated as a portfolio and their stock return performance is compared over a 12-month period using a BHAR strategy starting the second event-day after the Chapter 11 date. Event firms’ raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum. The null hypothesis is that there is no difference between the post-bankruptcy risk-adjusted abnormal returns of firms allocated to the “low growth” portfolio and the post-bankruptcy risk adjusted abnormal returns of firms allocated to the “high growth” or “not trending” portfolios. The alternative hypothesis is that there is a negative difference between the abnormal returns of firms allocated to the “low growth” portfolios and the risk adjusted abnormal returns of firms allocated to the “high growth” or “not trending” portfolios.

	Size and Book-to-market				Size and Momentum			
	Consistently low growth	Consistently high growth	Not trending	Anova and KW tests	Consistently low growth	Consistently high growth	Not trending	Anova and KW tests
Mean	-0.68	-0.50	-0.13	1.97	-0.22	0.08	-0.22	0.36
p-value	0.1258	0.2052	0.1923	0.1411	0.3958	0.8245	0.0268	0.7004
Median	-0.49	-0.41	-0.15	3.25	-0.17	-0.14	-0.30	0.25
p-value	0.0405	0.0395	0.0084	0.1969	0.3069	0.3060	0.0003	0.8847
N	30	17	203	-	30	17	203	-

Table 6.2 (cont.): Testing the Barberis, Shleifer and Vishny (1998) model – absolute financial performance

Panel B: In this panel I use the earnings per share measure to infer about the pre-bankruptcy trending regime of each event firm. I start by identifying the financial accounts of quarter 0, which are those reported by each sample firm right after its bankruptcy date. After this, I identify the accounts in quarter -1 (-4), which are those reported one quarter (four quarters) before the accounts of quarter 0. I then compute the variation in earnings per share between each consecutive quarter (quarter -3 and quarter -4, quarter -2 and quarter -3 and so forth). Firms are allocated to the “consistently low growth” (“consistently high growth”) group when all quarterly variations are negative (positive). The remaining companies are assigned to the “not trending” group. Firms in each group are treated as a portfolio and their stock return performance is compared over a 12-month period using a BHAR strategy starting the second event-day after the Chapter 11 date. Event firms’ raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum. The null hypothesis is that there is no difference between the post-bankruptcy risk-adjusted abnormal returns of firms allocated to the “low growth” portfolio and the post-bankruptcy risk adjusted abnormal returns of firms allocated to the “high growth” or “not trending” portfolios. The alternative hypothesis is that there is a negative difference between the abnormal returns of firms allocated to the “low growth” portfolios and the risk adjusted abnormal returns of firms allocated to the “high growth” or “not trending” portfolios.

	Size and Book-to-market				Size and Momentum			
	Consistently low growth	Consistently high growth	Not trending	Anova and KW tests	Consistently low growth	Consistently high growth	Not trending	Anova and KW tests
Mean	-0.07	0.21	-0.26	0.68	0.01	0.49	-0.27	2.00
p-value	0.6791	0.6291	0.0072	0.5083	0.9664	0.3462	0.0024	0.1371
Median	-0.09	-0.04	-0.22	1.41	0.09	-0.02	-0.32	5.35
p-value	0.6621	0.7002	<0.0001	0.4933	0.7243	0.8984	<0.0001	0.0688
N	25	11	254	-	25	11	254	-

Table 6.2 (cont.): Testing the Barberis, Shleifer and Vishny (1998) model – absolute financial performance

Panel C: In this panel I use the net income per share measure to infer about the pre-bankruptcy trending regime of each event firm. I start by identifying the financial accounts of quarter 0, which are those reported by each sample firm right after its bankruptcy date. After this, I identify the accounts in quarter -1 (-4), which are those reported one quarter (four quarters) before the accounts of quarter 0. I then compute the variation in net income per share between each consecutive quarter (quarter -3 and quarter -4, quarter -2 and quarter -3 and so forth). Firms are allocated to the “consistently low growth” (“consistently high growth”) group when all quarterly variations are negative (positive). The remaining companies are assigned to the “not trending” group. Firms in each group are treated as a portfolio and their stock return performance is compared over a 12-month period using a BHAR strategy starting the second event-day after the Chapter 11 date. Event firms’ raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum. The null hypothesis is that there is no difference between the post-bankruptcy risk-adjusted abnormal returns of firms allocated to the “low growth” portfolio and the post-bankruptcy risk adjusted abnormal returns of firms allocated to the “high growth” or “not trending” portfolios. The alternative hypothesis is that there is a negative difference between the abnormal returns of firms allocated to the “low growth” portfolios and the risk adjusted abnormal returns of firms allocated to the “high growth” or “not trending” portfolios.

	Size and Book-to-market				Size and Momentum			
	Consistently low growth	Consistently high growth	Not trending	Anova and KW tests	Consistently low growth	Consistently high growth	Not trending	Anova and KW tests
Mean	-0.02	0.21	-0.25	0.69	-0.18	0.60	-0.26	2.07
p-value	0.9102	0.6048	0.0226	0.5023	0.3932	0.2066	0.0086	0.1279
Median	-0.28	-0.03	-0.20	1.67	-0.12	0.08	-0.32	5.83
p-value	0.9063	0.7334	0.0002	0.4325	0.4964	0.2334	<0.0001	0.0541
N	23	12	213	-	23	12	213	-

Table 6.2 (cont.): Testing the Barberis, Shleifer and Vishny (1998) model – absolute financial performance

Panel D: In this panel I use the sales per share measure to infer about trending regime of each event firm. I start by identifying the financial accounts of quarter 0, which are those reported by each sample firm right after its bankruptcy date. After this, I identify the accounts in quarter -1 (-4), which are those reported one quarter (four quarters) before the accounts of quarter 0. I then compute the variation in sales per share between each consecutive quarter (quarter -3 and quarter -4, quarter -2 and quarter -3 and so forth). Firms are allocated to the “consistently low growth” (“consistently high growth”) group when all quarterly variations are negative (positive). The remaining companies are assigned to the “not trending” group. Firms in each group are treated as a portfolio and their stock return performance is compared over a 12-month period using a BHAR strategy starting the 252 trading days before the Chapter 11 date. Event firms’ raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum.

	Size and Book-to-market				Size and Momentum			
	Consistently low growth	Consistently high growth	Not trending	Anova and KW tests	Consistently low growth	Consistently high growth	Not trending	Anova and KW tests
Mean	-0.84	-0.38	-0.57	0.89	-0.18	-0.18	-0.19	0.03
p-value	0.0419	0.0417	<0.0001	0.4137	0.0215	0.0485	<0.0001	0.9718
Median	-0.47	-0.42	-0.40	0.51	-0.06	-0.12	-0.11	2.43
p-value	<0.0001	0.0202	<0.0001	0.7752	0.0160	0.0305	<0.0001	0.2956
N	30	17	203	-	30	17	203	-

Panel E: In this panel I use the earnings per share measure to infer about the trending regime of each event firm. I start by identifying the financial accounts of quarter 0, which are those reported by each sample firm right after its bankruptcy date. After this, I identify the accounts in quarter -1 (-4), which are those reported one quarter (four quarters) before the accounts of quarter 0. I then compute the variation in earnings per share between each consecutive quarter (quarter -3 and quarter -4, quarter -2 and quarter -3 and so forth). Firms are allocated to the “consistently low growth” (“consistently high growth”) group when all quarterly variations are negative (positive). The remaining companies are assigned to the “not trending” group. Firms in each group are treated as a portfolio and their stock return performance is compared over a 12-month period using a BHAR strategy starting the 252 trading days before the Chapter 11 date. Event firms’ raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum.

	Size and Book-to-market				Size and Momentum			
	Consistently low growth	Consistently high growth	Not trending	Anova and KW tests	Consistently low growth	Consistently high growth	Not trending	Anova and KW tests
Mean	-0.47	-0.20	-0.58	0.57	-0.04	-0.19	-0.20	2.18
p-value	<0.0001	0.4207	<0.0001	0.5639	0.3917	0.0344	<0.0001	0.1149
Median	-0.48	-0.34	-0.42	0.85	-0.07	-0.16	-0.10	6.32
p-value	<0.0001	0.1748	<0.0001	0.6533	0.0152	0.0322	<0.0001	0.0423
N	25	11	254	-	25	11	254	-

Table 6.2 (cont.): Testing the Barberis, Shleifer and Vishny (1998) model – absolute financial performance

Panel F: In this panel I use the net income per share measure to infer about the trending regime of each event firm. I start by identifying the financial accounts of quarter 0, which are those reported by each sample firm right after its bankruptcy date. After this, I identify the accounts in quarter -1 (-4), which are those reported one quarter (four quarters) before the accounts of quarter 0. I then compute the variation in net income per share between each consecutive quarter (quarter -3 and quarter -4, quarter -2 and quarter -3 and so forth). Firms are allocated to the “consistently low growth” (“consistently high growth”) group when all quarterly variations are negative (positive). The remaining companies are assigned to the “not trending” group. Firms in each group are treated as a portfolio and their stock return performance is compared over a 12-month period using a BHAR strategy starting the 252 trading days before the Chapter 11 date. Event firms’ raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum.

	Size and Book-to-market				Size and Momentum			
	Consistently low growth	Consistently high growth	Not trending	Anova and KW tests	Consistently low growth	Consistently high growth	Not trending	Anova and KW tests
Mean	-0.57	-0.96	-0.53	0.58	-0.14	-0.13	-0.19	0.37
p-value	<0.0001	0.2459	<0.0001	0.5590	0.0009	0.0478	<0.0001	0.6923
Median	-0.44	-0.39	-0.39	0.95	-0.07	-0.14	-0.10	0.58
p-value	<0.0001	0.0711	<0.0001	0.6215	0.0002	0.0342	<0.0001	0.7464
N	23	12	213	-	23	12	213	-

6.1.4 Summary and limitations

This section investigates whether the Barberis, Shleifer and Vishny (1998) model explains the stock return pattern associated with the announcement of corporate bankruptcy. My results suggest this is not the case. Specifically, I find that abnormal returns following Chapter 11 are not stronger for those firms with worst pre-event performance. According to Barberis, Shleifer and Vishny (1998), the opposite result should hold since, in this case, bankruptcy ought to act as a confirming signal of these firms' poor performance. Similarly, I do not find that bankruptcy prompts a reversal in the stock return pattern of the companies with the best pre-event performance. This result also contrasts with the predictions of the Barberis, Shleifer and Vishny (1998) model, which posits that disconfirming signals should lead to negatively autocorrelated returns.

Some may argue that the Barberis, Shleifer and Vishny (1998) model should not be used in my particular setting because it was not specifically developed to account for return patterns associated with information events. I would argue otherwise. To see why, consider the case of the EMH. Over the years, the academic community has claimed that the EMH is an encompassing theory that explains how financial markets work in *virtually every* conceivable scenario one might contemplate. Similarly, Fama (1998) claims that theoretical models are only of interest if one can use them to make *general* predictions about the behaviour of markets that are testable in practice. Chan, Frankel and Kothari (2004) rely on the exact same argument to justify using the key behavioural concepts of the Barberis, Shleifer and Vishny (1998) model (i.e., representativeness and conservatism) in explaining the momentum effect. In practice, Chan, Frankel and Kothari (2004) are the first to use an out-of-the sample test to check the predictive ability of the Barberis, Shleifer and Vishny (1998) model since, in its original form, the model uses earnings to motivate its results. Incidentally, the authors' findings also *do not* support the predictions of Barberis, Shleifer and Vishny (1998). In summary, although testing the Barberis, Shleifer and Vishny (1998) model outside the scope for which it was designed for may sound unreasonable, the fact is that this is the only scientific way to verify its ability to explain how real world financial markets work (Barberis and Thaler, 2005, pp. 64-65).

My results also shed (indirect) light on how the alternative model proposed by Daniel, Hirshleifer and Subrahmanyam (1998) would perform in the context I address. Clearly, this model has different behavioural foundations than that of Barberis, Shleifer and Vishny (1998). In fact, Daniel, Hirshleifer and Subrahmanyam (1998, p. 1841) posit that stock prices are determined by informed investors that are subject to two biases, overconfidence and biased self-attribution. Overconfident investors exaggerate the precision of their private signals about a stock's value, which is consistent with Edwards's (1968) conservatism bias (Chan, Frankel and Kothari, 2004, p. 11). In addition, biased self-attribution causes investors to down-weight public signals about value, especially when the public signals contradict their private signals. Hence, if sequences of similar public signals imply a positive correspondence between private and public signals, Daniel, Hirshleifer and Subrahmanyam (1998) predict investors will over-infer from a sequence of good (bad) news announcements in forming upward (downward) trending expectations (Chan, Frankel and Kothari, 2004, p. 11). This behaviour is akin to the representativeness bias documented by Tversky and Kahneman (1974). In short, Daniel, Hirshleifer and Subrahmanyam (1998) suggest that overreaction to private information and underreaction to public information tend to produce short-term continuation of stock returns but long-term reversals as public information eventually overwhelms the behavioural biases. It follows that, though based on different behavioural premises, the Daniel, Hirshleifer and Subrahmanyam's (1998) predictions are close to those of Barberis, Shleifer and Vishny (1998), with the former model sharing the same empirical successes and failures of the later, a point emphasized by Fama (1998, p. 289) and Chan, Frankel and Kothari (2004, p. 12). Hence, my results provide indirect evidence suggesting that the Daniel, Hirshleifer and Subrahmanyam (1998) would not be able to correctly capture the return pattern associated with the announcement of Chapter 11 bankruptcy.

There are, however, some limitations affecting this section's results. Firstly, I consider just a few accounting variables to determine whether or not a particular firm is trending before filing for Chapter 11. In order to verify the importance of this shortcoming, I rerun my analysis using different accounting measures like operating income per share, total assets per share and cash-

flow per share and find essentially the same results. Secondly, all tests use only one year of accounting data to catalogue each firm as trending or not trending before the event. Some may argue that this is a very short window that biases the results against finding evidence in favour of the Barberis, Shleifer and Vishny (1998) model. Following Chan, Frankel and Kothari (2004), I rerun my analysis using five years of accounting data reported before the bankruptcy announcement year. This alternative approach makes it impossible to implement the absolute financial performance test due to the very small number of firms with consistent positive performance in this five-year horizon. Nevertheless, the results obtained for the relative financial performance test are similar to those reported here.

Some other methodological aspects merit further attention. Perhaps the most important one relates to the small number of firms available to conduct the absolute financial performance test. In fact, in the worst scenario, only 11 firms are allocated to the “consistently high growth” portfolio. This creates an obvious problem with the interpretation of the one-way ANOVA test, which should be disregarded in this case. Nevertheless, the Kruskal-Wallis test is still valid in this context (e.g., Newbold, Carlson and Thorne, 2003, p. 595). There is also the question of how risk is factored into the analysis. All tests are based on size and book-to-market or size and momentum risk-adjusted returns since previous research shows that size, book-to-market and momentum are important risk factors priced by the market (e.g., Fama and French, 1992, 1993; Barber and Lyon, 1997; Carhart, 1997; Ang and Zhang, 2004). Nevertheless, given the special nature of bankruptcy, I rerun my analysis to control for additional risk-factors like industry, bankruptcy probability and impact of low-price stocks and find essentially the same results.

6.2 Corporate bankruptcy and the Hong and Stein (1999) model

Hong and Stein (1999) posit that under and overreaction are the result of the interaction between heterogeneous investors. In their basic model, there are two types of agents: newswatchers and momentum traders. Both are boundedly rational in the sense that they can only process a subset of all available information. Newswatchers make forecasts based on signals they privately observe about firms' fundamentals and are unable to evaluate current or past prices to make their investment decisions. A further critical assumption is that private information about firms diffuses gradually across the newswatchers population, i.e., newswatchers learn and process more fundamental information as time goes by. On the other hand, momentum traders rely only on the stock price pattern to devise their investment strategies. In fact, in Hong and Stein's (1999) basic framework, they are unable to process fundamental information as newswatchers do.

Under this setup, the authors show how both under and overreaction occur. The basic idea is that momentum traders exploit the fundamental information digested by newswatchers by engaging in positive feed-back strategies. Assume that newswatchers receive a partial signal suggesting that something positive is about to happen to a particular company. As a result, they adjust upwards their expectation about the company's fundamental value, setting a higher market price for its shares. In the next period, momentum traders verify that the stock price has gone up and, given that they follow a positive feed-back investment strategy, push the price upwards again. In the next round, another partial positive signal about the company is disclosed and the correspondent adjustment in price takes place.⁹⁷ Clearly, this pattern describes a situation of *underreaction*, where prices are slowly adjusting to a flow of continuous information. Hong and Stein (1999) predict that such process continues until: 1) the full positive signal is known by newswatchers and 2) momentum traders have pushed the stock price above its fundamental value. When this occurs, the market is actually *overreacting* to the initial information. At this point, Hong and Stein (1999) predict a price reversal. This happens because, in both situations, newswatchers have no reason to revise upwards their expectations

⁹⁷ This happens because information diffuses *gradually* among the newswatchers population.

thus interrupting the “momentum cycle”. In the end, prices slowly converge back to fundamentals as more and more momentum traders realize that the current market price is higher than the firm’s fundamental value.

6.2.1 Empirical implications

The Hong and Stein (1999) model was not developed to explain how markets deal with information events. In fact, as Hong, Lim and Stein (2000) emphasize, it is better suited for explaining market anomalies that are not event-driven like the momentum effect of Jegadeesh and Titman (1993). Yet, it is not difficult to use the concepts of the Hong and Stein (1999) model to derive testable predictions in alternative empirical settings. The most important aspect is acknowledging that the rate with which firm-specific information flows to the market determines the seriousness of the mispricing affecting firms’ securities (Hong, Lim and Stein, 2000). In the absence of value-relevant public information, the model predicts that mispricing is more likely to occur for the sub-set of firms for which information is scarce and/or hard to get and process. In the Hong and Stein (1999) world this means that, for these particular companies, newswatchers require more time to impound all their information into the stock price, which results in longer momentum cycles and higher degrees of mispricing.

What happens when a public information-event occurs? As Hong and Stein (1999) point out, an isolated event is likely to be meaningless unless supplementary information is released to the market. Such information is crucial because it enables the market to fully and clearly assess the value-relevance of the initial signal. As such, Hong and Stein (1999) claim that the degree of mispricing following a public event is conditional on complementary information being quickly revealed to and digested by the market. Drawing on this intuition, I propose to test the following null hypotheses to test the empirical implications of the Hong and Stein (1999) model:

H₆: There is no difference between the pre-event risk-adjusted abnormal returns of firms for which information is scarce/hard to get and the pre-event risk-adjusted abnormal returns of all other sample firms.⁹⁸

H₇: There is no difference between the post-event risk-adjusted abnormal returns of firms for which information is scarce/hard to get and the post-event risk-adjusted abnormal returns of all other sample firms.⁹⁹

Despite the similar wording, the implications of these two hypotheses are fundamentally different. The first is similar to that of Hong, Lim and Stein (2000), who test if the Hong and Stein (1999) theoretical model provides a good explanation for the momentum anomaly. Using size and residual analyst following as proxies for gradual diffusion of information, they show that momentum is more pronounced for those firms where information is more likely to diffuse gradually. Notice that, in this context, there is *no specific* information event conditioning the stock return pattern. In the context of this research, my first null hypothesis captures exactly the same effect.

My second null hypothesis is fundamentally different because it explores whether the Hong and Stein (1999) model has predictive ability when dealing with stock return patterns *driven* by a well-defined firm-specific event, something for which it was not initially designed for. To the best of my knowledge, this line of research has not yet been pursued in the literature.

⁹⁸ The alternative hypothesis is that there is a negative difference between the pre-event risk-adjusted abnormal returns of firms for which information is scarce/hard to get and the pre-event risk-adjusted abnormal returns of all other sample firms.

⁹⁹ The alternative hypothesis is that there is a negative difference between the post-event risk-adjusted abnormal returns of firms for which information is scarce/hard to get and the post-event risk-adjusted abnormal returns of all other sample firms.

6.2.2 What accounts for gradual diffusion of firm-specific information?

Testing the implications of the Hong and Stein (1999) model requires describing how firm-specific information is diffused to the market. Drawing on previous research, I focus my attention on six different factors that, *a priori*, should be important in this context: 1) firm size; 2) institutional ownership; 3) news coverage; 4) analyst following; 5) behaviour of insiders and 6) informativeness of firms' financial statements. The next paragraphs discuss how these factors can be used to proxy for the gradual diffusion of firms-specific information.

1. *Firm size*: Hong, Lim and Stein (2000) argue that the smaller the firm the harder it is to get timely and accurate information about it. This should be the case if investors face fixed costs of information acquisition and hence choose, in the aggregate, to devote more effort to learning about those stocks in which they can take large positions and trade more frequently. As a result, one should expect the market to misprice more severely smaller firms relative to larger ones. The problem is that size also proxies for other confounding factors that may affect the results. A clear example is that trading costs are higher for smaller firms (e.g., Lesmond, Schill and Zhou, 2004). Hong, Lim and Stein (2000) also emphasize this idea and warn against relying solely on size to make inferences about the relationship between the market's mispricing of stocks and gradual diffusion of information.

2. *Institutional ownership*: chapter 5 discusses the relevance of institutional investors for the development of this research. For the purpose at hand, suffice to say that institutions have access to a variety of news reports, analyses and the guidance of professional money managers that should put them in a better position to evaluate the fundamental value of the assets they decide to invest in. Consequently, these market participants' actions condition the rate with which firm-specific information flows to market. To see why, consider two companies that are fundamentally equivalent. The difference is that company A is owned only by individual investors while company B is owned in equal percentages by individuals and institutions. Institutional investors continuously require information about company B in order to dynamically adjust their position in the company. The same does not apply to company A because there are

no institutions holding its stock. Accordingly, the production of firm-specific information must be higher for company B than for company A. Clearly, the common trader cannot act on this proprietary information since, in principle, he does not have access to it. However, he can gain some insight into this information by looking at the trading patterns of institutional investors. In fact, these market participants will change their position in any given company whenever their private information signals that such adjustment is required. It follows that firms with lower institutional coverage should face more acute problems of gradual diffusion of information and thus be more exposed to mispricing.

3. *News coverage*: this is perhaps the cleanest variable that one can use to test the empirical implications of the Hong and Stein (1999) model. In fact, news unrelated to the announcement of financial statement information and analysts' disclosures provide relevant and additional information that market participants can use to determine their investment strategies (Frankel and Li, 2004). Clear support for this claim can be found in previous literature. For example, pre-emptive news releases affect the market's reaction to earnings announcements (Atiase, 1985). Additional research shows that news analysis stories can directly affect companies' stock prices (e.g., Chan, 2003). In a recent contribution, Ryan and Taffler (2004) find that reported corporate news-events drive an important proportion of the London Stock Exchange 350 largest firms' significant stock price changes and trading volume activity. Accordingly, problems with gradual diffusion of information (and mispricing) should be more severe for firms with lower news coverage.

4. *Analysts following*: this is the key variable used by Hong, Lim and Stein (2000) to test if the gradual diffusion of information argument drives the momentum anomaly. The authors posit that stocks with lower analyst coverage should, all else being equal, be the ones where firm specific information diffuses more slowly across the investing public. I adopt the same framework here.

5. *Behaviour of insiders*: the SEC defines an insider as an officer of the firm or a major stockholder that holds more than ten percent of the firm's outstanding stock.¹⁰⁰ Previous research almost unanimously reports that these market participants are better informed and earn excess returns when trading on their private information (e.g., Jaffe, 1974; Finnerty, 1976; Seyhun 1986 and 1998; Rozeff and Zaman, 1988; Lakonishok and Lee, 2001). Moreover, previous studies also find that insiders are able to profit from the announcement of corporate bankruptcy (e.g., Seyhun and Bradley, 1997; Ma, 2001). However, such information is revealed to the market when these market participants decide to trade. It follows that other investors can update their own beliefs about the future prospects of companies by examining how their insiders are trading. This suggests that mispricing is concentrated on firms with relative fewer active insiders since the amount of firm-specific information increases with insiders' activity.

6. *Informativeness of firm's financial statements*: Beginning with the seminal paper of Ball and Brown (1968), finance and accounting research has focused considerable attention on the value-relevance of firms' financial statements and many authors have emphasized their central importance in facilitating the existence of robust and efficient capital markets (e.g., Beresford, 1997; Sutton, 1997). As a result, it is widely accepted that financial statements are one of the most important sources of information for investors (Frankel and Li, 2004). Relating to this point, the SEC states "*the annual report to security holders has long been recognized as the most effective means of communication between management and security holders*".¹⁰¹ Importantly, Tversky and Kahneman (1974) and Hirshleifer and Teoh (2003) show that "*salience*" and "*availability*" are central aspects of how investors form their expectations, while Chan, Frankel and Kothari (2004) claim that financial information is both salient and easily available to a broad cross-section of investors. Nonetheless, from an information point of view, financial statements can be more important for some firms than others. To see why, consider the case of companies A and B. Both firms are in the same industry but are fundamentally different. In particular, assume that company A has wide analyst coverage, institutional investors hold the majority of the stock and it is regularly cited in the press. On the contrary,

¹⁰⁰ See <http://www.sec.gov/answers/form345.htm> and <http://www.sec.gov/answers/insider.htm> for details.

¹⁰¹ SEC Handbook, 2000, Section 102, para. 38,025.

the majority of company's B stock is held by individuals, the company has no analyst following and no news coverage. Now assume that both firms disclose their quarterly 10-Q report. It is easy to realise that, from an information perspective, this is a much more important event for company B than for company A. In fact, investors holding the stock of firm B may not have access to an alternative source of information and thus the financial accounts are of crucial importance to them. Of course, financial information is also important for those investing in company A, yet not *as much*. Several papers provide evidence in favour of this argument by finding an important variability in the cross-section of the informativeness of the firms' financial statements in a number of different research settings (e.g., Collins, Maydew and Weiss, 1997; Ely and Waymire, 1999; Francis and Schipper, 1999; Frankel and Li, 2004). As a result, I posit that mispricing is more likely to occur for those firms exhibiting higher dependence on financial accounts to disclose information to the market.

6.2.3 Data

Testing the Hong and Stein (1999) model requires information from a number of sources. Market data comes from the CRSP daily stock file and financial information is collected from COMPUSTAT's quarterly file. Institutional holdings data is gathered from Thomson Financial Network CDA/Spectrum Institutional holdings file. Data on analysts' coverage is from the I/B/E/S detail history file. Factiva is used to collect information on news coverage for each of my sample firms. This web-based product provides business news and information collected from more than 10,000 sources, including *The Wall Street Journal*, *The Financial Times*, *Dow Jones* and *Reuters* newswires and *The Associated Press*, as well as *Reuters Fundamentals* and *D&B company profiles*.¹⁰² Besides the depth of the sources covered by Factiva, the web-product also allows the researcher to conduct conditional keyword search about particular companies. The combination of these two factors makes Factiva ideal for compiling the data on news coverage for my sample firms.

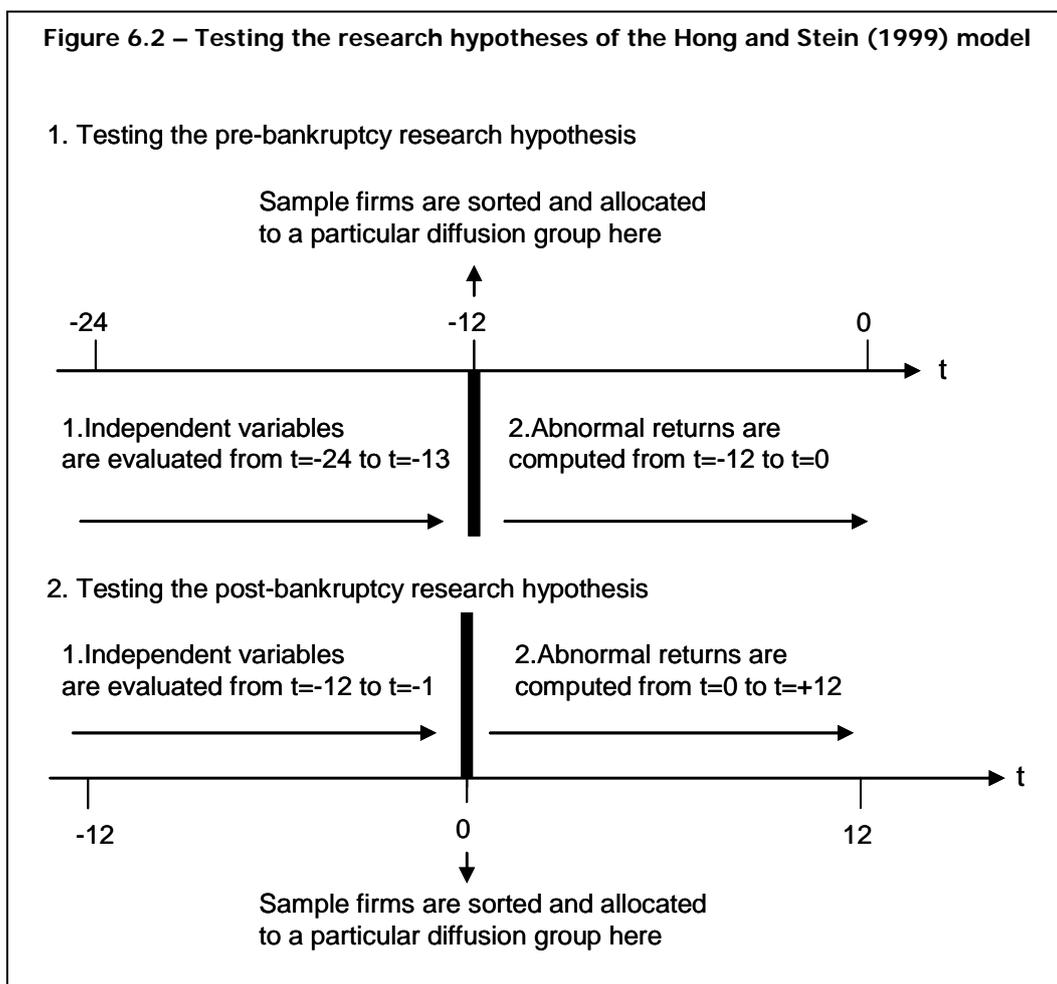
¹⁰² For more details go to <http://factiva.com/>.

I use the Thomson Financial Network Insider filing data files as my source of information for insiders' trading activity. Previous research emphasizes that working with these data is problematic. Drawing on Lakonishok and Lee (2001) and Frankel and Li (2004), I use several filters to ensure that my insider data are meaningful. Firstly, I do not include cases that Thomson Financial considers to be problematic (cleanse code A or S). Secondly, all duplicated and amended observations are excluded from the analysis. Thirdly, records with missing transaction dates are deleted. Additionally, I only consider cases with valid transactions prices and share volume. In particular, I drop all observations where the transaction price is not within 20 percent of the CRSP daily closing price or where the number of shares traded exceeds CRSP's daily trading volume. Fourthly, I eliminate all transactions that are not open market purchases or sells or that do not occur within a 25-month period centred on the bankruptcy date of one of my sample firms. Finally, I drop all non-management insider transactions. Management transactions refer to those trades made by officers, CEOs, CFOs, presidents, vice-presidents, treasurers, divisional officers, general partners and controlling persons. This is in line with Seyhun and Bradley (1997) and Ma (2001) who find that, for bankrupt firms, these top managers are better informed about the future prospects of their firms than all other insiders.¹⁰³

¹⁰³ Considering all corporate insiders yields essentially the same results. See the *Relationship Code Summary* table on WRDS's TFN Insider Filing Data support document for other possible insiders' codes.

6.2.4 Methodology

My methodology is very similar to that of Hong, Lim and Stein (2000). In their approach, the authors sort firms into classes for which information is, *a priori*, more or less likely to diffuse gradually. The major difference here lies in how I allocate firms into different information groups. Hong, Lim and Stein (2000) use a simple scheme, where only one independent variable is considered to achieve this objective (size or residual analysts coverage). In contrast, I create a more complex index using all six independent variables mentioned above. The details of my approach can be better understood with the help of figure 6.2:



In figure 6.2, time is measured in months, where $t = 0$ is the bankruptcy date. As figure 6.2 suggests, testing my research hypotheses entails two things. The first is computing and comparing abnormal returns of different diffusion information groups. I resort to the methodology described in section 3.3 to deal with this problem. The second is evaluating the

set of independent variables for each sample firm, which ultimately allows allocating each of them to a given diffusion group. Figure 6.2 shows that the major difference between testing my pre- and post-bankruptcy research hypothesis relates to the window employed to measure the independent variables and computing the abnormal returns. The method employed here ensures that the measurement of the independent variables does not overlap with the computation of abnormal returns, a key concern raised by Hong, Lim and Stein (2000). The next paragraphs detail how the independent variables are measured and how each sample firm is allocated to a particular gradual diffusion group.

6.2.4.1 Measuring the independent variables

1. *Firm size (Size)* - size is computed as the firm's market capitalization (number of shares outstanding times share price). For my pre-bankruptcy (post-bankruptcy) research hypothesis, I use the monthly average market capitalization between months -24 and -13 (-12 and -1).

2. *Institutional ownership (Inst)* – institutional ownership is measured as in chapter 5:

$$Inst_{i,t} = \frac{Shares\ held_{i,t}}{Shares\ outstanding_{i,t}} \quad (6.3)$$

where $Shares\ held_{i,t}$ is the number of shares of firm i held by the institutional investors in time t and $Shares\ outstanding_{i,t}$ is firm i 's outstanding shares in time t . For my pre-bankruptcy (post-bankruptcy) research hypothesis, this variable is computed with the most up-to-date information available on CDA/Spectrum Institutional Holdings file *just before* month -12 (0). Importantly, I set the value of $Inst_{i,t}$ to zero when there are no institutional investors holding stock of firm i .

3. *News coverage (News)* - the number of news articles published in the press is gathered with the help of Factiva's search tool. In particular, for each sample firm, I search all news

items that include the name of the company in the headline or leading paragraph, excluding all republished news and recurring pricing and market data. In order to compile the information for testing my two research hypotheses, this search is conducted twice, once for the period between month -24 and -13 and the other for the period between month -12 and -1. I focus my attention on the quantity rather than on the quality of the disclosures. In other words, no effort is made to separate potential “good” from “bad” news. This is consistent with previous research by Frankel and Li (2004) and my own objectives since the goal is simply verifying to what extent a particular company receives more or less attention by the media.

4. *Analysts following (Anfol)* - I measure the intensity of analyst activity as the number of analysts following the firm (e.g., Hong, Lim and Stein, 2000; Frankel and Li, 2004). In particular, for each sample firm, I identify from the I/B/E/S Detail History file all analysts with an I/B/E/S valid code providing estimates about the company in two different periods. The first encompasses months -24 to -13 and the second months -12 and -1. The number of analysts following is the simple count of the valid analysts' codes identified per firm in each of these periods. Importantly, in line with Frankel and Li (2004), this variable is set to zero for those companies where there are no analysts providing any type of forecast for the firm.

5. *Behaviour of insiders (Ins)* - I measure the intensity of insiders' activity as the number of insiders actively trading their firm's stock in a particular period. The approach here is very similar to that described for analysts in the previous point, but the information is now collected from the Thomson Financial Network Insider Filing Data files. There is, however, an additional complication relating to the fact that the insider data begins only in 1986. In effect, around 12 percent of my sample firms file for bankruptcy between 1980 and 1985 and thus, for these cases, I simply have no insider data available. Dropping all these companies from the analysis is one way to deal with this problem. Yet, this would generate an important bias in the results since I would be excluding *all* the earliest bankruptcy cases from this test. In order to overcome this problem, for all companies filing for bankruptcy before 1986, the number of

insiders is defined as the median value of this variable for all sample firms with available data.¹⁰⁴

6. *Informativeness of firm's financial statements (Fin)* – drawing on previous accounting research (e.g., Collins, Maydew and Weiss, 1997; Ely and Waymire, 1999; Francis and Schipper, 1999; Frankel and Li, 2004) this variable is measured using the adjusted R^2 from a firm-specific time-series regression. The regression model is given by:

$$P_{i,t} = \alpha_0 + \beta_1 E_{i,t} + \beta_2 BV_{i,t} + \varepsilon_{i,t} \quad (6.4)$$

where $P_{i,t}$ is the share price of firm i in time t , $E_{i,t}$ are the earnings per share of firm i in time t , $BV_{i,t}$ is book value per share of firm i in time t and $\varepsilon_{i,t}$ is the disturbance term, assumed to be white noise.¹⁰⁵ Several details about equation (6.4) need further clarification. For instance, I run the regression twice for each sample company, one for testing my pre-bankruptcy research hypothesis and another for testing my post-bankruptcy research hypothesis. For both test periods and each sample firm, I start by identifying the accounts in quarter 0, which for the first test (second test) are those disclosed by the firm *right after* month -12 (0). This is important because I require at least 8 quarters of financial information to run the regression, which can only be found once quarter 0 is defined. Furthermore, when testing both research hypotheses, only accounting information disclosed *prior* to quarter 0 is considered in the estimation of the regression's coefficients. This is essential since it prevents the overlap between the information used to estimate the adjusted R^2 and the abnormal returns employed to test both research hypotheses. Moreover, in all quarters and for all firms, $P_{i,t}$ is always measured two months after the quarter-end month, which ensures that the market price reflects the information conveyed by the firms' accounts. Finally, I set the value of

¹⁰⁴ To test the robustness of this assumption, I also consider setting the value of the variable for these firms at zero, the 25th percentile and the 75th percentile of the number of insiders trading for all firms with available data. Results are qualitatively similar to those reported here. Furthermore, dropping all cases from the analysis does not affect my overall conclusion in any meaningful way.

¹⁰⁵ COMPUSTAT's quarterly data items are as follows: earnings per share is Q19, book value of equity is Q59 and shares outstanding is (Q61*Q17).

the adjusted R^2 to zero for all companies that do not have enough information available to estimate equation (6.4). In simple terms, this is equivalent to saying that investors cannot use the firms' accounting information to define their investment strategies (Frankel and Li, 2004).

6.2.4.2 The gradual diffusion index

As figure 6.2 suggests, testing the Hong and Stein (1999) model requires allocating each sample firm to a particular information diffusion group. In this research, a firm-specific index built around the different independent variables summarized in the previous paragraphs is used to achieve this objective. I start by computing the values of each independent variable for all sample firms. Next, the companies are sorted into five classes based on their relative size.¹⁰⁶ This is a fundamental aspect of my methodology. In fact, Hong, Lim and Stein (2000) find that size and analysts following are highly correlated and suggest using a measure of analysts' coverage that explicitly controls for the effects of size. In other words, the authors emphasize that it is necessary to ensure that the independent variables are not all proxying for the same underlying factor, something that is not guaranteed when all of them are highly correlated. This argument is also valid in my setting. For instance, it is plausible that bigger firms receive more attention from the media, have more institutional investors interested in holding their stock and more analysts following. My approach creates five homogeneous groups of companies in terms of size, enabling me to control for the impact of such variable on the other independent variables.

¹⁰⁶ To be more precise, firms are sorted into quintiles based on their natural logarithm of size. In robustness tests, I also considered sorting the firms into quartiles and deciles based on the natural logarithm of size. Results are very similar to those presented below.

In the next step, firms within each size group are sorted into percentiles across the five remaining independent variables, one at a time. Each independent variable is then ordered in ascending order and percentiles are numbered from 1 (lowest) to 100 (highest). Firms in percentile one are given the ordinal value of one, firms in percentile two receive the value two and so forth. Crucially, this step is completed separately for each of the five independent variables. Once this task is completed, a gradual diffusion index is computed as follows:

$$GDI_{i,t} = Inst_{i,t} + News_{i,t} + Anfol_{i,t} + Ins_{i,t} + Fin_{i,t} \quad (6.5)$$

where $GDI_{i,t}$ is the size-adjusted gradual diffusion index for firm i in time t , and all other variables are the ordinal values for firm i in time t across the different independent variables mentioned in the previous section.

The last step of the process is allocating each sample firm to a given diffusion group. In particular, companies in the bottom quartile by their gradual diffusion index are labelled as “slow diffusion firms” while firms in the top quartile are labelled as “fast diffusion firms”. All other companies are assigned to the “middle diffusion” group. Companies in each group are treated as a portfolio and their stock price performance in the 12-month post-sorting period starting two days after the sorting date is compared with a one-way ANOVA and a Kruskal-Wallis test.

6.2.5 Results

Table 6.3 summarizes my results. The Hong and Stein (1999) model suggests that mispricing should be concentrated in those firms for which information diffuses more gradually. My results do not favour this prediction. For instance, panel A shows that, even when an information event is *not* conditioning the stock return pattern, the performance of the three information groups under analysis here *is not* statistically different. To be precise, in this setting and irrespective of the risk-adjustment technique, the results of the ANOVA and Kruskal-Wallis tests are not significant even at a ten percent level. This finding is at odds with that of Hong, Lim and Stein

(2000), who show that the profitability of momentum strategies steadily decreases with the rate with which firm-specific information flows to the market. Two reasons may help explain these contradictory findings. Firstly, the Hong, Lim and Stein (2000) main results *do not* consider the smallest firms present in the CRSP database (see page 269) whereas my analysis focuses particularly on this type of company (see table 3.2). Additionally, Hong, Lim and Stein (2000) do not specifically analyse highly distressed firms, which is precisely the object of my research.

Panel B reports what happens when one considers the performance of the three information portfolios after the announcement of bankruptcy. In line with panel A, I find that the mean and median post-event abnormal performance of these three groups *is not* statistically different. In other words, in this alternative setting, the p-values associated with the one-way ANOVA test and the Kruskal-Wallis test are *always* not significant at any meaningful level. Importantly, this conclusion holds for both size and book-to-market and size and momentum risk-adjusted returns.

Table 6.3.
Testing the Hong and Stein (1999) model

This table presents the results using the size-adjusted gradual diffusion index for testing the empirical implications of the Hong and Stein (1999) model with my 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. In the panels below, event firms are matched with firms with similar size and book-to-market or similar size and momentum. When the size and book-to-market benchmark is employed, for every event firm, I start by identifying 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 event firm. When the size and momentum benchmark is employed, for every event firm, I start by identifying 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 momentum closest to that of the event firm. In all panels below, the Slow diffusion, Fast diffusion and Middle diffusion columns present the mean and median risk-adjusted returns of the “slow diffusion”, “fast diffusion” and “middle diffusion” group, respectively. The two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the corresponding mean (median). In the panels below, the Anova and KW test column reports the result of a one-way ANOVA test (Kruskall-Wallis test) that checks the significance of the mean (median) difference in performance between the “slow diffusion”, “fast diffusion” and “middle diffusion” portfolios. For the one-way ANOVA test, the value of the F-test and its significance level are reported. For the Kruskall-Wallis test the value of the Chi-square test and its significance level are reported. In all panels below, *N* indicates the number of companies included in the “slow diffusion”, “fast diffusion” and “middle diffusion” portfolio.

Table 6.3. (cont.): Testing the Hong and Stein (1999) model

Panel A: In this panel I use the size-adjusted gradual diffusion index for testing the empirical implications of the Hong and Stein (1999) in the pre-bankruptcy period. I start by measuring the independent variables of the size-adjusted gradual diffusion index for each event firm as follows: 1) $Size_{i,(-24,-13)}$ - firm i 's monthly average market capitalization between event months -24 and -13; 2) $Inst_{i,-12}$ - firm i 's percentage of shares held by institutions twelve months before the bankruptcy date; 3) $News_{i,(-24,-13)}$ - firm i 's number of news articles published on the press between event months -24 and -13; 4) $Anfol_{i,(-24,-13)}$ - number of analyst following firm i between event months -24 and -13; 5) $Ins_{i,(-24,-13)}$ - number of insiders actively trading firm i 's stock between event months -24 and -13 and 6) $Fin_{i,-12}$ - informativeness of firm i 's financial statement twelve months before the bankruptcy date. Each event firm i is allocated to a size quintile based on its market capitalization. Next, within each size quintile, event firms are sorted into percentiles across the five remaining independent variables, one at a time. Each independent variable is then ordered in ascending order and percentiles are numbered from 1 (lowest) to 100 (highest). Firms in percentile one are given the ordinal value of one, firms in percentile two receive the value two and so forth. This step is completed separately for each of the five independent variables. The size-adjusted gradual diffusion index is then computed as follows: $GDI_{i,-12} = Inst_{i,-12} + News_{i,-12} + Anal_{i,-12} + Ins_{i,-12} + Fin_{i,-12}$, where $GDI_{i,-12}$ is firm i 's size-adjusted gradual diffusion index twelve months before the bankruptcy date, and all other variables are the ordinal values for firm i twelve months before the event date across the different independent variables. Event firms in the bottom quartile by their size-adjusted gradual diffusion index are labelled as "slow diffusion" firms while firms in the top quartile are labelled as "fast diffusion" firms. All other companies are assigned to the "middle diffusion" group. Firms in each group are treated as a portfolio and their stock return performance is compared over a one-year period using a BHAR strategy starting 252 trading days before the Chapter 11 date. Event firms' raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum. The null hypothesis is that there is no difference between the abnormal returns of firms allocated to the "slow diffusion" group and the abnormal returns of all remaining sample firms. The alternative hypothesis is that there is a negative difference between the abnormal returns of firms allocated to the "slow diffusion" group and the abnormal returns of all remaining sample firms.

	Size and Book-to-market				Size and Momentum			
	Slow diffusion group	Fast diffusion group	Middle diffusion group	Anova and KW test	Slow diffusion group	Fast diffusion group	Middle diffusion group	Anova and KW test
Mean	-0.54	-0.52	-0.45	0.39	-0.24	-0.17	-0.17	1.11
p-value	<0.0001	0.0007	<0.0001	0.6747	<0.0001	0.0002	<0.0001	0.3313
Median	-0.53	-0.34	-0.43	2.02	-0.10	-0.09	-0.10	0.36
p-value	<0.0001	<0.0001	<0.0001	0.3641	<0.0001	<0.0001	<0.0001	0.8332
N	88	87	176	-	88	87	176	-

Table 6.3. (cont.): Testing the Hong and Stein (1999) model

Panel B: In this panel I use the size-adjusted gradual diffusion index for testing the empirical implications of the Hong and Stein (1999) in the post-bankruptcy period. I start by measuring the independent variables of the size-adjusted gradual diffusion index for each event firm as follows: 1) $Size_{i,(-12,-1)}$ - firm i 's monthly average market capitalization between event months -12 and -1; 2) $Inst_{i,-1}$ - firm i 's percentage of shares held by institutions just before the bankruptcy date; 3) $News_{i,(-12,-1)}$ - firm i 's number of news articles published on the press between event months -24 and -1; 4) $Anfol_{i,(-12,-1)}$ - number of analyst following firm i between event months -12 and -1; 5) $Ins_{i,(-12,-1)}$ - number of insiders actively trading firm i 's stock between event months -12 and -1 and 6) $Fin_{i,-1}$ - informativeness of firm i 's financial statement just before the bankruptcy date. Each event firm i is allocated to a size quintile based on its market capitalization. Next, within each size quintile, event firms are sorted into percentiles across the five remaining independent variables, one at a time. Each independent variable is then ordered in ascending order and percentiles are numbered from 1 (lowest) to 100 (highest). Firms in percentile one are given the ordinal value of one, firms in percentile two receive the value two and so forth. This step is completed separately for each of the five independent variables. The size-adjusted gradual diffusion index is then computed as follows: $GDI_{i,0} = Inst_{i,0} + News_{i,0} + Anal_{i,0} + Ins_{i,0} + Fin_{i,0}$, where $GDI_{i,-12}$ is firm i 's size-adjusted gradual diffusion index at the bankruptcy date, and all other variables are the ordinal values for firm i one month before the event date across the different independent variables. Event firms in the bottom quartile by their size-adjusted gradual diffusion index are labelled as "slow diffusion" firms while firms in the top quartile are labelled as "fast diffusion" firms. All other companies are assigned to the "middle diffusion" group. Firms in each group are treated as a portfolio and their stock return performance is compared over a one-year period using a BHAR strategy starting the second event-day after the Chapter 11 date. Event firms' raw returns are adjusted using a control firm benchmark based on size and book-to-market or size and momentum. The null hypothesis is that there is no difference between the abnormal returns of firms allocated to the "slow diffusion" group and the abnormal returns of all remaining sample firms. The alternative hypothesis is that there is a negative difference between the abnormal returns of firms allocated to the "slow diffusion" group and the abnormal returns of all remaining sample firms.

	Size and Book-to-market				Size and Momentum			
	Slow diffusion group	Fast diffusion group	Middle diffusion group	Anova and KW test	Slow diffusion group	Fast diffusion group	Middle diffusion group	Anova and KW test
Mean	-0.24	-0.17	-0.28	0.97	-0.31	-0.17	-0.26	0.28
p-value	0.0610	0.0730	0.0006	0.3816	0.0123	0.0306	0.0052	0.7553
Median	-0.31	-0.18	-0.23	4.40	-0.33	-0.15	-0.39	1.48
p-value	0.0002	0.0058	<0.0001	0.1104	<0.0001	0.0012	0.0001	0.4770
N	88	87	176	-	88	87	176	-

6.2.6 Summary and limitations

This section explores whether the Hong and Stein (1999) model explains the post-bankruptcy drift uncovered in the previous chapters. My results suggest otherwise. Hong and Stein (1999) claim that, at every given point in time, investors systematically have more information about some firms than others. As such, mispricing should be concentrated on those firms for which information is hard to get and/or more difficult to interpret. I use my sample of bankrupt firms to test the model's main prediction and find that abnormal returns associated with these particular companies do not vary according to the inferred level of gradual diffusion of information. My conclusion holds both when I analyse the pre-bankruptcy period, where no particular information event conditions the stock return pattern, as well as after the bankruptcy announcement date.

One can argue that the Hong and Stein (1999) model should not be used to explain the stock return patterns associated with the announcement of bankruptcy since it was not initially designed to deal with information events. Incidentally, Hong, Lim and Stein (2000, p. 293) provide support for my line of research when writing "*The gradual-information-diffusion model of Hong and Stein (1999) was built for the express purpose of delivering both medium-term momentum and long-term reversals in stock returns; in the spirit of Fama (1998), then, it should be evaluated more on the basis of other, previously untested auxiliary predictions*". In effect, similarly to what I do here, Hong, Lim and Stein (2000) simply use the momentum anomaly to test, in practice, the predictive ability of Hong and Stein's (1999) theoretical model.

One important aspect that merits further discussion here is the methodology employed to test my post-bankruptcy research hypothesis. Figure 6.2 shows that, in this case, the independent variables are measured *before* the event date. One can argue that such research design is flawed because I use *pre-event* data to infer about the degree of gradual diffusion of information that occurs *after* the bankruptcy date. In order to assess the importance of this issue, I collect data for the different independent variables using a 6-month period *after* the bankruptcy announcement date for each of my sample firms. I use this information to sort the

firms into three diffusion groups as in section 6.2.4.2, and compare the result of this additional sort with that obtained when the information about the independent variables is collected *before* the event date. I find only minimal differences between the two sorts: there are six (two) companies that in the reported results are allocated to the "slow diffusion" ("middle diffusion") group that are reclassified as "middle diffusion" ("fast diffusion") firms when this alternative information is used. Hence, my findings seem robust to the period used to collect the information regarding the independent variables for testing my post-bankruptcy research hypothesis.

There is also the question of how risk is factored into the analysis. To be consistent with the procedure for the Barberis, Shleifer and Vishny (1998) model, I rerun my analysis controlling for additional risk-factors like industry, bankruptcy probability and impact of low-price stocks and find essentially the same results.

6.3 Summary of the chapter

This chapter explores to what extent the behavioural models of Barberis, Shleifer and Vishny (1998) and Hong and Stein (1999) explain why the market misprices the stock of bankrupt companies. Addressing this issue is important for two reasons. First, testing the predicting abilities of these models outside the settings for which they were designed for is the only scientific way to check their relative merit in explaining the workings of real world financial markets. Second, these models could help explain why the post-bankruptcy drift occurs in the first place.

I find that the Barberis, Shleifer and Vishny (1998) and the Hong and Stein (1999) models do not account well for the return patterns associated with a Chapter 11 announcement. As such, my findings suggest that, in the particular case of bankruptcy, the market is not affected by a representativeness/conservatism bias as posited by Barberis, Shleifer and Vishny (1998). Additionally, my results suggest that gradual diffusion of information does not explain why the market misprices the stock of firms undergoing a Chapter 11 reorganization in the post-event period.

My results are also interesting from a more fundamental perspective. In effect, when discussing the validity of existing behavioural theoretical models, Fama (1998, p. 291) writes: *“My view is that any new model should be judged on how it explains the big picture. The question should be: Does the new model produce rejectable predictions that capture the menu of anomalies better than market efficiency?”*, concluding that *“For existing behavioral models, my answer to this question is an emphatic no.”* I provide direct support to Fama's (1998) critique by finding that the predictions of two flagship behavioural models are not met in the context of Chapter 11 bankruptcy announcements, the most extreme bad news event in the corporate domain.

Chapter 7

Bankruptcies: Are They All Created Equal? The Case of Strategic vs. Non-strategic Chapter 11s

7.0 Introduction

Historically, bankruptcy has been associated with organizational demise and the destruction of shareholder value (e.g., Johnson, Baliga and Blair, 1986; Sirower, 1991). Even in the most optimistic scenario, a bankrupt company still has to face the direct cost of the proceedings that reduce its fundamental value. Such costs include out-of-the-pocket expenses for lawyers, expert witnesses, restructuring advisors, turnaround specialists and similar expenditures (e.g., Warner, 1977b; Ang, Chua and McConnell, 1982; Altman, 1984; Branch, 2002; LoPucki and Doherty, 2004; Bris, Welch and Zhu, 2006). Further, firms filing for bankruptcy have to bear indirect costs such as the diversion of scarce management time, additional lost sales during and after bankruptcy, constraints on capital investment and R&D spending, the loss of key employees and other unobservable opportunity costs (e.g., Altman, 1984; Opler and Titman, 1994; Maksimovic and Phillips, 1998; Pulvino, 1999; Branch, 2002). This explains why most firms usually work hard to avoid going bankrupt (Orr, 1998; Delaney, 1998, p. 3).

This traditional position, however, has been disputed in recent years, with an increasing number of scholars claiming that the introduction of the Bankruptcy Act of 1978 fuelled a major shift in the market's perception about bankruptcy (e.g., Sheppard, 1992 and 1995; Tavakolian 1994 and 1995; Delaney, 1998, p. 3). This Act made substantial progress towards improving the efficiency and effectiveness of the bankruptcy process in the US (Flynn and Farid, 1991; Moulton and Thomas, 1993). Included in the changes is a consolidation of the reorganization provisions under the Act's Chapter 11, which stands as one of the four operative chapters along with Chapter 7 (liquidation), Chapter 13 (for individuals with regular incomes) and Chapter 15 (for municipalities). As discussed earlier (section 2.4.1), Chapter 11 of the Bankruptcy Act of 1978 exists to permit the rehabilitation of the debtor's assets (Newton, 2003, p. 42). The idea is

that allowing a business to reconfigure its operations, without ongoing creditor pressure, will be of more value to society than a bundle of assets distributed among its creditors in liquidation (Altman and Hotchkiss, 2005, pp. 7-8).

An important feature of this statute is that it does not require a company to be *insolvent* before filing for reorganization (e.g., Johnson, Baliga and Blair, 1986; Farid and Flynn, 1992; Sheppard, 1992 and 1995; Salem and Martin, 1994; Tavakolian 1994 and 1995; Altman and Hotchkiss, 2005, p. 28). In practice, the Bankruptcy Act (1978) offers managers a mechanism that allows their organizations, almost at will, to fight nearly every undesirable financial obligation (Flynn and Farid, 1991; Sheppard, 1992 and 1995; Altman, 1993, pp. 89-90; Moulton and Thomas, 1993; Salem and Martin, 1994; Tavakolian 1994 and 1995; Delaney, 1998, p. 3).¹⁰⁷ There is also an additional incentive for managers who might be reluctant to file for protection under the 1978 Act; appointing a trustee to replace incumbent management is *not* a legal imperative. In fact, unless there are findings of dishonesty or incompetence, current managers are allowed to stay in office while developing a reorganization plan for the company (Newton, 2003, p. 91; Altman and Hotchkiss, 2005, p. 49). As a result, management has considerable flexibility in determining the proper circumstances and timing for filing for bankruptcy (Johnson, Baliga and Blair, 1986; Flynn and Farid, 1991; Moulton and Thomas, 1993; Sheppard, 1992).

The discretionary nature of the 1978 Act prompted many scholars to argue that bankruptcy in the US is no longer a stigma - it is the newest addition to managements' armoury (Flynn and Farid, 1991; Sheppard, 1992 and 1995; Moulton and Thomas, 1993; Salem and Martin, 1994). In fact, there have been many cases where firms used Chapter 11 in a non-traditional way (Johnson, Baliga and Blair, 1986; Garrison and Mason, 1988; Delaney, 1998; Orr, 1998). The term *strategic bankruptcy* is commonly used in the literature to describe such situations, which are characterized by solvent companies addressing the bankruptcy Courts not as a last resort

¹⁰⁷ Breaking penalizing labour contracts (e.g., Continental Airlines), shirk paying unprofitable leases (e.g., HRT) and reducing court-imposed damage awards (e.g., Texaco) are examples of undesirable financial obligations that have been resolved in the past by filing a strategic Chapter 11.

but as a planned business strategy (e.g., Sheppard, 1995; Delaney, 1998; Rose-Green and Dawkins, 2002).

Texaco is probably one of the best examples of this unconventional use of Chapter 11. On April 13, 1987, the company declared bankruptcy and went down in history as the largest corporate failure at the time. The most remarkable aspect, however, is that Texaco had a sound financial position when filed for Federal protection. In a letter addressed to its customers and suppliers released on its bankruptcy date, Texaco's managers stated that: "*Texaco Inc. is solvent and financially strong. The Chapter 11 petition will enable Texaco Inc. to conduct its business in the ordinary course as it continues to appeal this judgement. Again, we wish to emphasize that our Company is not affected and is honouring all its obligations in full. We are financially sound and our business will continue as normal.*" Clearly, by its own admission, Texaco is not the stereotypical bankruptcy case. Instead, the company used Chapter 11 as a weapon against one of its rivals, Pennzoil. As Delaney (1998, p. 145) clarifies, the objective was to protect the firm from a damage-award of 10.53 billion dollars awarded by a Court to be paid to its competitor Pennzoil. Over the years, other companies filed strategic bankruptcies to break labour contracts (e.g., Continental Airlines), resolve massive numbers of individual claims (e.g., Manville and A.H. Robins), avoid coping with pension funds' financial responsibilities (e.g., LTV), shirk paying unprofitable leases (e.g., HRT Industries) or even dealing with problems with the tax authorities (e.g., Whiting Pools).

It follows from the above paragraphs that firms filing a strategic Chapter 11 are, in their very nature, nothing like the typical company seeking protection from the Federal Bankruptcy Court. I would argue that this is a very important aspect for my own research. In fact, up until now, all my results were obtained under the implicit assumption that all my bankruptcy cases share a similar underlying motivation. Yet, this may not be the case. In this last chapter, I explore if there is a difference in the way the US equity market deals with strategic and non-strategic bankruptcies. Investigating this question should provide interesting insight into how the market deals with qualitatively diverse extreme bad news events.

The chapter is divided in four parts. The first presents the framework employed to disentangle between strategic and non-strategic bankruptcies. The second summarizes key information about these two types of Chapter 11. The third part examines how the market reacts to strategic and non-strategic bankruptcies. The last part concludes.

7.1 Defining strategic bankruptcy

Sheppard (1995) offers a good starting point for developing a classification schedule to separate strategic from non-strategic bankruptcies. According to the author, a strategic Chapter 11 complies with the following list of requisites:

1. There is one identifiable stakeholder-group against which the firm files the Chapter 11;
2. There is a single identifiable action the stakeholder-group was taking that the firm sought to thwart via the Chapter 11;
3. There is a specific goal that filing for Chapter 11 helps achieving;
4. The filing goes beyond simply staying the actions of creditors to collect debts.

I contacted Professor Kevin Delaney, a well-known scholar working in this area, and asked him if he had any input I could use to improve Sheppard's (1995) framework. Professor Kevin Delaney thinks of Chapter 11 as a continuous tool that is available to management. Highly distressed companies are at one extreme of this continuum. Managers of these firms use Chapter 11 to avoid facing liquidation, thereby minimizing the likelihood of losing their jobs and all their shareholders' value. Financially sound firms can also file for Chapter 11. These companies are at the other extreme of the continuum. Their managers use Chapter 11 as a weapon to maximize their shareholders' wealth at the expense of another group of stakeholders. I combined Sheppard's (1995) framework with the idea that Chapter 11 is a continuous tool for incumbent management and defined the following list of characteristics for a strategic bankruptcy:

1. Firms file a strategic Chapter 11 against one identifiable stakeholder-group (e.g., competitors, employees, retirees);

2. Filing for Chapter 11 must help the company achieve a specific goal that harms the interests of the stakeholders identified in the previous point (e.g., break labour contracts, avoid a lawsuit, reduce/eliminate pension responsibilities);
3. The filing *must not* be motivated by a clear short/medium-term financial problem.

My framework is basically a restricted version of Sheppard's (1995) classification schedule. However, my setting allows the researcher to focus on particular cases of the Chapter 11 continuum: those where the firm's viability as a going-concern is not at stake in the near future. This constitutes an important innovation when compared to Sheppard (1995). In effect, I do not allow highly financially distressed firms to be classified as strategic bankruptcies; this was possible under Sheppard's (1995) original framework.

I implement a three-stage process to classify all my sample firms as either a strategic or a non-strategic case.¹⁰⁸ Such process is centred on news articles relating to each of them, which I gather from three different sources: 1) Factiva; 2) Bankruptcydata.com and 3) Hoover's database.¹⁰⁹ Using Factiva's keyword-search tool, I collect sample firms' news articles for a one-year period before their Chapter 11 date and use that information to recreate each bankruptcy story. In particular, I try to identify a specific stakeholder-group against which the firm's management files the Chapter 11 and how such action benefits the company. I then verify if there are any signs of financial distress in the short-term history of the firm. This is done by searching the news articles for keywords like "*default on bond contract*", "*bond downgrade*", "*default on interest payment*", "*default on bank loan payment*", "*qualified audit opinion*", "*modified audit opinion*", "*trade credit problem*", "*technical default*", "*liquidity problem*" and "*renegotiation of credit line*". This phase yields a provisional list of strategic Chapter 11 cases.¹¹⁰

¹⁰⁸ Details about sample firms can be found on section 3.2.

¹⁰⁹ Details about Bankruptcydata.com and Factiva are available on chapters 3 and 6, respectively. Hoover's database stores firms' historical facts and some press releases. Go to <http://www.hoovers.com/free/> for further details.

¹¹⁰ This choice of keywords is based on extant research showing that the likelihood of bankruptcy is directly related with the occurrence of other public events. For instance, Beneish and Press (1995) find that firms in technical default are more likely to go into bankruptcy. They also show that the probability of bankruptcy increases after a debt service default. On the other hand, Campbell and Mutchler (1988), Chen and Church (1996) and Holder-Webb and Wilkins (2000) find that bankruptcy is more likely to occur after the issuance of a going concern opinion.

In the second step, I verify my initial results by analysing the information available on Bankruptcydata.com. In the typical case, this database only has news articles for a short window around the bankruptcy date, which makes it unsuitable for recreating the more longer-term history of the company. Nevertheless, in most cases, Bankruptcydata.com is very helpful in determining the reason why any given firm files its Chapter 11. By comparing the data from Factiva and Bankruptcydata.com, I am able to classify all my sample firms as either a strategic or a non-strategic bankruptcy. Importantly, these intermediate results are only confirmed in the last phase of the process if the information available on Hoover's database does not contradict my initial classification. Table 7.1 summarizes the number of strategic and non-strategic bankruptcies uncovered in my sample using the framework now discussed.

Table 7.1
Strategic vs. non-strategic bankruptcy cases

This table presents the number of strategic and non-strategic bankruptcy cases identified in my population of 351 non-finance, non-utility industry firms, fully listed on the NYSE, AMEX or NASDAQ that filed for Chapter 11 between 01.10.1979 and 17.10.2005 and remained listed on a major US stock exchange after their bankruptcy date. Firms are allocated to the strategic set if: 1) their managers use Chapter 11 against one identifiable stakeholder-group; 2) filing for Chapter 11 helps managers achieve a specific goal that harms the interests of the stakeholders identified in the previous point; 3) the filing is not motivated by a clear short/medium-term financial problem. All other firms are allocated to the non-strategic set.

	N°	% of total
Total number of sample firms	351	-
Strategic bankruptcies	32	9.1%
Financial bankruptcies	319	90.9%

Table 7.1 shows that only nine percent of my sample companies can be classified as a strategic Chapter 11.¹¹¹ This is an expected result. In fact, by implementing my classification schedule, I aim at maximizing the qualitative difference between what I term as strategic bankruptcy and

¹¹¹ Sheppard (1995) works with a total of 155 firms filing for Chapter 11 between October, 1979 and December, 1987. The author classifies 55 of these firms as a strategic bankruptcy (approximately 35 percent). Rose-Green and Dawkins (2002) identify 245 companies filing for Chapter 11 between 1980 and 1997, of which 19 are classified by the authors as a strategic bankruptcy (around 8 percent). Importantly, in sharp contrast with my own research, none of these papers require firms to continue trading after their Chapter 11 date.

all other Chapter 11s, which in turn should have a direct impact on how the market deals with these two bad news events.

It should be noted that several reasons explain why my sample firms file a strategic Chapter 11. For instance some of them want to break a labour contract (4 cases). Others want to enforce a contract with a key customer (2 cases). There is also the case where Chapter 11 helps fighting a Court-imposed award to a competitor (4 cases). A large number of firms file a strategic bankruptcy to resolve a massive number of individual lawsuits (10 cases). Some firms file for Federal protection to solve a merger problem (3 cases). Other firms file a strategic Chapter 11 to shirk paying unprofitable leases (2 cases). Finally, there are cases where Chapter 11 helps solving a problem with the firm's pension fund (3 cases). Besides these "typical" reasons for filing a strategic Chapter 11, I also uncover four other motivations for undertaking such action: 1) stop the cancellation of contract with the government (1 case), solve a mortgage dispute (1 case), break contract with a key supplier (1 case) and solve a lawsuit imposed by a key shareholder (1 case).

7.2 Strategic and non-strategic bankruptcies: are there any differences?

I begin my analysis by studying differences between strategic and non-strategic bankruptcies based on the summary sample statistics in table 7.2. I find that the typical company filing a strategic Chapter 11 has a better financial position than that of the average firm filing a non-strategic bankruptcy. For instance, panel A indicates that, for the set of strategic bankruptcies, sales, total assets and return on assets are higher while leverage is lower compared to non-strategic firms and this difference is statistically significant based on the t-tests and the Wilcoxon-Mann-Whitney tests for these variables. Panel A of table 7.2 also reveals that the mean (median) z-score for the strategic group is 2.30 (2.19) while its respective counterpart for the non-strategic set is 1.28 (1.25). Both the t-test and the Wilcoxon-Mann-Whitney test for this variable are significant at the 10 percent level. In his original work, Altman (1968) establishes a z-score cut-off point of 1.81 to separate between firms that clearly fall into the bankruptcy category from all other companies. Consequently, my results suggest that firms filing a strategic

Chapter 11 (non-strategic Chapter 11) *are not (are)* in any (an) immediate danger of failure when Altman (1968) z-score proxies for bankruptcy-risk.

Panel C of table 7.2 again shows that firms filing a strategic bankruptcy have a better financial position than that of the other bankrupt companies. Almost 40 (50) percent of the former have positive earnings (are paying dividends), a figure that is considerable higher than the 24 (24) percent obtained for the latter. Panel C additionally shows that only 31 percent of firms filing a strategic Chapter 11 are delisted in the 12-month period after their bankruptcy date. This figure is much higher for the non-strategic set: 58 percent. This result again suggests a relative lower degree of financial distress for strategic Chapter 11 firms (Dichev, 1998).

Panel B of table 7.2 summarizes key market variables. I find that the average firm filing a strategic bankruptcy is much bigger than its non-strategic counterpart. The mean (median) size difference between the two groups is 375 millions of dollars (53 millions of dollars), significant at the one percent level (one percent level). I find confirming evidence when sales and total assets proxy for size. This result helps explaining why the mean (median) stock price of the typical strategic bankruptcy is higher than its non-strategic equivalent, a phenomenon that holds for both the pre- and post-event periods. Interestingly, panel B of table 7.2 reveals that both types of Chapter 11 firms share a number of characteristics. For instance, the difference between the 12-month pre-event raw returns of the strategic and non-strategic set is not statistically significant at normal levels. Furthermore, both sets of companies have a very similar book-to-market ratio and are traded similarly in the pre- and post-event periods.

Table 7.2

Summary statistics – strategic vs. non-strategic bankruptcies

This table presents summary statistics relating to my population of 351 non-finance, non-utility industry firms, fully 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 allocated to the strategic portfolio if filing a strategic bankruptcy (n=32). Firms included in this portfolio respect the following conditions: 1) their managers use Chapter 11 against one identifiable stakeholder-group; 2) filing for Chapter 11 helps managers achieve a specific goal that harms the interests of the stakeholders identified in the previous point; 3) the filing is not motivated by a clear short/medium-term financial problem. All other firms are allocated to the non-strategic portfolio (n=319). 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

	Non-strategic (A)		Strategic (B)		Difference (A-B)			
	Mean	Median	Mean	Median	Mean	p-value	Median	p-value
Sales	423.1	92.4	2,324.1	356.2	-1,901.0	0.0682	-263.8	<0.0001
TA	454.4	79.6	2,562.9	190.5	-2,108.5	0.0710	-110.8	<0.0001
ROA	-20%	-7%	-6%	4%	-14%	0.0229	-10%	0.0286
Z-Score	1.28	1.25	2.30	2.19	-1.02	0.0581	-0.94	0.0575
CUR	154%	109%	310%	320%	-156%	0.0383	-211%	0.0286
LEV	45%	40%	39%	38%	7%	0.1151	2%	0.5321

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.

Panel B: Market related variables

	Non-strategic (A)		Strategic (B)		Difference (A-B)			
	Mean	Median	Mean	Median	Mean	p-value	Median	p-value
Size	125.8	31.0	501.1	84.0	-375.3	<0.0001	-53.0	0.0061
Book/Market	4.1	2.4	5.0	1.8	-0.9	0.5662	0.6	0.5514
Momentum	-0.06	-0.07	-0.05	-0.05	-0.01	0.4174	-0.02	0.3945
Pre price	4.59	2.82	8.69	6.38	-4.10	0.0064	-3.56	0.0020
Event Price	1.85	0.92	4.40	2.11	-2.55	0.0349	-1.19	0.0018
Pos Price	2.53	0.61	7.54	2.76	-5.01	0.0358	-2.15	<0.0001
Pre Volume	0.49%	0.33%	0.63%	0.39%	-0.14%	0.3136	-0.06%	0.2531
Event Volume	1.11%	0.56%	1.58%	1.12%	-0.47%	0.1433	-0.56%	0.0114
Pos Volume	0.55%	0.30%	0.65%	0.41%	-0.10%	0.8055	-0.11%	0.0742
Pre Tdays	251	252	242	251	9	0.0579	1	0.9070
Pos Tdays	228	244	251	252	-23	0.0074	-8	0.0176

Size: market capitalization (price times shares outstanding), in millions of dollars. Book/Market: book-to-market ratio. Momentum: 12-month pre-event average monthly raw returns. 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 7.2 (cont.): Summary statistics – strategic vs. non-strategic bankruptcies

Panel C: Other Characteristics

	Non-strategic (A)		Strategic (B)	
	Positive cases	% of Total	Positive cases	% of Total
EPS	76	23.8	12	37.5
Divid	75	23.5	16	50.0
Big8	257	80.6	30	93.8
Delist	185	58.0	10	31.3

Equity: book value of equity dummy (1 if positive, 0 otherwise). 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). 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

7.3 Market reaction to strategic and non-strategic bankruptcies

7.3.1 Initial evidence

I use an event-study similar to that of chapter 3 to verify if the market reacts differently to the announcement of strategic and non-strategic bankruptcies. However, in this application, I split my sample into two portfolios. The strategic portfolio refers to the 32 companies filing a strategic Chapter 11; the remaining firms are allocated to the non-strategic portfolio. Additionally, a t-test and a Wilcoxon-Mann-Whitney test are used to investigate if there is a difference in performance between these two portfolios.

Table 7.3 summarizes my results. I find that, for both portfolios, all mean and median BHARs computed for the (-252,-2) and (-126,-2) windows are negative and statistically different from zero at the one percent level. Additionally, the t-tests and the Wilcoxon-Mann-Whitney tests for differences in means and medians are not significant at any meaningful level. This suggests that, in the pre-event period, the market does not differentiate between strategic and non-strategic bankruptcies. My results are not consistent with those of Rose-Green and Dawkins (2002), who report stronger negative abnormal returns for their set of non-strategic Chapter

11s.¹¹² Two factors may help explain this disparity in results. First, Rose-Green and Dawkins (2002) report market-adjusted returns whilst I employ a control firm approach to compute the excess returns. Secondly, there is an important difference between how I identify my strategic bankruptcy cases and how Rose-Green and Dawkins (2002) achieve the same objective. In particular, I use a modified version of the Sheppard's (1995) classification framework to categorize each of my sample firms as either strategic or non-strategic Chapter 11s. As mentioned in section 7.1, my classification procedure relies on firm-specific news articles reported by the press up to a year before the event date that I collect from three independent (but complementary) sources. In contrast, Rose-Green and Dawkins (2002) simply take their strategic cases from a standardized database. In effect, on page 1321, the authors write: *"We classify bankruptcy filings as "strategic" if New Generation Research Inc.'s Bankruptcy Yearbook and Almanac indicates bankruptcy was filed for one of the following reasons: (1) alleged accounting improprieties, (2) asbestos liabilities, (3) labor (sic.) relations, (4) other litigation/contract problems, (5) pension disputes, (6) personal injury lawsuits, (7) patent lawsuits/problems, and (8) regulator, environmental, nuclear problems."* I contacted New Generation Research in an attempt to gain access to their list of strategic bankruptcies and investigate to what extent the distinct procedures for disentangling strategic from non-strategic Chapter 11s could help explain the difference in results. I found that New Generation Research does not classify bankruptcies as described by Rose-Green and Dawkins (2002) anymore and that nowadays they only maintain a list of asbestos-related bankruptcies.

Panel B of table 7.3 shows a strong and negative reaction to the event for both strategic and non-strategic bankruptcies. For the (-1,+1) window, the mean (median) market reaction for the strategic set is -25 percent, significant at the one percent level (-28 percent, $p < 0.0001$). The respective counterpart values for the non-strategic portfolio are -25 percent ($p < 0.0001$) and -27 percent ($p < 0.0001$). Importantly, both the t-test and its non-parametric equivalent for differences in means and medians are not significant at normal levels. The results for the

¹¹² For a (-251,-2) window, the mean BHAR for their strategic set is -62.94 percent, significant at a one percent level and the mean BHAR for the non-strategic (financial) sample is -94.50 percent, significant at a one percent level. The authors also report that the difference in means (medians) for this period is statistically significant at a five percent level (a five percent level).

complementary (-2,+2) window are very similar to those discussed here. My short-term findings are consistent with Rose-Green and Dawkins (2002).

Panel C of table 7.3 shows what happens after the bankruptcy announcement date. There is evidence of an *asymmetric* response of the market to the announcement of Chapter 11 conditional on the event's motivation. For the strategic (non-strategic) portfolio, all medium-term post-event BHARs are *positive (negative)*. Furthermore, for the strategic set, results are statistically significant for the (+2,+84) and (+2,+126) windows. For the non-strategic portfolio all mean and median BHARs are significant at the one percent level. Not surprisingly, the t-test (Wilcoxon-Mann-Whitney test) indicates that the sharp difference in mean (median) returns reported on panel C of table 7.3 is significant for all medium-term post-event windows.

Table 7.3

Market Reaction to Chapter 11 – strategic vs. non-strategic bankruptcies

This table presents buy-and-hold abnormal returns for my population of 351 non-finance, non-utility industry firms, fully 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 allocated to the strategic portfolio if filing a strategic bankruptcy (n=32). Firms included in this portfolio respect the following conditions: 1) their managers use Chapter 11 against one identifiable stakeholder-group; 2) filing for Chapter 11 helps managers achieve a specific goal that harms the interests of the stakeholders identified in the previous point; 3) the filing is not motivated by a clear short/medium-term financial problem. All other firms are allocated to the non-strategic portfolio (n=319). 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 (filing a strategic or a non-strategic Chapter 11), I 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. For the Non-strategic and Strategic columns, the two-tailed significance level from t-statistics (Wilcoxon signed rank-test) is reported below the mean (median). In the last two columns, the two-tailed significance level from t-statistics or a Wilcoxon-Mann-Whitney test are reported below the corresponding mean or median difference.

Panel A: Pre-event returns

	Non-Strategic (A)		Strategic (B)		Difference (A - B)	
	Mean	Median	Mean	Median	Mean	Median
(-252,-2)	-0.52 <0.0001	-0.44 <0.0001	-0.55 0.0020	-0.44 <0.0001	0.03 0.2993	0.00 0.5246
(-126,-2)	-0.44 <0.0001	-0.42 <0.0001	-0.41 <0.0001	-0.40 <0.0001	0.03 0.6332	0.02 0.4341

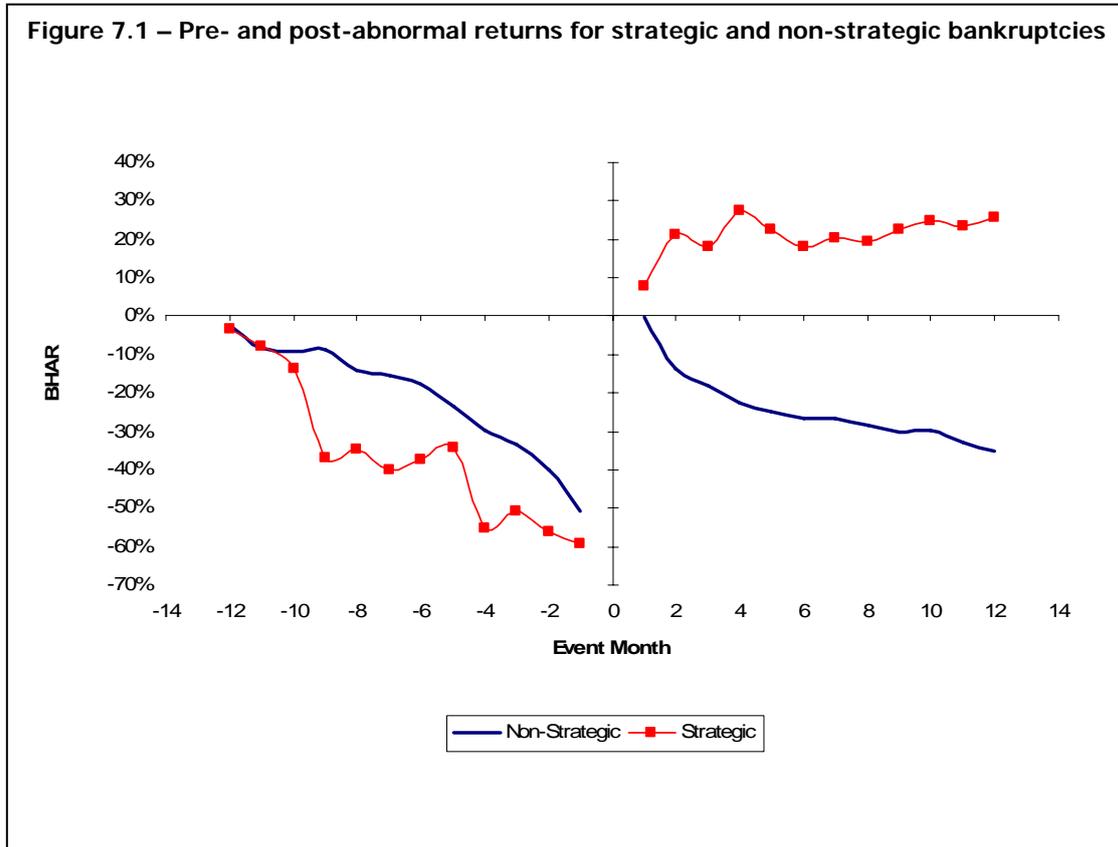
Panel B: Short-term market reaction

	Non-Strategic (A)		Strategic (B)		Difference (A - B)	
	Mean	Median	Mean	Median	Mean	Median
(-1,+1)	-0.25 <0.0001	-0.27 <0.0001	-0.25 <0.0001	-0.28 <0.0001	0.00 0.9581	0.01 0.8331
(-2,+2)	-0.28 <0.0001	-0.31 <0.0001	-0.26 <0.0001	-0.30 <0.0001	-0.02 0.7727	-0.01 0.7877

Panel C: Medium-term market reaction

	Non-Strategic (A)		Strategic (B)		Difference (A - B)	
	Mean	Median	Mean	Median	Mean	Median
(+2,+84)	-0.17 0.0023	-0.20 <0.0001	0.25 0.0142	0.27 0.0076	-0.42 0.0139	-0.47 <0.0001
(+2,+126)	-0.21 0.0007	-0.23 <0.0001	0.29 0.0102	0.35 0.0007	-0.50 0.0053	-0.58 <0.0001
(+2,+252)	-0.29 0.0003	-0.31 <0.0001	0.26 0.1925	0.30 0.0853	-0.55 0.0126	-0.61 0.0008

For illustrative purposes, figure 7.1 graphs the mean size and book-to-market risk adjusted BHARs over a period of 25 months centred on the bankruptcy announcement month for both the strategic and non-strategic sub-samples.¹¹³



In line with table 7.3, figure 7.1 shows an asymmetric market reaction to bankruptcy conditional on the motivation of the event. For the non-strategic set, a post-event drift follows a sharp pre-event decline in stock returns. On the other hand, there is evidence that filing a strategic Chapter 11 prompts a post-event reversal in stock returns.

¹¹³ Monthly returns are calculated following Kausar, Taffler and Tan (2008). To be precise, returns for 25 months centred on the bankruptcy announcement month are collected from CRPS monthly stock return file for both sample (strategic and non-strategic Chapter 11 sets) and control firms. The bankruptcy month is termed as the event month and excluded from the analysis. Equations (3.1) and (3.2) are then used to compute the abnormal returns presented above.

7.3.2 Robustness tests

At face value, the idea of the market reacting differently to strategic and non-strategic bankruptcies may sound odd. One explanation for my findings relates to possible methodological problems. As pointed out in chapter 4, controlling for size and book-to-market may be insufficient to understand the stock return pattern associated with a bankruptcy announcement. I try to overcome this issue by using the different benchmark samples presented in section 4.3, namely my control firms based on size and momentum, size and financial distress risk and industry, size and book-to-market. As above, I run an event-study separating my sample firms into a strategic and a non-strategic portfolio.¹¹⁴ However, in the robustness tests presented below, abnormal returns are computed using alternative matched samples as benchmark.

Table 7.4 presents my results. All panels indicate that the market does not differentiate between strategic and non-strategic bankruptcies in the pre-event period. I also find a strong and negative market reaction at the event date for both types of bankruptcy. More importantly, all robustness tests provide evidence in favour of an asymmetric response of the market to bankruptcy conditional on the Chapter 11's motivation. In effect, across the different panels, all post-event mean and median BHARs are always negative and statistically significant for the non-strategic portfolio. Additionally, I find that the post-event abnormal returns are always positive for the set of firms filing a strategic Chapter 11 and statistically significant up to 6-months after the bankruptcy date.

Overall, the stock return patterns uncovered with my robustness tests are very similar to those reported above. Accordingly, I would argue that the bankruptcy's motivation *does* matter. In particular, over time, the market seems to regard strategic bankruptcies much more positively than their non-strategic equivalents.

¹¹⁴ The insufficient number of strategic bankruptcy cases justifies why I do not implement a calendar-time portfolio technique to further check the validity of my results.

Table 7.4

Robustness tests – strategic vs. non-strategic bankruptcies

This table presents buy-and-hold abnormal returns for my population of 351 non-finance, non-utility industry firms, fully 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 allocated to the strategic portfolio if filing a strategic bankruptcy (n=32). Firms included in this portfolio respect the following conditions: 1) their managers use Chapter 11 against one identifiable stakeholder-group; 2) filing for Chapter 11 helps managers achieve a specific goal that harms the interests of the stakeholders identified in the previous point; 3) the filing is not motivated by a clear short/medium-term financial problem. All others are allocated to the non-strategic portfolio (n=319). All compounding periods are defined in trading days, where day zero is the formal Chapter 11 date. In each panel, a particular matched-sample is used to estimate the abnormal returns. In panel A, firms are matched according to size and momentum. Specifically, for each sample company, I 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 momentum closest to that of the sample firm. In panel B, firms are matched according to size and distress risk. Specifically, for each sample company, I 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 Altman (1968) z-score closest to that of the sample firm. In panel C, firms are matched according to industry, size and book-to-market. Specifically, the benchmark company is defined as the firm with the same COMPUSTAT two-digit SIC code, that lies on the same size decile as the sample firm and has the closest book-to-market ratio to that of the event company. In panels A, B and C, the Non-strategic and Strategic columns report the two-tailed significance level from t-statistics (Wilcoxon signed rank-test) below the mean (median). In the last two columns of panels A, B and C, the two-tailed significance level from t-statistics or a Wilcoxon-Mann-Whitney test are reported below the corresponding mean or median difference.

Panel A: Size and momentum

	Non-strategic (A)		Strategic (B)		Difference (A - B)	
	Mean	Median	Mean	Median	Mean	Median
(-252,-2)	-0.19 <0.0001	-0.10 <0.0001	-0.16 0.0371	-0.031 0.1102	-0.03 0.6867	-0.07 0.1688
(-126,-2)	-0.20 <0.0001	-0.18 <0.0001	-0.13 0.0148	-0.08 0.0201	-0.07 0.1968	-0.10 0.1677
(-1,+1)	-0.27 <0.0001	-0.28 <0.0001	-0.26 <0.0001	-0.24 0.0001	-0.01 0.9254	-0.04 0.9033
(-2,+2)	-0.30 <0.0001	-0.30 <0.0001	-0.30 <0.0001	-0.27 0.0001	0.00 0.9743	-0.03 0.8831
(+2,+84)	-0.17 0.0089	-0.20 <0.0001	0.39 0.0109	0.43 0.0071	-0.56 0.0006	-0.63 <0.0001
(+2,+126)	-0.22 <0.0001	-0.23 <0.0001	0.39 0.0269	0.35 0.0071	-0.61 0.0013	-0.58 <0.0001
(+2,+252)	-0.30 <0.0001	-0.36 <0.0001	0.23 0.3613	0.27 0.2153	-0.53 0.0459	-0.63 0.0068

Table 7.4 (cont.): Robustness tests – strategic vs. non-strategic bankruptcies

Panel B: Size and distress risk

	Non-strategic (A)		Strategic (B)		Difference (A - B)	
	Mean	Median	Mean	Median	Mean	Median
(-252,-2)	-0.67 <0.0001	-0.63 <0.0001	-0.75 0.0002	-0.72 <0.0001	0.08 0.6602	0.09 0.7334
(-126,-2)	-0.51 <0.0001	-0.55 <0.0001	-0.46 0.0002	-0.50 0.0005	-0.05 0.6248	-0.05 0.9019
(-1,+1)	-0.26 <0.0001	-0.27 <0.0001	-0.25 <0.0001	-0.23 0.0001	-0.01 0.8771	-0.04 0.6182
(-2,+2)	-0.28 <0.0001	-0.30 <0.0001	-0.26 <0.0001	-0.28 0.0001	-0.03 0.4720	-0.02 0.4487
(+2,+84)	-0.15 0.0007	-0.19 <0.0001	0.30 0.0041	0.35 0.0071	-0.45 0.0001	-0.54 <0.0001
(+2,+126)	-0.21 <0.0001	-0.23 <0.0001	0.39 0.0019	0.40 0.0021	-0.60 <0.0001	-0.63 <0.0001
(+2,+252)	-0.39 <0.0001	-0.40 <0.0001	0.23 0.2536	0.21 0.3771	-0.62 0.0043	-0.61 0.0004

Panel C: Industry, size and book-to-market

	Non-strategic (A)		Strategic (B)		Difference (A - B)	
	Mean	Median	Mean	Median	Mean	Median
(-252,-2)	-0.67 <0.0001	-0.60 <0.0001	-0.46 <0.0001	-0.51 <0.0001	-0.21 0.1131	-0.09 0.1469
(-126,-2)	-0.50 <0.0001	-0.49 <0.0001	-0.35 0.0005	-0.40 0.0003	-0.15 0.1154	-0.09 0.1981
(-1,+1)	-0.26 <0.0001	-0.27 <0.0001	-0.24 <0.0001	-0.25 <0.0001	-0.02 0.7572	-0.02 0.6642
(-2,+2)	-0.28 <0.0001	-0.31 <0.0001	-0.21 <0.0001	-0.23 <0.0001	-0.07 0.1843	-0.08 0.1739
(+2,+84)	-0.14 0.0012	-0.15 <0.0001	0.37 0.0014	0.31 0.0019	-0.51 <0.0001	-0.46 <0.0001
(+2,+126)	-0.21 <0.0001	-0.22 <0.0001	0.35 0.0461	0.29 0.0023	-0.56 <0.0001	-0.51 <0.0001
(+2,+252)	-0.38 <0.0001	-0.36 <0.0001	0.26 0.4252	0.21 0.5804	-0.64 0.0101	-0.57 0.0027

7.4 Summary and limitations

Up until now, this thesis has been developed under the implicit assumption that all bankruptcy cases share a common underlying motivation. However, extant research questions such as conjecture, arguing that different firms may file for Chapter 11 for different reasons. A clear distinction is made in the literature between strategic and non-strategic bankruptcies. Solvent firms addressing the Bankruptcy Court not as a last resort but as a planned business strategy characterize the first type of bankruptcy; companies in the verge of imminent failure account for the vast majority of non-strategic bankruptcies.

In this chapter, I explore to what extent this distinction affects the way the US equity market deals with such a catastrophic event. I show that the market does not differentiate between strategic and non-strategic Chapter 11s before and at the event date, with a sharp decrease in the stock price being noted for both types of firms in these periods. One explanation for the return patterns I document resides on Tversky and Kahneman's (1974) representativeness bias. People suffering from this behavioural bias tend to assume that things sharing a number of qualities are quite alike (Nofsinger, 2005, p. 64). Hence, it is quite possible that, in the pre-event period, investors have a common sentiment about firms that eventually file a strategic Chapter 11 and those that end up filing a non-strategic Chapter 11 because both types of firms possess parallel characteristics. In effect, my descriptive statistics demonstrate that these two sub-sets of firms share similar pre-event momentum and book-to-market ratios, two attributes that previous research has shown to be important for determining securities' market price (Jegadeesh and Titman, 1993; 2001; Fama and French, 1992). In short, for the aggregate market, firms filing both types of bankruptcy seem to fall within the same stereotype, i.e., that of a "loser" firm facing increasing problems that will eventually question its existence as a going-concern. This results in a similar stock price pattern in anticipation to the event for both strategic and non-strategic Chapter 11s.

Interestingly, I find an asymmetric longer-term market reaction to bankruptcy conditional on the underlying motivation for the filing. In particular, for the set of non-strategic bankruptcies, I

document a statistically significant downward post-event drift lasting at least one full year after the Chapter 11 date. In opposition, I find that filing a strategic Chapter 11 prompts a reversal in the stock return pattern, i.e., the post-event abnormal returns are positive and significant, a phenomenon that last up to six months. As such, my findings imply that, over time, the aggregate market values strategic and non-strategic bankruptcy announcement differently: the former is good news while the latter is bad news. To the best of my knowledge, this is the first time such a phenomenon is documented in the literature, which is of particular interest since it indicates that the longer-term market's reaction to bad news events is affected by the particular context surrounding firm-specific negative disclosures.

Some caveats should be taken into consideration while reading my results. Perhaps the most important one relates to how the strategic and non-strategic Chapter 11 cases are identified since the literature does not provide a clear guidance on this issue. The method employed here draws on existing research and an effort is made to keep the classification procedure as objective as possible, i.e., I resort to three independent sources to classify each of sample firms as either a strategic or non-strategic Chapter 11. However, some residual degree of subjectivity is likely to persist, thus affecting the overall results presented here.

A related issue is the small number of strategic bankruptcies available to work with, for which no easy solution exists. Another concern is the measurement of medium-term abnormal returns. As argued in chapter 4, this problem is always present in longer-term event-studies but is especially important here due to the small number of firms classified as strategic Chapter 11s. However, the fact that all parametric and non-parametric results are very close for this set of companies provides some assurance on the soundness of my findings. Additionally, all the evidence collected through a number of robustness tests seems to point to a similar conclusion as my primary event-study, which is also a reassuring result.

Chapter 8

Conclusion, Limitations and Further Work

8.0 Introduction

Finance scholars disagree on how real world financial markets work. On the one hand, the EMH advocates claim that investors are rational and care only about utilitarian characteristics (Statman, 1999). Additionally, they suggest that arbitrage ensures that market prices never go out of line even when some market participants are less than fully rational (e.g., Lee, 2001). As a result, in classical finance, securities' prices always reflect all available information (Fama, 1970). On the other hand, behavioural finance theorists argue that investors suffer from important cognitive biases and that arbitrage is both risky and costly (e.g., Shleifer and Summers, 1990; Shleifer and Vishny, 1997). In this alternative setting, prices may not reflect all available information and can deviate from their fundamental value for long periods of time (e.g., Barberis and Thaler, 2005, p. 1).

My thesis contributes to this ongoing debate by exploring how the US equity market reacts to bankruptcy announcements. This study is of interest for several reasons. Firstly, bankruptcy matters. Extensive evidence shows that, once an obscure event relevant for only the smallest firms in the greyest areas of the market, bankruptcy is nowadays a concern for virtually all existing companies (Altman and Hotchkiss, 2005, p. 3). Secondly, 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, Bhattacharya and Bamber, 2007), as is the market response to firm emergence from Chapter 11 (e.g., Eberhart, Altman and Aggarwal, 1999), there is a dearth of evidence on what happens to the stock price of firms subsequent to a few days after entering into bankruptcy proceedings (Altman and Hotchkiss, 2005, p. 83; Dawkins, Bhattacharya and Bamber, 2007). Thirdly, by exploring the most extreme event in the corporate domain, my thesis adds directly to behavioural research showing that the market has problems in assimilating bad news events (e.g., Bernard and Thomas, 1989, 1990; Michaely, Thaler and

Womack, 1995; Womack, 1996; Dichev and Piotroski, 2001; Chan, 2003 and Taffler, Lu and Kausar, 2004). Finally, at a more general level, this study's results help us comprehend better how financial markets work. For example, any cognitive biases affecting the pricing abilities of investors should become evident in the context I address. In effect, as emphasized by Hirshleifer (2001), psychological bias is more likely to exist when uncertainty is high and accurate feedback about a firm's fundamentals is inadequate. As such, misevaluation should be stronger for high uncertainty firms (Jiang, Lee and Zhang, 2005; Zhang, 2006), which is precisely the case of bankrupt companies. In addition, exploring how the market responds to bankruptcy also enhances our understanding about the arbitrage mechanism. The limited amount of information available about bankrupt companies (Espahbodi, Dugar and Tehranian, 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, Hotchkiss and Ruback, 2000) and the problems associated with trading costs and short sales constraints (D'Avolio, 2002) clearly point to the fact that, in this peculiar market, limits to arbitrage may be binding.

In the first empirical chapter of this thesis, chapter 3, I seek to answer the following question: does the US equity market quickly and accurately reacts to bankruptcy announcements? Chapter 4 examines the robustness of chapter 3's findings. In chapter 5, I investigate the role of limits to arbitrage in the pricing of bankrupt firms' stock. Chapter 6 explores to what extent well-known behavioural finance models explain the post-bankruptcy stock return performance uncovered in chapter 3. Finally, in chapter 7, I extend my study by analysing if the market's reaction to bankruptcy depends on the filing's motivation.

This final chapter is organized as follows. Section 8.1 summarizes and discusses my main empirical findings along with their implications and contributions to the literature and practice. In section 8.2, I present some limitations of my research. Section 8.3 outlines possible future developments of my work.

8.1 Summary, implications of results and contributions

The first step in this research is identifying a sample of 351 non-finance, non-utility industry firms that file for Chapter 11 between 01.10.1979 and 17.10.2005 and remain listed on a major US stock exchange after their bankruptcy date. In the first empirical chapter of this thesis, I use these companies to run an event study and investigate how the US equity market reacts to this extreme event. Consistent with previous research, I find negative abnormal returns both before and at the Chapter 11 date (e.g., Clark and Weinstein, 1983; Datta and Iskandar-Datta, 1995 and Dawkins, Bhattacharya and Bamber, 2007). These findings favour the idea that bankruptcy-related information is released to the market before the event date (e.g., Seyhun and Bradley, 1997; Dawkins and Rose-Green, 1998; Ma, 2001) and that bankruptcy is a key episode from an information perspective (e.g., Clark, and Weinstein, 1983; Datta and Iskandar-Datta, 1995 and Dawkins, Bhattacharya and Bamber, 2007).

More importantly, I make a direct contribution to the literature by exploring the market's longer-term reaction to bankruptcy announcements. To the best of my knowledge, only Morse and Shaw (1988) investigate a similar issue but, as argued in section 2.4.2.2, several shortcomings cast reasonable doubt about their conclusion. In sharp contrast to Morse and Shaw (1988), I find a strong, negative and statistically significant post-bankruptcy drift lasting at least one full year after the event date. Such drift ranges from -24 to -44 percent on average, depending on the benchmark adopted to measure the abnormal returns.

This finding is clearly at odds with the predictions of the semi-strong form of the EMH. Extant research, however, cautions against the dangers of measuring longer-term abnormal returns, emphasizing that some results are very sensitive to reasonable methodological changes (e.g., Brown and Warner, 1980; Kothari and Warner, 1997, 2007; Fama, 1998; Lyon, Barber and Tsai, 1999). Consequently, in my second empirical chapter, I explore to what extent the post-bankruptcy drift is not a mere statistical artefact. In particular, I re-run my initial analysis accounting for confounding factors like the post-earnings announcement drift, the post-first-time going-concern drift, the size effect, the momentum effect, the book-to-market effect, the

penny-stock effect, industry clustering and the level of financial distress. Crucially, I also use the calendar-time portfolio method to adjust my results for the cross-sectional return dependence problem highlighted by Fama (1998) and Mitchell and Stafford (2000). Albeit there is some weak evidence suggesting that a momentum factor is present and that the anomaly is more pronounced for smaller, low-price firms, the results of the robustness tests are, in general, very consistent with my main results. Accordingly, I conclude that the US equity market *does not* react on a timely and unbiased way to news contained in Chapter 11 bankruptcy announcements. This represents a direct contribution to the behavioural finance literature, especially for the strand of research documenting that the market is unable to deal appropriately with bad news events.

Finding a market-pricing anomaly is always a puzzling result. In effect, according to classical finance theory, arbitrage ensures that prices, on average, reflect their fundamental value. As such, in my third empirical chapter, I investigate to what extent the existence of limits to arbitrage explains why the market misprices the stock of bankrupt firms. Consistent with this hypothesis, I find that, in this market, only an "*illusory profit opportunity*" exists. To be precise, a sophisticated investor engaging in an arbitrage strategy involving the stock of bankrupt firms will likely lose, on average, around 11 percent of his investment over a 12-month holding period.

Implementation costs are not the only constraint that arbitrageurs face when investing in bankrupt firms' stock. In fact, in this thesis, I also investigate the role of noise trader risk in the pricing of this security. I find that, in a typical case, individual investors own around 90 percent of the equity while Chapter 11 is underway. It is widely accepted that such market participants are particularly vulnerable to psychological biases that impair their ability to make rational investment decisions (e.g., Shiller, 1984; Shefrin and Statman, 1985; De Long et al, 1990b; Shleifer and Summers, 1990 and Lakonishok, Shleifer and Vishny, 1994). Therefore, my results suggest that, even if arbitrageurs are able to overcome the problems with arbitrage implementation costs mentioned above, they still have to face high levels of noise trader risk.

In addition, I show that institutional investors are mostly absent from the market of bankrupt firms. Eight quarters prior to the event, institutions own an average of 25 percent of a bankrupt company's equity and drastically reduce their stockholdings as the Chapter 11 date approaches. This is also an important contribution to the literature since it favours the position of previous research claiming that institutions are more sophisticated and better informed than individuals (e.g., Lakonishok, Shleifer and Vishny, 1992; Nofsinger and Sias, 1999; Cohen, Gompers and Vuolteenaho, 2002; Ke and Ramalingegowda, 2005) and, as such, are able to deal more rationally with bad news events (Kausar, Taffler and Tan, 2008).

Overall, chapter 5 suggests that the market is "*minimally rational*" (Rubinstein, 2001) even in very extreme situations. In effect, in the case of bankrupt firms, arbitrage involving the stock of bankrupt firms is simply too risky and costly. As a result, the stock price of these firms drifts for long periods without being corrected by traditional market forces, which explains the persistence of the post-bankruptcy drift I uncover. This is also an important contribution, since I add directly to a growing body of literature documenting a similar phenomenon in different contexts (e.g., Barberis et al, 2001; Mendenhall, 2004; Taffler, Lu and Kausar, 2004; Klein, Rosenfeld and Tucker, 2006 and Kausar, Taffler and Tan, 2008).

Chapter 6 builds on the results presented above and addresses a more subtle question: why does the post-bankruptcy drift exist in the first place? I turn to behavioural finance theory in an attempt to provide an answer to this enquiry. I find that the Barberis, Shleifer and Vishny (1998) and the Hong and Stein (1999) models do not account well for the return patterns associated with a Chapter 11 bankruptcy announcement. Comparing the predictive abilities of these two models outside the particular setting for which they were designed for is a very important contribution to finance literature. In fact, as argued by Barberis and Thaler (2005, pp. 64-65), this is the only scientific way to test the models' relative merits. In addition, and at a more fundamental level, my results provide direct evidence in favour of Fama's (1998) critique about the reliability of existing models built around behavioural biases. In effect, my findings clearly suggest that more theoretical research is needed before behavioural finance can

challenge the EMH as an alternative way of understanding how real world financial markets really work.

I explore an interesting issue relating to the market's reaction to bankruptcy announcements in the last empirical chapter of this thesis. In particular, I argue that there is a clear distinction between strategic and non-strategic bankruptcies. Solvent firms addressing the Bankruptcy Court not as a last resort but as a planned business strategy characterize the first type of bankruptcy. Conversely, companies on the verge of imminent failure typify a non-strategic bankruptcy. Disentangling strategic from non-strategic bankruptcies is not a straightforward task and I contribute directly to the literature by identifying a simple yet reasonable classification schedule to achieve this objective.

I use this framework to separate my sample firms into two portfolios, conditional on their motivation for filing Chapter 11. After analysing both accounting and market related information, I find that companies filing a strategic bankruptcy usually have a stronger financial position than their non-strategic counterparts. There is also evidence of a considerable size difference in favour of the typical firm filing a strategic Chapter 11. The available data also reveals that companies filing strategic and non-strategic bankruptcies share similar pre-event momentum, book-to-market ratio and pre- and post-event trading patterns.

In a second phase, I run an event study to explore the stock return pattern around strategic and non-strategic bankruptcy announcements. I show that the market is unable to differentiate between these apparently similar bad news events with distinct underlying motivations in the pre- and event period, a result consistent with the representativeness bias of Tversky and Kahneman (1974). As the authors explain (p. 33), a person who follows this heuristic evaluates the probability of an uncertain event, or a sample, by the degree to which it is: 1) similar in essential properties to its parent population, and 2) reflects the salient features by which it is generated. Accordingly, one way of looking at my results is to consider that both in the pre-event period, and at the event date, the market treats all bankruptcy cases as part of the same

underlying population (i.e., those firms that will eventually fail in the near future, or have just failed), which, in turn, leads to a similar stock return pattern for both strategic and non-strategic cases.

I also demonstrate that the longer-term market reaction to bad news events is affected by the particular context surrounding firm-specific negative disclosures. Filing for Court protection for non-strategic reasons is clearly increasingly perceived by the market as bad news over time, while filing a strategic bankruptcy becomes recognized over time as a positive news event. This is an interesting result since, in contrast to the pre-bankruptcy period and at the filing event date, the market is able, albeit with a lag, to distinguish between the differential motivations for entering into Chapter 11 protection despite the same legal framework applying. It is not fooled by the apparent similarities between the two types of bad news event on this basis. This is an additional contribution of my work to the finance literature.

Importantly, I also contribute to the literature by finding that, in my case, the market takes time to digest both negative and “positive” bad news events and their implications for firm value: there is a strong post-event drift lasting up to 12-months after the announcement of both strategic and non-strategic Chapter 11 filings but in opposite directions. On the one hand, I confirm the results of previous research demonstrating that the market underreacts to negative disclosures (e.g., Womack, 1996; Dichev and Piotroski, 2001; Chan, 2003; Taffler, Lu, Kausar, 2004; Kausar, Taffler and Tan, 2008); on the other hand, I am the first to document that the market also overreacts to the announcement of Chapter 11 filing in the case of “positive” bad news events.

My thesis also has implications for practice. I would argue that individual investors are among those who can benefit the most from my results. In fact, for them, the message is clear: do not invest in the stock of bankrupt firms. Granted that, in some special situations, the upside potential is high. However, this study demonstrates that such an investment strategy is very risky and on average will result in steep losses for those pursuing it. However, if risk-seeking

traders do decide to invest in this treacherous market, they should concentrate their resources on large firms, filing Chapter 11 for strategic reasons. In effect, my results show that these particular bankruptcies have an interesting upside potential, which can be realized shortly after the event date. Moreover, trading strategies involving strategic Chapter 11s are simpler in the sense that they do not require the short-sale of the debtors' stock. This conclusion is also relevant for institutional investors, especially hedge funds. In fact, another way to read this result is that a thorough analysis of fundamental information may uncover interesting investment opportunities in this peculiar market. This point has also been raised previously by Altman (1999, p. 55) and Platt (1999, pp. 107-117).

My thesis also has implications for how the SEC governs the market for bankrupt firms. In the light of my overall results, I would argue that the best possible line of action for the SEC would be to force the delisting from all US stock exchanges of firms filing for bankruptcy. In effect, this would impede noise traders from losing their savings in highly speculative investments that, apparently, the majority of them do not understand. However, given US law, this may be unreasonable or simply not possible to achieve. Consequently, I would argue that improving the public's awareness about the risks of investing in bankrupt firms should be an immediate concern for the SEC since it is likely that more high-profile bankruptcies will follow in the US in the not so distant future.

8.2 Limitations

Like other empirical studies in finance, this thesis is subject to some limitations and my conclusions should be read with caution. Throughout the different empirical chapters, a number of specific shortcomings are identified and discussed. Hence, here I focus my attention on the major aspects that a reader should take into consideration while interpreting my results.

One of my primary concerns relates to my small sample size since I have only 351 companies available. Incidentally, this is not a small sample for studies exploring issues related with corporate bankruptcy. For instance, two very recent studies in this area, Dawkins, Bhattacharya

and Bamber (2007) and Kalay, Singhal and Tashjian (2007), use 272 and 459 firms in their tests, respectively. Nevertheless, in some situations, the number of companies that I have available does raise some concern about the statistical robustness of my results. This situation is particularly clear when the sample has to be divided into sub-groups for testing some of the research hypotheses of chapter 6. It is very important in chapter 7, where only 32 firms can be catalogued as strategic Chapter 11s.

A second problem relates to the possibility of generalizing my results. In fact, all bankruptcy cases used in this research are governed by the 1978 Bankruptcy Reform Act. As such, there are no guarantees that my findings hold outside this legal setting. In addition, as Altman and Hotchkiss (2005, pp. 55-78) emphasize, bankruptcy law differs dramatically among nations and the US has a particularly debtor-friendly regime. As such, my conclusions apply only to the US since all my sample firms are incorporated and traded in this country.

A third issue relates to the methodology for computing abnormal returns employed in this thesis. Chapter 4 discusses in detail the shortcomings affecting the methods that we have currently available for calculating and inferring about the statistical significance of longer-term excess returns. It also mentions the difficulty in testing market efficiency *per se*, given the existence of a joint-hypothesis problem. I would argue that these are pervasive problems, common to all researchers interested in measuring long-term abnormal returns. However, one should have them in mind when looking at the results presented here.

One particularly important methodological problem that hinders the soundness of my conclusions relates to how I correct for risk when computing my sample firms' abnormal returns. In effect, the all point of running an event study is to measure the impact of a specific event on the value of the affected firms (MacKinlay, 1997, p. 13) and, in order to do so, the researcher needs to control for the risk characteristics of the firm undergoing the event. As discussed in section 3.3.2, I attempt to achieve this objective by using a single-matched control firm approach based on specific characteristics that previous research identifies has being

important for the pricing of highly financially distressed companies. Nevertheless, it should be noted that bankrupt firms are very special and, as such, finding a similar firm in terms of risk/return characteristics is simply a very difficult (if not impossible) task. Put simply, when matching each of my bankrupt firms to some other non-bankrupt firm, I am always pairing a firm that is undergoing a Chapter 11 reorganization (and thus is facing profound financial problems) with another firm that might eventually be financially distressed but is surely not operating under the protection of the Federal Bankruptcy law. This is a very important issue and readers should be aware of it when interpreting the results presented here.

An additional methodological issue relates to the fact that throughout my analysis I compute equally weighted returns in lieu of value-weighted returns. Although carefully justifying the reason for this choice in section 3.3.1, it is always possible to argue that value-weighted returns should also be presented since they more accurately capture the total wealth effects experienced by investors (Fama, 1998, p. 296). Readers should take this issue into consideration when considering the results and contributions of this thesis.

I am also compelled to caution the reader about the methodologies I employ in chapters 6 and 7 of this thesis. In effect, I am able to contribute directly to the literature by developing particular methods for testing the accuracy of competing behavioural models in explaining the stock return patterns associated with the announcement of bankruptcy and by identifying a classification schedule for disentangling strategic from non-strategic Chapter 11s. Despite the fact that these contributions are always based on previous research, they are still innovations and thus require further scrutiny to infer about their validity.

8.3 Further work

There are several possible ways to develop and enrich this thesis' findings. Chapter 5 offers a starting point for this future research agenda. As argued in section 5.1.6, due to data restrictions, I was not able to clarify the role of vulture investors in the market for bankrupt firms. Addressing this issue in detail is especially interesting if one takes into consideration that, in the longer-run, the market reacts asymmetrically to strategic and non-strategic Chapter 11s. In fact, there are *a priori* reasons to believe that vultures will only pursue an active investment strategy when dealing with firms going bankrupt for strategic reasons, which may help explaining their particular post-bankruptcy stock return pattern. However, this is only a speculative idea that merits careful empirical inspection.

Exploring how the market reacts to other bad news events that occur when firms are highly financially distressed is another example of complementary work that should be of interest. Private debt workouts are a good example of this situation. In effect, these events are usually a first attempt to reorganize the firm's capital structure without the legal protection granted by the US bankruptcy law. It follows that, to some extent, private workouts are basically motivated by the same underlying financial factors that ultimately force firms to file for bankruptcy and thus should convey similar value-relevant information to the market.

Another question is determining to what extent the results uncovered here are specific to the US. In fact, it is commonly accepted that this country has the most debtor-friendly bankruptcy regime of the western world. As such, it would be interesting to verify if the market reacts differently to the announcement of bankruptcy in countries like France or Germany, where the law is more creditor oriented.

The introduction of the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 offers a similar opportunity. In effect, as Altman and Hotchkiss (2005, p. 47) point out, this new code is more creditor-friendly than its predecessor. Consequently, replicating this study with a

new sample composed of bankruptcy cases filed under the 2005 Code provides an ideal opportunity for testing the soundness of my results.

A complementary dimension that could be pursued is investigating how the different behavioural models considered in this thesis explain the post-bankruptcy emergence drift documented by Eberhart, Altman and Aggarwal (1999). In effect, the authors report positive excess returns following this good news event but fail to provide an explanation for this phenomenon. In contrast, my results show that limits to arbitrage are the key element in explaining the persistence of the post-bankruptcy drift I uncover. The event analysed by Eberhart, Altman and Aggarwal (1999) offers a privileged context within which to test the robustness of this finding.

A final research avenue that could be of interest is investigating how analysts deal with the bankruptcy event. Previous research by Espahbodi, Dugar and Tehranian (2001) shows that analysts' earnings forecasts for bankrupt firms are generally optimistic but that the forecast bias declines to insignificant levels by the year prior to bankruptcy filing. The authors also report that analysts tend to underreact to past forecast errors, a phenomenon that persists from four years before through the year of the bankruptcy and that is driven by firms with negative earnings-to-price ratios. More recently, Clarke et al (2006) document that analysts actively revise their recommendations downwards as bankruptcy approaches, which leads them to conclude that analysts are not biased when issuing recommendations about bankrupt firms. Interestingly, both papers focus on the pre-bankruptcy period thus failing to provide any insight into how analysts respond to the announcement of bankruptcy. Moreover, none of these papers explores to what extent analysts are self-selective in the face of bankruptcy (e.g., McNichols and O'Brien, 1997). This creates a research opportunity that I intent to explore in future research.

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