THE ECONOMIC CONSEQUENCES OF TAKEOVERS IN AUSTRALIA: A RE-EXAMINATION

by

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This thesis investigates and explains the divergent results reported in separate Australian studies on the economic consequences of corporate takeovers. Walter (1980) and Bishop, Dodd and Officer (1987) analyse the share market effects of takeovers and conclude that takeovers, on balance, are value-maximising investments. McDougall and Round (1986) analyse merging firms' financial statements and conclude that merged firms do worse post-takeover relative to both the pre-merger period and to matched non-merging firms.

While investigating the divergent results the thesis controls for research method weaknesses in the studies that might have contributed to their divergence. The share market event study component of this thesis includes controls for market risk, the systematic association between firm size and return, and survivorship bias. The accounting numbers based analysis incorporates a research method that facilitates the comparison of the accounting results with share market event study results as well as a model to estimate the downward bias in merging firms' post-takeover accounting rate of return.

Important novel contributions include the documentation of the significant impact of survivorship bias on calculations of market index returns and on estimated abnormal returns, assessments of the significant impact of positive asymmetry in the distribution of share market returns on estimated abnormal returns, and the presentation of empirical evidence on the reliability of accounting based measures of performance. While, on the whole, the evidence is consistent with the proposition that corporate takeovers are, on average, value increasing investments, the accounting based analysis leaves more room for equivocation on this point than does the share market based analysis.
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CHAPTER ONE
INTRODUCTION

1.0 AIM

This thesis investigates and explains the divergent results reported in three important studies of the economic consequences of takeovers in Australia. The three studies are a doctoral thesis by Walter (1980) and two commissioned studies, one by Bishop, Dodd and Officer (1987) and the other by McDougall and Round (1986).¹ The share market based studies of Walter and Bishop et al. find that takeovers help allocate capital to more valued uses. In contrast, McDougall and Round conclude from their analysis based on accounting numbers that merged firms perform worse than they did premerger and worse relative to matched non-merging firms.

In explaining the divergent results of the three studies this thesis controls for research method weaknesses in the studies. Several weaknesses are common to past studies on corporate takeovers. A contribution of the thesis is determining the extent to which the weaknesses influenced the earlier studies' results and developing means of overcoming them.

1.1 MOTIVATION

Several reasons motivate a reconciliation of the three studies' results.

(i) As the three studies recognise, takeovers are frequent and significant events in Australian capital markets and, as such, deserve close scrutiny. Over the period 1971 through 1985, the proportion of companies listed on the Australian Stock Exchange (henceforth, ASX) that were subject to a takeover bid each year never fell below 5% and exceeded 10% in 6 of the 14 years.² More tellingly, the aggregate share market value of the firms involved in takeover activity never fell below 10% of the total value of the share market over the period 1975 through 1984. In 1979, the aggregate value of firms involved in takeover activity was 43% of total share market

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¹ The McDougall and Round study was jointly commissioned by the Australian Institute of Management (AIM) and the National Companies and Securities Commission (NCSC). Bishop, Dodd and Officer's study was published under the aegis of the Centre for Independent Studies (CIS), which is an independent economic and social research institute.

² The Australian Financial Review (16 May 1986, p. 8). The statistics relate to the period covered in this thesis but the numbers have not changed much since 1985. Over the period 1986 through 1989, the number of companies subject to a takeover bid never fell below 8% of all listed companies in any one year (Australian Business, 17 January 1990, p. 56).
Over the 60 year period 1929 to 1989, over 40% of the delistings on the
ASX were due to the delisted companies being taken over [Martin (1993)].

The above numbers do not convey the whole impact of takeover activity. Shifts in
managerial policy and management itself are common after a takeover. Rationalisation of operations involving the retrenchment of staff often take place and
the target firm’s financial structure, asset base and product lines are frequently
revised. The extent of takeover activity and the accompanying significant changes
both explain why takeovers attract attention.

(ii) Informed opinion on the merit of more or less regulation of takeover activity
remains divided. Australian government regulation of takeover activity has, since
the early 1960s, vacillated between attempts at strict control to a practically laissez
d faire approach depending on the prevailing view of the benefits of a free market for
takeovers. The divergent results reported in the various studies on the economic
consequences of takeover has contributed to the vacillation in government policy. A
reconciliation of the divergent results in the three studies examined in this thesis will
inform the debate on the effects of takeovers and facilitate more consistent and
steadfast regulation of takeover activity.

(iii) Advances in theory, research design and quality of data available permit a more
rigorous and comprehensive analysis to be done on the sample firms included in the
three studies. Many of the data used in this thesis were either not available or not
used by the authors of the three studies being re-examined. Use of these more
comprehensive data coupled with improvements in research design allows more
powerful tests to be done on whether the takeover market, on average, allocates
 corporate assets to more valued uses. In sum, the innovations and improvements
incorporated in this thesis both facilitate the reconciliation of the divergent results
reported in the three studies examined and help determine more conclusively the

3 Bishop, Dodd and Officer (1987, p. 22).
4 A short history of the regulation of takeover activity in Australia is found in McDougall and
is not limited to the Government. On April 24 1986 The Australian Financial Review ran an editorial
which labelled McDougall and Round’s study “futile” and “dangerous” because it “may be used to
justify increased regulation of takeovers”. This was alarming because “policies that hinder takeovers
quite simply prevent resources being used by those who can get the most of them” (p. 14). By May
21 1991 The Australian Financial Review had turned full circle. On that day its editorial claimed that
“takeover raids, even unsuccessful ones, can have a painfully corrosive effect on companies, morale
often suffers and industrial performance flags ... The intangible cost of diverted management attention
and stultified growth is unlikely ever to be known” (p. 16). No major study on the effects of
takeovers in Australia had been released in the interim period.
5 Bob McComas, a partner in Clayton Utz, points out that the uncertainty created by frequent
revisions of the regulations on takeovers imposes a cost on the business community. Businesses are
not able to proceed with confidence on otherwise profitable takeovers because they fear a detrimental
change to the rules governing takeovers (“Unnecessary inquiry into takeover law”, The Australian
1.2 RESEARCH QUESTIONS AND THESIS CONTRIBUTION

In achieving the aims described above, this thesis addresses the following questions.

(i) Were the divergent results reported in the three studies due to sample bias?

(ii) Were the divergent results due to weaknesses in the three studies' research designs?

(iii) After correcting for weaknesses in research design, does the evidence indicate that the market for takeovers allocates corporate assets to more valued uses?

(iv) Is there a significant correlation between share market and unadjusted accounting based estimates of the gains from takeover?

The answers to the above questions are among the significant contributions of this thesis to the empirical research on takeovers. Equally important methodological contributions include extensive investigations of several issues.

(i) The impact of positive asymmetry in the distribution of share market returns on estimated abnormal returns.

(ii) The impact of survivorship bias on calculations of market index returns and on estimated abnormal returns.

(iii) The impact of the systematic negative association between firm size and returns on estimated abnormal returns.

(iv) The design of a model to estimate the downward bias in merging firms' post-takeover accounting rate of return.

(v) The design of an accounting based research method that facilitates comparison of the accounting results with share market event study results.

Finally, the comprehensive data bases and computer programs developed and tested for the purposes of this thesis also constitute a significant contribution. A range of interesting issues, not necessarily related to takeovers, may now be investigated as a result of the requisite data being accessible at a much lower cost.
1.3 THESIS STRUCTURE

Chapter Two surveys the literature on takeovers. The theory and evidence behind the two competing views on the economic consequences of a free market for corporate control are discussed and evaluated. The aim is to note areas where theory and evidence are in agreement and robust conclusions may be drawn in favour of one side or the other. While advances have been made, significant issues remain unresolved. These unresolved issues are reviewed towards the end of Chapter Two and the specific concerns addressed in this thesis are identified.

Chapter Three describes and defends the research method used to analyse the share market effects of corporate takeovers. Theoretical and empirical justifications of the research design are provided. In Chapter Four the share market based results are described and discussed.

Chapter Five begins with a defence of accounting numbers as valid indicators of firm performance. McDougall and Round's study is then criticised in light of this defence. The second half of Chapter Five begins with a description and defence of the research design adopted and ends with a description and discussion of the results obtained.

Chapter Six comprises a short overview of the thesis which highlights the major findings and their implications for the debate on corporate takeovers. It identifies unresolved issues and areas that look to be the most promising avenues for further research.
CHAPTER TWO
THE MARKET FOR CORPORATE CONTROL: A LITERATURE REVIEW

2.0 INTRODUCTION

The fundamental issue in the debate over takeovers is whether a free and active market for corporate control is beneficial to the economy [Horin (1986)]. Critics of takeovers contend that takeovers are wasteful of managerial resources, unnecessarily disruptive to the economy and serve mainly to build "corporate empires" which enhance management utility at the expense of shareholders and society at large. Supporters of takeovers view the market for corporate control as an important mechanism which helps deploy corporate resources to their most valued use, thus assisting in the optimal allocation of resources.

This chapter reviews the competing theories on takeovers and examines in detail the available empirical evidence from both event study and accounts based research. Event study research uses share market data to examine the wealth effect of takeovers while accounts based research uses information derived from financial statements.

The chapter is organised as follows: Section 2.1 introduces the current theories on takeovers and comments on their testable implications; Section 2.2 reviews the work done to date using "accounts" based research; Section 2.3 reviews the work done to date using "events study" research; Section 2.4 concludes the review.

2.1 TAKEOVER THEORY

2.1.1 The Pro-Takeover Perspective

In its most general form, the argument put forward by those in favour of a free market for corporate control states that takeovers, on average, channel corporate resources to higher valued uses [Bishop, Dodd and Officer (1986)]. Bradley and Jarrell (1988) observe that this is "nothing more than a simple extension of the neoclassical notion that there are mutual gains from voluntary trade - that through the process of voluntary exchange, resources flow to their highest value use" (p. 253).

The above argument does not specify the source(s) of the gains from takeover. A convenient way to introduce the various hypotheses which explain the possible sources of gains from takeover is via the "market for corporate control" framework.
From the "market for corporate control" perspective, management and investment (i.e., risk-bearing) are viewed as separate factors of production, each faced with a market for its services that provides alternative opportunities [Fama (1980)]. Investors evaluate alternative management teams on the basis of two criteria: (i) the extent to which return on the firm's assets is maximised, and (ii) the extent to which the potential conflict of interest between managers and investors is minimised. The two criteria are elaborated below.

2.1.1.1 Returns maximising criterion

In a takeover situation, competing management teams may promise to deliver a higher return on assets to shareholders on the basis of (a) efficiency gains, (b) monopoly gains, or (c) wealth transfer gains.

(a) Efficiency gains are the most commonly touted benefits of merger and are claimed to derive variously from synergy effects, economies of scale, or increased managerial efficiency.¹

(b) Monopoly gains arise when the effect of a takeover is to concentrate economic power which enables the merged firm to reap economic rents, possibly by facilitating collusive pricing policies within the industry or through an enhanced ability to engage in predatory pricing tactics which intimidate other firms in the industry.

(c) Wealth transfer gains can arise from tax policies which enable the merged firms to pay less tax in total than would be the case if they were separate entities. Examples include transfers of tax credits (in the U.S.A.) and investment allowances. Another form of wealth transfer gain occurs when the new management team has greater bargaining power than the old team and is thus able to extract greater concessions from the other factors of production, e.g., labour.

Monopoly sourced rents and wealth transfer gains are, using a Pareto optimal welfare criterion, suboptimal outcomes from takeover activity.² However, it is an empirical issue (addressed below) whether monopoly rents and wealth transfers are the prime reasons motivating the majority of takeovers. Further, even if it were proved that the ability to earn monopoly rents or affect wealth transfers motivated a substantial proportion of takeovers it is not obvious why takeover activity should be

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¹ Copeland and Weston (1988, pp. 683-6) discuss efficiency explanations in depth.
² This assumes that the outcomes do not conform to approved forms of wealth transfers. This is a moot point in relation to transfers of tax credits and investment allowances since it could be argued that they are intended to effect such transfers.
subject to greater regulation than other forms of investment activity. Such regulation is only justified if it can be shown that takeovers are a more efficient means of effecting such gains relative to other forms of investment activity, i.e., that a higher incidence of takeover relative to other forms of investment results in higher levels of wealth transfers or monopoly rents.

2.1.1.2 Minimisation of Conflicting Interests Criterion

A potential conflict of interest exists between managers and shareholders. Corporate managers derive a complex package of rewards from the firm, only one of which is to maximise net present value to the shareholders, whereas investors care only about maximising the net present value of their holdings. The conflict of interest between the two groups is mitigated via contracts which constrain managerial choice and by the existence of the managerial labor market, which monitors managers' performance in maximising firm value and helps tailor future contracts between parties with an interest in the firm.

Given costly and therefore imperfect monitoring, one would expect the existence of non-value maximising behaviour by managers, even with optimal employment contracts. However, in some instances, the non-value-maximising behaviour of managers is egregiously large. This is more likely to be true in firms characterised by diffuse share ownership and excess liquidity, which frees the manager from the discipline of the capital market.

Pro-takeover theorists argue that, under the above conditions, hostile takeovers reassert the interests of shareholders by reducing the incidence of non-value-maximising behaviour and transferring the resultant gains to the shareholders.

2.1.1.3 Conclusion: Pro-takeover Theory

In concluding this brief exposition of neoclassical takeover theory, it is worth emphasising that, in the neoclassical view, takeovers are just another part of the general process of resource allocation within the modern economy. As Copeland and Weston (1988) put it: "internal investments and external investments [are] alternatives that are not necessarily substitutable or mutually exclusive. Sometimes

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3 Value-maximising behaviour is defined as behaviour that maximises the net present value of the shareholders' interests in the firm.
4 Shleifer and Vishny (1988) discuss other common weaknesses in firms' internal control mechanisms.
5 The agency problem is overcome by the new management team paying the shareholders a share of the increase in firm value brought about by the new value-maximising strategies. The money to pay-off the shareholders is often borrowed under stringent conditions which effectively eliminate non-value-maximising behaviour by the new management team. In effect, the takeover can be viewed as a renegotiation of the contract between the firm's managers and its investors (risk-bearers).
circumstances suggest the use of internal investments; other times they suggest external investments" (p. 757).

From the neoclassical perspective, at the margin there is no reason to expect external investment to be more or less profitable than internal investment, if a rational, value maximising decision process drives takeovers. Whether or not the rational, value-maximising assumptions are justified will be considered in the sections reviewing the empirical evidence.

2.1.2 The Anti-Takeover Perspective

A common riposte of the anti-takeover theorists, to the proposition that the threat of takeover is a useful control over the actions of managers of potential targets, is that value-maximising behaviour on the part of the bidding firm's management does not necessarily follow. Bidding firms' managements have incentives to engage in takeover activity even when it does not maximise their firms' net present value.6

In the anti-takeover theorists' interpretation, managers of firms characterised by both weak internal control mechanisms and "free cash flow"7 further their utility through the acquisition of other firms even though they may not be able to increase the return on the acquired firms' assets. Competition between rival bidding firms for targets results in overpayment so that, at the margin, takeovers are negative net present value investments.8

At the heart of the anti-takeover arguments is the notion that capital markets are inefficient in valuing corporate assets so that takeovers do not necessarily result in corporate resources being put to their most valued use. Ostensible reasons for market inefficiency include:

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6 Frequently cited incentives include the perception that managerial compensation is positively correlated with firm size and the psychic rewards associated with running a large firm. See, for instance, Penrose (1959), Reid (1968), Mueller (1969), Mueller et al. (1980), McDougall and Round (1986) and Morck, Schleifer and Vishny (1990).

7 Jensen (1988, p. 28) defines free cash flow as "cash flow in excess of that required to fund all of a firm's projects that have positive net present values when discounted at the relevant cost of capital".

8 Negative NPV takeovers are not unambiguously suboptimal outcomes. Jensen (1988, p. 34) argues that if, given inadequate constraints on managerial choice, the alternative is a slow internal wastage of resources, then a negative NPV takeover which reduces the expected waste might, on balance, be a more optimal outcome. Reasons why losses associated with negative net present value takeovers are not expected to be greater than losses associated with internal waste include: (a) bidder management fear that investing in projects with greater waste than that anticipated by the market will make the bidder firm vulnerable to takeover because of the decrease in share price; and (b) in the event of takeover, excess funds are immediately released from the bidder firm's industry and presumably put to better use by the target's ex-shareholders. Further, the release of excess funds reduces the flexibility enjoyed by managers of the bidding firm and motivates them to perform.
(i) Market Myopia - securities markets discount long term projects at a higher rate than short-term projects even when the cost of capital is the same. One implication of this type of inefficiency is that it is possible for "corporate raiders" to acquire cheaply sound companies which have invested in projects with pay-offs in the long-term. The raiders profit by selling off the acquired companies' assets which the share market had hitherto valued at a discount to their true value.

(ii) Managerial Hubris [Roll (1986)] - occurs when managers over-value takeover targets as a result of a surfeit of confidence.9

(iii) Generalised Market Inefficiency in pricing corporate assets so that relative to future prospects some firms (often targets) are undervalued while other firms (often bidders) are overvalued.10

In summary, in the market failure perspective favoured by the anti-takeover theorists, corporate raiders take advantage of undervalued targets or their own overvalued securities to amass corporate assets which enhance their own utility without necessarily adding any real value to the economy. A high incidence of takeovers leads at best to a destabilising reshuffling of assets whilst at worst the target companies end up being run by people with less competence and interest in the acquired firm than the displaced managers. The anti-takeover perspective is also consistent with the view that a significant proportion of takeovers are motivated by the potential to effect wealth transfers or secure monopoly rents. It is argued that closer regulation of takeover activity can reduce the incidence of such takeovers while allowing socially beneficial takeovers to proceed.

2.1.3 Comment on the Testable Implications of the Theories

Ideally, given the concerns identified, the effects of takeovers should be measured at both the firm and society levels. However, developing testable implications of the effects on overall social welfare is difficult because of the problems in simulating credible counterfactual scenarios against which the actual case may be compared. Further, it is difficult to obtain agreement on appropriate measures of social welfare.

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9 Roll (1986, p. 200) contends that the hubris hypothesis is not inconsistent with strong-form market efficiency. In his view, takeovers are virtually unique transactions in that bids with positive valuation errors will typically be the winners in takeover contests so that the average winning bid will be an over-bid. This remains consistent with the notion that the market had originally priced the target firms "correctly". The alternative hypothesis is that managers do not overvalue targets but rather, they value targets in terms of the expected change to their total compensation package as a result of the takeover [Jensen (1988)]. This is a variant of the "managerialism" hypothesis discussed earlier.

10 See Scherer (1988, pp. 72-4) for a brief review of the reasons behind the capital market's alleged valuation errors. Shiller (1986) offers a more comprehensive account of why the notion of market efficiency should be doubted.
1968 was due to external acquisitions. An additional criterion, imposed to ensure diversification, is that in 1968 the firm had to be represented in ten or more 3-digit SIC industry categories or in five or more 2-digit SIC categories. Two control samples, one of industrial companies only and the other a mix of industrial and non-industrial companies, were selected at random from Fortune Magazine's lists of major companies.

Weston and Mansinghka compare earnings performance for the years 1958 and 1968. The measures used are (i) earnings (before interest and preferred stock dividends) to total assets, and (ii) net income to net assets. For 1958, they find the mean EBIT/total assets ratio of the control samples is significantly higher than the same ratio earned for the conglomerates (8.7% vs 16.7% and 12.6%). By 1968, there is no significant difference among the groups on this measure. A similar pattern is found for the mean net income/net worth ratio. In 1958, this ratio's mean is significantly higher for the control firms compared to the conglomerates (7.6% vs 12.6% and 10.7%) but by 1968 no significant difference exists.

Weston and Mansinghka interpret the above results to conclude that "an important economic function of conglomerate firms has been raising the profitability of firms with depressed earnings to the average for industry generally ... [T]he most appropriate test of the earnings performance of conglomerate firms is not superior earnings performance, but their ability to achieve average earnings performance" (p. 934).

Given that their sample of conglomerates achieved superior relative performance on their measures, Weston and Mansinghka's study is consistent with the hypothesis that, on average, takeovers reallocate corporate resources to more valued uses.

Utton (1974)

Utton (1974) measures the effect of industrial mergers on book measures of profitability. Two main questions are addressed: how does the profit performance of the merger intensive firm compare first with the average performance of firms in the same industry, and second, regardless of industry, with the performance of firms which do not merge?

Utton compares merger intensive firms and a random sample of firms which grew internally. Thirty-nine firms which undertook a merger between 1954 and 1966 are included. However, the comparison is made for the period 1966-70. To insure that the effects of other mergers undertaken before and after the sample period do not
In light of the above, most empirical work to date has focused on the effects of a takeover on firm performance, measured by various indicators. Measuring firm performance has been justified on the basis that it provides an indirect test of the efficiency of the market for corporate control [McDougall and Round (1986)]. Under this interpretation, evidence of widespread relatively poor performance by merging firms in the post-merger period supports the hypothesis that takeovers, on average, are not driven by value-maximising considerations but by managers furthering their narrow self-interest at the probable expense of the other investors in the firm. Good post-merger performance is consistent with value maximising managerial behaviour.

This thesis follows the above tradition in evaluating the effects of takeover. That is, the economic consequences of takeovers are assessed in terms of their impact on the wealth of the stakeholders in the firms involved. While the focus is principally on shareholder wealth effects, the effects of takeovers on other stakeholders are also canvassed. Examining the wealth effects of takeovers on firms' stakeholders other than shareholders is useful in determining whether or not the opportunity to effect wealth transfers or earn monopoly rents are the prime motivation for takeover activity.

2.2 ACCOUNTS RESEARCH: THE EVIDENCE

Accounts based studies typically focus not on just one measure of performance but several. Some studies define the various operational measures of performance differently so strict comparison of results is difficult. In light of this, the accounts studies are first reviewed separately (in chronological order), their results are then summarised and conclusions drawn. Criticism of the studies is limited to comments on sample selection criteria or to pointing out differences in interpretation. The methodological critique of accounts based research is reserved for Chapter Four.

2.2.1 Review of Major Accounts Based Studies

Weston and Mansinghka (1971)

Weston and Mansinghka (1971) evaluate the performance of conglomerates using measures presumed to reflect the interests of, respectively, managers, individual shareholders, and institutional investors. Their prime concern is economic efficiency. They compare the relative performance of conglomerate firms which merged between 1960 and 1968 to that of a control sample of firms which engaged in little or no merger activity. Weston and Mansinghka's merger sample includes 63 firms where 20 per cent or more of the increase in total assets between 1960 and
bias the results, two further criteria are imposed: (i) acquisitions made between 1954 and 1965 have to form at least 20 per cent of the 1965 size of the acquiring companies and (ii) 75 per cent of the acquisitions must have taken place between 1961 and 1965. The selection criteria for the 39 firms in the control sample ensures that an insignificant portion of their net assets is accounted for by merger activity.

The measure of profitability used by Utton is pre-tax profit. He does not investigate the effect of financing decisions on this measure. Further, the relative decline in profit due to amortisation of goodwill recorded on consolidation is considered negligible on the basis of Singh's (1971) analysis.

Relative to their industry average, 66 per cent of acquirers performed better one year after merger but this dropped to 57 per cent two years after merger. However, Utton notes that the proportion is probably insufficient to generalise the conclusion. Relative to the random sample, the merger intensive group's profit was two and a half times greater in the period 1961-65. The mean profit for the merger group was 11.5% and 13.6% for the periods 1966-70 and 1961-65 and 15.4% for the control groups in the same period. Utton interprets these results to conclude that, at best, mergers make no difference to performance.

Two points cast doubt on the generalisability of Utton's conclusion. First, it is likely that his selection criterion, that no more significant mergers must have taken place over the five year period in which the comparison was being made, meant that the more successful or competent acquirers would have been excluded. Second, Ravenscroft and Scherer's (1987) results (summarised below) show that the effect of amortisation of goodwill on consolidation on book profit is not negligible.

**Mueller et al. (1980)**

McDougall and Round (1986) base their research method on that used by Mueller et al. in seven countries to assess the economic consequences of mergers.\(^\text{11}\) Given that McDougall and Round's (and by extension, Mueller et al.'s) research method will be extensively criticised in Chapter Five, this review of Mueller et al.'s analysis focuses only on the results and conclusions drawn.

Mueller et al. find that merging firms have several characteristics in common even across countries. In general, acquiring firms are larger than the firms they acquire

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\(^{11}\) The countries were Belgium, Germany, France, the Netherlands, Sweden, Britain, and the United States.
and target firms smaller than their industry average (except in Britain and Germany). Acquiring firms are at least as profitable, if not more profitable, than target firms. Relative to target firms, acquiring firms are as or more highly leveraged. Acquiring firms have had higher growth rates than the targets (except in France, where they are similar).

Mueller et al. reject the hypothesis that merger takes place among relatively small firms to achieve the efficiency advantage of size, on the basis that the merging firms in their sample are, on average, as large or larger than randomly selected pairs of firms matched by industry. However, given that this result is not inconsistent with the hypothesis that economies of scale exist at the larger size of the merged firms, Mueller et al.'s finding cannot be used to reject the economies of scale motive.

Risk reduction is also rejected as a motive for merger, since no consistent result is reported across countries except with respect to leverage. Acquiring firms are found to be more highly levered than either the target companies or a control sample.

Finally, with respect to whether merger increases or decreases profitability, the most Mueller et al. commit themselves to is that mergers appear to have little effect on profitability in the post-merger period.

**McDougall and Round (1986)**

McDougall and Round (henceforth, MR) aim to evaluate the motives for, and effects of, takeovers in Australia. Their sample comprises 88 full (Part A) takeovers which occurred between 1970 and 1981. All firms in the sample are public companies in the retail, transport, and industrial sectors which were not involved in another takeover for three years before and after the takeover examined.

All the target and bidding firms are matched with "comparable" firms. MR test their hypothesis by comparisons of merging firms with the matched firms both individually and jointly. In most cases the comparisons are made for five years before and after the takeover but in some cases only three years' data could be obtained.

MR find: (i) the bidder firms are much bigger than targets on average. They concur with Mueller et al. in finding this inconsistent with scale economies as a motive for takeovers; (ii) bidder firms have higher profits pre-takeover and less profit variability relative to their respective target firms; (iii) post-takeover, the merged firms grow faster than their matched counterparts but the growth is not as fast as the acquirers' growth in the pre-takeover period; (iv) post-takeover profits are lower
than pre-takeover profits; and (v) post-takeover leverage is higher for the merged firms - which runs counter to the prediction of a risk reduction motive.

Based on the above findings, MR conclude that merged firms do worse post-takeover relative to both the pre-merger period and to the matched non-merging firms. Takeovers "appear to have been caused by so-called managerial motives, or by a desire to develop or enhance market power" (p. 189). Further comment on MR's study is left to Chapter Five.

**Ravenscroft and Scherer (1987)**

Ravenscroft and Scherer (1987) argue that current takeover theory implies at least two testable hypotheses. First, given that a significant proportion of takeovers are ostensibly directed towards replacing inefficient management, "the pre-takeover profitability of targets should be lower than that of non-target peer firms, other things ... being held equal" (p. 147). Second, both the introduction of efficient management and the possibility of "synergy" imply that post-takeover profitability should be greater than pre-takeover profitability.

Ravenscroft and Scherer test their hypothesis on 96 manufacturing company tender offer acquisitions which were included in the U.S. Federal Trade Commission's (FTC) Line of Business program (LB). They cite two advantages in using the disaggregated data provided by the program: (i) the acquired units can be singled out for special scrutiny, and (ii) the performance of other companies' non-acquired units of comparable size in the same industry provide well focused controls. The period over which the acquisitions took place is between 1950 and 1974 and the post-takeover performance is analysed for the three years 1975-77 which, on average, follow their specific sample of takeovers by nine years.

For 95 targets on which pre-takeover data are available, operating EBIT averaged 11.08% of assets in the two years prior to takeover. The time matched average for the 2-digit matched industries is 12.06%, the difference being significant at the 0.05 level (t= 1.90). In other words, the tender offer targets under-performed by about 8% relative to their own industry averages. There is no statistically significant difference in pre-takeover performance between targets that were the subject of hostile tender offers and those that were not.

Ravenscroft and Scherer's post-takeover analysis for which "clean" data were

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12 Ravenscroft and Scherer report that the FTC's LB surveys cover a maximum of 471 U.S. industrial companies that made three quarters of all manufacturing and mineral acquisitions, by asset value, between 1950 and 1976 (p. 148).
available covers 62 target companies with 153 LBs. They use a regression equation to control for market share, differences in merger accounting method ("purchase" or "pooling"), relative size of merging firms and level of (non-tender) acquired assets. The results show that over the 3 years 1975-77, LBs with a tender offer history were 3.10 percentage points less profitable than lines without such a history (whose average operating income/assets ratio was 13.34%). In other words, LBs which were the target of takeover activity were 23% less profitable, on average, than LBs not subject to takeover activity.

Ravenscroft and Scherer recognise that the accounting method used in recording acquired assets is important when interpreting the above result. If, as is typically the case, a premium over book value is paid in takeovers, the denominator of the ratio of operating income to assets is inflated for the merged firms relative to the denominators of the non-merged control firms (or LBs in this case). In an attempt to control for this, they use the ratio of cash flow (before deduction of depreciation) to sales as the dependent variable in an otherwise identical analysis. The results show that the 257 control LBs had a mean cashflow/sales ratio of 10.12% over the sample period while the acquired LBs were 1.09% less profitable. However, the 10.8% lower relative profitability exhibited by the acquired LBs was not statistically significant. Further, Ravenscroft and Scherer note that the post-takeover performance of the acquired LBs stands in much the same relationship to the non-acquired LBs as their pre-takeover performance. They conclude that this result, "[compels] an agnostic inference that takeover neither improved nor degraded the basic operating performance of target firms" (p. 154).

Herman and Lowenstein (1988)

Herman and Lowenstein (1988) analyse the efficiency effects of 56 hostile takeovers carried out over the period 1975-1983 by comparing pre- and post-merger rates of return and risk for bidding and acquired firms.

Profitability is measured by pre-tax returns on total capital (ROC) and after-tax returns on common equity (ROE) while risk is measured by the debt/equity ratio and fixed charge coverage. Data are obtained from published financial statements as contained in the COMPUSTAT data base prepared by Standard & Poors. In terms of risk, as measured by debt ratios and interest coverage, the targets displayed similar (high) levels of risk in the five years pre-merger. On an assets weighted basis the mean debt/equity ratio is 58.0% for bidders and 78.4% for targets.13

13 Bradley and Jarrell (1988, p. 257) note that Herman and Lowenstein's measures are out of line with the majority of studies on corporate leverage which typically indicate an average debt to
The fixed coverage ratio for targets averaged (a safe) 5.7 and actually increased over the last two years prior to merger. Bidders had a similar average coverage (5.2) in the three years prior to takeover. Herman and Lowenstein conclude that these results are inconsistent with "one of the common justifications for takeover" (p. 223) which is, that targets are underleveraged by the standards of the marketplace.\footnote{14}

In terms of profitability, Herman and Lowenstein find significant differences in the performance of firms involved in takeovers over the period 1975-1978 and those involved over the period 1981-1983. For the 21 targets in the former period, the average return on equity in the five years pre-merger was 8.8% but the mean return for the same interval for the 25 targets in the latter period was 15.9%. Further, the successful bidders over the period 1975-1978 not only did well pre-merger but continued to improve in performance post-merger. Their average ROE grew from 14% in the pre-merger year to 17% five years post-merger. The average pre-merger ROE for the bidders over the period 1981-83 was also 14% but fell to less than 9% in the two years after merger (no data are available for later years).

Herman and Lowenstein attribute the relatively poor performance of the latter period bidders to changes in the securities market which made it easier to attempt a takeover and encouraged a "casino mentality".\footnote{15} Although their analysis might be correct, a comparison of the merged firms' performances with all other firms over the same post-merger period would provide a much stronger test of their hypothesis. Further, Herman and Lowenstein's reasoning that the extraordinarily high premiums paid by the bidders to the targets' shareholders ought to have been reflected in "substantially higher rates of return than those of targets" (p. 231) overlooks the value-maximising principle that investment ought to proceed until the marginal rate of return equals the cost of capital.

\textbf{Healy, Palepu and Ruback (1990)}

One alleged weakness of financial statement based numbers as indicators of post-merger performance is that different methods of accounting for mergers often yield divergent performance values [Appleyard (1980), Meeks and Meeks (1981), and Stanton (1987)]. Given the apparent absence of effective constraints on managerial choice of accounting methods it can be argued that accounting data provide an

\footnote{14}{But see Bruner (1988) who, after controlling for the secular rise in leverage over the period 1962-1980 and accounting for cash balances, presents new evidence in support of the hypothesis that "slack-rich" bidders pair with "slack-poor" targets to create value.}

\footnote{15}{The term "casino mentality" is not used by them.}

Cash flow variables measure period-by-period performance which is affected by firm-specific and industry factors and so Healy et al. use industry performance as a benchmark to evaluate post-merger performance. The event-window over which performance is measured is \([-5,+5]\) years excluding the year of the merger. In so far that a proportion of the gains from takeover are achieved after year five, Healy et al.'s cash flow measures underestimate the gains (if any) from takeover. This is a potentially serious weakness of their study and is a direct cost of using cash flow variables. However, the underestimation bias does increase the power of their test since, if the managerial aggrandization hypothesis were true, one would expect the deleterious effects of takeover to be evident well within five years after merger.

Healy et al.'s sample comprises the largest 50 acquisitions which took place in the U.S. during the period 1979 to 1983. Pre-tax operating cash flows are used to measure improvements in operating performance. These are defined as "sales, minus cost of goods sold and selling and administrative expenses, plus depreciation and goodwill expenses" (p. 5). The cash flows are deflated by "the market value of assets (market value of equity plus book value of debt) to provide a returns metric that is comparable across firms" (p. 5). The accounting data of the target and bidding firms prior to the merger are added to obtain the pro forma performance of the combined firms. Comparisons between these sets of data and data from the merged entity in the post-takeover period provide a measure of the change in performance. Industry adjusted performance measures are calculated by subtracting the industry median from the sample firm value.

The results indicate that the median pre-tax operating returns for the sample firms were lower in the post-merger period when compared with the returns in the pre-merger period. Healy et al. exclude the year of the merger for two reasons, "first, many of the acquiring firms use the purchase accounting method, implying that in the year of the merger the two firms are consolidated for financial reporting purposes from only the date of the merger. Results for this year are therefore not comparable across firms or for industry comparisons. Second, year 0 figures are affected by one-time merger costs incurred during that year, making it difficult to compare them with results for other years " (p. 9). Healy et al. point out that by selecting mergers in which the target was large, they increased the probability of detecting economic consequences from takeover using accounting data. Further, firms which undertook very large acquisitions were less likely to undertake an equally significant takeover in the near future and so the probability of confounding events is reduced (p. 3). Finally, we may note that large takeovers are often the catalysts in reviving the takeover debate and their outcomes might be considered of special interest.

The change in equity value of the target and bidder firm at the merger announcement is excluded from the asset base in the post-merger years. Healy et al. argue that "this adjustment is important because an efficient stock market capitalises the value of any expected improvement in equity prices at the merger announcement" (p. 10).
merger period. The returns range from 26% to 27.5% in the five year pre-merger period while in the five year post-merger period they range from 19% to 23%. Intriguingly, although this finding is in line with other accounting based studies on takeovers, Healy et al. do not emphasise this point in their discussion.¹⁹ They find the comparison of the sample firms' performance with their respective industry performance to be of more interest. At least part of the explanation lies in their argument that industry-adjusted return is a more reliable measure of performance since they control for industry events unrelated to the merger.

As stated earlier, Healy et al. define industry adjusted cash flow return as the difference between the return for the merged firm and its median weighted average industry estimate.²⁰ The results show that merged firms have a superior pre-tax operating cash flow return on assets relative to their industry in the post-merger period. "Overall, the annual median pre-tax return in the five post-merger years is 2.8%. Stated differently, the merged firms' pre-tax operating returns in the post-merger period are about 15% larger than their industries' returns. In contrast, no superior industry-adjusted pre-tax operating cash flow returns occur in the five years before the merger; the median annual return is 0.4%" (p. 12).²¹

Healy et al. investigate several possible reasons for the improvement in industry-adjusted cash flow returns in the post-merger period. Monopoly rents are an unlikely source of the increased returns since Healy et al. find that the difference between pre- and post-merger industry adjusted operating margin is insignificant. Similarly, there is no significant difference in the capital and R&D expenditures of the sample firms and their industry counterparts in either the pre- or the post-merger period. There is some indirect evidence of a decrease in labour costs in the post-merger period but Healy et al. consider that the magnitude of the decrease in labour costs is not sufficient to account for the improved performance. The prime reason for the improved industry adjusted performance of the merged firms appears to be an increase in asset turnover. In the period [-5, -1] years the sample firms generated thirty cents less sales than their competitors for each dollar of assets. In contrast, over the period [+1,+5] years the sample firms generated ten cents more sales than their industry counterparts for each dollar of assets.

¹⁹ The finding is not foreshadowed in the abstract nor is it mentioned in the concluding discussion and summary.

²⁰ "Prior to the merger industry values for the sample firms are constructed by weighting median performance measures for the target and acquiring firms' industries by the relative asset sizes of the two firms at the beginning of each year. In all of the post-merger years target and industry cash flow returns are weighted by the relative asset sizes of the two firms one year before the merger" (pp. 11-12).

²¹ Healy et al. find that their results are not affected by two possible confounding factors (i) sensitivity to the definition of industry used in the analysis and (ii) relative declines in the market value of sample firms' assets. The last factor could cause a spurious improvement to Healy et al.'s asset deflated cash flow returns measure.
While Healy et al. conclude from their analysis that merged firms' improved industry adjusted performance is primarily a result of increased asset productivity, they note that their analysis still leaves open the question whether the improvement came about as a result of merger or whether it would have occurred even without a merger. If mergers are due to systematic undervaluation of target firms by the market, there will be improvements in cash flows regardless of merger activity. "Acquirers that anticipate the cash flow improvements will pay a premium to acquire the targets" (p. 22).

2.2.2 Summary and Comment on Accounts Research

In summary, the evidence across the accounts based studies is mixed. While Weston and Mansinghka (1971) find that merger raises acquired firms' below average performances to normal levels, most other studies find no significant difference in firms' performances post merger. The two notable exceptions are Healy, Palepu and Ruback (1990) and MR (1986) who both find that merged firms achieve lower returns in the post-merger years relative to their weighted average returns in the pre-merger years. However, on an industry-returns-adjusted basis, Healy et al. find that merged firms improve their relative profitability in the post-merger period. Notwithstanding Healy et al.'s concession that their study does not allow them to identify the reasons for the improved asset productivity of the merged firms, their conclusion that "expectations of economic improvements underlie the equity revaluations of the merging firms" diverges from the implication drawn by MR from their study which is that "takeovers appear to have been caused by so-called managerial motives".

As mentioned earlier, strict comparisons across the accounts studies are not possible because of differences in research methods. Probable reasons for the diversity in results include failure to control for the write-up of assets on acquisition in some studies [e.g., Utton (1974)], differences in sample types, differences in the accounting ratios employed, and measurement problems induced by acquirers being larger than their targets. These issues are discussed in Chapter Five.

Finally, perhaps the most significant aspect of the accounts research lies not in the results obtained, but in the interpretations placed on them by the researchers. With the exception of Weston and Mansinghka (1971) and Healy, Palepu, and Ruback (1990), all the accounts researchers interpret normal performance post-merger as a disappointment rather than the expected outcome in a competitive market for corporate control [e.g., Herman and Lowenstein (1988, pp. 227-228)]. In this
respect, accounts researchers differ significantly from market researchers. The difference is not logically compelled.

2.3 EVENT STUDY RESEARCH: THE EVIDENCE

One criticism directed at researchers involved in event studies of takeovers is that their conclusions tend to be more unequivocal than is warranted by their results. This criticism has been made most forcefully by Roll (1986) who alleges that Jensen and Ruback's (1983) comprehensive review of event study research on takeovers is open to a different interpretation than the one made by them, which is "that corporate takeovers generate positive gains, that target firm shareholders benefit, and that bidding firm shareholders do not lose" (p. 47).

Roll's contention is that the empirical evidence is at least equally supportive of an alternative hypothesis that "bidding firms infected by hubris simply pay too much for their targets" (p. 197) as it is of the hypothesis that takeovers are driven by value-increasing behaviour. Given that evidence in favour of Roll's argument is also consistent with many of the arguments marshalled by those who caution against a free market for corporate control, this review evaluates event studies of takeovers in light of both Roll's "hubris" hypothesis and what may be termed the "value-increasing" hypothesis.

The review is structured as follows. First, the evidence on the returns from takeover bids to shareholders in target firms is presented. This is followed by an analysis of the evidence on the returns to bidding firm shareholders. I then review studies which attempt to determine whether efficiency gains, or wealth transfers or monopoly rents are the predominant motive for takeovers. Finally, after a summary of the evidence presented, reasons for the striking inconsistencies in the returns to the shareholders of bidding firms are canvassed. This last discussion highlights the issues of concern that will be addressed in Chapters Three and Four.

2.3.1 Target Firms' Shareholders' Returns

2.3.1.1 Returns to Targets' Shareholders from Successful Bids

A critical concern early in the takeover debate was whether the shareholders of target firms were getting a "fair deal". The most consistent result across takeover studies is that target firm shareholders gain significant abnormal returns irrespective of the motive for takeover.

22 Pertinently, Weston and Mansinghka, and Healy, Palepu and Ruback all have strong backgrounds in share market research.
In a typical study Jarrell and Poulsen (1987) estimate the premiums paid in 663 successful takeovers from 1962 to December 1985. They find that premiums in the U.S. averaged 19% in the 1960s, 35% in the 1970s and 30% from 1980 to 1985. These figures are consistent with the results from 13 studies reviewed in Jensen and Ruback (1983). The 13 studies agree that targets of successful tender offers made before 1980 earned positive returns ranging from 16% to 30%.

Given the increase in leveraged buyouts and going private transactions in the 1980s, it is of interest that the target shareholders in such "takeovers" experienced the same magnitude of abnormal returns as in other takeovers. Lehn and Poulsen (1987) find premiums of 21% to shareholders in 93 leveraged buyouts taking place from 1980 to 1984. DeAngelo, DeAngelo and Rice (1984) find an average of 27% for leveraged buyouts between 1973 and 1980.

Australian evidence on returns to target shareholders prior to Bishop, Dodd and Officer (1986) (henceforth, BDO) is provided by Dodd (1976), and Walter (1984). Consistent with the U.S. evidence, Walter finds that 383 firms successfully acquired between 1966 and 1972, experienced an average increase of 28% in their cumulative abnormal return in the ten week period prior to and including the announcement week. Using a sample of 58 firms acquired during the period 1960-70, Dodd reports an average abnormal return of approximately 25% in the month of announcement.

BDO's study includes results for over 1400 takeover bids (for ordinary shares only) made between January 1972 and December 1985. Successful targets had CARs of about 20% on average over the period [-3,+3] months relative to the bid announcement month.

An intriguing finding in BDO's study is that their target companies did not, on average, display abnormally poor performance over the three years prior to the offers being made. BDO observe that "slightly more than half of the target firms earned positive abnormal returns over the period -36 [months] through -7 [months]. On average the targets were not firms experiencing severe financial difficulties" (p. 60). BDO's finding contrasts with those of Dodd (1976), Kummer and Hoffmeister (1978), Dodd (1980) and Asquith (1983) who find that over the two to three years prior to acquisition, target firms' shareholders realise negative abnormal returns.

The negative abnormal returns experienced by targets prior to a takeover has been claimed as evidence that target firms' managers are differentially inefficient or are
inadequately choice-constrained and so pursue non-value-maximising policies. The BDO's anomalous finding in this respect is attributable to measurement problems inherent in their research design. We revisit this issue in Chapter Three.

Notwithstanding the above, the positive abnormal return experienced by target firms on announcement of the takeover offer [Dodd and Ruback (1977), Kummer and Hoffmeister (1978), Bradley (1980), Mikkelson and Ruback (1985)] supports the conjecture that any positive abnormal return earned by the targets in the months leading to a takeover bid is more the result of the market anticipating the bid rather than the independent performance of the target firms. Australian evidence in support of the above is found in Walter (1984), Haw, Pastena and Lilien (1990) and Aitken and Czernkowski (1991). Walter (1984) finds that his sample of target firms experienced their largest weekly abnormal increase, 13.5%, in the announcement week itself. Haw, Pastena and Lilien (1990) and Aitken and Czernkowski (1991) find that target firms' shares are revalued upwards on disclosure of a substantial shareholding investment by prospective bidders.

2.3.1.2 Targets' Returns: Specific Tests of the Hubris Hypothesis

Positive abnormal returns to target firms on announcement of a takeover bid are consistent with both the value-increasing and hubris hypotheses of takeovers. Bradley, Desai, and Kim (1988) test the hubris hypothesis by measuring synergistic gains using matched pairs of target and acquiring firms involved in successful tender offers over the period 1963 to 1984. They define the total synergistic gain as the sum of the change in the wealth of the shareholders of the target and acquiring firms. The assumption is that if the total gain is greater than zero then a synergy effect is involved. Synergy effects are more consistent with the value-increasing hypothesis than the hubris hypothesis.

For their sample of 236 firms, Bradley et al. find that the combined value of the target and acquiring firms increased, on average, by a significant 7.43%, with 75 per

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23 Consistent with this hypothesis, Asquith (1983) finds that the pre-announcement negative returns for successfully acquired targets is greater than for unsuccessful targets. Presumably, the targets of unsuccessful bids were better positioned to resist a takeover offer because of, at least in part, their relatively better pre-bid share market performance.

24 Bradley et al. posit that the source of the synergy may come from "more efficient management, economies of scale, improved production techniques, the combination of complementary resources, the redeployment of assets to more profitable uses, the exploitation of market power, or any number of value creating mechanisms that fall under the rubric of corporate synergy" (p. 4).

25 As Bradley et al. point out (p. 5), this definition assumes that the takeovers in their sample had no effect on the wealth of the bondholders and other creditors of the firms involved. Kim and McConnell (1977) and Asquith and Kim (1982) provide evidence consistent with this assumption.
cent of the combined revaluations being positive. The mean total dollar gain created by the takeovers in the sample is $117 million (expressed in December 1984 dollars).

Although, prima facie, Bradley et al.'s results contradict the hubris hypothesis, their analysis of the distribution of the gains from takeover shows "that target shareholders capture the majority of the gains from tender offers. Ninety-five percent of the targets in the total sample experienced a positive abnormal return. The average abnormal return is 32% and the ratio of the mean dollar gain to targets to the mean dollar total gain ... is 91%. In contrast, the average abnormal return to acquiring firms is 0.97%, only 47% of the observations are positive, and the ratio of the mean dollar gain to the mean total gain ... is 15%" (p. 13). The total gains issue is discussed further in Section 2.3.3.

Bradley et al. go on to note that due to regulatory changes in the U.S. which have made takeovers more of an open auction affair, both the rate of return and the dollar gain to target shareholders have increased over time, whereas the returns to stockholders of acquiring firms have decreased.26 Consistent with Jarrell and Poulsen (1987), Bradley et al. find that over the period 1981-1984 acquiring firms suffered a significant abnormal loss.27 However, the increased competition which has decreased the returns to bidders and increased the returns to targets is not a zero-sum game: total synergistic gains are larger in multiple-bidder takeovers. Bradley et al. conclude that "the targets of multiple-bidder contests realise greater gains not only at the expense of the shareholders of acquiring firms but also from the greater synergistic gains that accompany these transactions" (p. 31). The losses to the successful bidding firm's shareholders do not sufficiently account for the total gains registered by the target firm shareholders; synergistic gains must account for the difference.

26 Jarrell, Brickley, and Netter (1988) attribute the secular decline in the returns to bidders to increased competition among bidders. Part of the increase in competition can, in turn, be attributed to changes to U.S. federal and state regulations which have imposed disclosure and delay rules that foster multiple-bidding, auction contests and pre-emptive bidding. Kim (1989) cites the passage of the Williams Amendment Act in 1968, the development of sophisticated tactics to repel takeovers (poison pills, targeted share repurchases, lock up provisions and supermajority amendments) and the advent of investment banks that specialise in raising funds to finance corporate investments as factors that, in the U.S., have led to target shareholders gaining control over the bidding process. Nathan and O'Keefe (1989) investigate the rise in takeover premiums since 1973 and conclude that it is unlikely that the passage of the Williams Amendment accounts for the increase in takeover premiums. The two other factors cited by Kim are the more probable cause.

27 Intriguingly, the significant abnormal loss to the portfolio of acquiring firms in the period 1981-1984 appeared to be driven by the negative returns to "white knights" in multiple-bidder contests. Bradley et al. define "white knights" as late (i.e., not initial) bidders in multiple-bidder contests who eventually acquire control. The mean CAR to all 73 successful bidders in multiple bidder contests over the period, [-5, + 5] trading days, centred on the bid announcement, was -1.33% (z = -1.44). The mean CAR to the 24 first bidder acquirers was 0.81% (z = 0.41), whereas the mean CAR for the 49 "white knights" was -2.38% (z=- 2.05) (p. 29). One implication of Bradley et al.'s results is that the hubris hypothesis applies to "white knights".
Although Bradley et al.'s results are, on balance, inconsistent with the hubris hypothesis, the negative abnormal returns to the acquirers in the 1980s remain a puzzle. We return to this problem in Section 2.3.2.

2.3.1.3 Targets' Returns: The Corporate Raider Hypothesis

"Corporate raider" is a term often used in the media to denote a person or company with a reputation for frequently engaging in takeover attempts. The term is generally meant to imply an "economic vandal" who raids target firms, strips them of their assets and makes off with the proceeds whilst leaving the target shareholders poorer for the experience. The raiders are said to be able to accomplish their goal because of "temporary failure" in the market which results in "good" firms being "undervalued". As pointed out by DeChow (1987), although the validity of such arguments is questionable, "the important factor is that these arguments may well be believed by a large sector of the community" (p. 24).

Holderness and Sheehan (1985) in the U.S., and DeChow (1987) and Casey, Dodd, and Dolan (1987) in Australia, address the corporate-raider issue by investigating the share price changes for targets and investors around the period when designated corporate raiders make acquisitions in the target companies. Consistent with Holderness and Sheahan (1985), DeChow finds that the total abnormal return to the target shareholders over the two day announcement period is a significant 5.74%. The cumulative abnormal return (CAR) to the target shareholders in the sample over a six week period beginning four weeks before the first public announcement is 14.93%. These results are inconsistent with the "economic vandal" hypothesis.

Casey, Dodd, and Dolan (1987) extend the time frame of their event study to provide a more precise measure of the wealth effects from corporate takeovers. Using the same sample of raiders identified by DeChow, they compare the returns generated by raiders and non-raiders. Based on their sample of 47 "raided" takeovers and 53 "non-raided" takeovers, Casey et al. report that across a 15 day event window, "the abnormal returns across all firms [acquirers and targets] are approximately equal regardless of whether a raider is involved or not" (p. 211). However, the targets of raiders do not do as well, on average, as the targets of non-raiders. Total CARs of 19.29% and 23.44% are earned by targets of raiders and non-raiders respectively. As noted by Casey et al., the ability of raiders to appropriate a larger share of the gains from takeover might well be why they are termed "raid...
Analysis of the returns experienced by unsuccessful targets is interesting because it enables one to evaluate the "information" hypothesis of takeovers.

Under the information hypothesis, the bidder possesses valuable information, concerning the target, that is not reflected in the target’s share price. Depending on the information, the bidding firm might conclude that the target company's shares are "undervalued". Consequently, the release of this information, through the takeover announcement, causes the market to revalue upwards the target firm’s shares.28

Another variant of the information hypothesis argues that the announcement of the takeover bid motivates management to implement value maximising strategies, with positive results on share price.

In either version, the prediction of the information hypothesis is identical. The announcement of the takeover offer releases positive information to the market concerning the target company. The increase in value in the target firm’s equity is expected to be permanent regardless of the outcome of the bid. However, because stock exchange delisting prevents the measurement of abnormal returns of successfully acquired target firms and because it could be argued that the positive returns in successful takeovers are attributable to, say, the synergy effects of the takeover, examining the returns of unsuccessful targets allows a more rigorous test of the information hypothesis.

In what may be, prima facie, seen as a confirmation of the information hypothesis, Dodd and Ruback (1977), Bradley (1980), Walter (1984) and Bishop, Dodd, and Officer (1986) find that target shareholders, on average, enjoy significant and permanent abnormal returns even if a takeover offer is unsuccessful. Bradley (1980) reports that the post expiration market price for unsuccessful offers is, on average, significantly greater than the cash offered by the bidding firm per target share outstanding. In other words, the market price of the target shares is greater than the amount the target shareholders would have received had they tendered their shares.

Although the revaluation of the equity of the targets of unsuccessful takeover offers is consistent with the information hypothesis, it is not sufficient evidence to reject the corporate control theories of takeover. The positive returns to unsuccessful

28 Stoughton (1988) demonstrates analytically that mergers can be socially beneficial due to risk reduction and information asymmetry even when there are no productive synergies and when positive premia are paid. He makes the strong assumption that both acquiring and target firms have perfect knowledge of each others' values. The gain comes from the resolution of the information asymmetry between the firms and the shareholders as the merger increases the credibility of information conveyed to the financial market.
targets may be due to the anticipation of a future higher valued bid.

In a specific test of the anticipation hypothesis, Bradley, Desai and Kim (1983) examine the returns experienced by unsuccessful targets that were divided into two groups: those that became the target of a subsequent successful bid and those that did not. They find, consistent with Walter (1984) and Bishop, Dodd and Officer (1986), that target shareholders realise a significant positive return on the announcement of a takeover offer and the return is not fully dissipated subsequent to the rejection of the offer by the target shareholders. However, the share price of a target firm that is not the target of a subsequent, successful takeover attempt falls back to its previous level. The share price of a target that receives a successful subsequent bid experiences an additional significant positive revaluation.

The above result is of interest because it implies that a permanent revaluation of the target shares requires that the target's resources be combined with those of an acquiring firm. That is, the gains to the stockholders of unsuccessful targets stem from the anticipation of a future successful takeover and not simply from the revelation of new information regarding the "true" value of the target's resources. BDO's finding, that the market downwardly revises the value of target companies only when the offerer terminates the bid, is consistent with the above interpretation. This is because it implies that offerer termination is considered a signal that the potential for synergy in the target firm is not as great as previously estimated (although BDO find that the price does not fall to its pre-offer level). Target firm induced bid withdrawal does not have any similar adverse portent and might even be viewed as a positive signal that the target firm expects a higher offer to be forthcoming. This would explain why the market does not revise downwards its valuation in such cases.29

Roll's (1986) objection to the above interpretation is that the evidence does not rule out that the gains to target shareholders represent wealth transfers from acquiring firms' shareholders and not necessarily synergistic gains.

Notwithstanding the above, we may note that the large returns accruing to both unsuccessful and successful targets indirectly refutes the managerial aggrandization hypothesis. If takeovers were motivated largely by managerial self-aggrandization,

29 Scherer (1988) contends that the above results do not refute the hypothesis that takeovers are the result of systematic undervaluation of targets by the market. He alleges that "[a]n equally plausible but more sceptical interpretation is that some tender offer targets are not truly undervalued, while others are. During the 'in play' period, targets are subject to intense scrutiny. Those that were truly undervalued but escape the first tenderer will prove attractive to another acquirer, while the small minority that were not undervalued will fall through the screen and see their share values return to the (correct) [sic] pre-announcement levels" (p. 74).
the sheer number of potential targets would ensure that the takeover premium is not as great as the 30% to 80% premiums recorded [Nathan and O'Keefe (1989)].

2.3.1.5 Returns to Target Firm Shareholders: Summary

In summary, the results across a large number of different studies indicate that target shareholders reap significant share market gains from takeover activity. These gains occur across all types of takeover activity and have increased in recent years as a result of a more open market for corporate control. At the very least, the evidence reviewed should dispel from informed debate any notion that the shareholders of target firms are "hapless victims" of "ruthless corporate raiders". Ironically, the evidence to some implies the opposite. The gains to target shareholders appear to have accrued, to large extent, at the expense of the shareholders of acquiring firms. The evidence reviewed so far suggests that an examination of the returns to bidding firms is crucial in determining whether the hubris hypothesis or the value-increasing hypothesis better characterises takeover activity, on average.

2.3.2 Bidding Firm Shareholder Returns

While event studies to date have shown that target firms' shareholders gain significantly from takeovers, the results for bidder firms' shareholders are mixed. Depending on the studies one cites, the evidence is as supportive of the hubris hypothesis as it is of the value-increasing hypothesis.

Assuming that the bid is unanticipated and the bid conveys no other information about the bidder, the hubris hypothesis predicts that bidder firms will experience (i) a price decline on announcement of a bid, (ii) a price increase on abandonment of a bid and (iii) a price decline on winning a bid [Roll (1986, p. 201)]. The predictions of the value-increasing hypothesis are the reverse of the hubris hypothesis.

2.3.2.1 Pre-Bid Period Returns to Bidding Firms

One result consistent across most studies is that regardless of the outcome of the offer, bidding firms tend to experience abnormally good performance in the months prior to the takeover announcement. For instance, BDO find that over the period three years through to one year prior to the bid announcement, bidding firms experience, on average, a 12.6% CAR and another 12.1% over the period 11 months prior through to the month of the bid. In their sample of bidding firms involved in over 1400 takeover offers, there were about 60% more bidders with positive CARs than with negative CARs over these periods. The bottom 25% of bidders had an average CAR total of -23% over the two periods while the top 25% earned an average CAR total of 79%.
BDO's figures in the year leading to the announcement are similar to those obtained by Dodd (1976). Using a smaller sample of 136 successful Australian takeovers made between 1960-1970, Dodd finds that the CAR to the acquiring firms fell 5.1% in the period [-25, -14] months prior to the takeover offer but rose 9.4% during the period [-13, 0] months. Asquith (1983) differs from the two Australian studies in finding that in the year and a quarter leading up to merger, successful bidders earn significantly greater positive abnormal returns than unsuccessful bidders. His U.S. based sample comprised 196 successful bidding firms and 89 unsuccessful bidding firms which earned CARs of 14.3% and 2.2% respectively. The bids in Asquith's sample were made between 1962 and 1975.

Roll (1986) interprets Asquith's results as being consistent with the hubris hypothesis: "[o]ne would expect a higher level of hubris and thus more aggressive pursuit of a target in firms that had experienced recent good times" (p. 206). On the other hand, Dodd (1976) notes that the finding of positive abnormal returns in the pre-bid period is consistent with the hypothesis that "[differentially efficient] firms experience abnormally good performance and have surplus funds to invest, and look to takeovers as a profitable avenue of investing those funds" (p. 20).

2.3.2.2 Announcement Period Returns

Given that the pre-bid returns to bidding firms reflect prior good performance unrelated to a takeover, the returns to bidders around the bid announcement date provide a potentially stronger test of the hubris hypothesis. Unfortunately, the results to date are so mixed as to be inconclusive.

Results consistent with the value-increasing hypothesis are found in Dodd and Ruback (1977), Kummer and Hoffmeister (1978), Bradley (1980), Bradley, Desai, and Kim (1983), Asquith, Bruner and Mullins (1983) and Jarrell and Poulsen (1987) who all investigate the abnormal returns to bidders in successful takeovers using U.S. data. The first four studies report significant positive returns that range from 2.4% to 6.7%, with a weighted average return of 3.8% for the (approximate) monthly period around the takeover offer date [Jensen and Ruback (1983)].

Asquith, Bruner and Mullins (1983) investigate the returns to 71 Fortune 1000 firms which had each made four or more bids over the period 1963 to 1979. They find the

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30 Although difficult to test, one reason why both successful and unsuccessful Australian bidders experience similar "high" CARs in the months prior to a bid's announcement might be the greater difficulty in making a successful takeover in Australia (at least in the periods relevant to BDO's and Dodd's samples). It could be that in Australia only companies doing extremely well feel confident they can mount a successful bid.
The average two day announcement day excess return for all bids was +0.9% with a t-statistic of +4.68. Significantly, when Asquith et al. divide the merger bids into two categories, bids where the equity value of the target firm is less than 10% of the bidding firm's value and bids where the target's equity is greater than 10%, a different pattern of returns emerges. In takeovers where the targets exceeded 10% of the bidder's size, the CAR for bidders over the period 20 days before the announcement through the announcement date is +4.1% with a t-statistic of 4.42. In the other category, the CAR for bidders over the same period is 1.7% with a t-statistic of 2. Asquith et al. observe that "incorporating relative size strengthens the evidence that mergers benefit bidding firms' stockholders" (p. 132).

The Jarrell and Poulsen (1987) study examines 663 takeovers covering 1962 to 1985. For the entire sample period, bidders realised, on average, small but statistically significant gains of about 1% to 2% in the immediate period around the takeover offer announcement date. Intriguingly, Jarrell and Poulsen note a decline over time in the gains to successful bidders in tender offers. Bidding firms' returns for the 1960s and 1970s are consistent with those reported in Jensen and Ruback's (1983) review. However, the 159 cases from the 1980s show statistically insignificant losses to bidders. Given that in constant dollar terms takeover activity reached its highest level in the mid-80s, the negative returns to bidders over the same period are a puzzle in terms of the value-increasing hypothesis but are consistent with the hubris hypothesis.

BDO's results for all bidding firms over the period three months before the announcement month through three months after it are consistent with the value-increasing hypothesis. Although, as expected, not all bids generated positive gains, over 60% of the sample experienced positive gains in this period, with the average being 6%. A breakdown of the returns to successful and unsuccessful bidders yielded findings which are potentially problematic for the value-increasing hypothesis. More discussion of these findings follows below.


Dodd's (1980) sample of 71 completed mergers over the period 1970-77 registered a significant average negative return of -1.09% to the bidders over the two day announcement period. Further, Dodd finds that the average CAR from 10 days before

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31 Jensen and Ruback (1983) note that a recheck of Dodd's data revealed no data or programming errors.
before the first announcement through 10 days after target shareholder approval of the merger is -7.22%. Similar results are reported by Walter (1984) who analyses the weekly returns to 271 successful Australian bidders. He finds that the offerers experienced an average CAR of 28.2% over the period [-100, 0] weeks relative to the offer announcement week. However, for the announcement period, [-1, +1] weeks, the average CAR declined by 1.3%. As Walter points out, "this adjustment is consistent with the hypothesis that identification or confirmation of a firm as an offerer is, on average, viewed as a disappointment" (p. 84).

A similar conclusion may be drawn from Casey, Dodd, and Dolan's (1987) study which investigates all the takeovers in BDO's (1986) study; takeovers which occurred between January 1981 and June 1986. Consistent with the U.S. studies for the same period, they observe a negative CAR of -2.42% for the two day announcement period and -5.99% for a 15 day window. As Casey et al. point out, this result differs significantly from the BDO findings, which document a mean CAR of 6.0% for all acquiring firms.

Given that bidder firms are generally several times larger than target firms, an appealing measure of the wealth effect of takeovers on bidders is the abnormal dollar return. Using this measure, Malatesta (1983) provides estimates of the cumulative abnormal dollar return for successful bidding firms over the years 1969-1974. He reports that for the period four months before until the end of the announcement month of the merger proposal, the acquiring firm's cumulative abnormal return is -$27.6 million, a loss which is statistically significant at the 5% level for a one-tailed test.32 Assuming that the period [-4, 0] captures the full impact of the merger, Malatesta concludes that, "the long run wealth effect of the event sequence culminating in merger is significantly negative for acquiring firms" (p.155).33 He interprets this as being consistent with the size-maximising hypothesis of takeovers.34

Dennis and McConnell (1985) also measure the abnormal dollar gains to acquiring firms around a 40-day interval surrounding merger announcement dates over the period 1962-1980. Contrary to Malatesta (1983), they report an average market

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32 Malatesta emphasises that this five month interval is not selected arbitrarily, "measured cumulative gains to acquired firms [in the sample] are maximised over this interval" (p. 177, emphasis in the original).

33 However, see also the discussion on Malatesta (1983) in the section on the Total Gains from Takeover.

34 Interestingly, Malatesta notes that when the abnormally poor pre-merger performance of acquired firms is taken into account (-8.5% over the period [-24,-4] months in his sample), "the long run sequence of events culminating in merger had a negative net impact on shareholder wealth" (p. 177). Although the mean CAR was not significant, he notes that the sample of only successful mergers was biased upwards.
adjusted change of $58.6 million which is marginally significant. Although this is consistent with the value-increasing hypothesis, it should be noted that the abnormal percentage return to acquiring firms' equity calculated by Dennis and McConnell changed sign depending on the period over which it is calculated. The announcement day return in their sample displays a marginally significant negative return of -0.34% but when calculated over longer periods the return is positive overall. Oddly, the greatest positive returns occur after the announcement day.

One possible reason why the announcement period returns to bidding firms are less impressive than the returns to target firms is that bidders with a reputation for making frequent takeover attempts have the anticipated effects of their prospective bids incorporated in their share-prices some time before the first public announcement of subsequent bids. Schipper and Thompson (1983) and Malatesta and Thompson (1985) were the first to analyse the returns to bidding firms from the time the firms signalled their intention to engage in an acquisition program. While the U.S. based results are consistent with a portion of the recorded gains being related to takeover activity, both studies find that their sample of bidders tended to release other value-relevant information simultaneously.

Casey, Dodd and Dolan (1987) refine Schipper and Thompson's research method by measuring the abnormal returns to seven Australian "corporate raiders" over three significant event dates: the announcement by the bidder of a substantial shareholding in the target, the announcement of the takeover bid, and the disclosure of the takeover outcome. Their results support the effects anticipation hypothesis in that the largest significant abnormal returns are achieved by the "raid ers" over the period, [-1, 0] days, around the substantial shareholder date. Insignificant abnormal returns are registered over the two day period ending with the bid announcement date but significant abnormal returns are achieved over the two day period ending on the date when the outcome of the bid is known and the uncertainty is resolved.

While Casey et al.'s results are consistent with the value-increasing hypothesis, it is possible that their findings are specific to "raid ers" who have a comparative advantage in selecting takeover targets. Telling evidence in favour of the hubris hypothesis is found in Mikkelson and Ruback (1985) who measure the effects on stock prices of corporate investments in 5% or more of another company's equity. Their evidence uncovers differences in returns between companies, dependent on whether the ostensible purpose of the investment is to effect a takeover. For the 370 acquiring firms whose stated purpose was not a takeover the two day announcement

35 The lodgement of the substantial shareholding notice is often the first indication to the market that a takeover might be imminent.
effect for acquiring firms was positive and statistically significant. In contrast, for 134 acquiring firms indicating an intention to effect a takeover, the announcement effect was negative and significant. Mikkelsen and Ruback find that the most favourable outcome for the bidding firm occurs when it sells the acquired shares in the market or to a third party, or when a targeted repurchase takes place. When the outcome is a completed takeover, the total abnormal return is zero for acquiring firms and is large and positive for target firms.

As discussed earlier, some researchers have posited that at least part of the reason for the secular decline in takeover related abnormal returns experienced by U.S. bidding firms is the passage of legislation which has had the effect of strengthening the bargaining position of targets and increasing competition among bidders [Jarrell and Bradley (1980), Bradley, Desai and Kim (1988), Nathan and O'Keefe (1989), Kim (1989)]. This explanation would be more credible were the pattern of returns to bidding firms not repeated in non-U.S. share markets. Limmack (1991) and Franks and Harris (1989) investigate the returns to shareholders in U.K. companies involved in takeover bids. In line with the U.S. results, they find that the target firm shareholders appear to capture most of the benefits from a takeover while the returns to bidding firms over the immediate period surrounding the bid announcement date suggest that takeovers are, on average, a zero NPV investment for the bidding firms.

2.3.2.3 Bidding Firms' Returns in the Post-Merger Period

The characteristic that takeovers are, on average, zero NPV investments for bidding firms is one of the implications of a perfectly competitive market. The finding is consistent with the value-increasing hypothesis. However, an analysis of the post-merger returns to acquiring firms poses a considerable challenge to this explanation.

Jensen and Ruback's (1983) comprehensive review of the event study evidence of takeovers drew attention to a puzzle in the results to date. While acquiring firms, in the various samples studied, registered strongly positive returns in the months leading to the bid and insignificant returns (on average) in the immediate period centred on the bid announcement date,\(^\text{36}\) there was a consistent, anomalous, secular decline in the abnormal returns to acquiring firms in the post-merger period. Six of the thirteen studies reviewed by Jensen and Ruback looked at the post-merger returns to acquiring firms over a period extending at least 12 months after the takeover bid. Five of the six studies indicated a decline in the acquiring firms' post-merger returns. The average decline across the six studies ranged from an insignificant \(-1.32\%\) to a significant \(-7.20\%.\) Only Mandelker's (1974) study

\(^{36}\) This pattern of results led Jensen and Ruback to conclude that "mergers are zero net present value investments for bidders" (p. 17).
displayed a (insignificant) positive return, of +0.60%.

Jensen and Ruback (1983) attribute the anomalous secular decline in the post-merger returns to acquiring firms as being due, possibly, to ex post selection bias, or non-stationary parameters or other forms of model mis-specification. However, subsequent studies in the U.S. and U.K. show that the post-merger secular decline is a robust finding.

In the U.K., Limmack (1991) and Franks and Harris (1989) note that the observed post-merger share price performance of their samples of acquiring firms is inconsistent with efficient capital market behaviour. Both studies find that the market model CAR to their portfolios of successful bidder firms reach -14.08% and -13% respectively over the 24 months after the outcome of the takeover is known. Franks and Harris observe that "the negative drift in bidder share prices is consistent with U.S. findings in earlier studies" (p. 244). We may note that the Franks and Harris sample covered the period 1955 to 1985 and so the hypothesis that regulatory action caused the secular decline in bidder returns is less plausible.

Franks, Harris and Titman (1991) and Agrawal, Jaffe and Mandelker (1992) contain extensive analyses of the post-merger anomaly using U.S. data. Franks et al. investigate model mis-specification as a possible cause of the anomaly. They point out that "correctly adjusting for risk requires a benchmark that is mean-variance efficient ... past event studies of post merger performance have generally used single-portfolio benchmarks that are now known to be inefficient" (p. 82). Using an eight-portfolio benchmark comprising four portfolios based on firm size, three based on dividend yield and one based on past returns, Franks et al. find that their sample of 399 merged U.S. firms exhibit no statistically significant abnormal performance in the post-merger period. Pertinently, when an equally weighted market index is used to adjust the returns to the same sample of firms, significant negative post-merger performance is recorded. Interestingly, Franks et al. note also that the eight-portfolio returns adjustment does not alter earlier studies' findings about bidder returns around the announcement period. "Acquirers appear to have no gains around the announcement date, but there is no evidence of significant losses" (p. 85).

Agrawal, Jaffe and Mandelker's (1992) investigation of the post-merger anomaly covers 937 mergers and 227 tender offers, which comprise virtually the entire population of acquisitions by NYSE firms over the period 1955 to 1987. Their study is of particular interest to this thesis since, along with Franks et al., they also control for firm size and beta estimation problems when estimating abnormal returns. Intriguingly, after incorporating the controls, Agrawal et al. find that the cumulative
average abnormal return (or, to use their term, CAAR) to the firms in their sample is "significantly negative for holding periods of two, three, four, and five years. For the five year period the CAAR is -10.26% (t = -2.37). These results are not driven merely by a few outliers. The percentage of positive abnormal returns over the five-year period is 43.97, which is significantly lower than 50 (z = -3.03). The median abnormal return over the five-year time period is -7.50%" (pp. 1610-1611).

Agrawal et al. also investigate whether the post-merger anomaly is limited to acquisitions over certain time periods. They subdivide their sample into five sub-periods: "(1) the fifties, (2) the sixties, (3) the seventies, (4) the eighties, and (5) the 1975-1984 sample period of Franks, Harris and Titman (1991)" (p. 1612). The analysis of subperiod returns is of interest because one may conjecture that the post-merger anomaly is attributable to market inefficiency in valuing corporate takeovers. Assuming that the market becomes more efficient over time, this explanation implies that the secular decline in acquiring firms' post-merger returns would disappear or at least decrease in later periods.

Contrary to the above prediction, Agrawal et al. find that in the fifties, sixties and eighties the CAAR to the firms in their sample decreased significantly by 15 to 23% over the five years after the merger. In the seventies the CAARs to their sample firms over the five-year post-merger period was insignificant. The five-year post-merger CAAR for firms involved in mergers which took place between 1975-1984 was also insignificant. This last result is consistent with that obtained by Franks, Harris and Titman (1991) for the same period. However, Agrawal et al. note that the overall result for this period is driven by the significantly positive return to acquiring firms for mergers which took place between 1975 and 1979. This is the only five-year period when acquiring firms' post-merger performance is significantly positive. Agrawal et al. conclude that Franks, Harris and Titman's results are specific to their time period.

Agrawal et al. point out that the persistence of the post-merger secular decline in CARs to acquiring firms has three implications. First, it casts doubt on the efficient capital markets paradigm. Second, it implies that much event study research which has focused on returns around relatively narrow event-windows has yielded downward biased estimates of the effects of takeover. Third, the secular decline in

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37 Agrawal et al. report that their results are robust to "(a) changes in the time period used for estimating β ..., (b) changes in the time period used for estimating the residual standard error for computing t-statistics, and (c) adjustment for firm size based on equity value of the acquirer at the end of the year of the completion of the acquisition rather than at the end of the previous year" (p. 1611).

38 Agrawal et al. observe, in footnote 17 (p. 1614), that the closeness between their results and those of Franks et al. is not surprising since both their research methods are similar; both studies adjust for size, though Franks et al. do not adjust for beta.
post-merger results supports those accounting based studies which show poor accounting performance after merger.

Despite the robustness of the post-merger anomaly across different studies which have adopted a variety of plausible control procedures, a couple of considerations caution against a ready acceptance of the anomaly as reflecting economic reality. Ruback (1988) suggests that while we have to accept the anomaly as a fact, we do not have to accept the explanation given. Ruback does "not believe there is an explanation for this phenomenon that is consistent with market price" (p. 262). It is unlikely that persistent opportunities to earn abnormal returns in the vicinity of 17% over two years exist.

Section 2.4 includes a discussion of one as yet untested factor - survivorship bias - that might account for the post-merger anomaly. In the interim, we may note Agrawal et al.'s finding that there was a negative relationship between announcement period returns and the post-merger returns. The negative relationship is incompatible with markets adjusting slowly to news about the takeovers since we would expect the direction of the adjustment to be consistent. Takeovers probably occur too frequently for us to accept that capital markets systematically over-react in one direction and then slowly revise that reaction.

2.3.2.4 Summary: Returns to Successful Bidders

In summary, the inconsistent returns to successful bidding shareholders across all studies do not favour conclusively either the hubris hypothesis or the notion that takeovers are, on average, a value-increasing investment for bidding firms. Positive abnormal returns to bidders in the prebid period are consistent with the hypothesis that bidding firms are typically above average performers. Analysis of bidders' abnormal returns around the announcement period are ambiguous in that some studies report positive abnormal returns while others report negative abnormal returns. The post-merger returns to successful bidders are more supportive of the view that capital markets are, at least in the case of takeovers, inefficient. However, the persistent nature of the negative abnormal returns and their magnitude suggest that the anomalous post-merger returns might be a function of flaws in research method. The potential flaws are discussed in Section 2.4. In the interim, a review of the returns to unsuccessful bidders offers further insights into the gains from takeovers.

2.3.2.5 Returns to Unsuccessful Bidders

Given the possibility that other positive information is released at the time of a takeover announcement (e.g., the bidding firm has "free cash flow" or financial
flexibility, or is undervalued in the case of cash offers), examination of the returns accruing to unsuccessful bidders provides a potentially more stringent test of the wealth effects of takeovers. Unfortunately, the results to date are ambiguous.


Dodd and Ruback (1977) report an insignificant announcement month return of 0.6% to 48 unsuccessful bidding firms in their sample. Normal returns continued to be earned for 60 months after the offer's announcement. Similarly, Ruback (1983) examines 48 unsuccessful bidder returns in the period 1962-82 and finds that the offers were zero net present value investments.

On the whole, the above results are not strongly supportive of either the hubris hypothesis, or the value-increasing hypothesis, since a zero net return on failure of a bid is consistent with both. However, Bradley, Desai and Kim (1983) find evidence in support of a synergy effect when the results for the bidders are further analysed.

Over a 41 day period centred on the bid announcement date, Bradley et al. find that the net effect on the bidders of making an unsuccessful bid is essentially zero. However, they note that the CAR continues to drift downwards so that over event days -20 to +140 the average CAR for the 94 unsuccessful bidders in their sample is -5.8%. The reason becomes apparent when the returns of failed bidders whose targets are eventually taken over by another firm are analysed separately from the returns to failed bidders whose targets remained controlled by the incumbent management.

Of the 94 unsuccessful acquisition attempts investigated by Bradley et al., 67 of them involved a rival bidding firm successfully acquiring control of the target firm. The 67 unsuccessful bidders experienced a significant CAR decline from day -20 through day 160 of -8.43%. For the remaining 27 unsuccessful acquisition attempts, the incumbent management team remained in power. The unsuccessful bidders of this subsample of targets experienced an insignificant CAR rise of 1.23% over the same 9 month period. Bradley et al. interpret these results to mean that in cases

39 This assumes that (a) the market does not associate failed bids with incompetent management, (b) takeovers have a significant impact on share returns to bidding firms and (c) the costs associated with the failed takeover bid are offset by some unidentified benefits.

40 A zero net change on failure of a bid does however suggest that the market does not view a failed bid as a reflection of incompetence.

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where a rival firm contests a takeover, the target firm might have unique resources which give the successful bidding firm an edge over the competition.

Limmack's (1991) U.K. based study of, inter alia, unsuccessful bidders supports the value increasing hypothesis of takeovers. A distinctive feature of Limmack's study is the estimation of share returns around two event dates: the bid announcement date and the bid outcome date. Share price reactions around the bid outcome date are of interest because prior to this date any share returns would be weighted by the probability of bid success. As Limmack notes, if the value-increasing hypothesis is true, "following bid outcome, returns to successful bidders would be expected to rise further, while returns to unsuccessful bidders would fall" (p. 248). The reverse would hold if, on average, management hubris dominates the decision to bid.

Limmack finds that both successful and unsuccessful bidders experience positive abnormal performance in the months leading to the bid announcement date. This, as stated earlier, is consistent with both the hubris and value-increasing hypotheses. However, there is a statistically significant difference in the abnormal returns to successful and unsuccessful bidders over the period from the bid announcement date to the outcome date. Successful acquirers achieve, on average, zero abnormal returns while unsuccessful bidders suffer a significantly negative abnormal wealth decrease of -3.99% over the same period. Limmack's findings are consistent with the share market (i) anticipating bid outcomes and (ii) equating failed bids with the loss of positive NPV investments. Interestingly, Limmack finds, using the market model as a proxy for expected returns, that successful and unsuccessful (i.e., failed) bidders experience abnormal losses of -8.46% and -20.23% respectively over the 24 months after the bid outcome date. The successful bidders' returns are statistically insignificant at the 10% level while the failed bidders returns are significant at the 5% level. As Limmack observes, "it appears that while the market may be ambivalent in its attitude to successful bidders, it has no sympathy with 'losers'" (p. 249). Limmack's results are remarkably similar to those obtained by Bradley, Desai and Kim (1983) who find that unsuccessful U.S. bidders in multiple bidder contests during the period 1963 to 1980 on average lost 8% of their pre-offer value. In contrast, the average gain to successful bidders over the same period was insignificantly different from zero.

The returns reported above were measured after using the market model as the estimate of expected returns. As indicated earlier in the discussion of Frank, Harris and Titman (1991), the market model is not necessarily mean-variant efficient when used as a proxy for the expected returns to companies involved in takeover bids and therefore some doubt is cast on the validity of Limmack's (1991) and Bradley, Desai
Evidence inconsistent with the view that takeovers maximise net present value is found in Dodd (1980). Analysing 66 unsuccessful merger bids, Dodd reports that the offerer shareholders incurred a significant abnormal return loss of -1.24% over the two day announcement period. Further, for merger proposals that were unsuccessful because the target withdrew from negotiations, he finds abnormal returns of 0.9% for 19 bidders on the day before and the day of the merger termination. These returns would be negative if the merger were a value maximising investment for the bidder. On the other hand, as Horin (1986) notes, Dodd's finding that 47 bidding firms earned positive abnormal returns of 1.38% as a result of bidders' termination is consistent with the hypothesis that the bidders were maximising value upon realising that their bids were overpriced. Another possible explanation for Dodd's finding of a significant negative return to his unsuccessful bidders over the announcement period is based on an adverse selection argument. The management of bidding firms which offered their shares in consideration (or cash that was funded by an equity placement) might have inadvertently sent a credible signal to the share market that they believed the bidding firm's shares were over-valued. Firms which seek to maximise the returns to their existing shareholders from a profitable takeover prefer to finance their investment using debt rather than equity [Carleton, Guilkey, Harris, and Stewart (1983), Bellamy and Lewin (1992)].

The Australian evidence on unsuccessful bidders is inconsistent with the returns reported by the U.S. and U.K. studies. Walter's (1984) and BDO's results both show that abnormal returns to unsuccessful bidders continue well after the announcement month. Walter's sample of 97 unsuccessful acquisition attempts made in the period 1966-72 displayed a 3.2% abnormal return for the period covering one week prior to the announcement through to the announcement week. In the 100 week post announcement period, the CAR rose an additional 21.3%. Walter partially reconciles this result with the U.S. evidence by noting that 19.8% of the increase was due to the inclusion of one company, Industrial Equity Ltd, in the final sample. Hence only 1.5% of the post announcement rise was not associated with this company. However, BDO's much larger sample displayed similar results to Walter's. In BDO's sample, unsuccessful bidders experienced large positive abnormal returns well before the takeover offer and, in contrast to successful bidders, continued to earn abnormal returns after the announcement month. For the seven month period centred on the announcement month, successful bidders' CAR averaged 6% while the CAR of unsuccessful bidders averaged 10%. 

and Kim's (1983) results. We return to this point in the next chapter and now review the studies whose results run counter to the value-increasing hypothesis.
One reason offered by both Walter and BDO to explain their anomalous results (that is, anomalous with respect to the value-increasing hypothesis) is that the unsuccessful firms might have managed to obtain a large portion of the target's shares and sold them to the eventual acquirer at a substantial profit. To quote BDO, "in effect, these [unsuccessful] firms are successful 'greenmailers'" (p. 52). Although BDO's explanation might fit their sample, Bradley, Desai and Kim's (1983) sample of 67 unsuccessful bidding firms recorded a significant drop of 2.84% over a 21 day period, centred on the announcement of a successful bid by a rival bidder.

The above explanation implicitly supports the information hypothesis regarding takeovers. The failed bidder firm's economic contribution lies in locating an "undervalued" target, only to be outbid by another firm which can put the target's assets to more profitable use than could the failed bidder. Walter's and BDO's results are also consistent with a non-value-increasing strategy on the part of the bidding firm's managers. A failed takeover, which otherwise would have been a negative NPV investment, could be seen as "good news" by investors, although one might expect their enthusiasm to be tempered by the inference of management incompetence reflected in the failed bid.

2.3.2.6 Summary: Returns to Unsuccessful Bidders

In summary, the more comprehensive U.S. and U.K. based analyses of the returns to shareholders of unsuccessful bidding firms have yielded results which are more consistent with the value-increasing hypothesis than with the hubris hypothesis. The Australian based evidence, which indicates that bidding firm shareholders gained rather than lost from the experience, is more supportive of the hubris hypothesis. It is possible that further analysis of the type conducted by Bradley, Desai and Kim on their sample of failed bidders might result in a reconciliation of the Australian studies' results with the U.S. and U.K. based studies.

2.3.3 The Total Gains from Takeover

An examination of the total gains from takeover is interesting if only because the "central prediction of the hubris hypothesis is that the total combined takeover gain to target and bidding firm shareholders is non-positive" (Roll, 1986, p. 202).

A problem in assessing the total gain from a takeover is that acquirers are generally several times larger than their targets. One possibility is that while the target might register a significant price reaction, the wealth effect on the bidder might be so trivial that it would be hidden in the noise of daily return volatility (Roll, 1986). Assuming this problem can be overcome, comparing the percentage change in the value of the
respective firms involved in the average takeover is inappropriate because of their unequal size. For example, if the dollar gain generated from a merger is split equally, the relative sizes of the bidder and target firms usually will lead to a smaller percentage abnormal gain for the bidding company.41

In an attempt to overcome the latter problem, Malatesta (1983) measures the change in total dollar value associated with a matched sample of target and bidders from 30 successful mergers. He reports a significant average abnormal return of $16.2 million in combined equity value in the month before and month of the outcome's announcement.42 Pertinently, Malatesta observes that the significance of the return to combined equity over the period is driven by the significance of the gain to the acquired firms involved. Overall, he interprets his results as providing "weak evidence that the successful resolution of these mergers had a positive impact on combined shareholder wealth" (p. 170).

Dennis and McConnell (1985) obtain results more strongly supportive of the synergy hypothesis. For the 108 U.S. mergers in their sample, they find that the combined firms' average CAR over a 41 day period centred on the merger announcement date is $70.8 million with a t-statistic of 2.51. Further, this result is not driven by the return to acquired firms' shareholders. In their sample, the gains are more equally divided between acquired and acquiring firms' shareholders.

In Australia, BDO find that for their entire sample of bidders and targets, weighted by their relative market capitalisation, the average abnormal return over the seven month period centred on the bid announcement is 6.3%. Translated into dollars, the total value created by the takeover offers in their sample is $7.2 billion. BDO note that these gains are not driven by very large gains in a few transactions; when the 5% of offers with the largest gains and losses are excluded, the total wealth created is still $6.5 billion. These results are consistent with the value-increasing hypothesis.

In conclusion, the analyses done to date of the total gains from takeovers are consistent with the value-increasing hypothesis and, by extension, do not support the hubris hypothesis. This conclusion is tempered by the consideration that the measured returns in the above study are sensitive to the choice of proxy for expected return and none of the studies surveyed has established via a control sample that the preferred proxy for expected return is unbiased. The likely flaws are discussed in

41 Asquith, Bruner and Mullins (1983) (discussed earlier) provide evidence which suggests that the bidders' abnormal returns depend upon the relative size of the target.
42 This is consistent with Bradley, Desai and Kim (1988) (discussed earlier) who also find evidence in support of synergistic gains.
Chapter Three but we may note a curious asymmetry in the takeover debate.

Virtually all the evidence suggests that target shareholders gain from a takeover yet the most vociferous opponents of takeovers tend to be the managers of the targeted firms. While their opposition might be explained in terms of narrow self-interest, the lack of support for curbs on takeovers from the shareholders of potential acquirers remains a puzzle in the context of the hubris hypothesis. One possible explanation hinges on the costs of organising an effective lobby against takeover but this would not explain why clauses forbidding takeovers are not inserted into company memoranda and articles if, on average, takeovers are not value-increasing. The lack of such clauses or even the lack of any drive to insert such clauses implies that takeovers do create value. Roll (1986) suggests one reason we do not observe such clauses might be that the "deadweight takeover costs" are relatively small and a "well diversified shareholder would receive the aggregate gain which is close to zero" (p. 214). However, the cost of a takeover is large, not least in the amount of managerial time and effort required to mount a successful bid. Further, were the hubris hypothesis true, investors would pay a premium for shares in companies which have clauses preventing the management from attempting a takeover but which still retain the potential to be a target.

2.3.4 Concluding Comment: Shareholder Returns from Takeovers

On the whole, it seems fair to conclude that while a review of the extensive number of event studies on shareholder gains from takeovers is informative, the exercise leaves considerable room for debate on the motives and gains from takeovers. While the highly significant positive abnormal return to targets is consistent with value-increasing behaviour, the inconsistent return to bidders implies that the hubris hypothesis retains plausibility. Moreover, it is possible that any gain to bidding shareholders from a takeover accrues not so much from the relocation of assets to more valued uses but rather stems from a lack of competition in the market for corporate control, wealth transfers from other stakeholders in the firm, and financial arbitrage opportunities.43 The next three sections discuss the empirical evidence on these issues.

2.3.5 Level of Competition in the Market for Corporate Control

Ruback (1983) defines a competitive acquisition market as one where the potential

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43 An extensive literature exists on the effect of anti-takeover amendments on the equity of potential targets. However, since virtually all the studies' results are consistent with the hypothesis that targets' shareholders gain substantially from takeovers and since this is now a widely accepted belief, these studies are not included in this review.
gain to unsuccessful bidders at the successful offer price is non-positive. He tests this implication using data on tender offers in which there are multiple bidders. Although the main purpose of Ruback's study is to assess the level of competition in the market for corporate control, his study also provides a test of the existence of rational, value-increasing behaviour on the part of bidding firms.

Using a sample of 48 unsuccessful bidders (from the period 1962-1981), Ruback estimates that had the unsuccessful bidders matched the price offered by the successful bidder the average potential gain would have been -$91 million and the average t-statistic -4.34. He interprets this as being consistent with competition in the takeover market since the potential gains are significantly less than zero. The results are also consistent with rational, value-increasing behaviour on the part of the unsuccessful bidders' managements.

Horin (1986) replicates Ruback's study using a sample of 72 unsuccessful Australian offerers. He estimates that the average outcome from the failed bidder matching the successful bid would have been a loss of $1.6 million. The amount is not significantly different from zero at the 5% level but this was driven by three outlier cases where the positive gains from matching the bid were estimated as being greater than $10 million. On a case by case basis, over 80% of the takeovers examined were consistent with a competitive market for corporate control; a result which, Horin points out, is comparable to Ruback's study.

The results from both the studies reviewed above may be acutely sensitive to the type of model used to estimate the bidding firms' expected returns and to the authors' estimates of the probability of the failed bidders being successful in their takeover bid had they matched the winning bidders' offers. Nonetheless, there are other grounds for assuming that the market for corporate control is very competitive on the bidding side. The discussion earlier noted that since the late 1960s the thrust of most changes to the regulation of takeovers in the U.S. has been to increase the relative bargaining power of target firms. Kim (1989) points out that "the three key words in the Williams Amendment [of 1968] are disclosure, delay and withdrawal" (p. 10). The greater ability of target firms to fend off bids which are less than value maximising for their shareholders suggests that it is difficult for bidding firms to achieve most of their gains by locating undervalued targets or by paying much less than the expected value of the gains from takeover. The persistent rise in U.S.

\[44 \text{In brief, the potential gains are calculated as the net present value of the takeover if it was successful at the failed bidder's offer price less the cash outflow from the higher price offered by the successful rival. Obtaining an estimate of the NPV of the takeover at the initial offer price is a non-trivial problem and the procedure is discussed at length by Ruback (pp. 145-146).}\]

\[45 \text{The reasoning behind the Williams Amendment has been incorporated in the regulation of takeovers in both Australia and the U.K.}\]
takeover bid premiums over the period 1974 - 1985 [Nathan and O'Keefe (1989)] and the insignificant abnormal returns registered by bidding firms over the bid announcement period are consistent with a highly competitive market for corporate control. We may note that the above situation characterises not just the U.S. takeover market but, based on the available evidence, also the Australian and U.K. markets for corporate control.

2.3.6 Market Power Hypothesis and Synergy Hypothesis

Bradley, Desai and Kim (1983) (discussed earlier) find results consistent with the hypothesis that gains from merger are due to synergy effects rather than information relating to "under-valued" targets. However, their results are also consistent with the hypothesis that monopoly power is the source of the gains from merger.

Stillman (1983) and Eckbo (1983) test the market power hypothesis by examining the abnormal returns of the target firms' rival producers. Given that monopoly benefits arising from merger accrue to the remaining rival producers as well, Stillman and Eckbo both predict that a finding of positive abnormal returns to rival producers on announcement of a takeover bid is consistent with the market power hypothesis.

Using a sample of 11 horizontal merger bids (attempted between 1964 and 1972) that were challenged by U.S. antitrust enforcement agencies, Stillman examines the abnormal returns on portfolios of the shares of rivals to the firms involved in the challenged mergers. In only two of the 11 cases were the results consistent with the hypothesis that the mergers facilitated the creation of monopoly profits. The rivals in the other nine mergers experienced no abnormal returns of any kind regardless of the outcome of the challenge although the firms directly involved did experience abnormal returns. On balance, Stillman interprets his results as being consistent with the hypothesis that horizontal mergers challenged by U.S. antitrust agencies do not have an appreciable effect on product prices.

Both Stillman and Eckbo recognise that a positive abnormal return to rivals on announcement of a merger is a necessary but not sufficient condition to conclude that a merger is anti-competitive. Positive information effects related to the signalling of more efficient ways of producing goods or lowering costs can also have a positive impact on rivals' shares.46 This insight is critical to Eckbo's interpretation of his results.

46 Horin (1986) notes that positive returns to rivals might also be a function of the increased probability that they will be takeover targets.
Consistent with the market power hypothesis, Eckbo finds that the rivals of 65 horizontal challenged mergers included in his sample earn, on average, a statistically significant abnormal return of 2.45% over a 31 day period around the merger proposal's announcement. However, the subsequent news of the anti-trust complaint is associated, on average, with a further 1.78% abnormal increase in the value of rival firms and a reduction in the value of the merging firms. Eckbo observes that this result contradicts the market power hypothesis but is consistent with the hypothesis that merger proposals release news of possible efficiencies, which benefits rival firms as well. Finally, it is pertinent to note that Eckbo interpreted his sample as being biased towards the collusion hypothesis. On average, 85% of all mergers during the sample period involved bidder and target firms with smaller asset sizes than the smallest merger in Eckbo's sample.

In summary, given no direct evidence in support of the market power hypothesis but some indirect evidence to the contrary, it is unlikely that most takeovers are driven largely by monopoly considerations. As Roll (1988) points out, "many corporate combinations simply could not be motivated by monopoly because they do not involve firms in the same or closely related industries" (p. 244). He gives the increase in leveraged buy-outs as one example.

2.3.7 Wealth Transfer Hypotheses and Value-increasing Hypothesis

A prominent concern in the takeover debate is whether the gains recorded by the equity holders in the firms represent a wealth transfer from senior security-holders in the same firms or from the taxpayer, or other stakeholders in the firm such as labour.

2.3.7.1 Evidence on returns to senior security-holders

Two early papers by Kim and McConnell (1977) and Asquith and Kim (1982) examine monthly and daily returns (Asquith and Kim) to the non-convertible bondholders of acquiring and acquired firms involved in conglomerate mergers. Kim and McConnell examine the returns around the month of merger while Asquith and Kim centre their observation on the initial announcement date. In both cases the abnormal returns to the nonconvertible bondholders are found not to be significantly different from zero.

A more comprehensive study by Dennis and McConnell (1986) examines the returns to all classes of merging firms' senior securities in addition to the returns to their common stock. Based on their sample of 132 mergers over the period 1962 to 1980, Dennis and McConnell find that, on average, acquired companies' common
shareholders, convertible and non-convertible preferred shareholders and convertible bondholders receive statistically significant gains in mergers as do acquiring companies' convertible preferred shareholders. However, on average, acquired companies' non-convertible bondholders and acquiring companies' convertible bondholders, non-convertible preferred shareholders and non-convertible bondholders do not receive statistically significant abnormal returns.

In an effort to reduce the impact on their results of the substantial difference in the average size of bidder and target firms, Dennis and McConnell report the dollar value of the changes in firm wealth. The market adjusted changes in the total dollar value of the firms was $30.1 million for acquired firms and $58.6 million for acquiring firms. Both were statistically significant. Most of the increase was attributable to the increase in the value of the common stock but the average market adjusted change in the value of convertible preferred stock, convertible bonds or non-convertible stock was not "trivial" although it was statistically insignificant.

Dennis and McConnell's finding that merger increases the value of the merged firms' existing bonds is not unexpected given that in most cases the merged entity's variance per dollar of operating cash-flow is less than the cash-flow variances of the individual firms prior to merger. The critical assumption is that the existing bondholders' debt ranks ahead of any securities used to finance the takeover. This assumption is valid if the takeover is financed through the issue of shares or debt which explicitly ranks behind existing debt. In the case of leveraged buyouts, i.e., takeovers financed through the issue of debt using the acquired entity's assets as security, it is not obvious that there is not a wealth-transfer from the existing bondholders to the shareholders; that is, that the recorded gains from takeovers come at the expense of the existing bondholders. The loss suffered by the pre-buyout bondholders is a result of the dilution of the value of their claims on the acquired firm's assets when the new debt ranks either equally or ahead of their claims.

Marais, Schipper and Smith (1989), among others, point out that a decrease in the value of the pre-buyout bondholders is not inevitable. "Existing securities face a number of outcomes that do not necessarily affect the wealth of different classes of

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47 The results for these were reported earlier.
48 Pure cash financed takeovers can also reduce the value of existing securities if the amount of cash paid by the acquirer significantly depletes the merged entity's ability to meet its debt payments. However, note Marais, Schipper and Smith's (1989) argument in the next paragraph.
49 Leveraged buyouts are, in essence, a debt for equity swap.
50 Marais, Schipper and Smith (1989) find that in a sample of 113 U.S. firms involved in leveraged buyouts the median pre-buyout leverage ratio was 0.263 and no firm's ratio exceeded 0.90. After the buyout, the median leverage ratio increased to 0.845 and for 43 of the 113 firms the ratio exceeded 0.90. The median increase in leverage ratio was just under 0.50 and over one fifth of the sample increased leverage by at least 0.70. These figures indicate the extent to which the capital structure of a company can change with a leveraged buyout.
security holders in the same way. Securities may remain outstanding or they may be redeemed for cash, converted into other securities, or renegotiated. Second, a buyout could further protect fixed payments on securities that remain outstanding with no change in terms if, for example, total operating cash flows increase after the buyout because of the incentive effects [on management] of the concentration of equity claims and the buyout financing" (p. 156). The cash-flow available to service debt could also increase because of wealth-transfers from other factors of production such as labour. The net effect of the increase in leverage and the other factors cited on the value of the pre-buyout bondholders is an empirical issue.

Marais et al.'s results indicate that existing bondholders in the U.S. do not lose from leveraged buyouts. However, Asquith and Wizman (1990) observe that Marais et al. analyse the potential value decreasing effects of leveraged buyouts on a sample of only 30 bonds and do not consider the effects of covenant protection, that is, they do not categorise bondholder returns by covenant protection. Further, Marais et al. follow only 10 bonds until completion of successful buyouts.

Asquith and Wizman's results, derived from their analysis of the effects of successful leveraged buyouts on 149 bonds, contrast with those of Marais et al. They conclude, from their analysis, that, on average, existing bondholders suffer a significant abnormal loss of -2.8%. However, the result is not universal and covenant protection explains much of the difference in bondholder wealth effects. Strongly protected bonds achieve abnormal returns of 2.1% (standard error 1.7%) while weak and unprotected bonds average abnormal returns of -2.0% and -5.3% respectively (standard errors 2.0% and 1.2%).

There are three points pertinent to Asquith and Wizman results. The first is that their findings confirm much anecdotal evidence reported in the business press regarding leveraged buyouts. On average, leveraged buyouts result in a wealth transfer from bondholders to shareholders. The second point is that the positive abnormal return to the strongly protected bondholders is consistent with the hypothesis that the incentive effects of leveraged buyouts and potential wealth transfers from non-security holders increase the net operating cashflows to the acquired firm (more on this point below). The third and most pertinent point is that the estimated wealth

51 The period over which the abnormal returns are estimated is from the month-end two months before the announcement until the month-end two months after the leveraged buy-out is successfully completed.

52 Asquith and Wizman classify strongly protected bonds as "(i) all bonds with a net worth restriction on the surviving firm in a merger and (ii) all bonds that limit total funded debt" (p. 200). Weakly protected bonds are defined as bonds that have none of the strong protection covenants but have covenants "(i) limiting senior funded debt or (ii) restricting dividends or special payouts to shareholders from retained earnings" (p. 200). Bonds with no protection have none of the above covenants.
transfers from bondholders explain only a small fraction of the shareholder gains from leveraged buyouts. Asquith and Wizman find that the bondholders in their sample incur abnormal losses of $678 million whereas shareholders gain an estimated abnormal return of $21,522 million. "Even if all debt, public and private suffered similar losses, the total loss to debt holders would be less than 7% of the total gain to stockholders" (p. 212).

Notwithstanding that, on the above evidence, leveraged buyouts do not appear to be motivated primarily by wealth transfers from debt holders, Asquith and Wizman's results could be seen as lending support to proposals that the leveraged buyout market be regulated in some way to protect the interest of debt holders. An intriguing finding by Asquith and Wizman cautions against ready acceptance of this argument. Asquith and Wizman find that despite the substantial and widely publicised increase in the incidence of leveraged buyouts in the 1980s the percentage of bonds in their sample without covenants is substantially higher for bonds issued after 1980 than for those issued earlier. If, within an unregulated context, lenders to firms eschew what appear to be relatively "cheap" and effective means of protecting the value of their loans, regulatory intervention would appear to impose an unwarranted cost on the market for debt.

2.3.7.2 Evidence on the Tax Motive for Takeovers

Gilson, Scholes and Wolfson (1986), among others, examine the claim that tax benefits under the U.S. tax system favour acquisitions. They conclude, from their theoretical study, that transaction and information costs might make takeovers the most attractive means of securing tax benefits (at least prior to the Tax Reform Act of 1986). The potential gains stem from three sources: (i) gains from the transfer of tax losses and credits; (ii) gains from the step-up in basis of the target's assets and the higher tax shield that results; and (iii) the gains from interest tax shields that are associated with merger-induced leverage increases.

Empirical research confirms Gilson et al.'s proposition that tax considerations explain a significant proportion of the abnormal gains achieved by U.S. firms' shareholders from takeovers and mergers. Evidence from two studies, Auerbach and Reishus (1988) and Hayn (1989), is discussed below.

Auerbach and Reishus (1988) examine the tax implications of 318 mergers and takeovers that took place in the U.S. between 1968 and 1983. They estimate that potential tax benefits are present in nearly a fifth of the mergers, with the average

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53 A similar conclusion is drawn by Marais et al. from their analysis.
54 The Tax Reform Act of 1986 eliminated most of these potential sources.
gain in these cases being just over a tenth of the target firm's value. However, the average is driven by a few large gains in particular mergers. On a case by case basis, the magnitude of the gain exceeded 10% of the target firm's equity in only 6.5% of the total sample.\(^{55}\) In the majority of cases where benefits were present, they came from losses and credits of the acquired entities.

The potential gain from asset basis step-up could be estimated for 275 firms. Of these 275 firms, only seven were estimated to produce a gain from basis step-up in excess of 5% of the target firm's value. Changes in leverage because of takeover are hard to assess. However, based on a sample of 162 mergers for which data were available, Auerbach and Reishus find that long-term debt as a fraction of long-term assets increases from an average of 30% to 32.1%. Auerbach and Reishus note that, given the magnitude of year-to-year changes in aggregate debt-to-value ratios over this period, the increase of 2.1% is small.

Hayn (1989) extends Auerbach and Reishus' analysis by, inter alia, examining the relationship between various specific tax attributes of target firms and shareholder returns. Her results show that, consistent with a tax motivation for acquisitions, target firms possessing the specific tax attributes that are of value to the acquiring firms achieve higher premiums. Further, takeovers possessing the specific tax attributes but whose tax status remains subject to a ruling from the Internal Revenue Service register lower abnormal returns than firms whose tax attributes are virtually certain of being exploitable. We may note that it is not obvious from Hayn's results that tax considerations are the main motivation for most takeovers. Of the 640 target firms in her sample only about 20% had net operating losses that could be exploited by the bidding firms.

Australian based research on the tax motivation for takeovers is scant. This in large part is probably because the Australian tax regime has not and does not facilitate the trafficking of tax shields across companies and so the question whether tax considerations have played a large part in motivating Australian takeovers has not attracted rigorous interest. Given the assumption that, unlike the U.S. situation, tax considerations play an insignificant role in explanations of most Australian takeover bids, it is pertinent to note that takeover premiums in Australia are of the same order of magnitude as U.S. takeover premiums. One implication of the similarity in shareholders returns in the Australia and the U.S. is that tax related gains are unlikely to be the main factor in explaining takeover bids. We may also note that

\(^{55}\) By way of comparison, Lehn and Poulsen document an average takeover premium to target firms' shareholders of 21% in 93 leveraged buyouts taking place over the period 1980-84. DeAngelo, DeAngelo and Rice (1984) find an average premium to target firm shareholders of 27% for leveraged buyouts between 1973 and 1980.
takeovers are a cyclical phenomenon and changes in the incidence of corporate
takeovers do not appear related to changes in tax laws (although it could be argued
that changes in economic climate affect the attractiveness of the tax related gains
from acquisition).

2.3.7.3 Wealth transfers from Other Stakeholders, e.g., Labour.

Shleifer and Vishny (1988), among others, observe that shareholder gains from
takeovers, in particular hostile takeovers, often seem to derive from wealth transfers
from non-management constituencies such as employees, suppliers and customers.
Takeovers offer an opportunity for the explicit and implicit contracts between the
stakeholders in the firm to be renegotiated but with management and (by extension)
shareholders having greater bargaining power than the other stakeholders.

Shleifer and Vishny identify two related issues that ought to be considered when
evaluating the consequences of wealth transfers caused by leveraged buyouts. The
first issue is that shareholder gains due to wealth transfers from other stakeholders in
the firm cannot be viewed as a net social gain unless one considers shareholders a
more deserving class of stakeholders. The presence of significant wealth transfers
implies that "it becomes impossible to gauge the social consequences of acquisitions
from shareholder returns alone" (p. 16). The second issue is that if all that takeovers
do is reassert the legitimate property rights of shareholders and management to the
cash flows of the firm then the ex post wealth transfers represent the righting of a
wrong. The problem with the second argument, Shleifer and Vishny note, is that
"shareholders do not buy an asset that entitles them to the maximum feasible profit.
Rather, they buy an asset whose price reflects the weaknesses of internal and
external control mechanisms and the consequent non-value-maximising behaviour of
managers ... Takeovers that radically change the environment from what
shareholders, managers and employees expect may be objectionable since they can
harm parties acting on the basis of an implicit contract" (p. 16).

Notwithstanding the above, Healy, Palepu and Ruback (1990) examine employee
growth rates and average pension expense per employee for firms involved in the
largest 50 takeovers in the U.S. during the period 1979 to 1983. They find that, on
average, the median number of employees in the merged firms declined in each of
the 5 years post-merger. Overall, industry adjusted growth rate is significantly less
than in the pre-merger period and average pension expense per employee declines to
the industry average after the merger. However, Healy, Palepu and Ruback conclude
from their analysis (parts of which were reported earlier), that the labor cost
reductions were not sufficient to account for the significant increase in post-merger
operating margins.
Healy, Palepu and Ruback’s results are consistent with those found in Brown and Medoff’s (1988) study on the impact of firm acquisitions on labour. Brown and Medoff’s study has been criticised on methodological grounds [Carliner (1988)]. However, their comment, that the popular press’ fascination on how labour fares in takeovers is not matched by extensive systematic investigations on the issue, remains valid today. The existing evidence does not allow one reasonably to be anything other than agnostic on the issue.

2.3.7.4 Summary and Conclusions: Wealth Transfer Hypothesis

In summary, the Ruback and Horin studies both support the proposition that the market for corporate control is competitive and, more importantly, that the actions of bidding firms are consistent with rational, value-increasing behaviour. The available evidence on the sources of gains from takeover is inconsistent with the monopoly, hubris, wealth transfer and information hypotheses but not with the synergy or corporate control hypotheses. The hypothesis that takeovers, on average, are value creating and that synergy is the source of the gain is the most consistent with the results to date. However, the fit is not perfect. Problems in implementing research designs with significant external validity plague most of the studies to date.

The most disturbing evidence against synergy being the source of the gain from a takeover is that the shareholders of acquired firms appear to gain most, if not all, of the economic benefits. Roll (1988) points out that while this would be consistent with a scenario where synergy benefits are available to several competing bidders but only in combination with a particular target, the opposite situation is equally likely: a bidder might have synergy whilst combining with several possible targets. In this case, we might expect competition among the targets to ensure that the bidder obtains most of the gains. Across many takeovers, we would expect an equal division of the gains between bidders and targets. This problem, along with the inconsistent returns recorded by bidders across different studies, samples, and time-periods is indicative of the measurement problems that exist with respect to the returns accruing to bidder firms. These issues are addressed in the next section.

2.4 EXPLANATIONS FOR THE INCONSISTENT RETURNS TO ACQUIRING FIRMS

Among the reasons examined as possible explanations are: (a) lack of control for method of payment, (b) polluted information effect, (c) differences in research

56 Roll (1988, p. 242) points out the same factors differ for target firms but nevertheless, the results agree.
methods, (d) the "size effect" anomaly and (e) survivorship bias in event studies.

2.4.1 Method of Payment Explanation

Bellamy and Lewin (1992), following Carleton, Guilkey, Harris and Stewart (1983), Travlos (1987) and Franks, Harris and Mayer (1988), suggest that failure to control for method of payment might explain the mixed results across the studies which have investigated the returns to bidding firms. Drawing from Myers and Majluf (1984), they argue that bidding firms which make cash offers send a credible signal to the market that the bidding firm is undervalued or that the prospective takeover is a positive NPV investment the benefits of which the managers prefer to reserve for the existing shareholders.57 Share exchange offers are consistent with the proposition that the bidding firm's managers believes its shares are overvalued and that they wish to help the existing shareholders at the expense of the potential new ones. The market is aware of its relative ignorance and bases its assessment of the bidding firm's managers' beliefs on the method of payment. That is, cash offers are interpreted as signalling "good news" about the bidding firm while share exchange offers are interpreted less favourably.

Bellamy and Lewin test their hypothesis on a sample of 393 Australian takeover offers which were made between January 1980 and July 1988. Their results support the hypothesis that method of payment affects the returns registered by bidding firms on announcement of a takeover bid and could explain the mixed results obtained in earlier studies. Two hundred and ten out of the 393 bidding firms in their sample had daily returns available. An event study using the unpartitioned sample of 210 firms revealed that over the period [-10,+10] days centred on the bid announcement date, none of the daily abnormal returns was significant. Upon partitioning, "it was found that the shareholders of bidding firms using cash offers earned a significant positive abnormal return of 1.3% on the day after the takeover announcement. For the full 21-day period around the announcement the CAR was 0.84%" (p. 143). In contrast, shareholders of firms making share exchange offers earned a significantly negative abnormal return of -2.97% on the day the bid was announced. However, the CAR over the period [-10,+10] days was an insignificantly positive 0.45%. These results are similar to those obtained by Travlos (1987) and Franks, Harris and Mayer (1988) in their U.S. based studies.

57 There is potential for misinterpretation in Bellamy and Lewin's exposition of the Myer and Majluf argument. Bellamy and Lewin state "that managers acting in the best interests of the current shareholders of a firm will raise the funds for a new investment by debt rather than equity when they believe that information asymmetry about the new investment would mean that the firm is currently undervalued" (pp. 140-1, emphasis added). Myers and Majluf (1984) posit that the information asymmetry is about the value of the bidding firm, not about the new investment. This underpins their explanation why firms might pass up positive NPV investments under certain circumstances.
Notwithstanding that the results Bellamy and Lewin discuss support their contention that method of payment explains, at least in part, the returns to bidding firms, it is not obvious that method of payment accounts for the more puzzling features in the pattern of returns to bidding firms found in earlier studies. In particular we may note that the method of payment hypothesis is supported best by the returns over the trading period immediately around the bid announcement date. Over the period [-10,+10] days the magnitude of CARs to both sets of bidders is much the same (0.84% to cash bidders and 0.45% to share exchange bidders) and positive (significance levels are not reported). More pertinently, the method of payment hypothesis does not explain the secular decline in the post-bid returns to bidding firms.

Finally, we may note that cash offers are not invariably associated with bidding firm undervaluation or positive NPV investments. Firms characterised by excess free cash flow and relative lack of shareholder control might make cash offer based takeover bids in order to further the interests of their managers. Jensen (1986) discusses the circumstances when this motive is the most probable cause of the takeover bid. The point is that controlling for method of payment does not cover all the confounding factors which are likely to have contributed towards the mixed results reported in earlier studies.

2.4.2 The Polluted Information Effect

Roll (1988), among others, argues that takeover bids are a "polluted" information item. They potentially convey information about a firm that is not necessarily related to the takeover event at all. A bid can signal either "good" news, e.g., that the bidder is flush with more funds than previously estimated or it can signal "bad" news, e.g., that managers "are going to use the company's cash in pursuit of an expensive and elusive target for which they may overpay" (p. 243).

As an explanation for the inconsistent returns recorded to bidders, the polluted information hypothesis is inadequate because across many firms one would expect the extraneous information items to cancel out, leaving only the average effect associated with the actual takeover. Further, the polluted information hypothesis does not explain the secular decline in abnormal returns to acquiring firms in the

58 Bellamy and Lewin note that a significantly negative return of -32.2% over the period from the announcement to six months after was earned by firms involved in share exchange offers. Puzzlingly, they do not report the return over the same period for the sample of bidding firms which made cash offers.

59 Ruback (1988), in his comment on Franks, Harris and Mayer's (1988) extensive empirical contribution vis-à-vis the method of payments explanation, notes that "the measured effects of takeovers may include factors that are caused purely by the financial restructurings involved... Unfortunately we have no accepted theory about the choice of takeover method" (p. 261).
2.4.3 Model Mis-specification and Differences in Research Methods

Inconsistent returns to bidders across different studies are compatible with (i) market inefficiency, (ii) differences in the market models used to estimate abnormal returns, and (iii) changes in unknown aspects of firms' variables around the time of merger which causes mis-specification of the models generating returns.

(i) Market inefficiency is an unlikely explanation for the anomalous persistence in post-bid returns recorded by bidders in takeovers. As Malatesta (1983) points out, given that takeovers are well known, it is "unlikely that investors systematically misinterpret the implications of these common phenomena" (p. 181). The market inefficiency explanation is discussed further in Chapter Three.

(ii) In comparing their results with other studies, both Malatesta (1983) and Walter (1984) argue that the estimated CAR is sensitive to model choice. For example, Walter's sensitivity test showed differences of up to 9.9% in estimated CAR depending on the model and time period used. Malatesta observes that the inconsistency between his (1983) results and Asquith's (1979) results disappears when Malatesta's data are re-analysed using Asquith's market model. (Malatesta found that acquiring firms' performance is insignificantly different from zero over the period 24 to 4 months prior to bid announcement. Over the same period, Asquith finds significant positive abnormal performance of approximately 9%.)

While it is true that estimated CAR is sensitive to model choice, Agrawal, Jaffe and Mandelker's (1992) results, among others, demonstrate that the anomalous returns to bidding firms are robust with respect to this issue. We may also note that the returns to target firms are subject to the same differences in estimation of abnormal returns, yet the results for target firms are consistent across virtually all studies.

(iii) Jensen and Ruback (1983), in discussing the anomalous post-merger decline in abnormal returns to acquiring firms, posit that the widespread finding might be a function of sample selection bias (e.g., the sample firms being inadvertently selected using ex post information) or of errors in estimates of the values of the parameters used in the asset pricing models (e.g., beta). Subsequent research by, inter alia, Agrawal, Jaffe and Mandelker (1992) (reviewed earlier) has eliminated the possibility that either sample selection bias or errors in the estimate of beta accounts for the anomaly. Agrawal, Jaffe and Mandelker (1992) find the secular decline in post-merger returns to acquiring firms is robust to plausible changes in the value of
beta as a result of takeover or related events.

2.4.4 The Size Anomaly

The credibility of event studies depends on the truth of the joint hypothesis that share markets are efficient and that adopted testing regimes are adequate. While most evidence to date is consistent with this joint hypothesis, a significant anomaly exists; after controlling for apparently all relevant factors, small firms persistently provide exceptional returns [Beedles, Dodd and Officer (1988)].

The size anomaly offers one possible explanation why bidder firms do not appear to register large gains from merger. Targets are generally several times smaller than bidders and so their exceptional returns relative to bidders might simply reflect the size anomaly. Further, the post-merger negative abnormal return drift reported in Dodd (1976), Dodd and Ruback (1977), Asquith (1983) and Malatesta might reflect the larger size of the merged firm relative to the smaller two entities which existed pre-merger.

Franks, Harris and Titman (1991) and Agrawal, Jaffe and Mandelker (1992) (reviewed earlier) control for firm size in their investigations of the post-merger anomaly. Agrawal et al. find that the anomaly is robust to controls for firm size. They conclude that the Franks, Harris, and Titman finding that the post-merger anomaly disappears after a mean-variance efficient control portfolio is implemented appears time period specific.

Agrawal et al.'s finding is not inconsistent with the proposition that the size-effect, at least in part, accounts for the post-merger decline in abnormal returns to acquiring firms. Accordingly, this thesis controls for, inter alia, firm size in estimating shareholder returns from takeovers. The size anomaly and the control procedures adopted are further discussed in Chapter Three.

2.4.5 Survivorship Bias

Persistent trends in adjusted share returns are a frequent finding in event study

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60 By construction, the sum of all abnormal returns across the market portfolio equals zero. Hence the exceptional returns to small firms comes at the expense of the large firms, as it were.

61 One further implication is that the negative abnormal performance registered by target firms prior to takeover is probably upward biased given the "small firm" effect.

62 The extent of this drift varies greatly across the studies. Dodd (1976) reports that successful offerers experienced consistent negative abnormal returns over the 24 month period after the announcement of the offer, with the CAR falling from 4.3% to -10.9%, a drop of 15.2%.

63 Although the anomaly is correlated with size, there is a consensus among financial economists that it is not size per se that accounts for the differential returns (Beedles, Dodd and Officer (1988, p. 12); See also Chapter Three.
research. Brown, Goetzmann, Ibbotson and Ross (1992) suggest that survivorship bias might explain the anomalous phenomenon. Survivorship bias occurs when the returns of samples of firms selected ex post are adjusted by the returns to portfolios comprising firms selected ex ante. If firms' survival depends on factors priced by the market we would, ceteris paribus, expect to observe systematic differences in the returns to the sample firms and the control portfolios. Given the selection procedures commonly used in event studies, it is plausible that survivorship bias does account, at least in part, for the anomalous returns to acquiring firms in the post-merger period. No event study of takeovers has yet controlled for survivorship bias. Further discussion on the issue is deferred till Chapters Three and Four.

2.5 CONCLUSION

The most striking aspect of both the accounts based and market based studies is not how much difference in results there is between the groups, but the large differences in results within the groups employing similar methods. Both types of study find results consistent with both sides of the takeover debate. The larger body of market based research probably reflects the relative tractability of this method. However, the severe measurement problems identified above and the inconclusive nature of the results suggest that the tractability of event study research might be more apparent than real.

In the next three chapters we examine possible reasons why both event studies and accounts based studies have yielded inconclusive results and what might be done to control the measurement problems inherent in both research methods.
CHAPTER THREE
EVENT STUDIES: REVIEW OF METHOD

3.0 INTRODUCTION

Chapter two reviewed, inter alia, event studies of takeovers and concluded that the presence of biases in measures of abnormal returns decreases our confidence in the results to date. While later studies have controlled for the more obvious biases, such as the size effect, none has controlled for survivorship bias.

In this chapter, the event study research method is reviewed. A key point in the review is that the Sharpe-Lintner-Black Capital Asset Pricing Model (CAPM) based event studies are sensitive to deviations from ideal experimental conditions. Empirical tests indicate that firms' share returns are systematically associated with specific (apparently non-risk related) characteristics of the firms. It is argued that the event study method remains a valid test of portfolio performance provided the systematic association of firm-specific characteristics with returns is controlled.

The experimental design used in this thesis incorporates controls for firm size, survivorship bias and positive asymmetry in distributions of returns. In describing and defending the experimental design, it is argued that the failure of earlier event studies of takeovers to control for these matters is the most likely explanation for the anomalous and inconsistent returns documented in these studies.

3.1 REVIEW OF METHOD

Capital markets may be defined as efficient with respect to an information set if superior returns can not be expected from trading on the basis of the information in the set. Market efficiency may be tested by examining changes in share prices after presentation of an information cue. Equilibrium share prices are dependent on capital market agents' perceived distribution of returns which, in turn, depends on the information available concerning companies' production, investment and financing decisions. A change in share price after presentation of the information cue implies that the market had not previously incorporated the information, i.e., it was inefficient with respect to that cue (if the cue was part of the information set available to the market).

Fama, Fisher, Jensen and Roll (1969) were the first to use the term "efficient market". They defined it as "a market that adjusts rapidly to new information". Ball (1990) discusses methodological implications of different definitions of market efficiency. The definition adopted here is consistent with the empirical research tradition.
Event studies are based on the ability to observe information efficient equilibrium prices. If capital markets are assumed to operate efficiently, then the difference in equilibrium share prices (i.e., the share return) before and after an event can be interpreted as the measure of the impact of that event on firms' performances. It is in this context that this thesis evaluates the effects of takeover bid announcements. The critical assumptions are (i) that capital markets are efficient and, (ii) that the asset pricing model used specifies correctly the informationally efficient equilibrium price. In Section 3.1.1 the evidence on the validity of these two assumptions is summarised and the implications for this thesis are discussed.

3.1.1 Market Efficiency: Argument and Evidence

A strong argument in support of the assumption that market efficiency prevails is that our knowledge of the motivations and behaviour of capital market agents is consistent with market efficiency. If information is essentially costless and investors are competitive, rational wealth maximisers, then information that is available without private cost should have no private benefit. Competition among agents ensures that share prices will fully reflect costless information [Ball (1978)].

An implicit assumption in most event studies is that the information released by the event of interest is costless to incorporate into share prices, that is, there are insignificant frictions in capital markets. This assumption underpins the use of relatively short time periods to evaluate the perceived impact of the event on investors.

In recent years an impressive body of evidence has emerged which is inconsistent with the assumption of frictionless capital markets. The evidence suggests that even information which appears to be relatively costless to acquire is not impounded instantaneously in the share price of some categories of firms. Muddying the waters, there is also a related body of evidence which indicates that the equilibrium pricing models used in event studies do not yield accurate predictions of firm value when operationalised. The operational analogues of the parameters that finance theory informs us are relevant in pricing shares are dominated empirically by other market and firm characteristics (e.g., firm size) when predicting share returns.

The evidence of frictions in capital markets is not necessarily inconsistent with market efficiency. An efficient market may be defined as one where the value of information not impounded in share prices is less than, or equal to, the cost of impounding that information. Rational pricing rather than frictionless operation is the sine qua non for an efficient market (as defined above). Similarly, the evidence that theory backed CAPMs yield inferior predictions relative to ad hoc pricing
models might reflect problems in obtaining accurate measures of the relevant pricing parameters rather than intrinsic flaws in the theoretical CAPMs.

Notwithstanding the above, it is clear that a consideration of the issues raised by the recent evidence on market efficiency is essential to the design and interpretation of any event study. It is probable that at least some of the anomalous results recorded in early studies on takeovers (e.g., the secular downward drift in the post-bid abnormal return to bidder firms) are a function of either inaccurate assumptions about share markets or the use of empirically inappropriate CAPMs. While some event studies on takeovers have modified certain aspects of the classic event study design in light of the recent evidence [Franks, Harris and Titman (1991) and Agrawal, Jaffe and Mandelker (1992)], none has reflected the full implications of the findings. The studies have either continued to depend on descriptively inaccurate CAPMs in assessing expected returns or have not modified the interpretation of their results in a manner consistent with the evidence. A significant contribution of this thesis lies in the justification of its research design and results in light of the most current evidence on market efficiency. For instance, the benchmarks used to assess the significance of results are based on observed empirical distributions rather than theoretical distributions.

In the rest of Section 3.1 the current evidence on market efficiency is reviewed briefly and the implications for this thesis' event study design are discussed. The review begins with a description of the classic Sharpe-Lintner-Black CAPM which provided the framework for the initial empirical research into the concept of market efficiency.

**The Sharpe-Lintner-Black CAPM: Description**

Sharpe (1964), Lintner (1965), and Black (1972) were instrumental in developing the classic two parameter CAPM to describe equilibrium share prices. The Black (1972) version of the two parameter CAPM states that in equilibrium:

\[ E(R_{it}) = E(R_{zt}) + \beta_{it}[E(R_{mt}) - E(R_{zt})] \]

where for period \( t \):

\[ E(R_{it}) \] = expected (value of the) rate of return on asset \( i \),
\[ E(R_{mt}) \] = expected rate of return on the market portfolio \( m \),
\[ E(R_{zt}) = \text{expected rate of return on an asset that has zero} \]
\[ \beta_{it} = \text{the relative risk measure, given by the ratio of the} \]
\[ \text{covariance between the rates of return on asset } i \text{ and} \]
\[ \text{on the market, to the variance of the rate of return on} \]
\[ \text{the market.} \]

Equation (1) asserts that expected return is a linear function of risk. Under certain conditions, the model implies:

\[ R_{it} = Y_{1t} + Y_{2t} \beta_{i} + \epsilon_{it} \]

where

\[ Y_{1t} = R_{zt}, \]
\[ Y_{2t} = R_{mt} - R_{zt}, \text{ and} \]
\[ \epsilon_{it} = \text{the error term.} \]

If the market is efficient then \( \epsilon_{it} \) is a random variable with a mean of zero, i.e.,

\[ E(\epsilon_{it}) = 0. \]

Tests of market efficiency involve three steps. First, estimates of the market factors (\( Y_{1t} \) and \( Y_{2t} \) in Black's model) and of the firm's systematic risk (\( \beta_{i} \)) are obtained. Second, the estimated parameters are used to predict asset \( i \)'s return in period \( t \). Third, the predicted returns are subtracted from the actual returns observed given the presence of each information cue and the differences are used to test market efficiency. If the market is efficient with respect to the cue used, the mean difference (also described as the average residual, abnormal return or excess return) should not be significantly different from zero.

It should be noted that the power of such tests is unknown since the null hypothesis is that the market is efficient with no specific alternative [Brenner (1977)]. Further, in all the tests there is the implicit assumption that the equilibrium model used is correctly specified. To elaborate, since abnormal returns are defined relative to the equilibrium returns estimated via some version of the CAPM, misspecification in the model or biased or inefficient parameter estimates will result in biased or inefficient estimates of abnormal return. In effect, a test of market efficiency is a joint test of the model used and of market efficiency. In all such tests either market efficiency or the validity of one's equilibrium model must be held as a maintained assumption in order to test the alternative.\(^2\)

\(^2\) Roll (1978) demonstrates that the dilemma is inescapable. The descriptive validity of the two parameter asset pricing model is dependent on the hypothesis that the market portfolio is mean-variance efficient. A test of this hypothesis is practically impossible.
A consequence of testing the dual hypothesis of market efficiency and model validity is that the interpretation of abnormal return is rendered ambiguous. Abnormal returns are consistent with either or both market inefficiency and model misspecification [Brenner (1977)]. In view of the fact that there is no serious alternative to the Sharpe-Lintner-Black CAPM, it is conventional to discuss abnormal returns in terms of the degree of market inefficiency they represent, i.e., abnormal returns are generally interpreted as being consistent with market inefficiency rather than being indicative of model misspecification.

Ball and Brown (1968) were the first to observe a "drift" in the apparent market response to earnings information released in companies' annual reports. This drift represented the first rigorous evidence that the two parameter Sharpe-Lintner-Black CAPM does not yield accurate predictions of capital market behaviour. Subsequently, a number of factors have been found to be systematically associated with firms' relative returns. In the interest of conciseness, the following summary focuses primarily on the evidence relating to the anomalous systematic association between (i) equity returns and earnings announcements and (ii) equity returns and firm size. However, the discussion does include all the relevant considerations that would have been covered in a more comprehensive review. Further, the "earnings effect" and the "size effect" present the most serious challenge to the efficient market hypothesis. It seems appropriate to defend the assumption of market efficiency against the strongest evidence accumulated against it.

3.1.1.1 The Earnings Effect

The "earnings effect" refers to the persistent abnormal return that apparently can be earned by portfolios selected on the basis of the sign and magnitude of publicly released earnings figures. In a typical study of this phenomenon, Rendleman, Jones and Latane (1982) document that portfolios consisting of firms whose reported earnings are more than two standard deviations above a prior estimate (calculated from a statistical analysis of past earnings) can earn abnormal returns of up to 8%. That is, there are no "serious" rivals to the Sharpe-Lintner-Black CAPM in the sense that all other rival models (e.g., arbitrage pricing models) are theoretically ad hoc.

To clarify, the systematic association of the factors with firms' relative returns is not to say that a proportion of firms' relative returns is independent of risk. The view adopted in this thesis is that risk is always relevant to pricing shares, all apparently non-risk related factors associated with relative returns are informative about risk. Other risk associated factors documented in various studies include: a "January effect" with higher average returns during the first half of January [Keim (1983)], a "weekend effect" which results in lower or negative average returns on Mondays [French (1980), Gibbons and Hess (1981)] and a "Tobin's q" effect which results in stocks of firms with higher growth opportunities (high Tobin's q) yielding returns lower on average than stocks of firms with lower growth opportunities [Solt and Statman (1985)]. This list is not exhaustive.

In his review of the anomalous earnings phenomena, Bernard (1991) concludes that the evidence summarised in his paper "presents a more serious challenge to the efficient markets hypothesis than could have been anticipated a few years ago" (p. 31).
over a period spanning 90 days from the earnings release date. Portfolios comprising firms which experience less deviation of their reported earnings from the prior estimates also register abnormal returns, but the average amount is less. The sign of the abnormal return is positively correlated with the sign of the deviation of the reported figure from the estimate. That is, positive prediction errors are associated with positive abnormal returns.

One obvious potential explanation for the earnings effect is bias in the estimates of the parameters of the asset pricing model. However, Rendleman, Jones and Latane (1982) find that the abnormal returns associated with the earnings effect are not very sensitive to potential errors in the estimate of risk (i.e., beta) or the risk adjustment technique. After ranking firms in their sample into deciles based on the deviation of their actual earnings from their estimated earnings, Rendleman et al. find that the typical portfolio has a beta of approximately 1.0. They conclude that "risk adjustment procedures are not the critical issue ... One must look elsewhere for an explanation of [the abnormal returns] " (p. 287). Rendleman et al.'s conclusions are supported by the results of similar investigations conducted by, inter alia, Foster, Olsen and Shevlin (1984) and Bernard and Thomas (1989).

It is possible that the anomalous post-earnings-announcement drift in share prices is due to the share market pricing a variable that is missing from the CAPM. Ball (1978) argues that one way earnings studies can limit the bias due to omitted variables in the CAPM is by avoiding earnings variables which are highly autocorrelated (p. 118). The rationale is that if there is a missing variable being priced by the market, then the use of a highly autocorrelated earnings variable (which might be proxying for the missing variable) to assign firms to portfolios will result in the sample firms being assigned to the same portfolio consistently over time. In this case one is unable to discriminate between the earnings change classification or the missing variable as the factor responsible for the abnormal return. The use of a non-autocorrelated earnings variable to assign firms to portfolios gives greater confidence that it is the earnings change classification rather than an unknown, omitted variable which is responsible for the apparently anomalous earnings effect.

Foster, Olsen, and Shevlin (1984) implement Ball's recommendation by constructing two sets of earnings expectations models. One set of models is based on the time

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6 Ball and Brown (1968) were the first to document an average upward (downward) drift in share prices following the announcement of unexpectedly higher (lower) earnings by firms. There are more than twenty (non-independent) studies of post-announcement-drift in share prices (Bernard (1991)). Bernard and Thomas (1989, 1990) and Bernard (1991) provide comprehensive reviews of the evidence relating to the earnings effect. This summary is based largely on these three works.
series of reported earnings while the other set of earnings expectations models is based on security returns. The rationale is that the time series based models yield earnings forecast errors which are significantly autocorrelated while, in contrast, the security returns based models assign firms to unexpected earnings change portfolios in a fashion that "better approximates the independence-over-time assumption" (p. 574).

In an apparent vindication of Ball's criticism, Foster et al.'s results show that the portfolios selected on the basis of security returns exhibit no systematic drift in share price in the 60 trading days following the release of the earnings figure. On the other hand, the portfolios selected on the basis of the time series based models do exhibit a systematic drift.

Foster et al.'s results are consistent with the proposition that the earnings effect is not a manifestation of market inefficiency but rather appears to be proxying for a variable priced by the market but not specified in the CAPM. However, Bernard and Thomas (1989) argue in favour of an alternative interpretation. They contend it is possible that share prices respond completely and immediately to earnings announcements for some firms but not for others. For the latter firms, the observed post-announcement drift in prices represents the completion of the response. In using security returns to assign firms to portfolios, Foster et al. relied on the magnitude of the excess return experienced by each firm over the sixty days up to and including the earnings announcement date. However, the firms most likely to experience the largest excess returns, and thereby be assigned to the extreme portfolios, were just those firms whose prices respond immediately and completely to their earnings announcements. Firms with a "delayed" price response to information were more likely to be scattered across the less extreme portfolios. As Bernard and Thomas observe, since the average unexpected earnings is small for such non-extreme portfolios, it is not clear that any drift for these portfolios would be empirically detectable. They conclude that "there may be no detectable drift for any of the [excess returns based] portfolios, even if stock prices respond with a delay to earnings news for some firms" (p. 9).

Notwithstanding the above, it could be argued that the post-earnings-announcement drift in share prices represents compensation for changing levels of a CAPM omitted risk factor that is priced by the market. This explanation cannot be ruled out entirely but Bernard and Thomas (1989) consider it implausible. They find no evidence to

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7 Foster et al.'s research design also enabled them to test the hypothesis that the use of hindsight information to construct portfolios might account for the earnings related abnormal returns reported in, for instance, Rendleman, Jones and Latane (1982). Their results indicate that the hindsight explanation does not account for the earnings phenomenon.
suggest that long (short) post-announcement positions in extreme good (bad) news shares were risky along any of the five dimensions specified in the Chen, Roll, and Ross (1986) test of arbitrage pricing theory. Further, they find little evidence that "any risk to which their [unexpected earnings based] portfolios were exposed had surfaced in the form of a loss" [Bernard (1991, p. 6, emphasis in original)]. Over the 13 years from 1974 through 1986, the estimated abnormal return to a zero-investment portfolio consisting of long (short) positions in extreme good (bad) news shares was positive all 13 times.8

The Bernard and Thomas argument and findings are consistent with the proposition that the assumption of frictionless capital markets is descriptively inappropriate for event studies. It is possible that costs to acting on information and other (perhaps non-obvious) barriers to market participants prevent information being incorporated into share prices as rapidly as the earnings effect studies presume. If, for instance, one assumes a link between the size of a company and the efficiency with which its shares are priced, then one would expect "larger systematic drifts for smaller firms than for larger firms" [Foster, Olsen and Shevlin (1984, p. 581)].

Foster et al. provide evidence consistent with the proposition that, for some (typically smaller) firms and for some (particularly non-institutional) investors, there are non-trivial costs to enabling share prices to reflect available information. Foster et al. find that the average annualised excess return to small firms with extreme values of unexpected earnings is about 40%. If size is ignored, the mean annualised return is about 25%.9 Remarkably, this difference arises after controlling for the anomalous but persistent association of firm size with excess return (see below).10 Foster et al.'s result is consistent with costs (e.g., transaction costs or the cost of imperfect diversification) impeding the incorporation of information into share price and with these unit costs being greater for smaller firms.

Bernard and Thomas (1989) marshal further evidence and argument in support of the proposition that investors face non-trivial costs in acting on information. They observe that the information cost explanation implies a limit to the size of the cumulative excess return that can be made.11 In line with this prediction, Bernard

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8 Bernard and Thomas (1991) report that "over 50 quarters, the estimated abnormal returns were positive as many as 46 times, and the gains in these quarters exceeded the cumulative losses in the four remaining quarters by a factor of 35 to 1" (p. 6). The long period over which the earnings phenomenon has persisted also leads one to discount Reinganum's (1981) suggestion that the abnormal returns observed constitute a biased sample.

9 The annualised excess returns do not take into account transaction costs.

10 Foster et al. find that firm size explains approximately 65% of the variation in the post-announcement abnormal return; "the smaller the firm size, the larger the post announcement cumulative abnormal returns, with positive (negative) earnings change portfolios having positive (negative) cumulative abnormal returns" (p. 598).

11 Ball (1978) notes that "the consistent interpretation of [the information cost] explanation is
and Thomas find that the post-earnings announcement "drift is bounded, as if the response to earnings is inhibited once prices are within a certain range of the 'appropriate level'" (p. 4).\footnote{One reason other studies have not observed the limit to the drift is that sufficiently extreme values of unexpected earnings were not examined separately. Bernard and Thomas note that the post-earnings-announcement drift in share prices grows larger up to the point where the sample firms are split into deciles on the basis of the magnitude of their unexpected earnings component. Beyond that point, no matter how extreme the average unexpected earnings component in the portfolios, the drift does not increase.} Significantly, the implied range over which share prices drift "varies inversely with firm size, as do transactions costs, and is within the bounds of transactions costs for individual investors (including bid-ask spreads and commissions)" (ibid., p. 4).

Bernard and Thomas also find that the estimated excess return on firms with lower than expected earnings is more than twice as large as the excess return on firms with higher than expected earnings. This finding is a puzzle for the explanations based on CAPM misspecification but it is consistent with the trading costs explanation. Short sales are more difficult to execute than share purchases (i.e., they are more costly) and so one would expect the drift for "bad news" firms to be larger. One problem with this explanation is that it begs the question, why were the trades that did take place mispriced?

The evidence, from the earnings effect studies, that market efficiency and firm size appear to be positively related, prompts a closer review of the association between firm size and share return. The next section covers this area.

3.1.1.2 The Size Effect

Banz (1981) and Reinganum (1981) were the first to document a negative relationship between firm size and share return after controlling for risk. Subsequently, a long list of researchers have confirmed their results across different share markets and time periods. For example, Beedles, Dodd and Officer (1988) confirm an inverse relationship between total equity value and return for Australian ordinary shares over the 1974-1984 period by comparing the returns to portfolios selected on the basis of size. Firms for which the relevant prices are available are categorised into size deciles and their (equally weighted) returns used to calculate the monthly returns on the portfolios.

In line with similar studies, including the present one (see Chapter Four), Beedles et al. find a remarkable difference in returns between the two extreme portfolios. Over the eleven year measurement period, the smallest firm portfolio returned 8.23% per
month over and above the return attributable to the riskfree rate, the market return and the level of systematic risk. The equivalent measure for the largest firm portfolio is minus (-) 0.73% per month. The pattern of abnormal return persists (although slightly reduced) after Beedles et al. make several technical adjustments in the estimation method to control for various biases which might affect the results.\footnote{Beedles et al. account for: "fluctuations in the returns of nominally riskless assets; varying levels of systematic risk of size ranked portfolios; the index selected to represent the common factor in the return generating mechanism; the form of returns, i.e., simple versus continuously compounded; shares that leave the data base for bankruptcy, winding up or other reasons; the third moment of the return distribution; and the levels of portfolios' unsystematic risk" (p. 1). However, their use of equal weights within their ten portfolios is potentially problematic since my preliminary results indicate that such a procedure results in significantly different returns being recorded relative to equivalent value-weighted portfolios.}

The factors which Beedles et al. find account for part of the magnitude of the size effect include nonsynchronous trading in small firms (which results in downwardly biased estimates of beta), higher per share transaction costs for small firms, less information available on small firms and the relatively illiquid market for small firms' shares.

The systematic association between firm size and expected return has particular relevance to event studies of takeovers since bidders are typically larger than targets by a number of magnitudes. Bishop, Dodd and Officer (1986) report that over the January 1972 to June 1985 period, bidding firms were, on average, about six times larger than target firms. Figures 3.1 and 3.2, and Table 3.1, provide more detail based on the sample firms used in this thesis. Salient features of the size-distribution of bidder and target firms include that (a) a sizeable majority of bidding firms came from the largest three size deciles and (b) that target firms were more evenly distributed across size deciles.

It is instructive to compare Beedles et al.'s results with the cumulative abnormal returns to the targets of takeover offers recorded by BDO. BDO found, in line with other studies, that target firms made the most spectacular gains from takeover. In BDO's study, the targets of successful and unsuccessful takeover bids earned 20.1% and 21.8%, respectively, over a six month period centred on the bid announcement date. However, when one compares these returns with the approximately 8% per month risk-adjusted average return earned by firms in the smallest size decile and when one considers that firms from the smaller size-deciles are well represented in the sample of target firms, it is clear that the size effect is a potentially confounding influence on BDO's results.

It is important to appreciate that it is not just the estimated returns to the target firms that are biased (upwards, in the case of targets) by the size-effect. By construction, the sum of all abnormal returns across an equally weighted market portfolio is zero.
Figure 3.1


Table 3.1

Frequency of Sample Firms in Each Size Decile\(^1\)

<table>
<thead>
<tr>
<th>Size Decile</th>
<th>Bidders</th>
<th>Targets</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>5</td>
<td>30</td>
</tr>
<tr>
<td>2</td>
<td>14</td>
<td>32</td>
</tr>
<tr>
<td>3</td>
<td>21</td>
<td>44</td>
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<td>4</td>
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<td>5</td>
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<td>6</td>
<td>32</td>
<td>59</td>
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<td>7</td>
<td>45</td>
<td>66</td>
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<td>8</td>
<td>50</td>
<td>63</td>
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<tr>
<td>9</td>
<td>108</td>
<td>57</td>
</tr>
<tr>
<td>10</td>
<td>159</td>
<td>41</td>
</tr>
<tr>
<td>Total</td>
<td>475</td>
<td>487</td>
</tr>
</tbody>
</table>

Note: 1. Based on the market capitalisation figures used to rank each sample firm at month \([0]\), i.e., the bid announcement month. For instance, if firm X announced its takeover bid for firm Y in March 1980, X's last available share price in January 1980 and the total number of X's fully paid shares as at the end of February 1980 is multiplied to obtain firm X's market capitalisation. The sample firms comprise all bidding and target firms which had data available over the period \([0,+3]\) months relative to the bid announcement month.
Figure 3.2

Ratio in Percent of Bidder Firms' Market Capitalisations Divided by Their Respective Target Firms' Market Capitalisations

Note: 1. Histogram based on data for 227 bidders and their respective targets. The histogram includes all bids for which market capitalisation data were available for both the bidder and target firms over the period [0, +3] months relative to the bid announcement month.
Hence the exceptional estimated returns to small firms comes at the expense of large firms, as it were. Given the overwhelming concentration of bidder firms in the largest three size deciles, the size effect, if uncontrolled, results in the estimated average abnormal return to bidder firms being downward biased. The operation of the size effect is one possible reason why bidder firms display, on average, a secular decline in their CAR in the post-bid period. A description of the procedure used in this study to control for the size effect is provided in Section 3.2.

Even if there was not a systematic association of firm size with equity return, the typically large absolute size of the majority of bidders and the relatively small size of their targets affects the power of an event study to detect abnormal share price performance by bidding firms. For large firms, the impact of a takeover is less likely to result in a significant deviation from expected return since the takeover related gain or loss is more likely to be insignificant relative to other expected gains and losses. The more intense and widespread scrutiny to which large firms are subject also implies that any takeover activity is less likely to come as a surprise to the share market. For large firms, it is probable that takeover effects are impounded in their share price over a longer period of time thus decreasing the detectability of abnormal performance. Finally, the concentration of bidding firms in the largest three firm size deciles means that any size-based control portfolio will also display some takeover related performance thus decreasing the power of an event study to detect takeover related abnormal performance. These issues are revisited in Chapter Four.

Notwithstanding the above, the surprisingly strong systematic association of firm size with share returns has led some researchers to suspect that firm size might even dominate relative risk as a predictor of share returns. Fama and French (1992) investigate this issue.

3.1.1.3 Firm size and $\beta$: Fama and French (1992)

In their comprehensive investigation of, inter alia, firm size and $\beta$, Fama and French (1992) find that estimated market risk "seems to have no role in explaining the average returns on NYSE, AMEX, and NASDAQ stocks for 1963-1990, while size and book-to-market equity capture the cross-sectional variation in average stock returns that is related to leverage and E/P" (p. 445, emphasis added). In other words, Fama and French find that the central prediction of the Sharpe-Lintner CAPM, "that average stock returns are positively related to market $\beta$" (p. 449), is not confirmed by their tests.

A review of the Fama and French research method facilitates an appreciation of the
implications of their finding for the design of the event study adopted in this thesis.

The Fama and French goal is to "evaluate the joint roles of market β, size, E/P [earnings yield], leverage, and book-to-market equity in the cross-section of average returns on NYSE, AMEX, and NASDAQ stocks" (p. 428). For each of the 330 months over the period July 1963 to December 1990, the cross-section of returns on stocks is regressed on the above variables. The time-series means of the 330 regression slopes are then analysed to assess the explanatory power of each of the variables, i.e., to see whether they are priced.

An important issue in the Fama and French study is estimating the values of the variables. Firm size (shares outstanding times share price), E/P, leverage and book-to-market equity (BE/ME) can be measured precisely for each firm. Estimating β is less straightforward because estimates of β are more precise for portfolios. Fama and French estimate Bs for portfolios and then assign a portfolio's β to each stock in the portfolio. The problem with this approach is that size and Bs of size portfolios are correlated [Brown, Keim, Kleidon, and Marsh (1983), Chan and Chen (1988)]. This means that the regression analysis lacks power to separate size from β effects in the time-series mean returns. The Fama and French solution is to first sort the sample firms into size deciles and then subdivide each size decile into 10 portfolios on the basis of pre-ranking Bs for individual firms. The pre-ranking Bs are estimated on 24 to 60 monthly returns (as available) in the 5 years before July of year t" (p. 431). This procedure allows for variation in β that is unrelated to size.

For each of the 100 portfolios identified by the above procedure, the 330 (equally weighted) monthly returns over the period July 1963 to December 1990 are then calculated. The Bs of the 100 portfolios are then estimated over the same period using the CRSP value-weighted portfolio of NYSE, AMEX and NASDAQ stocks as the proxy for the market. The full-period post-ranking β of a size-β portfolio is then assigned to each stock in the portfolio,14 i.e., the β of the centile portfolio to which a given stock belongs is used as the estimate of that stock's β in the regression analysis.

It is worth noting two characteristics of the 100 sample portfolios. First, "sorted on size alone, the post-ranking Bs range from 1.44 for the smallest ME portfolio to 0.92 for the largest. ... Across all 100 size-β portfolios, the post-ranking Bs range from 0.53 to 1.79, a spread 2.4 times the spread, 0.52, obtained with size portfolios alone" (p. 432). The implication is that forming portfolios on the basis of size alone

14 Fama and French argue that "the precision of the full-period post-ranking portfolio Bs, relative to the imprecise β estimates that would be obtained for individual stocks, more than makes up for the fact that true Bs are not the same for all stocks in the portfolio" (p. 432).
conceals the great variation in $\beta$s within the firms in each portfolio. It is not surprising that, under these conditions, there is a high correlation between $\beta$ and size measures. The second point is that the preliminary $\beta$ sort used to subdivide the firms within each size decile is not a refined size sort. Fama and French observe that "in any size decile, the average values of $\ln(ME)$ are similar across the $\beta$-sorted portfolios" (p. 432).

Prior to regressing firms' individual returns on their explanatory variables, Fama and French perform a couple of tests on their sample portfolios. In the first of their tests, Fama and French confirm that when portfolios are formed on size alone there is a strong negative relationship between size and average return and a strong positive relationship between average return and $\beta$. The two relationships are consistent because of the tight connection between size and the $\beta$ of the size portfolios. $\beta$ ranges from 1.44 for the smallest size portfolio to 0.90 for the largest size portfolio. Fama and French note that this test does not enable us to determine which of the two factors, firm size or $\beta$, is more closely associated with the systematic returns to the portfolios.

In a second test, Fama and French form 12 portfolios on the basis of firms' ranked market $\beta$. These portfolios exhibit a wider range of $\beta$ (from 0.81 to 1.73) than the portfolios formed on size alone. However, there is "no obvious relation between $\beta$ and average return" (p. 433). For example, the two extreme portfolios have nearly identical average returns (1.20% and 1.18% per month over the period 1963-1990).

A matrix analysis of the returns to the centile portfolios formed on the basis of size and $\beta$ confirms the lack of empirical association between $\beta$ and average return and the strong association of firm size with average return. Within each size decile, the 10 portfolios formed on the basis of post-ranking $\beta$s exhibit strong variation in $\beta$ yet average returns are flat and even decline slightly with increasing $\beta$. In contrast, average return and $\beta$ decrease with increasing size. As Fama and French note, the evidence indicates that "variation in $\beta$ that is tied to size is positively related to average return, but variation in $\beta$ unrelated to size is not compensated in the average returns of 1963-1990. The proper inference seems to be that there is a relation between size and average return, but controlling for size, there is no relation between $\beta$ and average return" (p. 438).

The regressions lend further support to the above. In brief, they show that when the variation in $\beta$ that is related to size is controlled, "the relation between $\beta$ and average return is flat, even when $\beta$ is the only explanatory variable [in the regression]" (p. 438). Interestingly, the regressions show a cross-sectional relation between average
return and book-to-market equity that appears stronger than the relation between size and average return. The combination of size and book-to-market equity subsumes the apparent association of leverage and E/P with average return.

The final two results described above are intriguing because Anderson, Lynch and Mathiou (1990) find that in Australia, over the period January 1975 to December 1984, the book-to-market relation with average return virtually disappears once firm size is controlled. The same is true for price-earnings. Anderson et al. posit that the association between earnings/yield and average return might hold only for large (in absolute terms) stocks. They point out that their results and some U.S. based results are consistent with this hypothesis. Anderson et al. note that the largest Australian firms are not significantly bigger than the smallest firms used in the U.S. studies.

The dominant role of firm size in predicting share returns is also supported by Brown, Keim, Kleidon and Marsh (1983). Brown et al. aimed primarily to test the "tax-loss selling" hypothesis of the "January effect" using Australian data. The January effect, documented with U.S. data by Brown, Kleidon and Marsh (1983) and Keim (1983), refers to the concentration of abnormally high positive returns in the first half of January. Keim's study shows that over the 1963-1979 period, a portfolio long in small firms and short in large firms could earn an average 30% per annum abnormal return. Remarkably, half of this return (15%) occurred in January. Further, of this half, 8% occurred in the first week with a fifth (3%) attributable to the first day. The tax-loss hypothesis maintains that the January effect results from tax laws that encourage a year's-end sale of shares that have declined in price so that the capital loss can be off-set against taxable income for that year. Small firms are more risky and therefore more likely to have experienced a price decline and be sold off. BKKM report that there is not one but two seasonals in Australian share returns (in December-January and July-August) across almost all size categories. Given the Australian tax year-end is June 30, this pattern is inconsistent with the tax-loss hypothesis. Further, the Australian size premium for smaller firms (about 4%), in contrast to the US. experience, appears fairly constant across all months.

3.1.1.4 Implications of the Anomalous Effects

One unequivocal implication from the preceding review of the firm size and earnings effects is that event studies which use the classic Sharpe-Lintner-Black CAPM rely on a predictively inappropriate characterisation of capital markets.

Ball (1991) points out that tests of efficiency are also "tests of particular models of equilibrium pricing with respect to information" (p. 17). There are sound reasons why we should not be surprised that the CAPM performs poorly for predictive
purposes. Ball (1990) notes that the CAPM and related models describe pure exchange equilibrium prices. They ignore supply (e.g., they do not say how betas might vary over time); are silent on seasonality; treat as exogenous all macro parameters (e.g., the expected return on the market portfolio); assume that returns distributions are continuous; and ignore information costs. Further, because the CAPM is a pure exchange model which only allows relative assessment of prices, it is susceptible to deficiencies in sampling from the population of firms. One implication is that abnormal returns achieved by samples of "non-normal" firms (e.g., firms which record earnings significantly different from their expected earnings) are not necessarily reliable indicators of market inefficiency.

The argument that the CAPM is unlikely to describe information-efficient equilibrium prices does not, of course, imply that the market is efficient. A descriptively inaccurate pricing model is consistent with both efficient and inefficient markets. However, a number of considerations suggest that, on balance, market efficiency - defined as rational response to information - is a valid characterisation of capital markets. These considerations are reviewed below.

A remarkable feature of the evidence on efficient market anomalies is the magnitude of the ostensible gains available by trading on the anomalies. Beedles, Dodd and Officer (1988) report that over the period January 1974 to December 1984 the average monthly return to the portfolio comprising firms in the smallest size (by market capitalisation) decile was 9.44% (i.e., 195% per year). Further below, this thesis records an average monthly return of 7.32% to the smallest size decile over the period February 1974 to January 1991. In the U.S., Bernard and Thomas (1989) estimate that every quarter the implied "pure profit from trading on the 20% extreme earnings-announcing stocks is 7.74% times 10% of the aggregate market value of all NYSE-AMEX stocks" (Ball 1992, p. 332). The scale and persistence of the apparent potential pure gains is difficult to reconcile with one's priors about the degree of competitiveness in capital markets. Another powerful argument against rejecting the assumption of rational pricing behaviour in share markets is the consistent and persistent finding that the past performance of professionally managed investment funds is not a reliable pointer to future performance by the same funds. The evidence on share market anomalies has been widely documented yet no fund manager has yet managed to demonstrate an ability to persistently capitalise on the findings.15

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15 Given the obvious marketing advantages available to anyone who is able to convince investors that they are able to pick future "winners", it is inevitable that investment funds with a good record in the recent past will point to that record as an indication of their relatively superior investment prowess. However, the same media that regularly publicise this strain of financial self-promotion often run items noting the virtual impossibility of predicting sustained positive investment fund performance. One such article which analysed the performance of the major Australian "Super
It begs the question to use as a defence of market efficiency the very evidence that
contradicts the premise. However, the evidence of the significant gains persistently
available from trading on publicly released information suggests that for certain
categories of stocks there are substantial frictions which impede the rapid
incorporation of information into their price. These frictions are not inconsistent
with market efficiency since efficiency is predicated on the assumption that gains
from trading on information outweigh the trading costs. The work by Bernard and
Thomas (1989, 1991) and others [reviewed in Bernard (1991)] makes clear that the
frictions are not easily traceable to obvious factors such as transaction costs but
might be due to less well understood constraints on trading.16

The earnings effect evidence is reconcilable with rational, profit-maximising market
behaviour if we assume the existence of substantial frictions in the market. On the
other hand, the size effect does not, prima facie, have the same rational association
with share prices that the earnings effect possesses. That is, the association of firm
size with stock returns is less obviously based on rational pricing behaviour. This
perspective is probably misguided. Ball (1992) argues that the evidence is more
consistent with size proxying for expected return than with size per se being priced.

Arguments for the above view include that information costs are likely to be a
decreasing function of market capitalisation which, in turn, implies that expected
return is likely to be a decreasing function of firm size. Evidence that information
acquisition and processing costs decrease with size is found in the typically higher
bid-ask spread for smaller firms. High bid-ask spreads are consistent with high
heterogeneity of investors' beliefs which is generally associated with high
information costs [Amihud and Mendelson (1986)]. Consistent with this argument,
Aitken and Ferris (1990) find that over the period January 1965 to December 1985
the size effect in Australian share prices disappears over a two to three month
investment horizon once brokerage fees and bid-ask spread are controlled.

In the U.S., Brennan, Jegadeesh and Swaminathan (1992) also find evidence
consistent with the proposition that information costs rather than size per se are
responsible for the differential returns to small and large firms after controlling for
estimated relative risk. In brief, Brennan, Jegadeesh and Swaminathan use the

Funds" concluded that "once again the message is clear: backing a fund manager solely because of its
past performance is a mistake. In most cases, it is just as likely to leave you weighed down with
heavy losses as buoyed by big winnings" (The Australian Financial Review, Tuesday September 28,
1993, p. 17).

16 Ball (1991, 1992) notes that research on the response of prices to information in competitive
markets is at a relatively nascent stage. Little research has been done in this area, "in part, because
the need has not arisen previously" (1992, p. 341).
number of analysts following each stock as a proxy for the degree of information available about each stock. Their results show that firms followed by many analysts respond more rapidly to market returns than firms followed by few analysts. Pertinently, there is no evidence that firm size plays an important role once account is taken of the volume of trading.

Another argument which implies that size proxies for expected return is based on the definitional point that size is a function of past equity returns. Size at a point in time proxies for beta changes up to that point. The evidence in Fama and French (1992) suggests that firm size is a much more efficient proxy for relative risk than empirical estimates of beta. Handa, Kothari and Wasley (1989) show that at least part of the size effect is due to errors in measuring beta from short-interval returns.

The preceding survey on efficient market anomalies and the ensuing discussion point to a couple of conclusions. The evidence remains anomalous with respect to certain characterisations of efficient capital markets. It is possible to reconcile the evidence with more circumscribed definitions of market efficiency which allow for the existence of market frictions. While careful interpretation of the evidence is consistent with the reconciliation, the fact remains that an a priori assumption of market rationality is a necessary component of the reconciliation. The alternative is to describe capital market behaviour as irrational. This characterisation is significantly at odds with the enormous resources devoted by capital market participants to analysing firms. It is equivalent to stating that random investment decisions are the driving force in setting share prices.

The view adopted in this thesis is that share return controls adopted in event studies are attempts to control systematic variation across securities in equilibrium expected returns. It is clear that at present the causes of the systematic variation are poorly understood. The evidence is unequivocal that the process by which information is incorporated into share prices is poorly modelled by the classic Sharpe-Linter-Black CAPM, at least for certain categories of firms. In particular, the proposition that information is rapidly incorporated into the model's equilibrium share prices is not valid across all firms. This is not to say that the share market is irrational in its behaviour but merely that the modelling of information efficient equilibrium prices needs to take into account the empirical association of certain readily observable firm characteristics (e.g., firm size) with systematic variation in share prices. Researchers cannot rely on theoretically sound but descriptively invalid static asset pricing models.17 In Section 3.2, the pricing models used in this thesis to evaluate

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17 Friedman (1952) discusses the methodological issues involved in adopting an instrumental view of theories. In brief, his thesis is that all theories are, to a certain degree, descriptively invalid. Empirical research requires the selection of those models of reality that yield the most useful
the information content of takeover announcement bid are described and defended in light of the preceding discussion. In the interim, we discuss one potentially highly significant but little addressed bias in event studies: survivorship bias.

### 3.1.2 Survivorship Bias

The extensive research on the size and price-earnings effects has largely discounted the explanation that flaws in research design or implementation account for their existence. The size and price-earnings effects have proved robust with respect to many different research designs and controls. However, Brown, Goetzmann, Ibbotson, and Ross (1992) suggest that at least a portion of the anomalous return recorded in event studies is attributable to survivorship bias.

In general, survivorship bias refers to the practice of predicting performance on the basis of ex post results. For example, Brown et al. contend that the recent evidence which implies that past mutual fund performance predicts future performance is subject to "survivorship bias". The evidence criticised by Brown et al. related to the performance of selected firms which had survived over a 10 year (or similar) period. The performance of the firms in the first five years was observed and the categorised as either a winner or loser. The performance of the firms over the next five years was also measured. It was found that firms that had won over the first period had a greater than 50% chance of being classified as a winner in the next period. The flaw in the studies, Brown et al. explain, is that winning firms were much more likely to have been run by managers who adopted a high risk investment strategy and won. If the managers had lost, they would not have appeared in the sample. The point is, only managers who adopted a high-risk strategy, and won in both periods, survived to be included in the sample. Other managers with high risk investment strategies who lost would not have been included in the sample. The extent of survivorship bias might be small but Brown et al. demonstrate analytically, and via a model, that "very mild survivorship criteria are sufficient to induce strong persistence in performance" (p. 14).

Survivorship bias in event studies operates somewhat differently but to the same effect as in the example cited by Brown et al. For example, inadvertent survivorship bias is introduced in event studies of takeovers when the returns to portfolios of bidder and target firms that have survived a particular time period are adjusted by the returns to a market portfolio comprising firms that have not necessarily survived over the full period investigated. To elaborate: the cumulative abnormal return to a portfolio may be defined as the sum of the average monthly predictions for one's purpose.
returns to all firms in the portfolio. If a firm does not have a return available for a particular month, its contribution to the portfolio return is excluded for that month. Ostensibly, apart from the fact of a takeover bid, the same selection criteria apply for both the sample portfolio and the control portfolio. However, bidding firms, by virtue of their pre-bid performance, have a higher probability of surviving over a particular period than the sample of firms comprising the control portfolio. If lower survival rates are correlated with lower rates of return, we may expect to observe an upward drift in the post-bid return to bidding firms (after controlling for the size effect). The upward bias is attributable to the higher rate of survival of bidder firms relative to the set of firms comprising the market portfolio. Note that the extent and even direction of survivorship bias is an empirical issue. It is possible that the downward pressure on average return due to firms dropping out from the sample because of bankruptcy or business failure is more than compensated for by the high returns to target firms which are delisted as a result of successful takeover bids. Evidence on this issue is presented in Chapter Four.

Survivorship bias has not received significant attention in the event study literature to date. In part, this reflects the greater influence of the size-effect in explaining the anomalous returns to bidding firms. Another probable reason for the lack of attention is the practical difficulty in constructing indexes over various periods that would be adjusted for survivorship. Some studies have used as proxies for the market portfolio indices comprising the top firms by market capitalisation and share volume traded (e.g., the Statex-Actuaries Accumulation Index). These indices are less vulnerable to survivorship bias, but they do remain open to charges that their share return characteristics differ systematically from the return characteristics of small and relatively illiquid sample firms. Evidence on this point is provided in Chapter Four.

This thesis controls for survivorship bias by ensuring that the sample firms' returns are adjusted by the returns to a control portfolio that comprises all firms with available returns over the relevant event period. That is, both the sample portfolio and the control portfolio comprise firms that have survived over a given chronological event window. The procedure to implement the control is described in Appendix A (see page 83). This study is the first to control for survivorship bias on the abnormal returns to firms involved in takeover bids. The implementation of the control and the associated construction of size and survivor adjusted market and individual firms' rates of return, for any given period of months between 1974 and 1993, are two of the significant contributions of this thesis.
3.2 RESEARCH METHOD: OUTLINE AND DEFENCE

Three key points emerging from the preceding review are (i) that systematic variation in share prices is strongly and predominantly associated with firm size, (ii) that the speed with which share prices incorporate information may be modelled as an increasing function of firm size and (iii) it is probable that survivorship bias significantly affects the adjusted return to sample firms. The respective implications for event studies are (i) that the portfolios used to model expected return need to incorporate a control for firm size, (ii) it is preferable that wide event-windows be analysed in order to ensure that measured abnormal returns across all samples of firms reflect the full impact of the event, and (iii) a control for survivorship bias ought to be implemented. A description of the research design used in this thesis, which addresses the above concerns, follows.

3.2.1 Research Method: Outline

In brief, the research is designed to estimate the cumulative abnormal or excess rate of return which could be achieved by holding separate portfolios of bidder and target firms over various periods ("event windows") relative to significant takeover event dates.

The choice of event date and width of event window are important issues for two reasons. One is that the first intimation the share market has about a takeover is usually some indeterminate time prior to the first official announcement [Aitken and Czernkowskki (1992)]. This suggests that the event-window ought to begin some months prior to the official bid announcement in order to estimate the complete share market reaction to the takeover bid. The other reason centres on the date that share market expectations are confirmed or dashed. In an efficient market we expect the share market response to a takeover bid announcement to be complete on this date since all remaining uncertainty about the bid outcome is eliminated.

While the preceding characterisation of the share market's pattern of response to takeover bids is relatively non-controversial, it is curious that many studies of takeovers, including BDO, report their findings as if the bid announcement date is

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18 One disadvantage of using wide event-windows is that more "noise" is captured in the estimated returns. This has implications for the design of tests of the significance of results. One advantage of using an empirical distribution of control portfolio returns (described later in the thesis) to compare the sample portfolio's returns is that the control portfolios and the sample portfolios are expected to have the same variance in returns under the null hypothesis. If the signal to noise ratio in the returns to the sample portfolio is relatively low over wide event-windows then this increases the probability that the null hypothesis will be wrongly accepted. As it turns out, the results in most cases are strong enough to reject the null hypothesis despite the operation of the bias in favour of accepting the null hypothesis.
the sole date on which share market uncertainty is resolved. The bid announcement date is important in that it confirms that a bid is being mounted. However, the mounting of a takeover bid may convey significant news to the share market not necessarily directly related to the takeover itself (e.g., perhaps that the bidder firm is confident about its future prospects or that the target firm has valuable resources) which will cause a more or less permanent revision in share prices. The date on which the bid outcome is confirmed - typically, some time after the bid announcement date - is the earliest date the share market has all its takeover related uncertainty resolved and so this date is also important when assessing the share market's response to the takeover per se. The bid announcement date may be used in the assessment of the share market's response to takeover bids but given that such bids often constitute "polluted" or ambiguous information signals about the firms involved it is also necessary, when attempting to assess the share market's reaction to the prospect of takeovers per se, to focus on the bid outcome date. In this thesis, results are reported based on both the bid announcement date and the bid outcome date as the critical uncertainty resolving event dates.

Expected Returns Models

Abnormal returns (ARs) in this study are defined as:

\[ AR_{it} = R_{it} - E(R_{it}) \]

where

- \( R_{it} \) = return on security i in period t, and
- \( E(R_{it}) \) = expected return on security i in period t.

In their paper, BDO (1987) use a market adjusted returns model to calculate the expected return for each firm in each month:

\[ E(R_{it}) = R_{mt} \]

where

- \( R_{mt} \) = the return on the market portfolio in month t.

The market adjusted returns model can be reconciled with the Sharpe-Linter-Black market and risk adjusted model under the assumptions that the beta of security i is equal to one and the intercept term is equal to zero (zero-one market model). This version of the market model is used by BDO on the grounds of, inter alia, simplicity. In order to facilitate a comparison with their results, this thesis also reports market adjusted returns to the sample firms.
In theory, the market adjusted returns model is not as efficient (i.e., it is less accurate) in estimating adjusted returns as the market and risk adjusted model since the expected systematic association of relative risk with return has not been explicitly controlled. However, the evidence in Fama and French (1992) that there is no empirical relationship between estimated beta and expected return once firm size has been controlled implies that the lack of an explicit control for systematic risk should not be a significant concern. The systematic empirical association of firm size with return is controlled by using the rate of return on a portfolio of firms in the same size decile (of which security i is a member) as the estimate of the market rate of return. In other words, the use of size deciles as a proxy for market rates of return controls directly for the market and size effects.

As stated earlier, survivorship bias is controlled by ensuring that for any given event period both the sample firm portfolios and the size-decile portfolios comprise only those firms with available data over the event period. That is, the sample portfolio and the control portfolio are confined to firms that survived over the complete event period.

3.2.2 Sample Selection

A prime objective of this study is to determine whether or not the divergent results reported in the BDO and MR studies can be reconciled by reanalysing the effects of takeover on the sample firms in each study via a common metric. Given this objective, the sample firms are selected from both the BDO and MR studies. Comparisons are made between (i) the sample and control portfolios, (ii) bidder and target portfolios selected from the BDO study, (ii) bidder and target portfolios selected from MR and (iii) bidder and target portfolios which combine sample firms from both studies. The comparison of results helps determine whether the two studies' divergent results are sample specific or due to differences in the modes of analysis. Further, given that the size and survivorship effects have been controlled, the abnormal returns yield less biased estimates of the gains or losses from takeover activity.

3.2.3 Data Sources and Period Covered

Sample firm data used to reanalyse the BDO study are taken from their Takeovers Database which has been made available by the Centre for Independent Studies (CIS). MR made available the sample firm data necessary to replicate and reanalyse their study. The MR study was jointly commissioned by the National Companies and Securities Commission and the Australian Institute of Management (Victorian Division) while the CIS commissioned the BDO study.
The share market data used in the event study are sourced from (i) the Share Price and Price Relative (SPPR) database compiled by the Centre for Research in Finance (CRIF) at the Australian Graduate School of Management (AGSM) and (ii) the Statex Database compiled by the Australian Stock Exchange (ASX).

The SPPR database is a historical record of the share prices and monthly rates of return of all Australian listed or previously listed companies. For each company, the share price, number of shares on issue and adjustments for dividends, share issues and reconstructions for fully paid ordinary shares are held on a monthly basis. CRIF calculates the monthly price relatives for each firm after making adjustments for dividends, share issues, etc. SPPR covers the period from 1974 to the release date of the latest update from CRIF. Importantly, SPPR retains delisted companies and incorporates a company name coding convention that enables changes in company names to be tracked. This means that company name changes or delistings do not prevent the compilation of a complete history of a given company's share market performance since 1974.

One caveat to the above is that the SPPR database does not provide a complete continuous historical record of the monthly price relatives for each company since 1974. A significant proportion of companies in the database have missing price relatives for some months over which their shares were traded. Pertinently, these missing price relatives do not appear to be randomly distributed. Missing price relatives are more probable in months when a price adjustment is necessary to take into account a distribution of dividends or a capital change. Smaller, less liquid or traded stocks are also more likely to have series of missing price relatives. Finally, periods leading up to a corporate restructuring are also more likely to have series of missing price relatives.

The non-random distribution of missing price relatives reduces our confidence that we can use the SPPR database to construct control portfolio returns that are unbiased. In an attempt to mitigate this problem, the SPPR database is augmented using the STATEX database compiled by the Australian Stock Exchange (ASX). Descriptions of the STATEX database and of the protocol followed to facilitate an effective merger of the SPPR and Statex databases are given in Appendix A (see page 83). The computer programs developed will prove useful for other events-studies on the Australian share market and constitute a significant contribution of this thesis.

The period covered by the BDO study is January 1972 through June 1985. The MR study examines takeovers which occurred between 1970 and 1981. This thesis uses monthly share market data which covers the period from January 1974 to December
1990. The use of January 1974 as the start date of the period covered is dictated by the non-availability of SPPR data prior to this date. At the time the data for this thesis were first collected the last month for which share market prices were available was December 1990.

3.2.4 Significance Testing

The significance of the measured abnormal returns to the experimental sample firms is assessed by comparing the sample portfolios' abnormal returns with the empirical distribution of abnormal returns on randomly selected portfolios. The advantage of this method of assessing significance is that it frees the researcher from making assumptions about the distribution of portfolio returns that may be inappropriate.

Foster, Olsen and Shevlin (1983) point out that the use of an empirical distribution to assess significance has three appealing properties relative to the standardised t-tests; (i) "it does not assume normality, (ii) it does not assume constant variance across securities or over time, and (iii) it does not assume cross independence in the residuals" (p. 586). These virtues of empirical distributions are particularly pertinent in view of the finding by, inter alia, Beedles (1986) that Australian equity returns are asymmetric. That is, the two-moment paradigm commonly used to assess investments does not accurately describe or predict the empirical distribution of Australian equity returns. Other sources of bias in assessing the significance of estimated abnormal returns to experimental sample firms are, as discussed, the systematic negative association of firm size with return and survivorship bias.

One of the contributions of this thesis is the reporting of the moments of the return distribution of randomly selected portfolios over various time periods. This provides previously unavailable benchmark figures that may be used in assessing the significance of estimated abnormal returns to experimental sample portfolios in both the current and future studies. Four different types of return distributions are generated: (1) the distribution of raw (i.e., unadjusted) returns; (2) the distribution of market (equal weighted) adjusted returns; (3) the distribution of market (value weighted) adjusted returns; and (4) the distribution of size-decile adjusted returns. Comparisons of the experimental returns against these distributions yields estimates of the bias introduced by the size-effect to experimental sample portfolios that have size profiles which differ from the size-profile of firms in the market portfolio.

The benchmark figures obtained and a review of their implications are reported in Chapter Four. In the interim, the procedure used to generate the empirical distribution of returns is described. It is illustrated by reference to an event study analysing the returns to sample firms over a period [-24,+24] months centred on the bid announcement month.
1. The first sample firm's bid announcement date is identified and the experimental time window is defined as the calendar time interval beginning 24 months prior to the announcement date and extending 24 months after the announcement date.

2. The set \( \{S\} \) of ASX listed firms with share returns available over the same period as the experimental time period is identified.

3. A random selection, with replacement, of a firm is done from \( \{S\} \) and the adjusted return to the selected firm over the experimental time window is placed into the first control portfolio. Step 3 is repeated 1000 times, each time placing the return to the randomly selected firm into the next portfolio in the sequence of 1001 portfolios.

4. Steps 1 to 3 are repeated for all the sample firms. The metrics calculated for the experimental sample portfolio and for each of the 1001 control portfolios include their mean abnormal return, their median abnormal return and the proportion of positive abnormal returns. The sample portfolio's metrics are then compared with the distributions of the mean abnormal return, the median abnormal return and the proportion of positive abnormal returns across the 1001 control portfolios.
Appendix A describes the share market data used in the event-study component of this thesis. The description includes summaries of the procedures followed in compiling the data.

**Objective and Scope**

The objective in compiling the sharemarket data is to build a database from which one may access, for any given period of months between January 1974 to December 1991: 19

(i) the cumulative return,
(ii) the market (equal weighted) adjusted cumulative return,
(ii) the market (value weighted) adjusted return, and
(iv) the size-decile adjusted cumulative return
to any and all ASX firms which were listed over the period of interest.

In addition, one may obtain, for the specified period:

(v) the equally weighted market return,
(vi) the value weighted market return,
(vii) the returns to all the size based decile portfolios,
(viii) the values of the 9 market capitalisations which demarcate the size based portfolios,
(ix) the six letter ASX codes for all the companies with available returns data,
(x) the number of firms with (a) positive market (equal weighted) returns, (b) negative market (equal weighted) returns, and (c) returns which equal the market (equal weighted) return
(xi) the number of firms with (a) positive market (value weighted) returns, (b) negative market (value weighted) returns, and (c) returns which equal the market (value weighted) return, and

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19 Due to data on the CRIF SPPR database extending only as far back as December 1973, the earliest month for which a cumulative return may be calculated is January 1974. The last month for which data are available depends on the currency of the latest versions of the SPPR and STATEX databases. At the time of writing, the database contained share price data up to December 1990. This means that one can obtain, from the database constructed, the cumulative returns to any and all ASX listed firms over any specified period in months between January 1974 and December 1990. Retrieval of data is contingent on the nominated companies having returns data available over the specified period.
(xii) the number of firms with

(a) positive decile adjusted returns,
(b) negative decile adjusted returns, and
(c) returns which equal the return to the decile portfolio in which they belong.

All the returns described above are adjusted for dividends and capitalisation changes. The adjustments are made under the assumption that the value of all dividends and rights issues are immediately reinvested in the issuing firm.

Two other data items used to calculate the market capitalisation of each firm at the beginning of the specified period may also be accessed: (a) the last quoted share price of each firm two months prior to the specified period (e.g., for the period January 1975 to October 1988, the pertinent price is the November 1974 share price) and (b) the total number of fully paid ordinary shares issued as at the month immediately prior to the specified period (e.g., for the period January 1975 to October 1988, this item would refer to the number of fully paid ordinary shares outstanding as at December 1974).

**Significant Definitions**

The cumulative return to each firm is defined as

\[ CR_i = \frac{P_1}{P_0} \]

where

- \( CR \) = cumulative return over the period,
- \( i \) = variable denoting a given ASX listed firm,
- \( P_1 \) = firm \( i \)'s share price at the end of a given period
- \( P_0 \) = firm \( i \)'s share price at the beginning of the given period.

The operational definition above applies if there are no dividends or rights issues distributed by firm \( i \) over the period of interest. If there are dividends or rights issues during the period then

\[ CR_i = \frac{XP_1}{P_0} \]
where

\[ X = \text{one plus the proportional increase in the investment weighting of firm } i \text{ as a result of re-investing in the same firm the value of all dividends and all other changes, adjusting for firm } i \text{'s change in basis of quotation.} \]

\[ X \text{ is calculated as follows:} \]

\[ X = 1 \times \left[ \frac{V_1}{xP_1} + 1 \right] \times \left[ \frac{V_2}{xP_2} + 1 \right] \times \cdots \times \left[ \frac{V_n}{xP_n} + 1 \right] \]

where

- \( V \) = the value per share of each dividend or rights issue,
- \( 1, 2, 3 \ldots n \) = subscript to denote ordinal sequence of the dividend or rights issues (e.g., \( v_1 \) was paid out before \( v_2 \) and so on), and
- \( xP \) = ex dividend or ex rights price (i.e., the value of the share after the dividend or rights issue has been paid).

To illustrate the above, assume the following facts in relation to dividends and rights issues made by firm \( i \) over the period March 1974 to October 1979: Firm \( i \)'s share price at the end of March 1974 was 110¢ and 200¢ at the end of October 1979. The October share price was set after the following dividends and rights issues were made.

<table>
<thead>
<tr>
<th>Date</th>
<th>Value per share of dividends or rights issue</th>
<th>Ex dividend or ex rights price of each share</th>
</tr>
</thead>
<tbody>
<tr>
<td>June 1974</td>
<td>10 cents</td>
<td>90 cents</td>
</tr>
<tr>
<td>June 1975</td>
<td>10 cents</td>
<td>140 cents</td>
</tr>
<tr>
<td>January 1979</td>
<td>20 cents</td>
<td>150 cents</td>
</tr>
</tbody>
</table>

\[ X = 1 \times \left[ \frac{10}{90} + 1 \right] \times \left[ \frac{10}{140} + 1 \right] \times \left[ \frac{20}{150} + 1 \right] \]

\[ = 1.349206 \]

From which it follows that

\[ CR_i = 1.349206 \times \frac{400}{300} = 2.45 \]

The procedure described above is less demanding of data relative to the alternative procedure of summing the natural log of each month's price relative in order to obtain the cumulative return. The minimum share data required are the share prices
at \( P_0 \) and \( P_1 \), the value per share of any dividends and rights issues received during the period over which the price relative is calculated and the associated ex dividend or ex rights share prices.

The cumulative market adjusted return to each firm is defined as

\[
CR_{mi} = CR_i - CR_m
\]

where

- \( CR \) = the cumulative return over the period,
- \( i \) = variable denoting each ASX listed firm, and
- \( m \) = variable denoting the market portfolio.

The cumulative return to the market portfolio, \( CR_m \), is calculated on both an equal weighted basis and a value weighted basis. The equal weighted cumulative market return, \( CR_{em} \), is defined as follows:

\[
CR_{em} = \frac{\sum CR_i}{n}
\]

where

- \( \sum CR_i \) = the sum of the cumulative returns to all firms with available data over the period and
- \( n \) = the number of firms with available data over the period.

The value weighted cumulative market return, \( CR_{vm} \), is defined as follows:

\[
CR_{vm} = \frac{\sum (MCAP_i \times CR_i)}{\sum MCAP_i}
\]

where

- \( MCAP_i \) = the market capitalisation of firm \( i \) (defined below),
- \( \sum (MCAP_i \times CR_i) \) = the sum of the weighted cumulative returns of all the firms with available returns data over the period and
- \( \sum MCAP_i \) = the sum of the market capitalisations of all the firms with available returns data over the period.
The cumulative size-decile adjusted return to each firm is defined as

\[ CR_{si} = CR_i - CR_{di} \]

where

- \( CR \) = cumulative return over the period,
- \( i \) = variable denoting each ASX listed firm,
- \( di \) = variable denoting the size-decile of which firm \( i \) is a member,
- \( si \) = variable denoting firm \( i \)'s decile adjusted return, and
- \( CR_{di} \) = the average cumulative return to firms within the size decile portfolio of which firm \( i \) is a member.

The portfolios of (size-based) deciles comprise firms which (a) have the relevant share price data available over the whole specified period(s) and (b) have the relevant data available to calculate their market capitalisation.

Market capitalisation for each firm is defined as

\[ MCAP_i = FPO_i \times P_{-2i} \]

where

- \( FPO_i \) = number of firm \( i \)'s fully paid ordinary shares on issue in the month preceding the specified period (e.g., for the period March 1977 to October 1979, the number of FPO shares on issue as at February 1977) and
- \( P_{-2i} \) = firm \( i \)'s last quoted share price on the second month preceding the specified period (e.g., for the period March 1977 to October 1979, the last quoted share price as at January 1977).

NB: The non-synchronous drawing of data used in the calculation of each firm's market capitalisation was deliberately designed in an attempt to avoid biases arising in measures of market capitalisation. The results obtained are not significantly affected by using synchronous dates.

Firms are assigned to their size-deciles on the basis of their market capitalisation, as defined above. Each firm remains within its assigned size-decile for the course of the specified period.
Adaptations Occasioned by Missing Data

The CRIF SPPR database does not comprise a complete record of monthly share market prices for all listed firms which have traded on the ASX since 1974. Some firms have share price or other data missing for some months. The distribution of the missing data is not random. Casual observation reveals that firms are more likely to have missing share price values in months in which a rights issue or dividend payment was made. Periods leading to a capital reorganisation or name change are also more likely to have missing data. Finally, the probability of observing missing data is higher for smaller, less liquid companies.

The non-random nature of the distribution of missing data in the CRIF SPPR database reduces one's confidence that any market indices constructed solely from the SPPR database would provide an adequate or unbiased control for the returns of sample companies. In order to ameliorate the impact of this problem the CRIF SPPR database is augmented with sharemarket data from the ASX's STATEX database.

The procedures followed to augment the SPPR database with data from the ASX STATEX database are described below. It is the first time that such an exercise has been attempted. The task has not proved to be trivial in execution.

Merging the CRIF SPPR and ASX STATEX Databases

(1) The first step is to match the SPPR companies with the corresponding STATEX companies. One hurdle is the difference in conventions adopted by CRIF and the ASX when companies change their names.

CRIF utilises a three to six character code based on the ASX code to identify securities in the SPPR database. "The three character code exactly mirrors the Exchange designation, whilst the six character code has been developed by CRIF to flag reused codes. For example, on 1st July 1981 Ansett Transport Industries shares were delisted from the boards of the Australian Stock Exchange. Its code at that time was "ati". On 11th June 1987 this code was assigned to Atlantic Limited when its shares were added to the Exchange boards. The former records for Ansett Transport Industries have subsequently been stored in the SP&PR file with a code of "ati--1" [CRIF Description of the SPPR database].

The records for each company listed in the SPPR database include two fields which
enable one to link forward or backwards to the codes of companies with name changes or to the codes of subsequent security types. "For instance "eld--1", Elder Smith Goldborough Mort Limited's name was changed in February 1982 to Elders IXL Limited, with code "exl". "exl" is recorded in the link forward field for "eld--1 ... The link back field for "exl", Elders IXL Limited, contains the code "eld--1" for Elder Smith Goldsborough Mort Limited" [CRIF Description of the SPPR database].

A list of all the companies' entries in the SPPR database which includes their full names, CRIF based tag and links to the forward and backward CRIF codes is generated. Note that on this list a company with three name changes will appear three times since each time the company changes its name a new company entry is generated on the SPPR database. This list provides the source data for another list which records the sequence of name changes for each company. The sequence begins with the company's latest three to six character CRIF identifier and ends with the earliest. On this second list, the company which changed its name three times appears just once. The creation of this list enables a company's share history to be traced continuously since it first listed despite the company having made numerous name changes.

The ASX STATEX database does not have a convention in place which enables a sequence of company name changes to be traced directly from its records. Data for the same company under its various names are stored in separate entries without any obvious links which could facilitate a connection between the entries. Companies are identified either by their current three letter ASX code or a number. One way that a STATEX company's complete share history since listing could be tracked is by matching the company with its corresponding SPPR entry and then tracing the SPPR entry's name changes.

Matching the SPPR and STATEX companies is done by comparing the company long names recorded in each database. While a computer program is able to locate exact matches and assist in the identification of approximate matches, "eye-balling" the data is the most effective method for matching companies across the two databases.

(2) After matching companies listed in both the SPPR and STATEX databases, the next step is the design of a protocol to merge the data from the two sources. Descriptions of the relevant data and the protocol follow.

**The SPPR Database**
The SPPR database contains, for each company, the share price, number of shares on
issue and adjustments for dividends, share issues and reconstructions for fully paid ordinary shares on a monthly basis. The share price recorded for a given month is meant to be the last available price for that month. If a price is not available the CRIF convention is to carry over the share price that was recorded in the previous month. In some instances, a "stale" share price is recorded over several consecutive months. The protocol adopted in this thesis treats a given month’s share price as valid only if the share traded at that price during the same month. The date field which indicates the date on which the share traded at the recorded price is used to check the validity of the price.

The STATEX Database

The STATEX database is extensive and stored at The University of Western Australia in the form of several Ingres database tables. The tables accessed for the purposes of this thesis include the "MonthlyPM", "MonthlyPN" and "Events" tables. The MonthlyPM table includes, for each company in a given month, fields which record the highest and lowest prices, the last price recorded, and the adjustment factor for capitalisation changes. The MonthlyPN table includes, for each company in a given month, fields which record capital dilution values, dividends per share, and volume of shares traded. Three caveats must be noted: (a) The share price data in the STATEX MonthlyPM and MonthlyPN tables are quoted in cents as opposed to the tenths of a cent basis used in SPPR. (b) The adjustment factor for capitalisation changes has to be divided by 1000 and the anti-log of the resulting number obtained in order to make it comparable to the adjustment factor found in the SPPR database. (c) Data for a maximum of only 270 months are available for each company. In the edition of the STATEX database used in this thesis the MonthlyPM and MonthlyPN tables cover the period March 1967 to September 1990.

The Events table in STATEX records firms' capital issue related events as they occur, e.g., new issues of shares, dividend payments, the coupon rate or interest rate on the face value of a fixed interest security. Our interest in this table is in working out the number of fully paid ordinary shares that are on issue in a given month for a given company. One complication is that every time a company issues additional capital, a new security number is created and all information relating to the latest issue, such as the total number of shares issued, is stored under the new security number. In some instances, the new security number subsumes the information stored under earlier issue numbers but not invariably. It has proved difficult to design an algorithm that would access the information in the Events table and sieve and combine the data so as to consistently yield the total number of fully paid shares that were on issue in any given month.
The most consistent algorithm, in terms of yielding an accurate estimate of the total number of fully paid shares issued by a given company in any given month, is one that focuses on two event codes in the Events table: "AI" indicates the total number of fully paid shares issued by a company while "@i" in the Events table indicates an increase in the total number of shares (usually as a result of an issue). The algorithm is described below by reference to company X. The algorithm only processes Events data that relate to fully paid ordinary shares.

(1) Start from the earliest date data is stored in the Events table for company X and scan through the dates until the first appearance of an "AI" value. Store this number as long as it is greater than 80,000. Until a change occurs, treat this number as the number of fully paid ordinary shares issued by company X.

(2) Continue scanning through the dates in the Events table until either an "AI" or "@i" value appears. If the "@i" code appears, add the associated number to the total number of fully paid ordinary shares issued by company X that has been carried forward. Until a change occurs, treat this new total as the number of fully paid shares ordinary shares issued by company X. If the "AI" code appears, replace the figure carried forward for the number of fully paid ordinary shares with the latest "AI" number.

(3) Repeat step (2) until the last date for which events data are available for company X.

Order of Precedence for SPPR and STATEX Data

For months where company data are available on both the SPPR and STATEX databases it is necessary to establish an order of precedence to determine which source will be used to get the data to calculate the required company returns, etc. The order of precedence and the reasons for each ranking of source (where it is not arbitrary) are described below.

Share Prices: Use the STATEX share price. If no STATEX price, use the SPPR price. The reason for this ranking is the impression, based on casual observation, that the STATEX prices are based on trades which took place closer to the end of the month than the trades on which the SPPR prices are based.

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AI values are not invariably accurate. The earliest recorded AI values for some companies imply that these companies' total number of fully paid issued shares were as few as 10!. Establishing a minimum value of 80,000 shares for the first AI value offers some safeguard against such absurd values being accepted.
Dividends: Use STATEX dividend value. If no STATEX dividend, use the SPPR dividend value if one is recorded. This protocol might give the impression that the STATEX database does not always record dividend payments when they are made. No instance was observed when a dividend was available on SPPR but not on STATEX. However, several instances were noted when STATEX recorded a dividend for a company but no data appeared at all in SPPR.

Capital Adjustments: Use SPPR adjustment and if it appears also use the SPPR share price since the adjustment would be based on that share price. If a capital adjustment value appears in STATEX but not in SPPR than accept the STATEX adjustment.

Number of Shares on Issue: If data for the Number of Shares on Issue are available on both SPPR and STATEX, take the higher value regardless of source. The reason for this rule is that both STATEX and SPPR occasionally have a lag in their recording of a share issue. Sometimes the STATEX lag is longer, other times the SPPR lag is longer.

Dividend Imputation Credits: Dividend imputation came into effect on July 1987. The last date on which a takeover bid was made in the sample of 1440 takeover bids analysed in the thesis is June 1985. This means that the difficulties in calculating share returns introduced as result of dividend imputation are not an issue for the results reported in the thesis. However, the implications of dividend imputation for calculations of share returns post July 1987 will need to addressed in any extensions of the research which cover periods beyond July 1987. It is a difficult problem to solve in an ideal fashion. At present we are unable to identify which dividends are franked in the share price data available within the Department of Accounting and Finance. Further, even if we are able to identify the franked dividends, we then need to address the problem of how much to "gross up" the declared dividends in calculating rates of return. Alex Clarke and Philip Brown, who are both within the Department of Accounting and Finance, have considerable expertise on this problem and the author of the thesis will discuss the issues
involved with them when extending the present research to incorporate periods beyond July 1987. There is a possibility that ignoring dividend imputation will not significantly bias the calculation of relative rates of return but this, of course, depends on the nature of the event being investigated and the characteristics of the sample firms.
CHAPTER FOUR
EVENT STUDY RESULTS

4.0 INTRODUCTION

Chapter Three described the event study research method and identified issues of concern. The main point made is that operational characterisations of market returns display systematic regularities that are not predicted by models of perfect and complete markets. These regularities must be controlled in order to obtain unbiased estimates of the impact of corporate events on share prices.

Chapter Four summarises and discusses the share market based findings from this thesis. It begins with a review of the salient features of the returns to ASX listed firms over the period January 1974 through December 1990. The primary aim of the review is to identify regularities in share returns over this period and discuss the implications of the regularities for our analysis of the returns to bidding and target firms. For example, the finding of significant positive asymmetry in distributions of returns implies that the median return in the distribution of sample firms returns might be a more appropriate performance metric than the mean return because the median return is less influenced by extreme, atypical values which are skewed in one direction. A fringe benefit of the review is that the robustness over time of the findings from earlier investigations of Australian share returns may also be tested.

Following the above review, the takeover related returns to BDO's and MR's samples of bidder and target firms are described and discussed.

4.1 ASX SHARE RETURNS 1974-1990: REGULARITIES AND IMPLICATIONS

Important findings from an review of the returns to ASX listed firms over the period January 1974 to December 1990 include that previously reported regularities in share returns such as the "January effect", the "July effect", the "firm-size effect" and positive asymmetry in returns are robust with respect to survivorship bias and time. The data from the early studies which confirmed the existence of these regularities in the Australian share market do not extend beyond 1984, if that [e.g., Praetz (1973), Officer (1975), Stokie (1982), Brown, Keim, Kleidon and Marsh (1983), Beedles

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1 The decision to review regularities in share returns over the period January 1974 through December 1990 was dictated by the availability of share market data. As noted in Chapter Three, the first month for which share returns data are available is January 1974, the last month for which returns data were available at the time of compilation is December 1990.
The results from this thesis confirm that the observed regularities persisted after publication of the early findings until at least December 1990 - the last month for which data were gathered in this thesis. However, there are qualifications. Details on these issues and other new findings are presented below.

4.1.1 Stock Return Seasonalities

Brown, Keim, Kleidon and Marsh (1983), henceforth BKKM, report the average rate monthly of return to a value-weighted market index and all size-based decile portfolios over the period March 1958 to June 1981. Important findings include (i) that the smallest-firm portfolio shows higher results across all months than do the other portfolios and (ii) that this average monthly premium of about 4 per cent seems about constant across all months. (iii) "For most portfolios, two strong seasonals emerge. December-January and July-August appear to earn consistently higher returns than do other months, with the largest returns in January and July" (pp. 115-18).

As Figure 4.1 and the numbers in Table 4.1 show, BKKM's findings for the years 1958 to 1981 are broadly consistent with the results from a similar investigation over the period 1974-1990 - note the robustness of the January and July-August regularities across time. There are some intriguing differences. The present study does not find a relatively high average return for December; however April and September emerge as months with high average price relatives. Tables 4.2 and 4.3 together indicate that the emergence of April as a month in which firms register, on average, a high price relative is largely a post-1980 phenomenon. Further, a review of the data on which Table 4.3 is based reveals that the post-1980 "April effect" does not appear to be due solely to an exceptional share market performance in April of one particular year. We also observe in Table 4.3 that the average March, April, July and August returns to an equal weighted market portfolio were higher than or equal to the average January return over the period 1981 to 1990. This apparent instability in seasonal "regularities" was also reported in BKKM and adds further weight to their caveat about the invariability of share returns "regularities". For instance, the results here suggest that event study researchers' concerns about the well publicised January effect ought to be extended to the comparatively high returns earned on the ASX every March, April, July and August over the period 1981 to 1990.

Further tests supports the above argument. For each year 1974 through to 1990, the January return to the equal weighted market portfolio are matched, consecutively, with the February, March, April, ..., December return to the same portfolio. The results from applying the Wilcoxon matched-pairs signed-ranks test show that there
Average Monthly Price Relatives 1974-1990

Figure 4.1
Average Price Relatives in Each Calendar Month to Firms in Each Size-Decile Over the Period 1974 to 1990

Table 4.1
Average Price Relatives in Each Calendar Month to Firms in (i) Each Size-Decile and (ii) Value and Equal Weighted Market Portfolios over the Period 1974 to 1990

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Figure 4.2
Average Price Relatives in Each Calendar Month to Firms in Each Size-Decile Over the Period 1974 to 1980

Table 4.2
Average Price Relatives in Each Calendar Month to Firms in (i) Each Size-Decile and (ii) Value and Equal Weighted Market Portfolios over the Period 1974 to 1980

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Figure 4.3
Average Price Relatives in Each Calendar Month to Firms in Each Size-Decile Over the Period 1981 to 1990

Table 4.3
Average Price Relatives in Each Calendar Month to Firms in (i) Each Size-Decile and (ii) Value and Equal Weighted Market Portfolios over the Period 1981 to 1990

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are only four months for which the average January return to the equal weighted market portfolio is significantly higher at the 5% level. The months are June, October, November, and December. It might be argued that the use of the equal weighted market portfolio rather than a value weighted market portfolio reduces the impact of the January effect since the effect is most pronounced in the smallest firms. However, when the January returns to the firms in the smallest size decile are matched, consecutively, with the February, March, April, ..., December returns to the same size-decile portfolio, similar results obtain using the Wilcoxon matched-pairs test. Consistent with these results, BKKM note the presence of "considerable sampling variation across months ... [such] that the null hypothesis of no difference across months cannot be rejected for portfolio 1 [smallest size decile] at conventional significance levels" (p. 115).

Notwithstanding the above, there is a firm-size effect. In line with BKKM, we find that there is a statistically significant higher average return to firms in the smallest size-decile relative to firms in the other size-decile portfolios across all months over the period 1974-1990.

It is improbable that the exceptionally high average monthly return to firms in the smallest size decile point to the existence of "free lunches". A review of the data reveals, unsurprisingly, that a large proportion of firms move in and out of the smallest size decile in every month which implies that a trading strategy which seeks to capitalise on the exceptional returns to the smallest size portfolio would incur exceptionally high transaction costs. One might consider investing in just a few firms in the smallest size decile in order to minimise transaction costs but it is unlikely to prove a profitable strategy. On average, in each month over the period 1975 to 1990, 29% of firms in the smallest size decile had a negative return. The exceptional average return to the smallest size decile is typically driven in most months by extreme positive returns to a handful of firms. The following analysis supports this proposition. On average, in every month over the period 1975 to 1990, 64% of firms in the smallest size decile had returns lower than the average return to the decile. More revealingly, on average, in every month over the same period 55% of firms in the smallest size decile had returns lower than the return to an equal weighted market portfolio (which includes firms from all size deciles).

The facts suggest that to capitalise on the high average return to firms in the smallest size decile one has to invest in virtually every firm in the decile and be prepared to incur high transaction costs. The strategy is unlikely to consistently yield high

2 A reminder: Each firm's market capitalisation at month t is defined as the product of its share price at month t-2 and the number of shares on issue at month t-1. See Appendix A for more details.
positive abnormal returns (or even consistent "normal" returns). This is because the capital base of firms in the smallest size decile is very low so even high monthly returns translate into small absolute dollar gains. One indication of the relatively low market capitalisation of firms in the smallest size decile is that in January 1985, the ratio of the market capitalisation of the largest firm in the smallest size decile to the market capitalisation of the smallest firm in the largest size decile was one over ninety one (i.e., 0.01). There were 86 firms in each decile in that month.

4.1.2 The Impact of Survivorship Bias

We noted in Chapter Three that the impact of survivorship bias on market index returns is an empirical issue. This section reports the results from an empirical investigation into the bias.

As discussed earlier, not controlling for survivorship will bias event study results if the returns to firms that do not survive differ systematically from the returns to surviving firms. Assuming there is a systematic difference, the bias manifests in the following way: The usual or "traditional" method of estimating one plus the return to the market portfolio is to sum the natural log of one plus the monthly return to the market and obtain the exponent of the sum; that is, the market return is defined as the sum of the natural logs of a series of short-term returns to the market. The market return over each month is defined as the mean of all listed firms' returns in each month. Expressed alternatively, the value weighted monthly return to the market portfolio is:

\[
R_{\text{mkt},t} = \frac{N_t}{\sum_{i=1}^{N_t} \frac{\text{MCAP}_{it}}{\sum_{j=1}^{N_t} \text{MCAP}_{jt}}} \times \left( \frac{\sum_{i=1}^{N_t} \text{R}_{it} \times \text{MCAP}_{it}}{\sum_{j=1}^{N_t} \text{MCAP}_{jt}} \right)
\]

where \(N_t\) is the number of firms in the market with a return in month \(t\) and MCAP stands for market capitalisation. The return to the market portfolio from month \(t_1\) to month \(t_n\) is:

\[3 \quad \text{In this instance, the shortest period for which a return may be calculated is assumed to be one month. The following argument is general and applies even if returns are available for periods shorter than one month. The only assumption is that returns over longer periods are defined as the sum of the natural log returns over the smallest unit period for which a return may be calculated.}\]

\[4 \quad \text{The "traditional" aspect of the market return definition that is of interest is not the weight attached to each firm's return. The aspect of interest in the traditional definition of market return is the implicit assumption that the incidence of survivorship in the market portfolio is similar to the incidence of survivorship in the sample firms or that, if there is a difference, the difference is immaterial.}\]
\[ R_{\text{mkt},t_1}^{t_n} = \exp \left( \sum_{t=t_1}^{t_n} \ln [1 + R_{\text{mkt},t}] \right) - 1. \]

Note that the returns to firms delisted or first traded sometime after month \( t_1 \) but before \( t_n \) will be reflected in the return to the market portfolio over the period \( t_1 \) to \( t_n \). If the returns to "survivor" firms, that is, firms which were listed over the full period, differ systematically from the returns to the "non-survivors", the two groups' market-adjusted returns will also systematically differ. 

The existence and severity of survivorship bias may be tested by comparing a distribution of, say, 6 month returns to the market portfolio calculated in the "traditional" way (i.e., as defined above) with a distribution of the same 6 month returns to the market portfolio with the qualification that the market portfolio is defined as those firms which were listed for the full 6 month period. Any systematic difference in the returns to survivors and non-survivors will show up as a significant difference in the two distributions of returns.

The above test is operationalised as follows: (i) For the period extending February 1974 to January 1991, the return to the value-weighted market portfolio is calculated for each month, that is, a set of 204 monthly price relatives is generated. (ii) The return to the value-weighted market portfolio over the period from the beginning of February 1974 to the end of July 1974 is calculated in the "traditional" way, that is, the natural logs of the market portfolio's value-weighted price relative in each of the 6 months are summed and the exponent of the sum obtained. (iii) The traditional return estimated in step ii is matched with the cumulative return, based on just "surviving" firms, to the value-weighted market portfolio over the period from the beginning of February 1974 to the end of July 1974. (iv) Steps ii and iii are repeated after rolling forward the returns estimation period by 6 months (e.g., the second pair of 6 month returns are estimated for the period August 1974 to January 1975). Step iv is repeated until we roll forward to the last 6 months for which a market return is available (i.e., the period August 1990 through January 1991).

The above procedure yields 34 matched pairs of 6 month returns to the market portfolio. In applying the Wilcoxon matched-pairs signed-ranks test to determine if there is any significant difference between the two distributions we find that the 6 month value-weighted return to the market portfolio calculated by the traditional

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5 Given that "non-survivors" in the present context include firms which are listed after month \( t_1 \) and remain listed until at least month \( t_n \), using the term "non-survivors" to identify them is less than fully apt. Nevertheless, it conveys the sense that these firms were not listed over the complete period of interest.
The Z-score is -2.8637 which indicates that the two-tailed probability that the two samples of returns were drawn from the same distribution is 0.0042. Pertinently, the significant statistical difference between the two samples of 6 monthly returns is driven not by a few large differences in the values of the 34 matched pairs of returns but rather by the small but more or less consistent (in direction) difference between the "traditional" returns and the "survivor" based returns. Over the 34 6-month periods for which market returns are calculated, the mean return calculated on a "traditional" basis is 9.1% while the "survivor" based returns average 8.4%. The lowest market return is recorded for the period August 1987 to January 1988: -31% on a "traditional basis" and -35% on a "survivor adjusted" basis. The highest return is recorded for the period August 1979 to January 1980: 52% on a "traditional" basis and 50% on a "survivor adjusted" basis.

We may suppose that the influence of survivorship bias increases over longer time periods because longer time-periods entail larger differences in the composition of market portfolio when it is defined as comprising just survivors and when it includes all firms listed for any time over the period of interest. To confirm this conjecture, 24-month value-weighted returns to the market portfolio are estimated following the protocol described above. Once again, there is a strong statistical difference between the distribution of returns calculated in the "traditional" way and the distribution of returns based on just survivors. In 7 of the 8 matched pairs of 24-month value weighted adjusted returns to the market portfolio, the survivors based return is lower than the return calculated by the "traditional" methods. As predicted, the difference in the mean values of the two distributions of 24 month returns is larger than the difference in mean values based on 6-month returns. The mean value in the distribution of "traditional" returns is 43.5% while the average value in the distribution of "survivor" based returns is 41.2%.

The unambiguous implication of the above results is that there is a systematic difference in the returns to firms which remain listed on the ASX for lengths of time equalling or exceeding 12 months (i.e., "survivors") and those firms which are either newly listed or delisted from the ASX. The evidence supports the proposition that, on average, non-surviving firms experience a higher return than survivors.

It is unlikely that the higher return, on average, experienced by non-surviving firms is driven by the return to newly listed firms.

6 Application of the Wilcoxon matched-pairs test indicates a two-tailed probability of 0.0929 that the "traditional" and "survivor adjusted" 24-month period returns to the market are drawn from the same distribution.
How (1993) reports evidence consistent with the proposition that newly listed firms in Australia, that is, Australian firms making their initial public offering (IPOs), earn negative abnormal returns, on average, over the first 20 trading days after their first day of trading. How's sample comprises 530 firms that went public during the eleven and a half year period from July 1979 to December 1990. Her results are consistent with those reported by Ritter (1991) who analyses the relative performance of 1,526 IPOs that went public in the U.S. over the period 1975 to 1984. Ritter finds that the performance of his sample firms significantly underperformed a set of comparable firms matched by size and industry in the 3 years after going public.

Given the evidence that, on average, IPOs experience a negative return after listing, the higher return to non-surviving firms relative to surviving firms must be driven by an exceptionally high return to delisted firms.

The information in Table 4.4 strongly suggests that, in fact, firms which delist from the ASX experience, on average, strongly positive abnormal returns. Over 40% of the firms which exit from the ASX boards do so as a result of a takeover or merger. Mergers and takeovers are, as revealed in Table 4.4, by far the most common reason why companies are delisted from the ASX boards. Company name changes, which account for just under 30% of company name delistings are the next most common category. However, when we consider that our survivor-adjusted portfolios includes companies which remained listed after changing their corporate name (i.e., "pseudo" delistings), the proportion of "real" delistings accounted for by mergers and takeovers rises even higher.

The reason why the net effect of excluding firms that delist results in a decrease in the recorded mean return to the market portfolio is that, typically, the large proportion of firms that delist as result of takeover experience remarkably high positive abnormal returns in the months leading to their eventual delisting. These high returns more than compensate for the downward pressure on the mean return to the market portfolio attributable to those firms which delist because of bankruptcy or business failure. Further, while the shares of target companies are usually heavily traded so that the high returns earned are registered on the ASX prices prior to delisting, the shares of companies which are delisted because of bankruptcy or business failure are not.

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7 A reminder that non-surviving firms are idiosyncratically defined in this thesis as newly listed and delisted firms.
8 In Australia, the terms merger and takeover are generally used interchangeably.
9 Anecdotal evidence suggests that many company name changes are associated with corporate reorganisations which frequently include company expansion through the acquisition of new lines of business. In other words, company name changes are often prompted by activities which resemble the essential aspects of takeovers and mergers.
Table 4.4
ASX Delistings 1920 - 1989: Reasons and Frequencies

<table>
<thead>
<tr>
<th>Reason for Delisting</th>
<th>Frequency</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Stock exchange order</td>
<td>84</td>
<td>1.83</td>
</tr>
<tr>
<td>Fail to pay listing fee</td>
<td>94</td>
<td>2.05</td>
</tr>
<tr>
<td>Fail to issue accounts</td>
<td>23</td>
<td>0.50</td>
</tr>
<tr>
<td>Non-compliance with listing</td>
<td>17</td>
<td>0.37</td>
</tr>
<tr>
<td>requirements</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Insufficient spread of</td>
<td>81</td>
<td>1.76</td>
</tr>
<tr>
<td>shareholdings</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fail to issue accounts &amp; pay listing</td>
<td>6</td>
<td>0.13</td>
</tr>
<tr>
<td>fees</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Combination of above</td>
<td>4</td>
<td>0.09</td>
</tr>
<tr>
<td>Company request/ Director's Request</td>
<td>214</td>
<td>4.66</td>
</tr>
<tr>
<td>Shareholder resolution</td>
<td>4</td>
<td>0.09</td>
</tr>
<tr>
<td>Trust/Option holder request</td>
<td>5</td>
<td>0.11</td>
</tr>
<tr>
<td>Appointment of liquidator</td>
<td>3</td>
<td>0.07</td>
</tr>
<tr>
<td>Liquidator request</td>
<td>26</td>
<td>0.57</td>
</tr>
<tr>
<td>Receiver/Manager request</td>
<td>10</td>
<td>0.22</td>
</tr>
<tr>
<td>Scheme of Arrangement</td>
<td>60</td>
<td>1.31</td>
</tr>
<tr>
<td>Wound-up/Liquidation</td>
<td>405</td>
<td>8.81</td>
</tr>
<tr>
<td>Voluntary liquidation</td>
<td>25</td>
<td>0.54</td>
</tr>
<tr>
<td>Winding-up order</td>
<td>3</td>
<td>0.07</td>
</tr>
<tr>
<td><strong>Merger</strong></td>
<td>93</td>
<td>2.02</td>
</tr>
<tr>
<td><strong>Takeover</strong></td>
<td>1852</td>
<td>40.30</td>
</tr>
<tr>
<td><strong>Assets acquired/Absorbed</strong></td>
<td>5</td>
<td>0.11</td>
</tr>
<tr>
<td><strong>Name Change</strong></td>
<td>1353</td>
<td>29.44</td>
</tr>
<tr>
<td>Change in Nature of the Entity</td>
<td>4</td>
<td>0.09</td>
</tr>
<tr>
<td>(e.g. privatised)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share Exchange</td>
<td>2</td>
<td>0.04</td>
</tr>
<tr>
<td>Court order</td>
<td>5</td>
<td>0.11</td>
</tr>
<tr>
<td>Request by Other</td>
<td>3</td>
<td>0.07</td>
</tr>
<tr>
<td>Cessation of trading</td>
<td>1</td>
<td>0.02</td>
</tr>
<tr>
<td>Failure to carry out operations</td>
<td>2</td>
<td>0.04</td>
</tr>
<tr>
<td>Maturity of options/debentures</td>
<td>27</td>
<td>0.59</td>
</tr>
<tr>
<td>Repayment of Capital</td>
<td>13</td>
<td>0.28</td>
</tr>
<tr>
<td>Reconstruction or</td>
<td>41</td>
<td>0.89</td>
</tr>
<tr>
<td>Reorganisation of Company</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Relisted</td>
<td>33</td>
<td>0.7</td>
</tr>
<tr>
<td>Share conversion</td>
<td>5</td>
<td>0.11</td>
</tr>
<tr>
<td>Closure of Aust'n. Share Registry</td>
<td>6</td>
<td>0.13</td>
</tr>
<tr>
<td>Redemption (early)</td>
<td>22</td>
<td>0.48</td>
</tr>
<tr>
<td>Nationalised</td>
<td>5</td>
<td>0.11</td>
</tr>
<tr>
<td>Other reason</td>
<td>12</td>
<td>0.26</td>
</tr>
<tr>
<td>No Official Reason</td>
<td>48</td>
<td>1.04</td>
</tr>
</tbody>
</table>

**Total** 4596 100.00%

Source: Data compiled from the Australian Stock Exchange delisting records by Gavin Martin of the Department of Accounting and Finance, The University of Western Australia, 1993.
similar reasons are typically suspended or do not experience recorded trades. The point is that firms which delist have a greater chance of having their returns on record if these returns are positive than if the returns are negative.

The methodological implications of the last point made should be emphasised. The evidence of survivorship bias documented in this thesis does not support or imply the existence of market inefficiency in the sense that persistent opportunities to make abnormal gains exist or that obviously sub-optimal trading strategies characterise behaviour of traders in the share market. Rather, the evidence is consistent with the existence of an incomplete market\textsuperscript{10} for shares in some circumstances, in particular, circumstances where a "shock" event affects listed firms. It is likely that the risk of such 'shocks' occurring is incorporated or reflected in the prices of shares and what might appear to be abnormal returns to a particular stock or trading strategy might simply be the normal return for bearing such risk.

The body of evidence and argument reported in this section constitutes a significant contribution of this thesis. To date, no study has reported or even posited survivorship bias as a component of the explanation for the well publicised secular decline in the post-bid and post-outcome cumulative abnormal returns to successful bidding firms. Portfolios comprising bidding firms are also typically portfolios which comprise firms having a high probability of survival. However, the market portfolios against which the performance of the sample "bidder portfolios" are compared typically constitute firms with lower survival rates, on average. The evidence presented in this section suggests that under this regime, a post-bid secular decline in the estimated cumulative abnormal returns to the bidder portfolios is the expected result\textsuperscript{11}. This issue is discussed further in Section 4.2.

4.1.3 Asymmetry in Equity Returns

Stokie (1982) and Beedles (1986) confirm the existence of persistent asymmetry in Australian share returns. The pervasiveness of returns asymmetry in Australian share returns is such that Beedles (1986) is moved to conclude with the admonition that "in light of the normative and empirical support that exists, attention to asymmetry would appear to be properly a matter of routine" (p. 44).

Positive asymmetry in equity returns implies that relying solely on the first and second moments to evaluate portfolio returns can be inappropriate. The mean and

\textsuperscript{10} Beaver (1989, p. 50) defines "complete markets" as those where the price for any commodity or claim is publicly observable.

\textsuperscript{11} Another implication of this line of reasoning is that the positive pre-bid abnormal returns to bidder portfolios recorded by most studies are also downwards biased.
variance no longer describe completely the distribution of returns to portfolios and exclusive reliance on them could lead to inaccurate inferences about performance. For instance, one of the measures BDO use to assess the performance of firms involved in takeover activity is the proportion of positive CARs achieved by their sample of firms over various periods (e.g., the proportion of bidding firms which had positive CARs over the period [-3,+3] months relative to the bid announcement month). In BDO's analysis, the implicit assumption is that the expected proportion under the null hypothesis of no abnormal performance is fifty percent. However, positive asymmetry in the return distributions of individual firms implies that the mean return to the firms comprising the market portfolio will be higher than the median return. Other things being equal, we may expect less than 50% of firms will, over any given period, achieve an adjusted return higher than the market return (where the market return is defined as the mean of its constituent firms' returns). Generalisations about the performance of sample firms based solely on the sign or magnitude of their estimated abnormal return are thus likely to be invalid since the modal abnormal return under the null hypothesis of no extra-ordinary performance is other than zero.

The benefit of assessing the significance of the experimental sample portfolio's returns by comparing them against an empirical distribution of portfolio returns is that this procedure addresses the above concerns. The control portfolios' returns incorporate any non-event related regularities or patterns in returns so an unbiased assessment of performance may be done by comparing the performance metrics of the sample portfolio with those of the empirical distribution. If a sufficiently large number of control portfolios are constructed, the law of large numbers ensures the empirical distribution of returns approximates the normal and standard parametric tests of significance may be applied.

Notwithstanding the above, the extent of asymmetry in Australian share returns is such that the empirical distribution of the returns to 1001 portfolios each comprising

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12 Agrawal, Jaffe and Mandelker (1992) make the same assumption when commenting on the negative drift in post-merger returns to bidder firms. Their estimate of the cumulative average abnormal return (CAAR) to their sample of bidder firms over the 5 year period after merger is -10.26%. They conclude "these results are not driven by merely a few outliers. The percentage of positive abnormal returns over the five-year period is 43.97%, which is significantly lower than 50 (z = - 3.03). Pervasive positive asymmetry in U.S. share returns is well documented [Beedles (1986)].

13 This is true for both equal weighted and value weighted portfolios and for the size-decile portfolios. Incidentally, positive asymmetry in equity returns is not restricted to smaller or larger firms. For each of the 192 months over the period January 1974 to December 1990 the number of positive size-decile adjusted returns in each size-decile was matched with the number of negative size-decile adjusted returns. A Wilcoxon matched-pairs signed ranks test revealed that across all size-deciles, the proportion of negative adjusted returns was significantly higher (at the 1% level) than the proportion of positive adjusted returns.

14 Other things being equal, the larger the number of firms in each portfolio the less the number of portfolios that need to be constructed for the empirical distribution of returns to approximate the normal (assuming that the Central Limit Theorem holds).
randomly selected firms is not normally distributed. This may be observed from Figure 4.4 (b) which illustrates the empirical distribution of one period returns generated by the following protocol. (i) The set \( \{B\} \) of bidding firms which have a return available for the period \([-24, -6]\) months relative to their bid announcement date is identified (492 bidders in BDO's sample met this criterion). (ii) For each bidder, the set \( \{R\} \) of returns to all listed firms over each bidder's event-window is identified (i.e., 492 sets of \( \{R\} \) are identified). (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the (492) sets of \( \{R\} \) so identified. (iv) The return to each control portfolio is defined as the mean of its constituent returns. Using the Kolmogorov-Smirnov one-sample test of goodness of fit, the probability that the distribution of mean returns to the 1001 portfolios approximates the normal is less than 1 in 10,000.\(^{15}\)

The reason for the distribution of mean returns not approximating the normal is the magnitude of the positive asymmetry in the returns distributions of individual firms. Over any given 16 months a few firms record an extreme positive return. The majority of the 1001 portfolios comprising randomly selected firms include a proportionate number of these firms. A minority of the 1001 portfolios include a disproportionate number of the exceptional performers. The portfolios which include a less than proportionate number of extreme positive returns achieve a mean return that is less than the average mean return to the 1001 portfolios. However, because there are relatively few extremely poor performers in any of the portfolios their deviation from the average mean return is less than the deviation achieved by those portfolios which, by chance, include a disproportionately high number of extreme positive returns. The outcome is a distribution of the mean returns to the 1001 control portfolios that is positively skewed.

Using a non-normal empirical distribution of control portfolio returns to assess the significance of the experimental portfolio return is problematic. If the sample firms record a return much below the median return in the empirical distribution, the presence of positive asymmetry in the empirical distribution increases our confidence that the null hypothesis may be rejected.\(^{16}\) On the other hand, the\(^{15}\) Note that adjusting the returns to the firms by subtracting the market return (calculated either on a value weighted or equal weighted basis) will not affect the shape of the distribution since this procedure simply moves the whole distribution of returns leftwards. We have also established that positive asymmetry is prevalent across all size deciles so we may expect the distribution of size-adjusted returns to be non-normal. For confirmation see Figures 4.5 (a), 4.6 (a), and 4.7 (a) which illustrate the returns distributions to the same 1001 portfolios calculated on a market (equal weighted) return adjusted basis, a market (value weighted) return adjusted basis and a size-decile return adjusted basis.

\(^{16}\) One caveat: Beedles (1986) reports that while positive asymmetry in the distribution of firms' returns predominates, negative asymmetry is not rare. About 6 to 7% percent of firms have returns distributions that are significantly (at the 5% level) negatively skewed. It is possible that the experimental sample firms will coincidentally include a higher than proportionate percentage of such firms. However, the evidence cited by Beedles gives no reason to expect negative asymmetry to
Figure 4.4
Distribution of the Mean Cumulative Return to 1001 Portfolios

(a) Uncensored (non-winsorised) returns data
(492 randomly selected firms in each portfolio)

Mean 69%; Median 68%; Std Dev 9.8%; Kurtosis 1.76; Skewness .972

(b) Winsorised returns data
(490 randomly selected firms in each portfolio)

Mean 55%; Median 55%; Std Dev 4.8%; Kurtosis .141; Skewness .165

The return to each portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set \(B\) of bidding firms which had returns available over the period \([-24,-6]\) months relative to their respective bid announcement months is identified. (ii) For each bidder's event-window, the set \(R\) of returns to all listed firms is identified. (iii) Each of the 1001 portfolios includes a return, drawn independently at random with replacement, from each of the sets of \(R\) identified.

Uncensored (non-winsorised) returns are based on the full set of returns available in each period. Winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the population mean return. Two of BDO's bidders achieved a return greater than 3 standard deviations away from the population mean return over their event-window. This is why set \(B\) in Figure 4.4 (a) has two less members than set \(B\) in Figure 4.4 (b).
Uncensored (non-winsorised) returns data
(492 randomly selected firms in each portfolio)

Returns expressed as the natural log of unity plus the 16 month periodic return.

Mean 52%; Median 51%; Std Dev 5.65%; Kurtosis 1.10; Skewness 0.75
probability of making a Type II error increases, particularly if the expected sign of the adjusted returns to the sample firms is positive.

A number of methods may be used to impose normality on the distribution of returns to the control portfolios. One is to increase the number of control portfolios so that the law of large numbers eventually ensures the generation of a normal distribution. Computing power constraints limit the application of this solution. Generating significantly more than 1001 control portfolios for every event-window for all the different experimental sample portfolios being evaluated remains, at present, computational infeasible given the computing resources available to me.

Expressing the mean portfolio returns in continuously compounded form rather than using the discrete form draws in the extreme observations and is another method of facilitating the generation of a normal distribution [Fama (1976)]. Figure 4.4 (c) displays the distribution yielded when the control portfolio returns are expressed as the natural log of unity plus the 16 month periodic return. As expected, the transformation greatly reduces the distribution's kurtosis and skewness but, consistent with Beedles (1986) results, significant asymmetry still prevails.

Notwithstanding the above, an appealing feature of expressing returns in discrete form rather than in continuously compounded form is that discrete returns lend themselves to more intuitively accessible interpretations. A discrete return of, say, 20% over 3 months implies that an investment of $100 in the relevant portfolio would have grown to $120 at the end of the 3 months. This interpretation of portfolio returns is the most common basis for evaluating relative performance.

In view of the above, the solution adopted in this thesis is to censor (or "winsorise") extreme returns recorded in any period. The justification is as follows. Positive asymmetry in returns distributions is the product of a few extreme positive returns in each period. Asymmetry is prevalent at significant levels in all size-deciles and does not appear associated with any particular type of firm. This implies that the validity of event study results is not affected by censoring extreme returns - either negative or positive - in an effort to impose symmetry on the return distribution of randomly selected portfolios.

It might be argued that the extreme returns censored are caused or related to the event in question. This possibility increases the probability of a Type II error being made, that is, the probability of wrongly accepting the null hypothesis of no significant difference in the returns to the sample firms and randomly selected firms is increased. This issue is of particular concern when reviewing the post-bid
abnormal returns to successful bidding firms. As reported earlier, previous studies have found a significant secular decline in the post-bid returns to successful bidding firms even though the expected result in an efficient market is a finding of no significant post-bid abnormal performance. A key contention of this thesis is that the reported secular decline is likely the product of inappropriate bench-marks being used to adjust the sample firms. It could be argued that censoring extreme returns data - including negative returns - which might be associated with takeover activity inappropriately facilitates the production of results which support the above contention.

As stated earlier, there is no a priori reason to believe that censoring extreme returns will disproportionately affect the returns recorded by bidding firms involved in takeovers. Equally, there are no compelling reasons to believe otherwise. The dilemma is avoided by reporting results obtained from both the complete set of returns data and the winsorised set. Where the two sets of results diverge, further analysis is presented to explain the divergence.

The returns data are censored as follows. In every period for which returns data exists, the population, \( R \), of available returns is identified.\(^{17}\) The standard deviation of the population of returns in each period is calculated and returns that are greater than three (3) standard deviations away from the mean return are deleted. The censored population of returns, that is, the returns less than or equal to three standard deviations from the original population mean return, comprises the new set of returns from which the size-decile and market portfolios are compiled.\(^{18}\)

Figures 4.5 (b), 4.6 (b) and 4.7 (b) describe the returns distributions after the censoring procedure is applied.\(^{19}\) Based on the Kolgomorov-Smirnov one-sample test of goodness of fit, the values underpinning all three distributions are normally distributed although some positive asymmetry remains evident. This last characteristic suggests that the censoring filter employed did not eliminate positive asymmetry in firms' distribution of returns. The following analysis based on the winsorised population of firms' returns confirms this hypothesis. For each of the 192 months over the period January 1974 to December 1990 the number of positive

\(^{17}\) The total number of periods include every combination of consecutive monthly periods between January 1974 and December 1990.

\(^{18}\) Given a normal distribution, deleting values greater than 3 standard deviations away would result in 0.26% of all values being deleted. In the present case, an average of 1.15% of returns were deleted across all periods.

\(^{19}\) Note that winsorising has resulted in the number of firms in each control portfolio being reduced from 492 to 490. Two sample bidder firms were among those firms which recorded a return which deviated by more than 3 standard deviations from the mean return to the market portfolio. This eliminated them from the sample portfolio. In order to provide an unbiased estimate of the gains from takeover, the winsorising procedure has to be applied to both the sample firms and all other firms.
Figure 4.5
Distribution of the Mean Market (Equal Weighted) Adjusted Return to 1001 Portfolios

(a) Original (non-winsorised) returns data
(492 randomly selected firms in each portfolio)

Mean 0.1%; Median -1.1%; Std Dev 9.8%; Kurtosis 1.76; Skewness .972

(b) Winsorised returns data
(490 randomly selected firms in each portfolio)

Mean 0.3%; Median 0.03%; Std Dev 4.8%; Kurtosis .141; Skewness .165

---
a Firm i’s market (equal weighted) adjusted return in period t = firm i’s return in period t less the return in period t to an equally weighted portfolio comprising all listed firms with available data. The mean market (equal weighted) adjusted return to each portfolio is defined as the mean of its constituent market (equal weighted) adjusted returns. Selection procedure: (i) The set \( B \) of bidding firms which had returns available over the period [-24,-6] months relative to their respective bid announcement months is identified. (ii) For each bidder’s event-window, the set \( R \) of returns to all listed firms is identified. (iii) Each of the 1001 portfolios includes an adjusted return, drawn independently at random with replacement, from each of the sets of \( R \) identified.
b Original (non-winsorised) returns are based on the full set of returns available in each period. Winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the population mean return. To clarify; In (b), the market portfolio is compiled after the winsorising procedure has been applied. Two of BDO’s bidders achieved a return greater than 3 standard deviations away from the population mean return over their event-window. This is why set \( B \) in Figure 4.5 (a) has two less members than set \( B \) in Figure 4.5 (b).
Figure 4.6
Distribution of the Mean Market (Value Weighted) Adjusted Return to 1001 Portfolios

(a) Original (non-winsorised) returns data (492 randomly selected firms in each portfolio)

Mean 29.8%; Median 28.6%; Std Dev 9.8%; Kurtosis 1.760; Skewness .972

(b) Winsorised returns data (490 randomly selected firms in each portfolio)

Mean 16.2%; Median 16.2%; Std Dev 4.8%; Kurtosis .141; Skewness .165

a Firm i's market (value weighted) adjusted return in period t = firm i's return in period t less the return in period t to a value weighted portfolio comprising all listed firms with available data. The mean market (value weighted) adjusted return to each portfolio is defined as the mean of its constituent market (value weighted) adjusted returns. Selection procedure: (i) The set \( B \) of bidding firms which had returns available over the period \([-24, -6]\) months relative to their respective bid announcement months is identified. (ii) For each bidder's event-window, the set \( R \) of returns to all listed firms is identified. (iii) Each of the 1001 portfolios includes an adjusted return, drawn independently at random with replacement, from each of the sets of \( R \) identified.

b Original (non-winsorised) returns are based on the full set of returns available in each period. Winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the population mean return. To clarify; in (b), the market portfolio is compiled after the winsorising procedure has been applied. Two of BDO's bidders achieved a return greater than 3 standard deviations away from the population mean return over their event-window. This is why set \( B \) in Figure 4.6 (a) has two less members than set \( B \) in Figure 4.6 (b).
Figure 4.7
Distribution of the Mean Size-Decile Adjusted Returns to 1001 Portfolios

(a) Original (non-winsorised) returns data
(492 randomly selected firms in each portfolio)

Mean 0.1%; Median -1.1%; Std Dev 9.4%; Kurtosis 1.958; Skewness .998

(b) Winsorised returns data
(490 randomly selected firms in each portfolio)

Mean 0.4%; Median 0.2%; Std Dev 4.6%; Kurtosis .082; Skewness .132

---

a Firm i's size-decile adjusted return in period t = firm i's return in period t less the return in period t to the size-decile of which firm i is a member. The mean size-decile adjusted return to each portfolio is defined as the mean of its constituent size-decile adjusted returns. Selection procedure: (i) The set \( \{B\} \) of bidding firms which had returns available over the period [-24,-6] months relative to their respective bid announcement months is identified. (ii) For each bidder's event-window, the set \( \{R\} \) of returns to all listed firms is identified. (iii) Each of the 1001 portfolios includes an size-decile adjusted return, drawn independently at random with replacement, from each of the sets of \( \{R\} \) identified.

b Original (non-winsorised) returns are based on the full set of returns available in each period. Winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the population mean return. To clarify; in (b), the size-decile portfolios are compiled after the winsorising procedure has been applied. Two of BDO's bidders achieved a return greater than 3 standard deviations away from the population mean return over their event-window. This is why set \( \{B\} \) in Figure 4.7 (a) has two less members than set \( \{B\} \) in Figure 4.7 (b).
size-decile adjusted returns in each size-decile is matched with the number of
negative size-decile adjusted returns. A Wilcoxon matched-pairs signed ranks test
reveals that across all size-deciles, the proportion of negative adjusted returns is
significantly higher (at the 1% level) than the proportion of positive adjusted returns.

Notwithstanding the above, in the absence of any guide as to how fine the filter for
extreme returns ought to be it might be argued that eliminating returns in excess of
three standard deviations from the population mean return is fine enough. Censoring
returns any less than three standard deviations from the mean increases the risk of
censoring returns that were associated with the event of interest. The next section
includes a description of the method used to control for the confounding effect of the
residual positive asymmetry in the distribution of firms' returns.

4.2 POSITIVE ASYMMETRY AND TESTS OF SIGNIFICANCE

Section 4.1.3 confirmed the existence of positive asymmetry in returns in the returns
metrics used to assess firm performance and described the procedure employed to
reduce its impact. Section 4.2 outlines the procedure used to estimate the absolute
gains or losses from takeover activity and justifies the various metrics used to assess
the significance of the results in light of the findings in Section 4.1.3. The returns to
BDO's bidding firms over the period [-24, -6] months relative to their bid
announcement month are analysed by way of example. These results are
summarised on Table 4.5 which is standard in its format. All results are based on
winsorised data, unless otherwise noted. Winsorised results are reported in the top
half of Table 4.5

4.2.1 Estimating the Absolute Gains (Losses) from Takeover

If other things were equal, one method of estimating the absolute gain from takeover
activity is by comparing the grand mean return to the 1001 control portfolios with
the mean of the returns to the firms comprising the sample portfolio. The grand
mean of the 1001 control portfolios is an unbiased estimate of the average market
return across the sample firms' respective event-windows. For instance, over the
period [-24, -6] months relative to the 490 bidders' respective bid announcement
dates the estimated average return to the market portfolio is 54.71% (see Table 4.5).
Over the same event-window the 490 bidders earned an average return of 65.20%.
On the assumption that the only systematic significant difference between the market
portfolio and the sample portfolio is the sample firms' involvement in takeover
activity one may infer that bidder firms typically experience exceptional

20 A reminder that all returns are adjusted for survivorship, i.e., the unbiased estimate of the
average market return is not as conventionally defined.
<table>
<thead>
<tr>
<th></th>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios Returns &gt; Sample Return</th>
<th>Sample Firms' Median Return (%)</th>
<th>Median of 1001 Control Median Returns &gt; Sample Median</th>
<th>No. Control Firms' PAR c</th>
<th>Sample Firms' PAR c</th>
<th>Median value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
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<td>54.71</td>
<td>22</td>
<td>42.96</td>
<td>30.73</td>
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<td>0.82</td>
<td>0.75</td>
<td>0</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
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<td>22</td>
<td>-0.60</td>
<td>-14.83</td>
<td>0</td>
<td>0.49</td>
<td>0.39</td>
<td>0</td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>26.74</td>
<td>16.25</td>
<td>22</td>
<td>9.35</td>
<td></td>
<td>0</td>
<td>0.57</td>
<td>0.48</td>
<td>0</td>
</tr>
<tr>
<td>SizeDec Adj</td>
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<td>0.36</td>
<td>0</td>
<td>5.50</td>
<td>-11.89</td>
<td>0</td>
<td>0.55</td>
<td>0.41</td>
<td>0</td>
</tr>
</tbody>
</table>

**Panel A: Censored Returns:** Number of Experimental Sample Firms = 490

**Panel B: Uncensored Returns:** Number of Experimental Sample Firms = 492

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.
b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population (R) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.
c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
performance in the pre-bid period. For our sample of bidder firms, the mean abnormal performance is in the order of 10.49% (= 65.20% - 54.71%) over the period [-24,-6] months relative to the bid announcement month.

The accuracy of the above procedure may be assessed by comparing its result - an average abnormal return of 10.49% to the bidder firms - with the average market (equal weighted) adjusted return to the bidder firms which is 10.78% (see Table 4.5). The market (equal weighted) adjusted return to each bidder firm controls directly for the actual market return over each bidder's event-window. The benefit of the direct control is that accuracy is not dependent on generating a large number of control portfolio returns in order to obtain an accurate estimate of the mean return to the market portfolio over the bidders' respective event-windows. Nevertheless, the difference of 0.29% (=10.78-10.49) in the estimates of the average abnormal return to the sample bidders firms yielded by the two procedures is not attributable to differences in estimation accuracy between them. An explanation follows.

A market (equal weighted) adjusted return is defined as the return to firm i less the mean return to all the firms that survived over the event window. As noted earlier, positive asymmetry in firms' distribution of returns implies that the mean return to the market portfolio will be higher than the median return. One implication is that the distribution of market adjusted returns to portfolios comprising randomly selected firms also has a mean return higher than the median return. The mean value of the distribution of control portfolio adjusted returns is an unbiased estimate of the positive bias in the sample portfolio's adjusted return attributable to positive skewness in firms' distributions of returns. From Table 4.5 we can see that the grand mean of the market adjusted returns to 1001 control portfolios is equal to 0.28%. If we subtract this estimate of the positive bias from 10.78% (the mean market (equal weighted) adjusted return to the sample portfolio), the resulting value 10.50% (=10.78% - 0.28%) is within .01% of the estimate of the mean abnormal return to the sample firms yielded by the first method. The point is that constructing an empirical distribution of returns is the only method to control for both systematic variation in returns attributable to market wide factors and for the positive bias in estimated returns caused by positive skewness in returns.

21 In fact, the mean return to the firms in the market portfolio will be positive while the median return will be negative. This last proposition is true because of the asymmetry and, by construction, the sum of all market (equal weighted) adjusted returns across the market portfolio equals zero.

22 Unless, of course, the number of firms in each portfolio equals the number of firms in the market.

23 It needs to be stressed that the none of the above implies that the attempt to eliminate positive asymmetry in firms' distribution of returns by winsorising extreme values is otiose. The key assumption is that positive asymmetry is a theoretically anomalous component of the distribution of market returns that is not systematically related to takeover activity. Thus eliminating or reducing positive asymmetry in firms' return distributions - including the sample firms' returns distribution - allows a more accurate assessment of the gains from takeover since it reduces an anomalous upward
Notwithstanding the above, market adjusted returns do not yield an unbiased estimate of the absolute gains from takeover because the return to the market portfolio does not control for the systematic negative association between firm size and equity returns. This implies, for instance, that estimates of the abnormal returns to large firms from takeover activity are systematically downward biased.

It might be argued that one benefit in using a value-weighted market index rather than an equal-weighted market index to control for expected return is that the downward bias in estimates of abnormal returns to large firms is mitigated. The problem is, if firm size is associated with equity returns then, unless the size-distribution of the sample firms correlates highly with the size-distribution of the firms comprising the value-weighted index, it is unlikely that the expected abnormal returns sum to zero under the null hypothesis of no event-related impact on performance.

Figure 4.6 (b) illustrates the problem by displaying the distribution of market (value weighted) adjusted returns to the same portfolios whose market (equal weighted) adjusted returns distribution is graphed in Figure 4.5 (b). In Figure 4.6 (b) the return to each control portfolio is defined as the simple average of the market (value-weighted) adjusted return to each firm comprising the portfolio. Firms from all the size-deciles are represented about equally in each control portfolio because of the random selection criterion. Given (i) that firms from the largest-size decile dominate the market (value-weighted) portfolio's return and (ii) that the recorded returns to smaller firms are higher so that they typically record positive market (value-weighted) adjusted returns, it is unsurprising that every single one of the 1001 control portfolios has a positive average abnormal return. In the calculation of the average return to each control portfolio, the positive abnormal returns to the more numerous smaller firms dominate the negative abnormal returns to the few large firms which drive the return to the value-weighted market portfolio.

The existence of the positive bias in market value-weighted adjusted returns, much less estimates of its magnitude, are rarely noted in event-studies employing value-weighted market indexes despite the high probability of the bias significantly influencing results. For instance, the mean market (value weighted) return to BDO's sample of bidders over the period [-24,-6] months relative to the bid announcement date is 26.74%. This appears impressive until one considers that 16.25% - the grand
mean of the market (value weighted) adjusted returns to 1001 control portfolios - is an unbiased estimate of the expected return over the same period to a portfolio comprising 490 randomly selected firms.24

Given that BDO's sample of bidder firms are typically large firms which have a closer correspondence in size to the firms which drive the return to the value weighted index than does a randomly selected sample of firms, it might be argued that the above comparison overstates the extent of the positive bias (if any) in the estimated abnormal return to the bidder firms. That is true enough but the point is, without a direct control for firm size, the extent of the bias is unknown. Further, while the extent of the positive bias in estimated abnormal returns to bidder firms is reduced because of their typically larger size, target firms, which are generally smaller firms, have the bias accentuated. Unless firm size is controlled directly, one may not make valid inferences about the absolute gains from takeover on the basis of the estimated adjusted returns to the sample firms. This is why the preferred basis for assessing the gains from takeover in this thesis are size-decile as well as survivorship adjusted returns. Using the return to each sample firm's respective size-decile as the proxy for expected returns controls directly for the systematic association of firms' returns with their size and the return to the market portfolio.

From Table 4.5 we see that the mean size-decile adjusted return to BDO's sample of 490 bidders over the period [-24,-6] months relative to the bid announcement month is 17.46%. Removing the estimated bias attributable to positive skewness in firms' distribution of returns yields an estimate of 17.10% (=17.46% - 0.36%). That is, an unbiased estimate of the average abnormal return earned by BDO's sample of bidder firms over the period [-24,-6] months relative to the bid announcement date is 17.10%.

4.2.2 Assessing Significance

We may be confident that the estimated average abnormal return of 17.10% to the bidder firms is not a random occurrence because we can see, from the third column of Table 4.5, that none of the 1001 control portfolios achieved a higher average size-decile adjusted return to their sample of firms over the same period. Even on a raw returns basis, that is, without controlling for the negative association between firm size and equity returns, only 22 out of 1001 control portfolios achieve a higher mean

24 The grand mean of the market (value weighted) returns to the 1001 control portfolios, 16.25%, is an unbiased measure of the positive bias in returns to a randomly selected portfolio. Adjusting the mean market (value weighted) return to the sample bidders by the estimate of the positive bias yields 10.49% (= 26.74% - 16.25%) which, as expected, is the same estimate of the mean abnormal return yielded by controlling for expected return by the return to the market (equal weighted) portfolio.
return to their sample of firms.25

Notwithstanding our confidence in the significance of the average abnormal return to the sample portfolio, the metric is possibly an unreliable indicator of the benefits of takeover related activity to most firms in the sample. One reason is that the mean abnormal return might be driven by extreme abnormal returns achieved by a few sample firms. In the present instance, we might suspect this to be the case since it is remarkable that the bidder firms did so well even on a market adjusted basis despite their abnormal returns being downwards biased as a result of the size-effect.

In order to confirm that the performance of the sample firms are not driven by extreme returns to a few firms, the comparison of the median return to the sample portfolio with the distribution of the median returns to the 1001 control portfolios is also reported (see the middle three columns of Table 4.5). The justification is that the median return to the sample firms is not affected by extreme values in the distribution of sample firms' returns. If the majority of sample firms experience a positive (negative) abnormal return as a result of takeover then one may expect the median abnormal return to the sample firms to be higher (lower) than the median abnormal return to a majority of the control portfolios.26

The results reported in Table 4.5 indicate that we may be confident that the exceptional average return of the bidder firms in the pre-bid period is not driven by extreme returns to a few bidders. None of the 1001 control portfolios has a median size-adjusted return that is higher than the median size-adjusted return to the 490 bidder firms. The exceptional pre-bid period performance of the bidder is confirmed even after controlling for the equally weighted return to the market portfolio. None of the 1001 control portfolios include a median market (equal weighted) adjusted return that is higher than the median market (equal weighted) adjusted return of the 490 bidder firms.

Examining the median returns that are adjusted by the return to the value-weighted market portfolio reveals results that are consistent with the analysis based on size-adjusted returns but with an interesting difference. While only one out of the 1001 control portfolios has a median return higher than the median return to the 490

25 Note that the ranking of portfolio returns remains unchanged after adjusting the raw returns by the return to either the value-weighted or equal-weighted market portfolio. The reason is that the distributions of mean market adjusted returns to the control portfolios differ from the distribution of mean raw returns only in having a constant subtracted from the raw returns. The constant in each case is equal to the mean return to the market portfolio across the sample firms' event-windows.

26 In order to identify the median return, the returns in each portfolio are ranked in ascending order. If n (= number of firms in each portfolio) is an odd number, the median return is defined as the return to the firm ranking (n-1/2) + 1. If n is an even number, the median return is defined as the average of the returns to the firms ranked n/2 and (n/2) + 1.
sample bidders, the median of the 1001 control portfolios' median returns is 9.25% \((-2.64\% \text{ minus } -11.89\%)\) higher than the median of the 1001 control portfolios' size-adjusted median returns (see the sixth column in Table 4.5). The control portfolios register higher returns on a value-adjusted basis than on a size-adjusted basis. This outcome is not surprising because the size-effect leads us to expect that, in most periods, the number of firms which outperform the market (value weighted) return is greater than the number of firms that outperform either the size-adjusted market return or market (equally weighted) return. The value weighted return to the market portfolio is driven by the return to a few firms that together comprise a large proportion of total market capitalisation. The median value-adjusted return to most of the 1001 control portfolios is therefore higher than the median size adjusted or market (equally weighted) adjusted return to the same portfolios. One implication of the above is that, unless the size-effect has been controlled, the median is a poor indicator of relative performance. In consequence, the analyses that follow in Section 4.3 focus on just the size-adjusted median return metric when seeking confirmation that the mean return achieved by the sample firms is not driven by extreme and unrepresentative returns to a few firms.

### 4.2.3 An Alternative Method of Assessing Statistical Significance

If all significant systematic variation in share returns is controlled the mean and median return measures are sufficient to assess the significance of the returns to the sample firms. However, it is not uncommon for event-studies to report the proportion of positive adjusted returns in their samples. The motivation is similar to that behind the use of the median return. That is, event-studies report the proportion of positive adjusted returns in their sample in order to establish that the mean return to the sample firms is not driven by a few extreme and unrepresentative values.

As noted earlier, the validity of the above test depends on the existence of an appropriate benchmark. For instance, positive asymmetry in firms' distribution of returns implies that less than 50% of firms will, over any given period, achieve a return higher than the mean return to either their respective size-deciles or the market portfolio.

In order to impress an appreciation of the validity of the above argument this thesis reports the proportion of positive adjusted returns (henceforth, the PAR ratio) to the sample firms and uses as a benchmark for assessing significance the distribution of the PAR ratios of the 1001 control portfolios. In most instances, for the size-decile adjusted returns, we expect the result of this test to be consistent with the test of significance based on median values. The market (value weighted) adjusted returns may yield inconsistent results more frequently because they do not control for the
negative association between firm size and return; it is possible for the sample firms all to record positive adjusted returns yet have a median adjusted return that is lower than the median adjusted return to a majority of the 1001 control portfolios. This is most likely when the sample firms include a significantly disproportionate number of large firms.

The results reported in the last three columns of Table 4.5 show that the sample firms PAR ratio is higher than the PAR ratio recorded by every single one of the 1001 control portfolios. This is true regardless of the method used to adjust for expected returns.

The downward bias in the mean abnormal return attributable to positive asymmetry in firms' distribution of return may be gauged by reviewing the minimum and maximum values of the distribution of PAR ratios to the 1001 control portfolios. On a market (equal weighted) adjusted basis the best performing portfolio achieved a PAR ratio of just .45 while the worst performing portfolio recorded a PAR ratio of .31. The sample bidders recorded a market (equal weighted) adjusted PAR ratio of .49. On a market (value weighted) adjusted basis, the maximum PAR ratio was .55 while the minimum PAR was .41. The sample bidders recorded a market (value-weighted) adjusted PAR ratio of .57. On a size-decile adjusted basis, the maximum PAR ratio achieved by the 1001 control portfolios was .47 while the minimum PAR ratio was .41. The 490 sample bidders registered a PAR ratio of .55. By any criterion, the sample bidder firms performed exceptionally well as a group in the pre-bid period.

4.3 EXPERIMENTAL SAMPLE RESULTS

The findings reviewed below are unequivocal. The share market evidence, after controlling for the size-effect and survivorship bias, strongly endorses BDO's conclusion that, "on average, shareholders gain considerably when they own shares of companies involved in takeover transactions" (p. 10, Executive Summary). MR's accounting based findings on the performance of their sample of firms is also confirmed by the share market based study. MR's sample firms performed poorly in the pre-bid period; on average, the involvement of MR's sample bidding firms was viewed unfavourably by the share market; and the post-bid performance of the bidding firms was mediocre. The returns to MR's sample of firms, when compared with the returns to the much larger sample of firms included in BDO's study, are consistent with the proposition that MR's selection criteria inadvertently led to a disproportionate number of inexpert bidding firms being included in their sample. Finally, and importantly, the results recorded in this section are consistent with the
operation of efficient capital markets. A large proportion of earlier event-studies on
takeover activity reported results which were inconsistent with both the pro-takeover
perspective and (disturbingly) with the prevalence of reasonable market efficiency.
The results in this thesis suggest that the failure to control for phenomena such the
size-effect and survivorship bias explains the findings from many earlier studies.

The material below enlarges on the above precis. Returns to the portfolios
comprising bidding firms are discussed first, followed by a review of target firm
portfolio returns. The last section summarises and concludes the discussion of
results. To facilitate the exposition, unless otherwise specified, all sample firms are
drawn from BDO's database and all results discussed are based on winsorised returns
data. However, where notable differences exist in the winsorised and non-
winsorised results they are highlighted. In any event, both sets of results are fully
reported in the tables.

4.3.1 Review of Bidding Firms' Returns

4.3.1.1 Pre-bid announcement returns

One result consistent across most studies is that regardless of the outcome of the
takeover bid offer, bidding firms tend to experience abnormally good performance in
the months leading to the takeover offer. Dodd (1976) notes that the finding of
positive abnormal performance in the pre-bid period is consistent with the
hypothesis that "[differentially efficient] firms experience abnormally good
performance and have surplus funds to invest, and look to takeovers as a profitable
avenue of investing those funds" (p. 20).

The results in this thesis are consistent with Dodd's surmise. Table 4.5, reviewed in
passing in Section 4.2, summarises the analysis of returns to the portfolio comprising
bidding firms (regardless of takeover offer outcome) over the event-window [-24,-6]
months relative to the bid announcement month. A striking feature of the results in
Table 4.5 is that the exceptional performance of the bidding firms in the pre-bid
period is strong enough to offset the downward bias in returns to the bidding firms
that operates as a result of the size-effect and survivorship. Note that on an equal
weighted basis, value-weighted basis and size-adjusted basis, the bidder portfolio
experienced a mean return higher than the mean return to at least 979 of the 1001
control portfolios.27 Confirmation that the strong result is not driven by a few
extreme positive values is given by the PAR ratio performance metric. None of the

27 Henceforth, "equal weighted" returns refer to returns after subtracting the mean return to the
market (equal weighted) portfolio. "Value-weighted" returns refer to returns after subtracting the
mean return to the market (value weighted) portfolio. "Size-adjusted" returns to firm i refer to returns
after subtracting the return to the size-decile (equal weighted) portfolio of which firm i is a member.
1001 control portfolios had a higher proportion of positive adjusted returns than the portfolio comprising the sample bidder firms. Note as well that on a size-adjusted basis (i.e., the least biased measure of relative performance), the winsorised and non-winsorised results are consistent with each other.

The clear implication of the results in Table 4.5 is that bidder firms are typically exceptional performers in the pre-bid period. This finding is consistent with both the hubris hypothesis and the competing "value-increasing" hypothesis that bidder firms are typically characterised by above average management teams which look for other avenues in which to capitalise on their skills.

We may note, in support of the value-increasing hypothesis, that of the 876 bids analysed in BDO's study (for which the identity of the bidding firm is known), only 22% involved firms which made just a single bid over the period January 1972 to June 1985. Fifty nine per cent of the 876 bids involved firms which made more than 2 bids over the period (Industrial Equity Limited accounted for 58 of the 876 bids). This analysis is consistent with the majority of bidding firms finding their experience of takeover activity beneficial to performance. If unwarranted hubris, resulting from outstanding pre-bid performance, motivated most takeovers then we might expect the initial experience of takeover activity to act as a sobering influence on the management of bidding firms which would decrease the incidence of their re-entry in the takeover market as bidders.

A review of the returns to bidding firms over the period [-6,-1] months relative to the bid announcement month adds further support to the value-increasing hypothesis. Over this period we expect the share market to receive the first intimation that the bidder is planning a takeover bid. Assuming this to be the case, the returns to bidding firms over this period offers an indication of how the share market typically views the prospect of the bidding firm engaging in takeover activity.

Results summarised in Table 4.6 show that over the immediate pre-bid period, bidding firms typically continue to experience strong positive abnormal performance

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28 The relatively high PAR ratio of the portfolio of bidding firm is also consistent with these firms having a lower level of risk. However, given the exceptionally high mean return recorded by the bidder firms over this period one cannot make valid inferences about the risk-profile of the bidder firms. An insignificant mean return coupled with an above average PAR ratio is indicative of a low risk portfolio that is fairly priced by the market. See the analysis of the bidding firms post-bid returns.

29 If, despite the cautionary impact of poor performance from a takeover, bidding firms persisted in their hubris we might expect these firms to involuntarily re-enter the takeover market as target firms. We leave this line of investigation for future research.

30 It might be argued that this perspective puts the cart before the horse; it could be that bidding firms time their takeover bids to take advantage of recent increases in their valuation. That is, positive performance by the bidding firms in the immediate pre-bid period might be unrelated to, or in spite of, impending takeover bids.
which is consistent with the share market approving the prospect of future takeover activity. The average size-decile adjusted return to the bidding firms over the 5 month period immediately prior to the bid announcement month is 8.58% or 8.51% (= 8.58% - 0.07%) after subtracting the positive bias attributable to positive skewness in firms' distribution of returns. We may be confident that the positive mean abnormal return to the bidder firms is not driven by a few unrepresentative values since none of the 1001 control portfolio has a higher PAR ratio than the sample firms. This is true even if the size-effect is not controlled.

The pre-bid returns to MR's sample of bidder firms support the hypothesis that these firms are not representative of typical bidders. In Table 4.7 we see that over the period [-24, -6] months relative to the bid announcement month, the 37 bidding firms for which data were available achieved a mean size-adjusted return of -14.88% (= -15.28% - -0.40%). A higher mean return over the same period is achieved by 779 control portfolios. The sample bidders' size based PAR ratio of .38 is lower than the size based PAR ratio of 574 control portfolios.

Notwithstanding the above, Table 4.8 shows there is some evidence that the prospect of takeover improves the share market's rating of MR's sample bidders. Over the period [-6,-1] months relative to the bid announcement month, that is, over the period when news of the imminent announcement of a takeover bid is increasingly likely to be leaked to the share market, MR's bidder firms record a mean size-adjusted return of 3.29%. Only 281 control portfolios achieve a higher size-adjusted mean return over this same period. Further, only 149 control portfolios record a higher size-based PAR ratio than the 0.54 registered by the bidder portfolio. While the relative performance of MR's bidder firms over the 5 months immediately period preceding the bid announcement is not as outstanding as the relative performance of BDO's much larger sample of bidder firms over the same period, the analysis does suggest that even otherwise unlikely bidder firms are viewed more favourably by the share market once they enter the takeover market. In other words, the returns evidence over the period [-6,-1] months is consistent with the proposition that takeovers are, in general, considered value-increasing investments by the share market. Analysis of the returns to bidder firms over the bid announcement period enable a more rigorous test of this hypothesis. The alternative hypothesis is that bidder firms are more likely to be successful firms which look to takeovers as one

31 As discussed in Section 4.2, the mean return to the 1001 control portfolios is an unbiased estimate of the positive bias in the sample portfolio's mean return attributable to positive skewness in firms' distributions of returns. Henceforth, results reported in discussion will be after adjusting for the positive bias.
Table 4.6

All Bidders; Returns [-6,-1] Months Relative to the Bid Announcement Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Firms' Returns &gt; Sample Portfolio Return (%)</th>
<th>Median of 1001 Control Portfolios' Median Return (%)</th>
<th>No. Control Firms' Median Return (%)</th>
<th>Sample PAR c</th>
<th>Median value of 1001 Control Portfolios PARs ≥ Sample PAR</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Censored Returns: a Number of Experimental Sample Firms = 518</td>
<td>Raw Return b 20.27 12.83 0 13.04</td>
<td>.74</td>
<td>.66</td>
<td>0</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj 7.38 -0.05 0 1.72</td>
<td>.54</td>
<td>.44</td>
<td>0</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj 10.42 2.99 0 4.46</td>
<td>.58</td>
<td>.48</td>
<td>0</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj 8.58 0.07 0 2.90</td>
<td>-2.91</td>
<td>0</td>
<td>.57</td>
<td>.45</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Uncensored Returns: a Number of Experimental Sample Firms = 520</td>
<td>Raw Return b 20.89 17.27 93 13.07</td>
<td>.74</td>
<td>.67</td>
<td>0</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj 3.48 -0.13 93 -1.79</td>
<td>.47</td>
<td>.38</td>
<td>0</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj 10.62 7.01 93 4.37</td>
<td>.57</td>
<td>.48</td>
<td>0</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj 7.46 -0.02 18 1.79</td>
<td>-4.87</td>
<td>0</td>
<td>.54</td>
<td>.42</td>
<td>0</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.
b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population (R) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of (R) identified.
c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios Returns (%)</th>
<th>No. Control Portfolios Returns &gt; Sample Return</th>
<th>Sample Firms' Median Return (%)</th>
<th>Median of 1001 Control Median Returns (%)</th>
<th>No. Control Median Returns &gt; Sample Median</th>
<th>Sample Firms' PAR c</th>
<th>Median value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>43.81</td>
<td>68.28</td>
<td>912</td>
<td>39.49</td>
<td>.86</td>
<td>.81</td>
<td>281</td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>-25.40</td>
<td>-0.83</td>
<td>912</td>
<td>-20.09</td>
<td>.35</td>
<td>.35</td>
<td>565</td>
<td></td>
</tr>
<tr>
<td>Val Wtd Adj</td>
<td>-6.40</td>
<td>18.17</td>
<td>912</td>
<td>0.96</td>
<td>.51</td>
<td>.46</td>
<td>308</td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>-15.28</td>
<td>-0.40</td>
<td>779</td>
<td>-23.09</td>
<td>-14.85</td>
<td>801</td>
<td>.38</td>
<td>574</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel A: Censored Returns: a Number of Experimental Sample Firms = 37</th>
</tr>
</thead>
</table>

| Panel B: Uncensored Returns: a Number of Experimental Sample Firms = 37 |

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population \( [R] \) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of \( [R] \) identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
Table 4.8
MR's Bidders; Returns [-6,-1] Months Relative to the Bid Announcement Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)&lt;sup&gt;b&lt;/sup&gt;</th>
<th>No. Control Portfolios</th>
<th>Sample Firms' Returns &gt; Sample Return (%)</th>
<th>Median of 1001 Control Portfolios' Median Returns (%)</th>
<th>No. Control Firms' PAR&lt;sup&gt;c&lt;/sup&gt;</th>
<th>Sample Firms' PAR&lt;sup&gt;c&lt;/sup&gt;</th>
<th>Median value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return&lt;sup&gt;b&lt;/sup&gt;</td>
<td>16.61</td>
<td>17.94</td>
<td>488</td>
<td>9.96</td>
<td>.69</td>
<td>.69</td>
<td>610</td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>-1.84</td>
<td>-0.5</td>
<td>488</td>
<td>-0.76</td>
<td>.46</td>
<td>.43</td>
<td>435</td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>5.91</td>
<td>7.25</td>
<td>488</td>
<td>4.18</td>
<td>.63</td>
<td>.54</td>
<td>212</td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>3.05</td>
<td>-0.24</td>
<td>281</td>
<td>3.12</td>
<td>-2.84</td>
<td>77</td>
<td>149</td>
<td></td>
</tr>
</tbody>
</table>

Panel A: Censored Returns:<sup>a</sup> Number of Experimental Sample Firms = 35

Panel B: Uncensored Returns:<sup>a</sup> Number of Experimental Sample Firms = 35

---

<sup>a</sup> Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

<sup>b</sup> The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population (R) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of (R) identified.

<sup>c</sup> PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
profitable avenue for investment.

4.3.1.2 Returns over the bid announcement period

Tables 4.9 and 4.10 describe, respectively, the returns to the bidding firms over the periods [-3,+3] months and [-1,+1] months relative to the bid announcement month. The aim in reviewing the returns over these event-windows is to isolate the share market reaction to the confirmation of a takeover bid offer. If, on average, the share market views the event of a takeover offer adversely we can expect a downward revision in share price (i.e., a negative abnormal return) over the announcement period. The reverse applies if the share market, on average, views takeover offers in a positive light, that is, if the share market considers that takeover offers convey positive news about the bidder firms. As discussed earlier, the news signalled does not have to be directly associated with the prospect of an actual takeover. A takeover bid might be interpreted by the share market as a credible signal of overweening hubris on the part of the bidding firms' or a credible signal of financial slack in the bidding firms coupled with the prospect of imminent successful expansion.

The reason for reviewing the announcement period returns over the longer event-window, [-3,+3] months, is that we can expect that for some bidders, particularly the larger, more extensively analysed firms, the news of the takeover offer will be general within the share market well before notice of the bid offer appears in the financial press. Reviewing the returns over the shorter period [-1,+1] months, therefore probably yields a downward biased estimate of the share market reaction to the bid offer announcement. Further, the earnings announcement evidence reviewed earlier suggests that the share market reaction to news might not be immediately incorporated in reported share prices for some firms. Extending the event-window to three months after the takeover offer is publicised in the media provides a possibly more complete estimate of the share market reaction, although it does add noise to the experiment.

Notwithstanding the above, the results reported in Tables 4.9 and 4.10 are consistent with the share market viewing takeover bid announcements in a positive light, on average. From Table 4.9 we see that over a seven month period centred on the bid announcement date, i.e., the period [-3,+3] months relative to the bid announcement month, the mean size-adjusted return to the bidder firms is 6.99% (= 7.07% - 0.08%). The mean value-weighted adjusted return to the bidder firms is 4.99% (= 9.62% - 4.63%) which indicates that the positive return over the seven month period is robust enough to more than off-set the size-effect. An examination of the performance metrics associated with the median return and the PAR ratio is
Table 4.9

All Bidders; Returns [-3,+3] Months Relative to the Bid Announcement Month

<table>
<thead>
<tr>
<th></th>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolio Returns (%)</th>
<th>No. Control Portfolios Returns &gt; Sample Return</th>
<th>Sample Firms' Returns Median (%)</th>
<th>Median of 1001 Control Median Returns (%)</th>
<th>No. Control Median Returns &gt; Sample Median</th>
<th>Sample Firms' PAR c</th>
<th>Median value of 1001 Control Paros PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Panel A: Censored Returns: a</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Raw Return b</td>
<td>22.06</td>
<td>17.07</td>
<td>11</td>
<td>12.64</td>
<td>.71</td>
<td>.67</td>
<td>0</td>
<td></td>
<td>0</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>4.92</td>
<td>-0.06</td>
<td>11</td>
<td>-0.84</td>
<td>.49</td>
<td>.43</td>
<td>0</td>
<td></td>
<td>0</td>
</tr>
<tr>
<td>Val Wtd Adj</td>
<td>9.62</td>
<td>4.63</td>
<td>11</td>
<td>3.88</td>
<td>.57</td>
<td>.49</td>
<td>0</td>
<td></td>
<td>0</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>7.07</td>
<td>0.08</td>
<td>2</td>
<td>2.10</td>
<td>-3.75</td>
<td>0</td>
<td>.54</td>
<td>.44</td>
<td>0</td>
</tr>
<tr>
<td>Panel B: Uncensored Returns: a</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Raw Return b</td>
<td>23.15</td>
<td>22.95</td>
<td>432</td>
<td>12.66</td>
<td>.71</td>
<td>.67</td>
<td>24</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>0.32</td>
<td>-0.12</td>
<td>432</td>
<td>-4.99</td>
<td>.43</td>
<td>.38</td>
<td>11</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Val Wtd Adj</td>
<td>10.19</td>
<td>9.98</td>
<td>432</td>
<td>3.86</td>
<td>.56</td>
<td>.49</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>6.28</td>
<td>0.23</td>
<td>11</td>
<td>0.80</td>
<td>-5.93</td>
<td>0</td>
<td>.53</td>
<td>.42</td>
<td></td>
</tr>
</tbody>
</table>

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population [R] of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
Table 4.10

All Bidders; Returns [-1,+1] Months Relative to the Bid Announcement Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios</th>
<th>Sample Firms' Returns &gt; Sample Return (%)</th>
<th>Median of 1001 Control Portfolios' Median Returns &gt; Sample Return (%)</th>
<th>No. Control Firms' Median PAR c</th>
<th>Sample value of PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>9.53</td>
<td>6.93</td>
<td>26</td>
<td>4.11</td>
<td>.61</td>
<td>.63</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>2.56</td>
<td>-0.04</td>
<td>26</td>
<td>-0.21</td>
<td>.49</td>
<td>.46</td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>3.89</td>
<td>1.30</td>
<td>26</td>
<td>-0.58</td>
<td>.53</td>
<td>.47</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>3.26</td>
<td>-0.09</td>
<td>7</td>
<td>0.05</td>
<td>-1.35</td>
<td>22</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a  Number of Experimental Sample Firms = 529

Panel B: Uncensored Returns: a  Number of Experimental Sample Firms = 532

---

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of funds identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
consistent with positive performance being typical within the sample of bidder firms; irrespective of the type of adjustment effected, none of the 1001 control portfolios achieve a higher PAR ratio. About 54% of the sample firms achieve a positive size-decile adjusted return while the median size-decile PAR ratio of the 1001 control portfolios is just .44.

The returns to bidding firms over the narrower event-window, [-1,+1] months relative to the bid announcement date are, in the main, consistent with those for the [-3,+3] months event-window (see Table 4.10). The respective mean size-adjusted return and mean value-weighted return to the bidder firms are 3.35% (= 3.26% -0.09%) and 2.59% (= 3.89% - 1.30%). Under both test regimes the PAR ratio of the bidder portfolio is higher than the PAR ratios of at least 981 of the 1001 control portfolios. Intriguingly, the same performance metrics based on non-winsorised returns are not wholly consistent with these results. Note that the non-winsorised grand mean value adjusted return to the 1001 control portfolios over the period [-1,+1] months relative to the sample bidders' bid announcement month is 3.64% while the mean value adjusted return to the sample bidders over the same period is 3.61%. Despite 52% of the sample bidders experiencing a positive return over the [-1,+1] period, a PAR ratio higher than that of all but 35 of the 1001 control portfolios, the sample bidders have a mean value adjusted return lower than the comparable grand mean for the 1001 control portfolios.

Positive asymmetry in distributions of returns and the absence of control for the size-effect explain the divergence in results between the winsorised and non-winsorised data. We noted in Section 4.2 that positive asymmetry in firms' distributions of returns causes the mean return to the market portfolio to be higher than the median return to the firms in the market. Typically, there is a considerable difference between the mean return and the median return such that, other things being equal, significant positive performance by a group of firms is required to raise their average return above the mean market return. The group of sample bidders experience strong positive performance over the period [-1,+1] months. Further, the strong performance is not confined to a few of the bidders, as is evidenced by the relatively high PAR ratio of the bidders' portfolio. However, the extreme positive returns to the sample of bidder firms are not sufficiently high to off-set the extreme positive returns accruing to a small minority of firms in the market portfolio. This results in the mean return to the market portfolio being higher than the mean return to the sample bidder firms despite the higher incidence of positive performance among the bidder firms. We can see from the top half of Table 4.10 that once the impact of positive asymmetry is reduced by winsorising returns, the strong performance of the sample bidder firms is clearly manifest in the value-adjusted returns.
On a size-adjusted basis the stronger relative performance of the sample bidder firms is evident in both the winsorised and non-winsorised returns. The downward bias in mean returns attributable to positive asymmetry is still present in the non-winsorised size-adjusted returns but the systematic negative association between firm size and return has been controlled. The performance of the bidder firms over the period [-1,+1] months is sufficiently strong to off-set the downward bias attributable to positive asymmetry.

In summary, an analysis of the returns to bidder firms over the immediate period around the bid announcement date is strongly consistent with the hypothesis that the share market considers that a takeover bid announcement signals, on average, good news about the bidding firm.

The bid announcement period returns to MR's sample of bidding firms diverge from the returns to BDO's sample of firms. The bid announcement period returns to MR's bidders are only weakly consistent with the hypothesis that the share market judged these firms to have announced a prospective investment that, on average, is significantly value-increasing. Specifically, over the period [-3,+3] months relative to the bid announcement the mean size-adjusted return to MR's bidders is 1.20% (= 1.47% - 0.27%) and the PAR ratio is .48 (refer Table 4.11). Four hundred and eight control portfolios achieve a higher size-adjusted mean return over the same period while a PAR ratio higher than .48 is recorded by 392 control portfolios. Interestingly, over the shorter event-window [-1,+1] months relative to the bid announcement month, MR's bidders record a size-adjusted return of 2.08% (= 2.02% - 0.06%), a return higher than that recorded by 311 control portfolios (refer Table 4.12). It is likely that the better performance registered by MR's bidders over the short event-window relative to their performance over the long event-window (i.e., [-3,+3] months) is driven by a couple of outliers. The size-based PAR ratio of MR's bidder firms is .39 over the period [-1,+1] months. A higher PAR ratio is attained by 804 control portfolios over the same period.

In sum, the unexceptional returns achieved by MR's sample of bidders around the bid announcement period contrast with the strong, significantly exceptional returns earned, on average, by BDO's sample of bidders over the same period. The difference in performance reinforces the argument that MR's sample of bidders included a disproportionate number of firms that lacked comparative advantage in the mounting of a takeover.
Table 4.11
MR's Bidders; Returns [-3,+3] Months Relative to the Bid Announcement Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios</th>
<th>Sample Firms' Returns &gt; Sample Return (%)</th>
<th>Median of 1001 Control Median Returns (%)</th>
<th>No. Control Median Returns &gt; Sample Median</th>
<th>Sample Firms' PAR c</th>
<th>Median value of 1001 Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>9.00</td>
<td>12.00</td>
<td>636</td>
<td>7.53</td>
<td></td>
<td>.61</td>
<td>.65</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>-2.78</td>
<td>0.20</td>
<td>636</td>
<td>-3.81</td>
<td>.39</td>
<td>.42</td>
<td>765</td>
</tr>
<tr>
<td>Val Wtd Adj</td>
<td>2.24</td>
<td>5.23</td>
<td>636</td>
<td>-0.20</td>
<td>.48</td>
<td>.52</td>
<td>659</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>1.47</td>
<td>0.27</td>
<td>408</td>
<td>-0.78</td>
<td>-3.31</td>
<td>319</td>
<td>.48</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a Number of Experimental Sample Firms = 31

Panel B: Uncensored Returns: a Number of Experimental Sample Firms = 31

| Raw Return b                  | 9.00                                        | 17.86                  | 763                                      | 7.53                                       |                                           | .61              | .65                           | 700               |
| Eq. Wtd Adj                   | -8.36                                       | 0.49                   | 763                                      | -9.52                                      | .29                                       | .39              | 917                           |                   |
| Val Wtd Adj                   | 1.85                                        | 10.70                  | 763                                      | -0.42                                      | .48                                       | .52              | 385                           |                   |
| SizeDec Adj                   | 0.42                                        | 0.41                   | 410                                      | -0.04                                      | -4.64                                     | 187              | .52                           | 189               |

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.
b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population [R] of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.
c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
### Table 4.12

MR's Bidders; Returns [-1,+1] Months Relative to the Bid Announcement Month

| Return to Sample Portfolio (%) | Mean of 1001 Control Portfolios’ Returns (%) | No. Control Portfolios | Median of Sample Firms’ Returns (%) | No. Control Median Returns (%) | Sample PAR c | Median value of 1001 Control PARs | No. of Control PARs ≥ Sample PAR |
|--------------------------------|---------------------------------------------|------------------------|-------------------------------------|--------------------------------|
| Raw Return b                   | 6.87                                        | 5.47                   | 358                                 | 2.70                           | .61          | .64                                 | 672                            |
| Eq. Wtd Adj                    | 1.24                                        | -0.17                  | 358                                 | -3.46                          | .39          | .45                                 | 789                            |
| Val Wtd Adj                    | 3.10                                        | 1.69                   | 358                                 | -0.55                          | .45          | .48                                 | 652                            |
| SizeDec Adj                    | 2.02                                        | -0.06                  | 311                                 | -1.33                          | -1.46        | 483                                 | .39                            |

#### Panel A: Censored Returns:

Number of Experimental Sample Firms = 33

#### Panel B: Uncensored Returns:

Number of Experimental Sample Firms = 34

---

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms’ returns, including the sample firms’ returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms’ respective event-windows correspond is identified. (ii) For each period, the population (R) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of (R) identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
As discussed earlier, the share market's reaction to the takeover announcement might constitute a polluted or mixed signal about its evaluation of takeovers per se. While the results so far indicate that, on balance, takeover bid announcements are viewed favourably it could be that actual takeovers are not relished by the stock market but the release of other positive information by the bidder firms during the bid process swamps the negative impact of the prospect of an actual takeover. Given that the share market reaction to the prospect of an actual takeover is not complete until the date that the bid outcome is finalised, the share market reaction around the bid outcome month provides a more stringent test of its evaluation of actual takeovers.

Tables 4.13 and 4.14 summarise the returns to successful bidders over the periods [-3,+3] months and [-1,+1] months relative to the month the bid offer outcome was publicised. They are consistent with the share market revaluing upwards the returns to successful offerers, that is, the returns are consistent with the share market viewing confirmed takeovers as, on average, value-increasing investments.

The size-adjusted returns estimated for the period [-3,+3] months relative to the bid outcome month provide the strongest support for the value-increasing hypothesis. On a size-adjusted basis, only 19 out of 1001 control portfolios achieve a higher mean return over this period. The PAR ratio on a size-adjusted basis to the sample of successful bidders is 55%, a ratio not achieved by any of the 1001 control portfolio. The median PAR ratio to the 1001 control portfolios is just .44.

The value adjusted returns display less solid support for the value-increasing hypothesis of takeovers. This is to be expected given that bidders are typically large firms and that the value adjusted returns do not control for the size-effect. Nevertheless, the value adjusted returns are consistent with the value-increasing hypothesis. Only 106 of the 1001 control portfolios have a higher mean value adjusted return than the mean value adjusted return to the bidder firms and none of them achieve a PAR ratio on a value-adjusted basis of .58. The median PAR ratio to the 1001 portfolios is just .49.

Once again it is a moot point which of the two event-windows yields the most accurate and unbiased estimate of the returns to the successful bidders. In favour of the [-3,+3] months event-window it may be said that it is probable that for the more extensively analysed bidders the bid outcome is accurately predicted by the share market at least a couple of months before the bid outcome is officially publicised in the print media. Given that this is the case, the returns over the period [-1,+1]
Table 4.13

Successful Bidders; Returns [-3,+3] Months Relative to the Bid Outcome Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios Returns &gt; Sample Return (%)</th>
<th>Sample Firms' Median Return (%)</th>
<th>Median of 1001 Control Median Returns &gt; Sample Median (%)</th>
<th>No. Control Firms' PAR c</th>
<th>Sample value of 1001 Control PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>20.17</td>
<td>16.46</td>
<td>106</td>
<td>12.02</td>
<td>.69</td>
<td>.65</td>
<td>68</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>3.69</td>
<td>-0.02</td>
<td>106</td>
<td>2.26</td>
<td>.53</td>
<td>.43</td>
<td>1</td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>8.48</td>
<td>4.77</td>
<td>106</td>
<td>4.70</td>
<td>.58</td>
<td>.49</td>
<td>0</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>6.28</td>
<td>0.10</td>
<td>19</td>
<td>3.61</td>
<td>3.66</td>
<td>0</td>
<td>.55</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a Number of Experimental Sample Firms = 302

| Raw Return b                  | 21.85                                     | 22.62                     | 456                            | 12.02                          | .69                  | .65                              | 93                              |
| Eq. Wtd Adj                   | -0.27                                     | 0.49                      | 456                            | -3.15                          | .45                  | .38                              | 5                               |
| Val W'td Adj                  | 9.73                                      | 10.49                     | 456                            | 4.74                           | .57                  | .49                              | 3                               |
| SizeDec Adj                   | 7.20                                      | 0.48                      | 107                            | 3.66                           | -.58                 | 0                                | .55                             |

Panel B: Uncensored Returns: a Number of Experimental Sample Firms = 304

a Uncensored returns are based on the full set of raw returns in each period. Censored or winorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population (R) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of (R) identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
### Table 4.14

Successful Bidders; Returns [-1,+1] Months Relative to the Bid Outcome Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Firms' Returns &gt; Sample Return (%)</th>
<th>Sample Median of 1001 Control Portfolios Returns (%)</th>
<th>No. Control Firms' Returns &gt; Sample Median Return (%)</th>
<th>Median value of 1001 Control Portfolios PAR c</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Censored Returns:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Raw Return b</td>
<td>5.13</td>
<td>4.29</td>
<td>278</td>
<td>2.58</td>
<td>.55</td>
<td>.60</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>0.93</td>
<td>0.09</td>
<td>278</td>
<td>-0.83</td>
<td>.48</td>
<td>.46</td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>2.51</td>
<td>1.67</td>
<td>278</td>
<td>-0.39</td>
<td>.51</td>
<td>.49</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>1.89</td>
<td>0.19</td>
<td>119</td>
<td>-0.94</td>
<td>-1.18</td>
<td>384</td>
</tr>
<tr>
<td><strong>Panel B: Uncensored Returns:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Raw Return b</td>
<td>6.61</td>
<td>6.71</td>
<td>459</td>
<td>2.87</td>
<td>.55</td>
<td>.61</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>-0.06</td>
<td>0.04</td>
<td>459</td>
<td>-3.52</td>
<td>.44</td>
<td>.41</td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>3.80</td>
<td>3.90</td>
<td>459</td>
<td>0.10</td>
<td>.51</td>
<td>.48</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>2.72</td>
<td>1.12</td>
<td>112</td>
<td>-0.86</td>
<td>-2.21</td>
<td>89</td>
</tr>
</tbody>
</table>

---

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population (R) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
months relative to the bid announcement being publicised provide a downward biased estimate of the share market reaction to confirmation of a takeover bid's success.

In light of the above, it is not surprising that while the results reported in Table 4.14 for the event-window [-1,+1] months are consistent with the value-increasing hypothesis, they are not as strongly supportive of it as the returns reported for the event-window [-3,+3] months relative to the bid outcome month. On a size-adjusted basis 119 of the 1001 control portfolios record a higher mean return than the successful bidders and 214 control portfolios achieve a higher PAR ratio than the portfolio comprising the successful bidders. The plausible explanation for the weaker evidence of exceptional performance over the shorter event-window is that the share market anticipated the bid outcome for a substantial proportion of the successful bidders and so the publication of the bid outcome did not convey "news".

If confirmation of the success of a takeover bid is seen as good news by the share market one would expect bidders which fail to obtain a majority position in the shares of their target firms to experience negative returns on confirmation of the failure. This, other things being equal, is one implication of the value-increasing hypothesis.32

A review of the returns to unsuccessful bidders over the period [-3,+3] months relative to the bid outcome month reveals that the implications of the value-increasing hypothesis for unsuccessful bidders is contradicted by the evidence. Table 4.15 shows that bidding firms involved in 67 clearly identified unsuccessful bids in BDO's sample of takeover bids experienced a size-adjusted mean return of 13.94% (= 13.90% - -0.04%) which is higher than the mean size-adjusted return achieved by all but 25 of 1001 control portfolios.

Walter's (1980) results are consistent with the above. However, he posits that the strong positive performance of "failed" bidders is attributable to the "failed" bidders selling their stakes in their respective targets to other bidders at a price higher than their offer price, i.e., the unsuccessful bidders are successful "green-mailers". One implication of this interpretation is that the returns to unsuccessful bidders are not a fair test of the value-increasing hypothesis unless one separates the "successful

32 An unsuccessful bid may represent an optimal outcome for the bidder (on the presumption that the unsuccessful bid included a fair price for the target and that an increase in offer price would have made the takeover a negative NPV investment). However, the optimum outcome for the bidder would have been acceptance of the takeover bid (in the absence of positive returns from a "greenmail" strategy) on the presumption that takeovers, on average, represent value-increasing investments. Given that the share market had incorporated the probability of a positive NPV investment occurring into the bidder's share price then we would expect, other things being equal, a decrease in bidder share price on confirmation that the positive NPV takeover would not eventuate.
Table 4.15

Unsuccessful Bidders; Returns [-3,+3] Months Relative to the Bid Outcome Month

<table>
<thead>
<tr>
<th></th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios Returns &gt; Sample Return (%)</th>
<th>Sample Firms' Returns &gt; Sample Return (%)</th>
<th>Median of 1001 Control Portfolios' Returns Median</th>
<th>No. Control Portfolios Returns &gt; Sample Median</th>
<th>Sample Firms' PAR c</th>
<th>Median value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Return to Sample Portfolio (%)</td>
<td>Mean of 1001 Control Portfolios' Returns (%)</td>
<td>No. Control Portfolios Returns &gt; Sample Return (%)</td>
<td>Sample Firms' Returns &gt; Sample Return (%)</td>
<td>Median of 1001 Control Portfolios' Returns Median</td>
<td>No. Control Portfolios Returns &gt; Sample Median</td>
<td>Sample Firms' PAR c</td>
<td>Median value of 1001 Control Portfolios PARs</td>
<td>No. of Control PARs ≥ Sample PAR</td>
</tr>
<tr>
<td>Raw Return b</td>
<td>27.47</td>
<td>15.38</td>
<td>46</td>
<td>15.87</td>
<td>.72</td>
<td>.63</td>
<td>96</td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>11.94</td>
<td>-0.15</td>
<td>46</td>
<td>3.93</td>
<td>.60</td>
<td>.45</td>
<td>10</td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>16.15</td>
<td>4.05</td>
<td>46</td>
<td>9.95</td>
<td>.64</td>
<td>.49</td>
<td>3</td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>13.90</td>
<td>-0.04</td>
<td>25</td>
<td>6.10</td>
<td>-3.19</td>
<td>12</td>
<td>.61</td>
<td>.45</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a  Number of Experimental Sample Firms = 67

Panel B: Uncensored Returns: a  Number of Experimental Sample Firms = 67

---

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population [R] of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
green-mailers" from the other unsuccessful bidders. In support of his hypothesis Walter notes that of the total post-bid value adjusted increase of 21.3% in his sample of 97 unsuccessful offers, only 1.5% was not associated with the investment performance of Industrial Equity Limited (henceforth, IEL). IEL accounts for 58 of the 876 bids in BDO's sample for which the identity of the bidder is known. In terms of frequency of entry into the takeover market, IEL is the best qualified firm to wear the "corporate raider" tag, at least over the period 1974 to 1985. It also accounts for 11 of the 67 unsuccessful bids for which the identity of the bidder is established by BDO. In view that Walter's finding confirms the expertise of IEL in entering and exiting from the market for corporate control in a manner which maximises its gains, it is plausible that IEL's inclusion in the sample of unsuccessful bidders conveys a misleading picture of the typical returns to unsuccessful bidders around the bid outcome date.

Table 4.16 summarises the returns to unsuccessful bidders over the period [-3,+3] months relative to the bid outcome month after excluding the 11 bids involving IEL. The evidence is unchanged in all major respects. While the exclusion of IEL decreases the mean size-adjusted return to the unsuccessful bidders from 13.94% (= 13.90% - -0.04%) to 9.23% (= 8.80% - -0.43%), the bidders' mean size-adjusted return is higher than the mean size-adjusted return to 910 of the 1001 control portfolios. Further, on a size-adjusted basis, the PAR ratio of .59 achieved by the portfolio of unsuccessful bidders is higher than the PAR ratio achieved by all but 15 of the 1001 control portfolios. The value-adjusted returns are consistent with the size-adjusted return. The winsorised returns are also supported by the non-winsorised returns. The implication of these results is that bidders are not penalised by the share market for making unsuccessful takeover bids. One might even draw the inference from the evidence that unsuccessful bids are rewarded.

Given the strength and consistency of the other evidence in favour of the value-increasing hypothesis of takeovers, one would have to draw a very long bow to infer that the evidence on the returns to unsuccessful bidders significantly weakens the argument for a free market for corporate control. The sample of unsuccessful bidders is much smaller than the sample of successful bidders. In this context we may note that the outcome of 167 out of 876 bids made by identified bidders could not be determined by BDO. It is likely that at least some of these bids were unsuccessful.

There is no reason to infer that the returns to the unsuccessful bidders which could not be identified by BDO systematically diverge from the returns to the unsuccessful bidders that could be identified. However, as the experience of IEL indicates, failure
Table 4.16
Unsuccessful Bidders Excluding IEL Ltd; Returns [-3,+3] Months Relative to the Bid Outcome Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios &gt; Sample Return</th>
<th>Sample Firms' Sample Return (%)</th>
<th>Median of 1001 Control Portfolios Median Returns &gt; Sample Return (%)</th>
<th>No. Control Firms' PAR c</th>
<th>Sample Value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>22.79</td>
<td>15.70</td>
<td>140</td>
<td>12.06</td>
<td>.68</td>
<td>.64</td>
<td>298</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>6.50</td>
<td>-0.59</td>
<td>140</td>
<td>1.48</td>
<td>.54</td>
<td>.43</td>
<td>85</td>
</tr>
<tr>
<td>Val Wtd Adj</td>
<td>10.38</td>
<td>3.29</td>
<td>140</td>
<td>4.03</td>
<td>.59</td>
<td>.48</td>
<td>53</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>8.80</td>
<td>-0.43</td>
<td>91</td>
<td>2.92</td>
<td>-3.40</td>
<td>68</td>
<td>68</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a Number of Experimental Sample Firms = 56

| Raw Return b                  | 22.79                                         | 21.68                                  | 327                             | 12.06                                                         | .68                    | .64                                        | 341                              |
| Eq. Wtd Adj                   | 0.78                                          | -0.33                                  | 327                             | -3.99                                                         | .38                    | .39                                        | 348                              |
| Val Wtd Adj                   | 9.93                                          | 8.82                                   | 327                             | 3.63                                                          | .59                    | .48                                        | 63                               |
| SizeDec Adj                   | 6.99                                          | -0.04                                  | 188                             | 1.90                                                          | -5.60                  | 42                                         | 31                               |

Panel B: Uncensored Returns: a Number of Experimental Sample Firms = 56

---

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.
b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population (R) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.
c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
to achieve a particular takeover does not necessarily imply either a withdrawal from the takeover market or the sustaining of abnormal losses. As noted earlier, a more accurate or representative assessment of the consequences of failed takeover bids for bidding firms probably requires the sub-division of the unsuccessful bidders into different categories based on their different aspirations or aims. For some firms, failed takeover bids might represent the culmination of a successful green-mailing campaign. For other firms, a failed takeover bid might thwart plans for a period of successful expansion.

Indirect support for the proposition that failure to ultimately achieve control of a target does not imply the failure of a corporate strategy to increase value is found in the analysis of the returns to bidders which withdrew their takeover bids (henceforth, withdrawn bidders). The likely reasons for withdrawing a bid offer include that the bidder finds, subsequent to the bid offer being made, that it has overvalued the target firm. If this is the case, then it is unlikely that the target will be subject to a higher bid from a rival bidder and the best move is to withdraw the offer. If the bidder has not over-valued the target and there is a possibility of another higher value bid forthcoming from a rival bidder then the value-increasing tactic is to persist with the bid offer and sell whatever shares one garners from the bid to the rival bidder at a profit. The point is, withdrawal of a bid offer sends a signal to the share market that the bidder is not going to achieve the synergy benefits anticipated when the bid was made and that it is likely that the bidder will sustain a loss on its offer as a result of being locked into a minority shareholding in the target firm (assuming that it does capture a few shares prior to withdrawing its offer). A withdrawn bid also sends an adverse signal to the share market about the extent of the bidding firm's management expertise in the takeover market.

The returns to withdrawn bidders over the period [-3,+3] months relative to the bid outcome month are consistent with the above interpretation. The returns are summarised in Table 4.17. Over the bid outcome period, the 67 withdrawn bidders in BDO's sample that had data available earned a mean size-adjusted return of -5.49% (= -5.78% - 0.29%) , a mean return lower than that earned by 860 control portfolios over the same period. Further, the median return in 926 out of 1001 control portfolios is higher than the median return to the firms comprising the withdrawn bidder portfolio. These results are inconsistent with the hubris hypothesis that bidding firms typically over-bid for target firms. If the hubris hypothesis were valid then one would expect, on average, an upward revaluation in the withdrawn bidders' share prices on announcement of the bid outcome.
<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios Returns &gt; Sample Return (%)</th>
<th>Sample Firms' Mean Returns &gt; Sample Return (%)</th>
<th>Median of 1001 Control Portfolios Median Returns &gt; Sample Return (%)</th>
<th>No. Control Firms' PAR</th>
<th>Sample Firms' PAR</th>
<th>Median value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>11.08</td>
<td>20.07</td>
<td>944</td>
<td>7.05</td>
<td>.63</td>
<td>.70</td>
<td>924</td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>-8.77</td>
<td>0.22</td>
<td>944</td>
<td>-9.86</td>
<td>.36</td>
<td>.40</td>
<td>844</td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>-4.74</td>
<td>4.25</td>
<td>944</td>
<td>-6.65</td>
<td>.43</td>
<td>.46</td>
<td>710</td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>-5.78</td>
<td>0.29</td>
<td>860</td>
<td>-9.67</td>
<td>-4.72</td>
<td>-7.03</td>
<td>658</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>.39</td>
<td>.41</td>
<td>580</td>
<td></td>
</tr>
</tbody>
</table>

**Panel A: Censored Returns:** Number of Experimental Sample Firms = 67

**Panel B: Uncensored Returns:** Number of Experimental Sample Firms = 68

---

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population [R] of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
Examination of the post-bid returns to bidding firms is important for at least one reason. It is not an uncommon belief that the share market is prone to "speculative bubbles" which result in bidders' prospects (and consequently, shares) being systematically overrated for considerable periods. This belief is often associated with the view that a correction to the share market's unjustified over-valuation will be observed only in the long-run, that is, the share market does not act quickly to eliminate systematic biases in estimates of firms' prospects.

Efficient market sceptics who hold to some version of the above sketch will not be persuaded by the evidence here which supports the value-increasing hypothesis. Strong positive abnormal returns to bidding firms around the pre-bid and possibly bid periods are, in their view, consistent with takeovers being, on average, negative net present value investments. The most reliable share market evidence on takeovers is based on the evaluation of returns to successful bidders in the post-bid period since this is when the consequences of takeover become apparent and when the share market may make an informed assessment of them.

While some might consider that there are ample grounds to largely discount or even dismiss the views of the efficient market sceptics, considerable force is lent to their argument by the widespread and robust finding that bidding firms experience significantly negative abnormal returns over one to three years after acquisition of the target (e.g., Dodd (1976), Malatesta (1976), Jarrell and Poulsen (1987), Franks, Harris and Titman (1991), Limmack (1991) and Agrawal, Jaffe and Mandelker (1992)). As Jensen and Ruback (1983) comment: "These post-outcome negative abnormal returns are unsettling because they are inconsistent with market efficiency and suggest that changes in stock prices during takeovers overestimate the future efficiency gains from mergers" (p. 20).

An important contribution of this thesis is the investigation of a previously untouted explanation for the secular decline in the post-bid returns to bidding firms: survivorship bias. As demonstrated in Section 4.1.2, portfolios that comprise just surviving firms are, other things being equal, expected to record lower returns than portfolios that comprise both survivors and non-survivors. Firms that have tendered successful bids are typically firms that have done well over the event-windows and have an above average chance of surviving over any given period. Notwithstanding the successful bidders' typically exceptional performance over the pre-bid and bid periods, in an efficient market we expect these firms to achieve, on average, normal returns in the post-bid period. However, because of their above average incidence of
survival these bidder firms record a negative market adjusted return in the post-bid period (when the market portfolio includes both surviving and non-surviving firms).\textsuperscript{33}

The results from the pilot investigation (or replication) of BDO's study supports the above explanation. Over the period \([+7,+24]\) months relative to each successful bidder's bid announcement month, the mean value adjusted return to the bidder firms is \(-2.96\%\) when adjusted returns are calculated by the traditional method, that is without controlling for survivorship.\textsuperscript{34} Nine hundred and ninety six out of 1000 control portfolios achieve a higher mean return over the same period when these returns are calculated without controlling for survivorship. These results are consistent with the post-bid returns to bidding firms reported in earlier studies (e.g., Agrawal, Jaffe and Mandelker (1992)).

In sharp contrast to the above, once survivorship is controlled, the mean value-adjusted return to the sample of successful bidders over the period \([+7,+24]\) months relative to the bid announcement month is \(12.48\%\) (see Table 4.18). In order to isolate the impact of survivorship bias, the mean value-adjusted return quoted is based on the non-winsorised data and without adjusting for the positive bias in mean return caused by positive asymmetry in firms' distributions of returns (that is, without subtracting the grand mean of the 1001 control portfolios' mean value-adjusted returns). The pilot investigation results are also based on non-winsorised data and do not incorporate a control for positive asymmetry.

One interesting observation that may be made from the results in Table 4.18 is that the post-bid size-adjusted returns to bidder firms are not negative on either a survivorship adjusted or non-survivorship adjusted basis. However, the size-adjusted returns do indicate the existence of a downward bias. Note that the survivor and size-adjusted mean return to bidder firms is \(5.55\%\) while on just a size-adjusted basis the mean return to the bidder firms is \(3.44\%\). This point is of interest because it suggests that there are two independent downward biases operating when we estimate the adjusted returns to bidding firms. One of them is the systematic

\textsuperscript{33} Importantly, it is implicit in this explanation that over the pre-bid and bid periods the downward bias in the adjusted returns to the bidding firms portfolio attributable to survivorship is more than offset by these firms' otherwise exceptional performance. It is only in the post-bid period, when the bidder firms do not otherwise experience exceptional performance that the effect of the bias becomes evident.

\textsuperscript{34} Specifically: For each month over the period \([+7,+24]\) months relative to the bid announcement date, the mean value-adjusted return to the sample of bidders is estimated. That is, in each month over the event-window each sample firm's adjusted price relative is calculated by subtracting the value-weighted market price relative from the sample firm's corresponding monthly price relative and adding 1. The natural logs of the mean monthly price relatives to the sample portfolio are then summed to obtain the mean return to the successful bidders over the period \([+7,+24]\) months.
Table 4.18

Successful Bidders; Returns [+7,+24] Months Relative to the Bid Announcement Month; Returns Calculated on Both a Survivor Adjusted and Non-Survivor Adjusted Basis; Non-Winsorised Data

<table>
<thead>
<tr>
<th>Survivor Adjusted</th>
<th>No.Control Portfolios</th>
<th>Non-Survivor Adjusted</th>
<th>No.Control Portfolios</th>
</tr>
</thead>
<tbody>
<tr>
<td>Return to Sample Portfolio (%)</td>
<td>Returns &gt; Sample Return</td>
<td>Returns &gt; Sample Return</td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>12.48</td>
<td>868</td>
<td>-2.96</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>5.55</td>
<td>254</td>
<td>3.44</td>
</tr>
</tbody>
</table>
negative association between firm size and return and the other is survivorship bias.

Agrawal, Jaffe and Mandelker (1992) conclude, from their observation of a persistent decline in the post-merger returns to bidding firms even after controlling for size, that the size-effect is not the cause of the decline. The results reported here suggest that their conclusion is only partly true. The size-effect and survivorship both contribute towards the estimated decline in the post-merger abnormal return to bidding firms. Controlling for just one of the biases does not yield unbiased estimates of performance. However, the results reported in this thesis seem to indicate that for firms listed on the ASX the size-effect is responsible for the major portion of any estimated downward bias in returns to bidder firms. It might that in the larger U.S. capital market survivorship bias plays a larger role.

Notwithstanding the preceding analysis, a more appropriate period to evaluate the post-bid performance of bidding firms is after the takeover outcome is known. As discussed earlier, the share market can make its final adjustment in response to the prospect of a takeover only after the bid outcome is known. Prior to knowing the bid outcome the market's expectations of a given takeover will be discounted by the probability of success that it attaches to the takeover bid.

As it happens, the share market's post-outcome response to successful takeover bids is consistent with the value-increasing hypothesis. The mean value adjusted return to BDO's sample of successful bidders over the period [+6,+24] months relative to the bid outcome month is 3.05% (= 15.43% - 12.38%, refer Table 4.19). This mean return is equalled or bettered by 297 out of 1001 control portfolios which suggests that on a value adjusted basis successful bidding firms achieve normal performance in the post-bid period. However, note that the mean size-adjusted return to the successful bidders is 9.31% (= 9.45% - 0.14%) over the period [+6,+24] months. Only 61 out of 1001 control portfolios achieve a higher size-adjusted mean return.

While the earning of significant positive size-adjusted returns, on average, by successful bidders in the post-outcome period refutes the hypothesis that the share market systematically overestimates the gains from takeovers, the result remains anomalous with respect to the EMH. Takeovers are frequent occurrences, so we do not expect an efficient market to systematically either over- or under-estimate their benefits. In this context, note that if we define a significant return achieved by the sample firms as one that lies within 50 values of the extreme of the distribution of the 1001 control portfolios' mean returns then the size adjusted post-outcome return to the successful bidders just marginally escapes being classified as significant. One
Table 4.19
Successful Bidders; Returns [+6,+24] Months Relative to the Bid Outcome Month

<table>
<thead>
<tr>
<th></th>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios Returns (%)</th>
<th>No.Control Portfolios Returns &gt; Sample Return (%)</th>
<th>Sample Firms' Median Return (%)</th>
<th>Median of 1001 Control Portfolios Returns &gt; Sample Return (%)</th>
<th>No. Control Firms' PAR c</th>
<th>Sample Value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Raw Return</strong> b</td>
<td>55.62</td>
<td>52.56</td>
<td>297</td>
<td>40.74</td>
<td></td>
<td>.79</td>
<td>.72</td>
<td>2</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>3.04</td>
<td>-0.01</td>
<td>297</td>
<td>-4.56</td>
<td></td>
<td>.44</td>
<td>.39</td>
<td>29</td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>15.43</td>
<td>12.38</td>
<td>297</td>
<td>6.47</td>
<td></td>
<td>.55</td>
<td>.46</td>
<td>0</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>9.45</td>
<td>0.14</td>
<td>61</td>
<td>-1.03</td>
<td>-12.10</td>
<td>3</td>
<td>.49</td>
<td>.41</td>
</tr>
</tbody>
</table>

**Panel A: Censored Returns:** a  **Number of Experimental Sample Firms = 336**

<table>
<thead>
<tr>
<th></th>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios Returns (%)</th>
<th>No.Control Portfolios Returns &gt; Sample Return (%)</th>
<th>Sample Firms' Median Return (%)</th>
<th>Median of 1001 Control Portfolios Returns &gt; Sample Return (%)</th>
<th>No. Control Firms' PAR c</th>
<th>Sample Value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Raw Return</strong> b</td>
<td>57.66</td>
<td>67.60</td>
<td>776</td>
<td>40.78</td>
<td></td>
<td>.79</td>
<td>.72</td>
<td>1</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>-9.17</td>
<td>0.77</td>
<td>776</td>
<td>-16.52</td>
<td></td>
<td>.38</td>
<td>.34</td>
<td>47</td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>16.51</td>
<td>26.45</td>
<td>776</td>
<td>5.12</td>
<td></td>
<td>.55</td>
<td>.46</td>
<td>0</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>8.71</td>
<td>0.76</td>
<td>213</td>
<td>-2.33</td>
<td>-17.63</td>
<td>0</td>
<td>.49</td>
<td>.37</td>
</tr>
</tbody>
</table>

**Panel B: Uncensored Returns:** a  **Number of Experimental Sample Firms = 337**

---

a  Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.
b  The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population [R] of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.
c  PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
may classify the result as significant in an economic if not statistical sense.\textsuperscript{35}

Further investigation reveals that the significant positive mean size-adjusted return to successful bidders in the post-outcome period is driven largely by IEL's performance. IEL accounts for 18 of the 336 bids for which a return over the period [+6,+24] months post-outcome is available for the successful bidders. If the bids involving IEL are eliminated the mean post-bid size-adjusted return drops from 9.31\% (= 9.45\% - 0.14\%) to 4.42\% (= 4.42\% - 0.0\%, see Table 4.20). Two hundred and ten control portfolios achieve a mean return that either equals or is higher than 4.42\%. In sum, after eliminating IEL, the post-outcome performance of the successful bidding firms is in line with expected performance in an efficient market.\textsuperscript{36}

In justifying the deletion of IEL from the sample of successful bidders, one may note that the operation of an efficient market does not imply that individual firms do not achieve persistent exceptional share price performance. The share market might take a long period to assess a company's prospects with persistent (in hindsight) upward revisions in share price. It is only ex post that a judgment can be made as to whether the revisions were too conservative in particular instances. The correct implication of an efficient market is that, on average, across all firms the assessment of prospects is unbiased.

One finding of particular interest in the post-bid returns is that the portfolio of successful bidding firms has a higher PAR ratio than the overwhelming majority of control portfolios. This is true across all three types of adjustments, equal, value or size based and for both winsorised and non-winsorised results. The remarkable aspect of this finding is that while 621 control portfolios achieve a higher mean value-adjusted return (winsorised data), only 6 out of 1001 control portfolios register a higher PAR ratio. Similarly, the size-adjusted mean return to the sample firms is

\textsuperscript{35} Firms increase in size following a successful takeover and some might argue that the increase in size warrants an investigation into whether the merged entity's returns ought to be adjusted by the returns to a higher ranked size-decile portfolio. The problem in the present instance is that the direction of the bias which we would seek to correct by using the above procedure is opposite to the results being investigated. Given the empirically established negative association between firm size and return, re-allocating the merged entity to a higher ranked portfolio would cause it to record higher positive abnormal returns, other things being equal.

\textsuperscript{36} Eliminating IEL from the sample of bidding firms does not affect the conclusions drawn earlier about the performance of the bidding firms (successful or unsuccessful) in the pre-bid periods. While the exceptional performance of the bidding firms over these periods is decreased by the elimination of IEL, the mean size-adjusted return to bidding firms over the periods [-24,-6] months and [-6,-1] months relative to the bid announcement month remains higher than the mean size-adjusted return achieved by all 1001 control portfolios over either period. Over the [-3,+3] months relative to the bid outcome month, eliminating IEL from the sample of successful bidders decreases their mean size-adjusted return by 1.5\% (from 6.3\% to 4.8\%). While only 19 control portfolios achieve a mean size-adjusted return higher than 6.3\%, 54 control portfolios register a return greater than 4.8\%.
## Table 4.20
Successful Bidders excluding IEL; Returns [+6, +24] Months Relative to the Bid Outcome Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios Returns &gt; Sample Return</th>
<th>Sample Firms Median Return (%)</th>
<th>Median of 1001 Control Median Returns &gt; Sample Median</th>
<th>No. Control Firms' median PAR c</th>
<th>Sample value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>50.80</td>
<td>52.88</td>
<td>621</td>
<td>37.59</td>
<td>.79</td>
<td>.72</td>
<td>0</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>-2.19</td>
<td>-0.11</td>
<td>621</td>
<td>-7.38</td>
<td>.42</td>
<td>.39</td>
<td>90</td>
</tr>
<tr>
<td>Val Wtd Adj</td>
<td>10.24</td>
<td>12.33</td>
<td>621</td>
<td>3.61</td>
<td>.53</td>
<td>.46</td>
<td>6</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>4.42</td>
<td>0.00</td>
<td>210</td>
<td>3.76</td>
<td>-12.44</td>
<td>11</td>
<td>.47</td>
</tr>
</tbody>
</table>

**Panel A: Censored Returns:**
**Number of Experimental Sample Firms = 318**

| Raw Return b                   | 52.98                                         | 67.35                                         | 905                            | 37.89                                                  | .79                           | .72                                           | 3                                 |
| Eq. Wtd Adj                    | -14.40                                        | -0.02                                         | 905                            | -18.18                                                 | .36                           | .34                                           | 242                               |
| Val Wtd Adj                    | 11.43                                         | 25.81                                         | 905                            | 2.55                                                   | .53                           | .46                                           | 6                                 |
| SizeDec Adj                    | 3.61                                          | -0.08                                         | 307                            | -3.63                                                   | -17.66                        | 0                                             | .47                               |

**Panel B: Uncensored Returns:**
**Number of Experimental Sample Firms = 319**

---

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population (R) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of (R) identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
lower than the mean size-adjusted return to 210 control portfolio but only 12 control portfolios have a higher PAR ratio. One inference consistent with this finding is that the bidder firms have lower risk than other firms. This is to be expected given their larger size is correlated with higher levels of diversification and that their respective takeovers contribute, on average, to further diversification of operations. The evidence suggests that the bidder portfolio's low systematic risk is fairly priced by the market in the post-bid period so that while the mean return earned by the successful bidders is unexceptional the portfolio comprising them has a significantly high PAR ratio relative to the control portfolios.

Diversification as a means of reducing a firm's systematic risk has long been touted as one of the major motivations for takeover. This thesis is the first to document rate of return evidence that is consistent with the "lower-risk" hypothesis. The finding is inconsistent with MR's conclusion that "the desire to reduce profit variability could not have been a major determinant of takeover activity" (p. 180).

We now turn to the analysis of MR's bidders' returns over the post-bid period. See Table 4.21. Over the period [+6,+24] months relative to the bid outcome month MR's bidders earn an average size-adjusted return of -4.73% (= -4.99 - -0.26). It is not a significantly exceptional mean return for a portfolio comprising 39 firms. Out of 1001 control portfolios, 620 portfolios, each comprising 39 randomly selected firms, earn a higher mean size-adjusted return over the same period. MR's bidders' portfolio's size-based PAR ratio of .44 is lower than the PAR ratio of 465 control portfolios.

One feature that emerges from the analysis of the returns to MR's bidding firms is that mean return earned by these firms is not statistically significant over any of the event-windows reviewed so far. MR's bidder firms earn, on average, a negative return over the period [-24, -6] months relative to the bid announcement month, positive returns over the periods [-6,-1], [-3,+3] and [-1, +1] months relative to the bid announcement month, and a negative return over the period [+6, +24] months relative to the bid announcement month. The question remains whether on the whole MR's bidder firms profit from their entry into the takeover market. A review of their returns over the period [-24,+24] months relative to the bid announcement month indicates that it is unlikely that MR's bidder firms experienced, on average, an increase in share price as a result of takeover (refer Table 4.22). Over the period [-24, +24] months, MR's bidder firms earned a mean size-adjusted return of -32.13% (= -32.46 - -0.33), an achievement bettered by 736 control portfolios. The bidder portfolio's size-based PAR ratio of .35 was less than the PAR ratio of 606 out of 1001 control portfolios. In sum, when assessed against the usual magnitude of returns recorded by bidder firms over long event-windows, the share market returns
Table 4.21
MR's Bidders: Returns [+6,+24] Months Relative to the Bid Announcement Month

<table>
<thead>
<tr>
<th></th>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Firms' Returns &gt; Sample Return (%)</th>
<th>Sample Firms' Median Return (%)</th>
<th>Median of 1001 Control Portfolios' Median Returns &gt; Sample Median (%)</th>
<th>No. Control Firms' PAR c</th>
<th>Median Value of 1001 Control Portfolios' PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>34.23</td>
<td>43.17</td>
<td>731</td>
<td>26.11</td>
<td>25.98</td>
<td>496</td>
<td>.74</td>
<td>.69</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>-9.38</td>
<td>-0.44</td>
<td>731</td>
<td>-7.58</td>
<td>-11.55</td>
<td>336</td>
<td>.44</td>
<td>.41</td>
</tr>
<tr>
<td>Val Wtd Adj</td>
<td>1.93</td>
<td>10.87</td>
<td>731</td>
<td>-3.88</td>
<td>19.38</td>
<td>666</td>
<td>.49</td>
<td>.46</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>-4.99</td>
<td>-0.26</td>
<td>620</td>
<td>-5.76</td>
<td>-9.61</td>
<td>351</td>
<td>.44</td>
<td>.41</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a Number of Experimental Sample Firms = 39

Panel B: Uncensored Returns: a Number of Experimental Sample Firms = 39

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.
b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population [R] of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.
c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
Table 4.22

MR's Bidders; Returns [-24, +24] Months Relative to the Bid Announcement Month

<table>
<thead>
<tr>
<th></th>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Firms'</th>
<th>Sample Median of 1001 Control Median Returns (%)</th>
<th>No. Control Firms'</th>
<th>Sample PAR</th>
<th>Median Value of 1001 Control PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return</td>
<td>118.94</td>
<td>182.80</td>
<td>931</td>
<td>104.67</td>
<td>.91</td>
<td>.88</td>
<td>357</td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>-63.73</td>
<td>0.12</td>
<td>931</td>
<td>-43.66</td>
<td>.26</td>
<td>.32</td>
<td>858</td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>6.82</td>
<td>70.67</td>
<td>931</td>
<td>0.19</td>
<td>.5</td>
<td>.47</td>
<td>490</td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>-32.46</td>
<td>-0.33</td>
<td>736</td>
<td>-19.88</td>
<td>-42.95</td>
<td>174</td>
<td>.35</td>
<td>.35</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a Number of Experimental Sample Firms = 34

Panel B: Uncensored Returns: a Number of Experimental Sample Firms = 34

---

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population [R] of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
earned by MR's bidders as a result of their involvement in takeovers may be judged to be disappointing. This empirically grounded conclusion supports the argument that MR's sample selection criteria systematically excluded firms with a comparative advantage in managing takeovers.

4.3.2 Review of Target Firms' Returns

4.3.2.1 Pre-bid period returns

One implication of the value-increasing hypothesis of takeovers is that relatively poor performing firms are the typical targets of better managed bidder firms. While well managed firms might also find themselves targets as a result of synergies afforded by combining their particular resources with those of their bidders, we would expect poor performers to be the more typical target of takeover bids.

A review of target firms' returns over the period [-24,-6] months relative to the bid announcement month reveals evidence consistent with the above argument. From Table 4.23 we see that over this period, target firms earn an average size-adjusted return of -7.02% (= -7.09% - 0.07%)\textsuperscript{37}, a mean return lower than that achieved by 953 out of 1001 control portfolios. Seven hundred and sixty nine control portfolios record a size-based PAR greater than the target firms' portfolio's size-based PAR ratio of .39. The poor performance of the target firms is evident on a value-adjusted basis as well. The value-adjusted return of -8.02% (= 6.89% - 14.92%) earned by the target firms over the period [-24, -6] months is lower than the value-adjusted mean return to 966 control portfolios.\textsuperscript{38}

An intriguing aspect of the above findings is that they diverge substantially from those reported by BDO. BDO conclude from their results that "the sample of target companies does not, on average, display abnormally poor performance over the three years before the offers were made ... On average the targets were not firms experiencing severe financial difficulties" (p. 60). This conclusion provides us with one of the starkest examples of the pitfalls that are encountered when one does not assess the validity and significance of event study results against an empirical

\textsuperscript{37} A reminder that, as discussed in Section 4.2, the mean return to the 1001 control portfolios is an unbiased estimate of the positive bias in the sample portfolios mean return attributable to positive skewness in firms' distributions of returns. Results reported in discussion are after adjusting for the positive bias.

\textsuperscript{38} As with all other mean returns quoted in discussion so far, the target firms' mean return of -8.02% is after adjusting for the positive bias in portfolio mean returns attributable to positive asymmetry in firms' distributions of returns. Specifically, the grand mean of the 1001 control portfolios' mean returns, 14.92%, which is a fair estimate of the bias, is subtracted from the mean value-adjusted return of 6.89% recorded by the sample target firms. Note that if the bias is not taken into account and one does not have an empirical distribution of returns to compare the sample portfolio's returns it is easy to fall into the trap of considering that the target firms were, on the basis of their estimated mean value-adjusted return, above average performers in the share market.
<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios Returns &gt; Sample Return (%)</th>
<th>Sample Firms' Median Return (%)</th>
<th>Median of 1001 Control Portfolios' Median Returns (%)</th>
<th>No. Control Firms' Median Returns &gt; Sample Median</th>
<th>Sample Firms' PAR c</th>
<th>Median value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>46.77</td>
<td>54.79</td>
<td>966</td>
<td>29.06</td>
<td>.77</td>
<td>.75</td>
<td>110</td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>-8.09</td>
<td>-0.07</td>
<td>966</td>
<td>-16.09</td>
<td>.38</td>
<td>.38</td>
<td>631</td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>6.89</td>
<td>14.92</td>
<td>966</td>
<td>-7.90</td>
<td>.44</td>
<td>.47</td>
<td>942</td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>-7.09</td>
<td>0.07</td>
<td>953</td>
<td>-12.72</td>
<td>-12.28</td>
<td>569</td>
<td>.39</td>
<td>769</td>
</tr>
</tbody>
</table>

**Panel A: Censored Returns:** Number of Experimental Sample Firms = 570

**Panel B: Uncensored Returns:** Number of Experimental Sample Firms = 576

---

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population (R) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of (R) identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
benchmark distribution. The value-adjusted return of -8.02% quoted above is after subtracting the grand mean return of 14.92% to the 1001 control portfolios over the period [-24,-6] months relative to the bid announcement month. The grand mean return is, as explained in Section 4.2, a fair estimate of the upward bias in the mean return to a portfolio attributable to positive asymmetry in returns. If we do not subtract this return, the value-adjusted mean return to BDO’s targets is 6.89%. This is a reasonable performance only if we do not take into account that the expected mean value-adjusted return to a randomly selected portfolio is 14.92%.

The share market performance of MR’s sample of target firms over the period [-24, -6] months relative to the bid announcement month mirrors the performance of BDO’s targets over the same period (refer Table 4.24). While MR's targets' size-adjusted return of 33.33% (= -33.41% - -0.08%) is lower than the mean return of -7.02% (= -7.09% - -0.07%) earned by BDO's targets, MR's targets' performance relative to the performance of the 1001 control portfolios is not much worse than BDO's relative performance.39 Nine hundred and sixty seven out of 1001 control portfolios have a higher mean size-adjusted return than MR's sample of target firms.

A robust finding in the takeover literature is that news of an impending takeover bid for a specific target is leaked to the share market as early as six months prior to the bid announcement month. In any event, over the period [-6,-1] months relative to the bid announcement month target firms' shareholders experience a spectacular turn-around in their fortunes (see Table 4.25). The mean size-adjusted return to the target firms' over this period is 6.58% (= 7.91% - 1.33%), a mean return not bettered by any of 1001 control portfolios. The target firms' portfolio's size-based PAR ratio of .56 over this period is also not bettered by the PAR ratio of any of the 1001 control portfolios. The speed and magnitude of the turn-around lends additional support to earlier studies' conclusions that the positive returns to target shareholders "are a direct result of the merger proposal and do not reflect any pre-merger good performance by these firms" [Dodd (1980), p. 134].

Interestingly, MR’s sample of target firms do not display a similar sharp turn-around in fortune over the period [-6,-1] month relative to the bid announcement (see Table 4.26). While MR's targets' mean size-adjusted return of 2.34% over this period is an improvement over their negative mean return earned over the previous 15 months, a higher mean size-adjusted mean return is earned by 331 control portfolios. This finding is consistent with this set of targets being viewed as unlikely generators of

---

39 MR's sample of target firms with available data over the period [-24,-6] months relative to the bid announcement month numbered only 25 while BDO sample of target firms with data available over the same period numbered 570. Given the disparity in sample sizes, the mean returns to the two sample portfolios are not comparable.
Table 4.24
MR's Targets: Returns [-24,-6] Months Relative to the Bid Announcement Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios</th>
<th>Sample Firms' Returns &gt; Sample Return (%)</th>
<th>Median of 1001 Control Portfolios Median Returns &gt; Sample Return (%)</th>
<th>No. Control Median Returns &gt; Sample Median</th>
<th>Sample Firms' PAR c</th>
<th>Median value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>32.54</td>
<td>66.48</td>
<td>965</td>
<td>25.20</td>
<td>.92</td>
<td>.84</td>
<td>143</td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>-34.17</td>
<td>-0.23</td>
<td>965</td>
<td>-22.44</td>
<td>.24</td>
<td>.36</td>
<td>916</td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>-13.03</td>
<td>20.91</td>
<td>965</td>
<td>-6.11</td>
<td>.44</td>
<td>.48</td>
<td>722</td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>-33.41</td>
<td>-0.08</td>
<td>967</td>
<td>-13.93</td>
<td>.32</td>
<td>.36</td>
<td>770</td>
<td></td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a Number of Experimental Sample Firms = 25

Panel B: Uncensored Returns: a Number of Experimental Sample Firms = 25

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a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.
b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population [R] of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.
c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
Table 4.25

All Targets; Returns [-6, -1] Months Relative to the Bid Announcement Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios Returns &gt; Sample Return</th>
<th>Sample Firms' Median Return (%)</th>
<th>Median of 1001 Control Median Returns &gt; Sample Median</th>
<th>No. Control Firms' PAR c</th>
<th>Sample value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>19.93</td>
<td>12.15</td>
<td>0</td>
<td>12.68</td>
<td>.71</td>
<td>.66</td>
<td>5</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>7.79</td>
<td>0.00</td>
<td>0</td>
<td>3.98</td>
<td>.55</td>
<td>.44</td>
<td>0</td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>10.62</td>
<td>2.83</td>
<td>0</td>
<td>6.31</td>
<td>.59</td>
<td>.48</td>
<td>0</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>7.91</td>
<td>1.33</td>
<td>0</td>
<td>4.00</td>
<td>-2.77</td>
<td>0</td>
<td>.56</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a Number of Experimental Sample Firms = 573

Panel B: Uncensored Returns: a Number of Experimental Sample Firms = 582

| Raw Return b                  | 25.99                                       | 16.80                                          | 9                               | 13.18                                         | .72                      | .67                             | 4                                |
| Eq. Wtd Adj                   | 9.32                                        | 30.30                                          | 9                               | -0.03                                         | .50                      | .39                             | 0                                |
| Val W'td Adj                  | 16.21                                       | 7.02                                           | 9                               | 6.30                                          | .59                      | .48                             | 0                                |
| SizeDec Adj                   | 9.47                                        | 0.24                                           | 8                               | 29.65                                         | -4.89                    | 0                               | .52                              |

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.
b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population [R] of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.
c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)&lt;sup&gt;b&lt;/sup&gt;</th>
<th>No. Control Portfolios' Returns &gt; Sample Return (%)</th>
<th>Sample Firms' Median Return (%)</th>
<th>Median of 1001 Control Median Returns &gt; Sample Median (%)</th>
<th>No. Control Firms' PAR&lt;sup&gt;c&lt;/sup&gt;</th>
<th>Sample value of 1001 Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return&lt;sup&gt;b&lt;/sup&gt;</td>
<td>15.80</td>
<td>13.14</td>
<td>310</td>
<td>11.16</td>
<td>.69</td>
<td>.69</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>2.74</td>
<td>0.08</td>
<td>310</td>
<td>-1.04</td>
<td>.50</td>
<td>.42</td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>7.63</td>
<td>4.98</td>
<td>310</td>
<td>2.81</td>
<td>.54</td>
<td>.50</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>2.57</td>
<td>0.23</td>
<td>331</td>
<td>0.36</td>
<td>-3.16</td>
<td>230</td>
</tr>
</tbody>
</table>

**Panel A: Censored Returns:**

*Number of Experimental Sample Firms = 26*

| Raw Return<sup>b</sup>        | 15.80                                         | 17.34                                         | 476                           | 11.16                                         | .69                             | .73                                                   | 691                       |
| Eq. Wtd Adj                   | -1.34                                         | 0.20                                          | 476                           | -8.05                                         | .35                             | .38                                                   | 687                       |
| Val W'td Adj                  | 7.02                                          | 8.56                                          | 476                           | 1.79                                          | .54                             | .50                                                   | 461                       |
| SizeDec Adj                   | -1.57                                         | 0.21                                          | 485                           | -5.42                                         | -5.04                           | 538                                                   | .38                       |

**Panel B: Uncensored Returns:**

*Number of Experimental Sample Firms = 26*

---

<sup>a</sup> Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

<sup>b</sup> The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population [R] of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.

<sup>c</sup> PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
synergistic gains and, in consequence, unlikely attractors of high takeover premiums.

4.3.2.2 Bid-announcement period returns

Over the period [-3,+3] months relative to the bid announcement month BDO's target firms record their most extraordinary performance. Table 4.27 describes their results. The 473 targets with a return available over this period record a mean size-adjusted return of 24.74% (= 25.01% - 0.27%) which is higher than the mean size-adjusted return attained by any of 1001 control portfolios. More tellingly, while the median value in the distribution of the 1001 control portfolios' size-based PAR ratios is .45 the size-based PAR ratio of the targets' portfolio is .73. None of the 1001 control portfolios achieve a PAR ratio as high as .73. The unambiguous conclusion one may draw is that regardless of the effects of takeover activity on other firm stakeholders, the shareholders of target companies experience extraordinary positive returns, on average, on announcement of a takeover bid.

The last conclusion applies as well to MR's sample of target firms (see Table 4.28). Over the period [-3,+3] months MR's target firms achieved a mean size adjusted return of 32.85% (= 33.14% - 0.29%) which was bettered by only 8 out of 1001 control portfolios. The target portfolios size-based PAR ratio of .71, which was either equalled or bettered by only 22 control portfolios, reinforces MR's conclusion that the shareholders in their sample of target firms benefited, on average, from takeover. We should note that only 14 out of a possible 51 target firms had a return available over the period [-3,+3] months relative to the bid announcement month. MR's sample comprised just targets of successful bids. It is probable that most target shareholders sold their shares early and by month [+3], trades were not recorded on the ASX for the majority of targets companies in MR's sample.

4.3.2.3 Post-bid outcome period returns

The compelling evidence that shareholders in target firms are enriched by takeover bid announcements prompts an interest in the share market returns earned by target companies that were subject to withdrawn or unsuccessful bids. In line with BDO, we find that targets of unsuccessful or withdrawn bids do not lose their takeover related gains once the takeover bid outcome becomes public. Over the period [-3,+3] months relative to the bid outcome becoming publicised the targets of unsuccessful takeover bids earn a mean size-adjusted return of 5.92% (= 6.39% - 0.47%) which is beaten by only 157 control portfolios (see Table 4.29). Moreover, the unsuccessful target firms' portfolio's size-based PAR ratio of .51 is equalled or bettered by only 226 control portfolios. Over the same event-window, targets
Table 4.27

All Targets; Returns [-3, +3] Months Relative to the Bid Announcement Month

<table>
<thead>
<tr>
<th></th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios</th>
<th>Sample Firms' Returns (%)</th>
<th>Median of 1001 Control Portfolios' Returns &gt; Sample Return (%)</th>
<th>Median of 1001 Control Portfolios' Median Returns &gt; Sample Return (%)</th>
<th>No. Control Sample Firms' PAR c</th>
<th>Median value of 1001 Control Portfolios PARs</th>
<th>No. of Control Sample PARs</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>40.61</td>
<td>15.86</td>
<td>0</td>
<td>32.07</td>
<td>.84</td>
<td>.66</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>28.99</td>
<td>4.23</td>
<td>0</td>
<td>22.85</td>
<td>.75</td>
<td>.48</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>25.01</td>
<td>0.27</td>
<td>0</td>
<td>19.53</td>
<td>-3.7</td>
<td>.73</td>
<td>.45</td>
<td>0</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a  Number of Experimental Sample Firms = 473

Panel B: Uncensored Returns: a  Number of Experimental Sample Firms = 496

| Raw Return b     | 50.71                                       | 20.79                  | 0                         | 33.33                                                         | .85                                                               | .67                         | 0                                           |                           |
| Eq. Wtd Adj      | 29.91                                       | -0.01                  | 0                         | 15.93                                                         | .70                                                               | .39                         | 0                                           |                           |
| Val W'td Adj     | 38.85                                       | 8.92                   | 0                         | 24.58                                                         | .76                                                               | .49                         | 0                                           |                           |
| SizeDec Adj      | 30.45                                       | -0.08                  | 0                         | 18.37                                                         | -5.78                                                             | .73                         | .42                                        | 0                          |

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population (R) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of (R) identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
Table 4.28

MR's Target firms; Returns [-3,+3] Months Relative to the Bid Announcement Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios * Returns &gt; Sample Return</th>
<th>Median of Sample Firms' Sample Return (%)</th>
<th>Median of 1001 Control Median Returns (%)</th>
<th>No. Control Median Returns &gt; Sample Median</th>
<th>Sample Firms' PAR c</th>
<th>Median value of 1001 Control PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>52.07</td>
<td>18.75</td>
<td>8</td>
<td>35.97</td>
<td>.79</td>
<td>.71</td>
<td>330</td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>33.49</td>
<td>0.16</td>
<td>8</td>
<td>20.79</td>
<td>.64</td>
<td>.43</td>
<td>55</td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>36.09</td>
<td>2.77</td>
<td>8</td>
<td>21.60</td>
<td>.64</td>
<td>.43</td>
<td>90</td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>33.14</td>
<td>0.29</td>
<td>8</td>
<td>20.06</td>
<td>-4.96</td>
<td>6</td>
<td>.71</td>
<td>.43</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a Number of Experimental Sample Firms = 14

Panel B: Uncensored Returns: a Number of Experimental Sample Firms = 16

| Raw Return b                   | 68.82                                       | 22.57                                         | 27                                 | 43.25                                    | .81                                         | .69             | 155                              |                               |
| Eq. Wtd Adj                    | 46.43                                       | 0.85                                          | 27                                 | 24.00                                    | .63                                         | .38             | 25                               |                               |
| Val W'td Adj                   | 55.56                                       | 9.32                                          | 27                                 | 26.59                                    | .69                                         | .44             | 63                               |                               |
| SizeDec Adj                    | 49.03                                       | 0.15                                          | 20                                 | 23.14                                    | -6.32                                       | 0               | .75                              | .38                           |

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population (R) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of (R) identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
Table 4.29

Targets Subject to Unsuccessful Bids; Returns [-3,+3] Months Relative to Bid Outcome Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns &gt; (%)b</th>
<th>No. Control Portfolios</th>
<th>Sample Firms' Returns &gt; (%)</th>
<th>Sample Firms' Median Return (%)</th>
<th>Median of 1001 Control Median Returns &gt; Sample Median</th>
<th>No. Control Firms' PAR c</th>
<th>Sample value of 1001 Control PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>21.06</td>
<td>15.95</td>
<td>175</td>
<td>7.84</td>
<td>.65</td>
<td>.64</td>
<td>44</td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>5.38</td>
<td>0.27</td>
<td>175</td>
<td>-3.59</td>
<td>.49</td>
<td>.45</td>
<td>248</td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>9.05</td>
<td>3.94</td>
<td>175</td>
<td>-9.70</td>
<td>.48</td>
<td>.48</td>
<td>545</td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>6.39</td>
<td>0.47</td>
<td>157</td>
<td>-2.75</td>
<td>0.39</td>
<td>199</td>
<td>.51</td>
<td>226</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a Number of Experimental Sample Firms = 75

Panel B: Uncensored Returns: a Number of Experimental Sample Firms = 77

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population [R] of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
subject to withdrawn bids earn a mean size-adjusted return of 6.33% (= 6.25% -0.08%), a performance which only 106 control portfolios are able to surpass (see Table 4.30). Curiously, while 812 out of 1001 control portfolios have a size-adjusted median return higher than the withdrawn target's portfolio median return of -7.62%, only 499 control portfolios have a size-based PAR ratio that either equals or is higher than the withdrawn targets' portfolio's PAR ratio of .43. One interpretation of these results is that in those instances where the share market judges the withdrawal of a takeover bid is an adverse signal of the target's "quality", a sharp downward revaluation occurs. However, for the most part this is not the conclusion drawn by the market and, on average, a positive return is earned by target firms subject to withdrawn bids.

In explaining their results, which are consistent with those reported here, BDO follow Walter (1980) in positing three reasons why target firms which are not taken over retain the positive abnormal returns earned as a result of a takeover bid being made for them; "One reason is that the offer, even though it did not effect a change in control, triggered a change in the target firm's investment strategy and the market estimates this new strategy will increase future profitability. Alternatively, the permanent increase in the value of the target could reflect the market's expectation of another unsuccessful takeover bid now that the target has been identified as an avenue for potential value creation. Of the 118 unsuccessful targets [in BDO's sample], 57 were subject to a subsequent takeover offer and 42 of these were successful. Further, the bid may have caused valuable information about the target company to be released to the market, thus causing the [permanent] revaluation" (p. 62).

The returns to targets of unsuccessful bidders over the period [-24, +24] months relative to the bid outcome month are consistent with Walter's and BDO's interpretation (see Table 4.31). Over this period the 53 targets of unsuccessful bids which had data available recorded a size-adjusted mean return of 52.78% (= 54.57% - 1.79%). Only 124 out of 1001 control portfolios earned a higher size-adjusted mean return over the same period. Further, only 29 control portfolios had a size-based PAR ratio that equalled or was higher than the unsuccessful targets' portfolio's size-based PAR ratio of .51.

In sum, the evidence overwhelmingly supports the proposition that shareholders of target firms benefit from takeover activity. Given a regulatory regime similar to that which prevailed in Australia from 1974 to mid-1985, concern about the effect of takeovers on the welfare of target firm shareholders is misplaced. Target firms' shareholders benefit unambiguously from a free market for corporate control.
Table 4.30

Targets Subject to Withdrawn Offers; Returns [-3, +3] Months Relative to the Bid Outcome Month

<table>
<thead>
<tr>
<th></th>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)b</th>
<th>No. Control Portfolios Returns &gt; Sample Return</th>
<th>Sample Firms' Median Return (%)</th>
<th>Median of 1001 Control Median Returns (%)</th>
<th>No. Control Median Returns &gt; Sample Median</th>
<th>Sample Firms' PAR c</th>
<th>Median value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>20.91</td>
<td>15.74</td>
<td>156</td>
<td>5.96</td>
<td></td>
<td></td>
<td>.60</td>
<td>.70</td>
<td>942</td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>4.97</td>
<td>-0.20</td>
<td>156</td>
<td>-5.86</td>
<td></td>
<td></td>
<td>.47</td>
<td>.42</td>
<td>249</td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>7.57</td>
<td>2.40</td>
<td>156</td>
<td>-0.81</td>
<td></td>
<td></td>
<td>.49</td>
<td>.45</td>
<td>376</td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>6.25</td>
<td>-0.08</td>
<td>106</td>
<td>-7.62</td>
<td>-4.36</td>
<td>812</td>
<td>.43</td>
<td>.42</td>
<td>499</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns:a Number of Experimental Sample Firms = 53

Panel B: Uncensored Returns:a Number of Experimental Sample Firms = 56

a Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.

b The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective event-windows correspond is identified. (ii) For each period, the population [R] of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of [R] identified.

c PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
Table 4.31

Targets Subject to Unsuccessful Bids; Returns [-24, +24] Relative to the Bid Outcome Month

<table>
<thead>
<tr>
<th>Return to Sample Portfolio (%)</th>
<th>Mean of 1001 Control Portfolios' Returns (%)</th>
<th>No. Control Portfolios Returns &gt; Sample Return</th>
<th>Sample Firms' Returns &gt; Sample Return (%)</th>
<th>Median of 1001 Control Portfolios' Median Returns (%)</th>
<th>No. Control Portfolios Median Returns &gt; Sample Median</th>
<th>Sample Firms' PAR c</th>
<th>Median value of 1001 Control Portfolios PARs</th>
<th>No. of Control PARs ≥ Sample PAR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Raw Return b</td>
<td>223.70</td>
<td>170.40</td>
<td>124</td>
<td>125.16</td>
<td>.85</td>
<td>.77</td>
<td>131</td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>54.96</td>
<td>16.64</td>
<td>124</td>
<td>-1.90</td>
<td>.49</td>
<td>.34</td>
<td>26</td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>105.75</td>
<td>52.44</td>
<td>124</td>
<td>42.40</td>
<td>.58</td>
<td>.45</td>
<td>50</td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>54.57</td>
<td>1.79</td>
<td>124</td>
<td>0.79</td>
<td>-48.95</td>
<td>29</td>
<td>.51</td>
<td>29</td>
</tr>
<tr>
<td>Raw Return b</td>
<td>312.07</td>
<td>229.90</td>
<td>96</td>
<td>127.23</td>
<td>.85</td>
<td>.77</td>
<td>132</td>
<td></td>
</tr>
<tr>
<td>Eq. Wtd Adj</td>
<td>82.65</td>
<td>0.48</td>
<td>96</td>
<td>-45.82</td>
<td>.44</td>
<td>.27</td>
<td>7</td>
<td></td>
</tr>
<tr>
<td>Val W'td Adj</td>
<td>188.93</td>
<td>10.68</td>
<td>96</td>
<td>47.01</td>
<td>.59</td>
<td>.46</td>
<td>30</td>
<td></td>
</tr>
<tr>
<td>SizeDec Adj</td>
<td>122.46</td>
<td>0.96</td>
<td>80</td>
<td>125.27</td>
<td>2.25</td>
<td>3</td>
<td>.52</td>
<td>.33</td>
</tr>
</tbody>
</table>

Panel A: Censored Returns: a  Number of Experimental Sample Firms = 53

Panel B: Uncensored Returns: a  Number of Experimental Sample Firms = 54

a  Uncensored returns are based on the full set of raw returns in each period. Censored or winsorised returns are based on the sub-sample of returns that are less than or equal to three standard deviations away from the full population mean return. All listed firms' returns, including the sample firms' returns, are subject to censoring.
b  The return to each control portfolio is defined as the mean of its constituent returns. Selection procedure: (i) The set of periods to which the sample firms' respective events-windows correspond is identified. (ii) For each period, the population (R) of returns to all listed firms is identified. (iii) Each of the 1001 control portfolios includes a return, drawn independently at random with replacement, from each of the sets of (R) identified.
c  PAR = positive adjusted returns. PAR ratio = number of positive adjusted returns divided by the total number of adjusted returns in each portfolio.
CHAPTER FIVE
ACCOUNTING NUMBERS: REVIEW OF METHOD

5.0 INTRODUCTION

The use of accounting numbers to assess the performance of firms involved in takeovers is problematic. BDO cite the long time it takes for the effects of a takeover to be reflected in accounting numbers, the variety in accounting practices between companies, and systematic bias in accounting based firm performance measures as their reasons for preferring the capital market rate of return over accounting numbers in assessing the performance of their sample of firms.

The practical difficulties in using accounting numbers to assess firm performance often give rise to the mistaken view that accounting numbers are absolutely inferior to capital market rate of return as a measure of firm performance. Given this misconception, Section 5.1 reviews the efficacy of accounting numbers in yielding valid measures of firm performance. A key point made is that the power of unadjusted accounting numbers to assess firms' relative performance is high under certain conditions and for certain comparisons. Section 5.2 summarises briefly the key findings in the MR study and critiques the research method adopted. The focus is on the validity of the widely reported criticisms of the study. Section 5.3 describes and defends the research design adopted in this thesis. Results and conclusions are reported in Section 5.4.

5.1 ACCOUNTING NUMBERS AND FIRM PERFORMANCE: A REVIEW

Two issues are important: (i) the appropriateness of using accounting data to assess firm performance and (ii) ways of overcoming or avoiding the systematic biases in accounting performance indicators caused by the methods commonly used to account for a takeover. These two concerns are discussed separately below.

5.1.1 Accounting Numbers as Performance Indicators

Views on the ability of accounting data to convey valid information about firm value range from Fisher and McGowan's (1983) extreme scepticism to Ou and Penman's

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1 A report in The Australian (17 June 1986, p. 14) suggested, falsely, that McDougall and Round's choice of an accounting based evaluation of their sample's performance was a "second best" option forced on them. For some information sought by McDougall and Round the use of accounting numbers is unavoidable.

2 An oft cited quote from Fisher and McGowan (1983, p. 90) reads as follows. "There is no way in which one can look at accounting rates of return and infer anything about relative economic
(1989) claim that "share price changes are poor predictors of future earnings relative to financial statement information" (pp. 112-113).

The inadequacy of accounting earnings as a measure of firm performance is usually based on the inability of the accounting rate of return to proxy accurately for economic rate of return. It is probable that this comparison is inappropriate.

It is well established that return on investment (ROI) based on accounting data will not equal economic rate of return except under conditions of perfect and complete markets. However, under such conditions it is unclear what is the additional role of accounting earnings since market values are identical for everyone and can be easily ascertained. As Beaver notes, "once value is determined, an earnings concept is redundant" (1989, p. 75). The provision and use of accounting data are better understood from their role as a source of information in imperfect and incomplete markets (i.e., the conditions which characterise observable markets). This informational perspective on accounting data is briefly elaborated below.

In imperfect and incomplete markets, prices of goods are costly to observe and some goods may not be valued at all since a market for them does not exist. As Ball (1975) and Beaver (1989) point out, one effect is that observed market values do not necessarily possess the same properties that they do in perfect and complete markets. For example, market value maximisation is not necessarily the unanimous choice of firm stakeholders because "markets are no longer rich enough to sort out the individual heterogeneity of preferences and beliefs . . . An important implication is that any concept of earnings [including capital market rates of return] that is valuation based is no longer a well defined concept [Beaver (1989), p. 88, emphasis in original].

In imperfect and incomplete markets the role of accounting earnings may be to convey information about the firm not available elsewhere. In the present context, economists (and others) who believe that analysis of accounting rates of return will tell them much (if only they can overcome the various definitional problems which separate economists and accountants) are deluding themselves". 3 See, for example, Harcourt (1965), Solomon (1966), Fisher and McGowan (1983), Salamon (1988), and Kelly and Tippett (1989).

Beaver (1989, p. 50) defines a perfect market as one where "(i) trading of commodities and claims takes place at zero transactions costs, (ii) no firm or individual has any special advantage or opportunity to earn abnormal returns on its investments, and (iii) prices are invariant to the actions of any individual or firm". A complete market means "that markets exist for all commodities or claims, and hence the market price for any commodity or claim is publicly observable" (ibid.).

The informational perspective can be contrasted with the valuation perspective to accounting earnings which most critics of accounting ROI have adopted. The distinction, as Ball (1975, p. 21) points out, derives from the difference between the "notion of a price, and its role in a market mechanism [and] the notion of an estimate of a price". It seems more reasonable to compare the usefulness of an estimate of a price with the best available alternative rather than with an ideal measure.

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the insight yielded is that accounting earnings are more appropriately judged against practical and available alternatives rather than against ideal measures (e.g., economic income) which may be impossible to attain. The informational perspective implies that the usefulness of accounting earnings is an empirical issue. Unfortunately, as Ball (1975), Beaver (1989) and Penman (1991) have noted, the issue is still very much an open one.

Notwithstanding the lack of a well grounded theory on the relationship between accounting data and firm value, there are substantive arguments in support of the valid use of accounting data to assess firm performance. One line of argument relies on evidence of the use made of accounting information in capital markets to demonstrate the utility of accounting data. The other line of argument is more general and relies on a priori evidence. Both lines are explored below.

5.1.1.1 Capital markets and accounting data: the evidence

The association between capital market rate of return and accounting ROI has been used as a test of the information content of accounting earnings. Three results from this research are pertinent. (i) The release of annual accounting reports causes an abnormal revision (i.e., after controlling for risk and market return) in share price of about 14.6%. Changes in earnings alone capture about half of that (i.e., about 6.9%) [Ball and Brown (1968)]. (ii) Accounting earnings account for 7% of the across-firm variance in annual abnormal returns, on average. (iii) Two year accounting earnings are associated with about 12% of the variance in share returns over the corresponding two years; five year accounting earnings explain approximately 30% of the variance in five year returns; and ten year accounting earnings summarise over 60% of the variance in share returns over the same ten year period [Easton, Harris and Ohlson (1989)].

The "good news" in the above results is (i) that there is some informational content in accounting numbers and (ii) the association between accounting earnings and accounting numbers to yield accurate estimates of price should take into account information costs. Is a system which provides more up-to-date estimates of price always to be preferred over a less timely reporting system after taking into account cost? (Ball presumes an inverse relationship between cost and the extent to which price information is dated.) Black (1980), for one, argues that when one considers the information available to accountants, accounting earnings perform "a remarkably good job" as estimates of firm value (p. 19).

Barnes (1987), in a review article on the use of financial ratios, observes that it is axiomatic in much research that accounting ratios are good indicators of a firm's financial and business performance but, typically, little argument is provided in support of the axiom (probably because of the lack of a widely agreed theory relating the two).

Baruch Lev (1989), in his comprehensive review, describes this line of investigation as constituting "the most concerted research effort in accounting history". Ball and Brown's (1968) study launched the approach.
share prices is stronger the longer the period over which the association is measured. Unfortunately, the import of the "bad news" threatens to overwhelm that of the "good news". The "bad news" is that accounting earnings explain only about 6 to 7 per cent of the variance in firms' abnormal share returns. This has proved to be a robust result over the 26 years of research since Ball and Brown (1968).

Notwithstanding the above, there is much to suggest that the disappointingly low power of accounting earnings to explain annual abnormal share returns is probably a misleading indicator of the utility of accounting numbers in capital markets. Roll (1988) points out that the notion of most financial economists that "with hindsight, they could explain most asset-price movements with authenticated information" (p. 542) is not supported empirically. Roll finds in his research that over two thirds of the daily variability in individual firms' share price is unexplained by returns models incorporating either a CAPM based index or a set of five Arbitrage Pricing Theory (APT) variables. Patell (1989) observes that Roll's finding alerts us to the possibility that the CAPM and various APT models "are significantly incomplete as fundamental valuation models at the individual firm level" (p. 197). The large body of evidence, discussed in Chapter Three, which is anomalous with respect to the CAPM is consistent with Patell's conjecture. Given flawed valuation models, it is unreasonable to expect accounting researchers to explain more than a modest proportion of share returns from accounting earnings.

Finally, it is worth noting that at least one prominent researcher thinks that inferring the information content of accounting numbers from the degree to which share prices move in response to changes in the numbers is "putting the cart before the horse". Penman (1991) argues that the discovery of phenomena anomalous with respect to the EMH such as persistent post-earnings drift implies we ought to be less sanguine about market efficiency. He states that "a healthy scepticism for the EMH" would lead to researchers placing more emphasis on developing a theory about how to "generate value indicators from observables about firm activities without references to prices" (pp. 11-12). In the present context, the implication of a more sceptical attitude to the EMH is that capital market rates of return are not necessarily less biased than accounting based estimates of return.

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9 Easton et al. suggest that the reason for the stronger association of longer term multi-year earnings with multi-period share returns is that short-term errors in the accrual process average out over the years. Easton et al.'s result must impress those who view share returns as being unequivocally the best measure of firm performance and doubt the capacity of accounting numbers to reflect anything of value about firm performance even over the long term.

10 Patell also points out the probability that accounting earnings are directed towards more than one market (e.g., the debt market, the labour market and the political market). Compromises necessitated by satisficing these different markets probably result in a bottom line earnings figure that is less than optimal in any one market.
5.1.1.2 A priori arguments in support of the utility of accounting data

(i) One reason for expecting accounting earnings to reflect firm performance is that the accrual process is designed to reorganise "the apparent timing of economic events so as more faithfully to represent economic reality" [Patell (1989, p. 199), citing Ijiri (1967)]. The appropriateness of this reorganisation of economic data is often challenged (implicitly on occasion). For instance, Lev (1989) criticises accounting researchers for "cavalierly" accepting reported earnings at face value and not questioning the "quality" of the earnings. However, it is not obvious that the researchers' method is a weakness. One view of annual report data is that they are products directed towards different markets and the final products represent the equilibrium outcome. If researchers were to adjust the reported figures in an effort to attain an ill-defined "true" measure of earnings, they would effectively be second guessing the reporting system in an ad hoc fashion. The point is not that the accounting system is mistake free but that without a well specified theory relating accounting data to firm valuation, researchers cannot justify attempts to manipulate the reported figures in the accounts in order to increase the "quality" of earnings. For more on this issue, see below.

(ii) Notwithstanding the results reported in Section 5.4, the enormous attention given to, and importance placed on, annual report data by share market analysts is consistent with these data being highly useful in setting capital market prices. Indeed the same rational behaviour, wealth maximising assumptions that underpin the Efficient Market Hypothesis and associated share market research support the notion that accounting earnings are useful. Why would so much time and effort be

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11 Evidence which supports the proposition that accrual accounting data are more informative than merely reporting cashflows is found in Ball and Brown (1968), Beaver and Dukes (1972) and Wilson (1986, 1987). But see Bernard and Stober (1989), who found that Wilson's results were sample specific.

12 Mian and Smith (1990) make a similar point in stating that rational evaluation of the observed variation in choice of accounting method is problematic in the absence of a well specified theory on why cross-sectional variation is to be expected.

13 Lev (1989, footnote 35) cites a 21% increase in General Motors' 1987 reported income figure, caused entirely by a switch to longer depreciation periods for the company plant and equipment, as one example of poor quality earnings which ought to be adjusted by a researcher but probably would not be adjusted. However, if General Motors had adopted a policy of lengthening the useful life of its plant and equipment, then the reported earnings figure is the correct one. (A 1992 report in Time magazine stated that one of the reasons General Motors has become less competitive relative to Japanese car manufacturers is that it failed to modernise its plant and equipment in the late 1980s.)

14 Chang, Brain and Most (1983) conducted a survey on the usefulness of annual reports. They found that the corporate annual report was ranked number one in importance (out of ten items) by financial analysts in the U.S. and New Zealand and number two by analysts in the United Kingdom (U.K. analysts ranked communications with management as being number one). Foster (1986, p. 12) observes that Chang et al.'s results are typical of many similar surveys. The high degree of importance placed on the annual report by analysts is another reason for believing that the apparent low influence of accounting data on share prices revealed in various studies significantly underestimates the true effect.
spent in preparing, reporting and analysing annual reports if they had no informational content? The utility of accounting data should be assessed relative to alternative measures of performance [Long and Ravenscroft (1984)] and the high influence of annual report data on the pricing decisions made by share market analysts is consistent with the data being an efficient source for measures of performance.15

(iii) Another reason for using accounting data to assess firm performance is that they are the only evidence that is admissible to those who are sceptical of stock market efficiency.16 This group constitutes a significant proportion of those interested in the returns from takeovers. However, it is not necessary to be sceptical of market efficiency in general in order to prefer accounting measures of performance over capital market rates of return. Some researchers do not believe that the benefits expected from most takeovers are realised. Measuring share price reactions around the period of a takeover might reflect the winning bidder's hubris rather than well grounded expectations [Roll (1988)]. Only ex post evidence is relevant in this case and accounting data remain the only tractable, systematic source of such information. It is also worth noting that when firms are evaluated over several accounting periods then some of the alleged deficiencies of accounting data disappear or are attenuated. For example, over (under) accruals in one period are eventually balanced by under (over) accruals in other periods.

(iv) Finally, one cannot avoid resorting to accounting data for certain types of information which are commonly expressed in its terminology, e.g., the debt to equity and sales to total assets ratios.

In sum: the utility of accounting numbers in assessing firm performance is more appropriately evaluated in the context of imperfect markets where information is costly rather than compared with ideal notions of economic income. The issue is an empirical one but little progress has been made because we lack an acceptable theory relating firm valuation to accounting data. However, capital market rates of return are not unambiguously to be preferred over accounting data. The extensive use made of accounting data is consistent with them yielding valid estimates of firm performance and for some purposes their use is unavoidable.

15 A particularly apposite piece of supporting evidence: DeAngelo (1988) reports results that "dissident stockholders who wage a proxy contest for board seats typically cite poor earnings rather than poor stock price performance as necessitating the proposed hostile management change" (p. 3). However, DeAngelo argues that the dissidents resort to accounting data because the share market anticipates the positive effects of the proxy contest and "this empirical regularity [of stock price increases] limits dissidents' ability to cite stock prices as evidence of managerial efficiency" (p. 4).
Given that accounting data can be used validly to assess firm performance, a question arises: can accounting earnings be used as a relative measure of the performance of firms involved in takeover activity? The usefulness of accounting earnings in particular contexts is no guarantee that they are appropriate in other contexts. This issue is addressed in the next section.

5.1.2 Issues in Accounting Based Assessments of Merged Firms' Performances

The analysis of firms' post-merger performance involves two issues. One issue involves the systematic biases present in the accounting based research methods commonly used to assess firms' performance in the post-merger period. Typically, the research methods yield downward biased estimates of the merged firms' performance relative to both the sample firms' performance in the pre-merger period and to matched control firms in the post-merger period. The other issue concerns the non-statistical interpretation of the difference (if any) between the sample firms' performance and the benchmark adopted. The first issue has received most of the attention in the literature but many of the problems raised are avoidable if proportional attention is paid to the second issue. The second issue is discussed first.

5.1.2.1 Non-statistical interpretation of accounting based performance measures

MR (1986), in discussing the interpretation of their statistical tests, rightly note that "there is a difference between statistical significance and economic or operational significance". A difference between samples that is significant in a statistical sense might have little operational or policy-relevant significance" (p. 91). MR do not explain with specific examples how this could be so but one can envision particular cases. For example, the economic policies available to the government to reduce the incidence of takeovers leading to monopoly power might not be sufficiently sensitive to discriminate between that type of takeover and others which promote a more efficient allocation of corporate assets. In this instance, a statistically significant finding that the incidence of firms possessing monopoly power is slightly higher among firms involved in takeovers would have little or no operational or policy significance.

MR's caveat is salutary. In this section attention is drawn to another methodological concern when analysing accounting ratios. An economic interpretation of a firm performance indicator requires that it be couched in terms of a returns metric, i.e., firm performance is assessed relative to an investment outlay. Ikenberry and Lakonishok (1991) note that the conventional analysis using mean accounting ratios as measures of the performance of sample firms implies an unrealistic investment
Ikenberry and Lakonishok illustrate their point with an example. Consider two firms with equal earnings in a given year of $1.00. In the next year, earnings in the first firm grow to $1.10 and earnings in the second firm grow to $1.50. The mean earnings growth rate of the two firms, calculated in the conventional way, is 30%. Note that the growth rate is completely independent of the prices at which the two shares were traded in the first year. Now assume that the first firm traded at $10.00 per share and the second firm traded at $20.00 per share. The implied investment strategy consistent with providing the same growth as the mean growth for the two firms (30%) is a purchase of one share in each company, i.e., an investment of $10.00 in the first firm and $20.00 in the second firm. The problems with this investment strategy become apparent when we consider that the investment weight placed on each firm is proportional to the price-earnings ratio.

Weighting each investment in proportion to the ratio of the share price and the accounting variable of interest, as the conventional analysis implies, is problematic when we wish to integrate the analysis of different accounting variables. The implicit investment weights placed on each variable for the same firm will not necessarily be the same. The results obtained for each accounting variable might be incommensurable. Another difficulty caused by the conventional approach to financial analysis is that the procedure effectively results in market information being used to weight the investment in each stock. For example, if earnings growth was the variable of interest, firms with higher price-earnings ratios would have a higher weight placed on them. This could impart an upward bias in the calculation of the mean growth rate in experimental samples similar to the ones investigated in this thesis. This is because firms involved in takeover activity might have relatively high price-earnings ratios in anticipation of higher earnings in future years. A fair assessment of the mean earnings growth in an experimental sample requires that an equal investment weight be placed in each firm. The conventional analysis does not permit a fair assessment in experimental samples where a systematic bias exists in the ratio of the share price to the accounting variable of interest.

In this thesis Ikenberry and Lakonishok’s (1991) procedure to ensure an equal investment in each firm is adopted. The explicit assumption is an equally weighted buy-and-hold investment strategy. At the beginning of the period being analysed (i.e., the fifth year before the bid announcement year for each sample firm) an equal dollar amount is assumed to be invested in each firm. Given the investment, the proportional ownership in the firm is identified. Suppose, following Ikenberry and Lakonishok, we are analysing net income. Knowing the proportional ownership in
each sample firm enables us to calculate the pro-rata level of income attributable to
this investment. Summing the pro-rata net incomes of all the sample firms provides
the measure of aggregate net income attributable to our investment strategy.

The above procedure, further discussed in Section 5.3, avoids the unrealistic
investment assumption implied by the conventional method of analysis. It avoids
also the problems caused by sample ratio denominator values that are very close to
zero or negative. These outliers can have a significant impact on the experimental
sample’s mean ratio value and thus convey a misleading picture. A number of
techniques are available to reduce or eliminate the impact of these outliers but their
application is subjective and ad hoc [Lev and Sunder (1979)].

5.1.2.2  Systematic biases in accounting measures of firms' post-
merger performance

Several writers have recognised that the accounting based research methods
commonly used to assess firms' performances in the post-merger period incorporate
probable systematic biases. These biases are discussed below. One strength of
the present study is that the research design adopted and the characteristics of the
experimental sample firms enable this thesis to avoid the systematic biases
identified.

Purchase vs pooling of interest

There are two principal methods to account for a takeover: the purchase (or
acquisition) method and the pooling (or merger) method [Stanton (1987)]. Under
the purchase method, the successful bidder records the acquired firm at its fair or
market value. In most cases, this is taken to be the consideration paid. Typically,
the bidder pays a premium over the book value of the assets acquired. Under the
pooling method, the assets and liabilities of the merging firms are combined at the
book values recorded in their respective accounts. The rationale for the method is
that the merger is viewed as a uniting of interests rather than as an acquisition
[Goodwin (1986)]. The merger is brought about through an exchange of shares
which is structured to leave the ownership interest in both companies largely
unchanged. Share capital issued is recorded at par value. Any difference between
the par value of capital issued and the par value of capital received is adjusted via,

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17 Lev and Sunder (1979) describe two techniques aimed at "a systematic treatment of
outliers". "Trimming involves the removal from a sample of an equal number of the smallest
and largest observations and then proceeding as if the trimmed sample were a complete one".
"Winsorisation" involves changing the outlier's value to the value of the nearest observation not
seriously suspect as arising from a measurement error (pp. 207-8).

18 The writers include Meeks (1977), Appleyard (1980), Meeks and Meeks (1981), Ikeda and
Doi (1983), and Stanton (1987).
first, the capital reserves, and then through the revenue reserves [Goodwin (1986)].

As Stanton (1987), among others, points out, the purchase and pooling methods of accounting for a takeover result in significantly different accounting rates of return even where cashflows remain identical. Under the purchase method, the assets acquired typically have higher depreciation and amortisation expenses charged against them than they did prior to the takeover. This is because the premium over book value of assets acquired which is typically paid by the successful bidder for the target firm is apportioned to a revaluation of the assets acquired or goodwill. The net effect is for the observed accounting rate of return (ARR)\(^{19}\) to decrease in the post-merger period even when the net cashflows of the merged entity are identical to the sum of the net cashflows of the two previously separate firms. No such downward bias affects the ARR of firms which adopt the pooling method. Depreciation and amortisation expenses are unaffected by the merger because assets are not revalued and goodwill on acquisition does not arise.

The pooling method is not widely used to account for acquisitions.\(^{20}\) The principal method used to account for acquisitions in most countries, including Australia, is the purchase method. This poses a problem for accounting based studies of post-merger performance. The problem arises because most of the studies, e.g., Mueller (1980), and MR (1986), use the "pooled" results of matched control firms as benchmarks to assess the performance of the sample merged firms. The "pooled" results of the merged firms in the pre-merger period are also used as another benchmark to evaluate performance in the post-merger period. As indicated above, comparing the ARRs of firms which use purchase accounting with pooled ARRs gives a systematic bias in favour of the firms with the pooled results [Stanton (1987), Appleyard (1980)].

The extent of the downward bias in the observed post-merger ARR of the merged firms (relative to the pooled ARR of the two firms had they not merged or relative to the pooled ARR of two matched non-merging firms) is an empirical issue. However, estimates of the bias using typical values for the excess of purchase consideration over book value of assets acquired and for the pooled ARR are useful.

\(^{19}\) The conventional definition of ARR is net profit before interest and tax divided by (depreciated) net assets. One reason commonly advanced for using ARR is that it is largely independent of changes in borrowings and therefore enables a fair comparison of pre and post-merger changes in operating performance.

\(^{20}\) AAS 21 (issued in December 1985) bans the use of the pooling method. Standish (1972) and Leo (1984) provide evidence to suggest that its use was quite common prior to 1980. However, Goodwin (1986) on reanalysing the data argues that the evidence presented is more consistent with the use of the purchase method to account for acquisitions rather than the pooling method. In a survey of publicly listed companies over the period 1980 through 1983, Goodwin finds that the pooling method was used by only one company.
These estimates give some indication of what might be regarded as significant deviations between the observed ARR of the merged firms and the pooled ARR of the same firms in the pre-merger period and also the pooled ARR of matched non-merging firms [Appleyard, (1980)]. As indicated earlier, the latter two measures are the benchmarks commonly used to assess merged firms' performances.

Estimates of the expected (downward) bias in post-merger ARR relative to the benchmarks described above are reviewed in Appendix B. The salient points arising from Appendix B are first, that the extent of the downward bias in the observed post-merger ARR of sample firms could easily account for the deviations between sample merged firms' average ARR and the average (pooled) ARR of the control firms found in previous studies. The estimated downward bias could also account for the decline in the average post-merger ARR of the merged firms relative to their average (pooled) ARR in the pre-merger period, also found in prior studies. Second, the bias cannot be adjusted by a straightforward shift to the right in the population distribution of post-merger ARRs. Assuming that the cash-flows of the two merging firms remain identical with or without the merger, the higher the (pooled) ARR the greater is the bias in the observed post-merger ARR. A similar relationship holds for the premium paid by the acquirer for the target firm's assets. The greater the premium paid over the book value of the acquired firm's assets, the greater the bias.

Other sources of bias

The purchase and pooling differences in the amount of depreciation and amortisation expense charged against profit in the post-merger period are not the only sources of bias in accounting based evaluations of post-merger performance. However, they are the only identified biases that exert an unequivocally significant influence. Determining the influence (and even direction) of other possible biases is less straightforward. Some of them are discussed below.

The appropriateness of using data relating to the first year of the post-merger period is a subject open to debate. Appleyard (1980) argues that unless the acquirer's accounting year end coincides with the financial year in question and the acquisition occurs at the same date, there is a downward bias in the ARR in the first year post-merger. This is because for the first year post-merger only the fraction of the acquired firm's profits earned after the merger are included in the merged entity's total profits. However, all the acquired firm's net assets are included in the merged entity's asset base. Even if operating efficiency and performance remained the same after the takeover, there is a drop in the ARR since it is usually calculated using end of year assets.
Meeks and Meeks (1981) take a contrary view. Meeks and Meeks, unlike Appleyard, assume that the merged entity's total assets is calculated as the average of its opening and closing total assets figures rather than being defined as the closing total assets figure. In this event, the opening assets of the acquired firm are not included in the denominator of the ARR ratio (unless the acquisition took place at the very beginning of the acquirer's financial year). However, the acquired firm's closing assets are included in the calculation of the average total assets figure. The benchmark Meeks and Meeks use is the weighted average ARR of the two firms (i.e., the pooled ARR). As Meeks and Meeks point out, the question as to whether the observed ARR exceeds or falls short of the pooled ARR depends on whether the amount of the acquired firm's profits included in the numerator exceed or fall short of the increase in the average total assets of the merged entity. On the basis of stylised facts obtained from Meeks' (1977) study of British takeovers in the late sixties and early 1970s, Meeks and Meeks conclude that the likely direction of the bias is for the observed ARR to exceed the pooled ARR. The issue remains moot in the Australian context.

A similar problem arises with respect to inter-company sales. Some merged firms engage in substantial trade with each other prior to merger. Appleyard notes that in the pre-takeover period the selling company would record the profit made on the sales while the purchase price, inclusive of the profit, would be recorded in the purchasing company's books in so far as the goods remained unsold. However, in the post-takeover period the profit that would normally be recorded by the first inter-company sale is not recorded since the two firms are now one entity. To the extent that the goods remained unsold outside the group, there is a downward bias in the observed ARR.

Meeks and Meeks attach less weight to the above issue than does Appleyard. They observe that the impact of the problem largely disappears after the first year post-merger, based on the assumption that by the end of the second year the merged entity can realise all profits from intercompany transactions from the first year, which more or less balances the unrealised profit from the current year's inter-company transactions. In Meeks and Meeks' view, a more serious problem is raised by the merging firms' ability to divert either some of their products or services from third parties to each other. This has the effect of increasing the profit-sales ratio (on the

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21 This assumes that the acquisition was financed through the issue of new capital rather than from the acquirer's existing asset base.
22 Meeks and Meeks define the weighted average ARR of the two firms as the sum of 12 months' profit flow for each firm divided by the average total assets of the two firms together.
23 This is based on the reasonable assumption that the two firms' combined ARR is less than one. Eliminating the unrealised inter-company profit from both the numerator (earnings before interest and taxes) and the denominator (total assets) thus has the effect of decreasing the ARR.
assumption that all the intra-company profits are realised in each accounting period). Meeks and Meeks argue that an increase in the sales/profit ratio caused by diverting sales to related companies does not allow a rise in efficiency to be inferred.

Meeks and Meeks' reasoning is correct if all that occurs is simply a diversion of sales and products to a related firm. However, among the possible benefits of related company transactions are lower transaction costs. Lowering these costs facilitates better co-ordination between different operating units and thus paves the way for real efficiency gains.

Notwithstanding their differences on the direction of the biases caused by the problems identified above, both Appleyard and Meeks and Meeks agree that the problems can largely be overcome by ignoring or discounting the accounting results for the year of merger. This is the approach adopted in this thesis. Less tractable are the biases associated with the initial reorganisation costs of the takeover. These costs, which include the opportunity cost of managerial time, can be substantial yet are generally written off immediately (when recognised) rather than capitalised and amortised. The bias against the merged firms becomes apparent when we consider that the benefits associated with the costs may not be realised for some years.

The above discussion implies that a revision of the interpretation of some prior accounting based studies is necessary. This is particularly true of studies whose samples included merged firms which adopted the purchase method of accounting and systematically amortised the premium paid for the assets. A finding, based on observed ARRs, that merged firms do not outperform a sample of control firms in the post-merger period is consistent with real improvements in performance by the merged firms which contributes towards their registering higher ARRs. It might be that the performance induced increase in ARR counter-balances the systematic downward bias in the observed post-merger ARRs of the experimental samples.

The significant systematic downward bias in firms' post-merger ARRs caused by the purchase method of accounting for takeovers raises two questions pertinent to this thesis. (i) In analysing the accounting based earnings for the sample firms in this thesis, should the earnings be adjusted to take into account the systematic biases identified above? (ii) Do the biases account for the indifferent (as opposed to improved) performance of MR's experimental sample of firms relative to both their own performance in the pre-merger period and to the performance of the matched

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24 The profit-sales ratio is typically used as one measure of a firm's operating efficiency. Sales figures are not widely available for Australian firms over the period studied and so this ratio is not included in the analysis. It is discussed here for completeness.
control firms in the post-merger period? Both questions are addressed below.

5.1.3 Why No Adjustments Are Made for Systematic Biases in the Experimental Sample

One reason for not adjusting for any bias in firms' post-merger earnings figures is that over the major portion of the period analysed, January 1974 through June 1985, there was effectively no standard treatment for accounting for takeovers [Gibson and Francis (1975), Goodwin (1986), Carnegie and Gibson (1987)]. One consequence of a lack of a standard treatment for accounting for takeovers is that it is virtually impossible to re-cast the accounts so as to remove the effect of any perceived bias. For example, one common treatment of goodwill prior to the introduction of AAS 18 was to capitalise goodwill as an asset which was subsequently amortised on an ad hoc basis [Kirkness (1987)]. Some companies made more than one acquisition over a given period. The abbreviated information disclosed in the annual report makes it impossible to trace the unsystematic write-offs of goodwill back to the specific acquisitions to which they related. Further, where assets were revalued on acquisition, no indication is given in the annual report of the method or time period over which the revalued portions of the assets were depreciated.

Another consequence of the lack of an enforced standard treatment for accounting for acquisitions is that it is probable that there is no need to adjust for a systematic bias against the merged firm. This point is elaborated below.

Anderson and Zimmer (1991) argue that prior to the introduction of AAS 18, "the accounting policy adopted for (purchased) goodwill correlated with firms' 'underlying' or 'economic' goodwill. Purchased goodwill arises as the difference between purchase price and book value of assets in an acquisition ... [E]conomic goodwill is that proportion of the market value of the firm not explained by assets-in-place" (p. 2). Immediately after a takeover, purchased goodwill closely

25 The exposure draft of AAS 18, Accounting for Goodwill, was released in May 1983. This represented the first attempt by Australian accounting authorities to influence the treatment of goodwill specifically. One of its stated objectives was to reduce the diversity of goodwill accounting procedures. It is argued below that while AAS 18 was partially successful in achieving this objective (Kirkness (1987)) other considerations suggest that, effectively, real diversity in the accounting treatment of goodwill existed at least until the end of the 1985 financial year, i.e., the end of the period studied in this thesis.

26 Goodwin and Harris (1992) report that out of a sample of 90 companies selected from the top 150 (by market capitalisation) companies in 1989, fewer than 10 disclosed their amortisation period for intangibles. They also note that while most identifiable intangibles appeared to be valued at cost, the actual valuation method was not always clearly evident because of a lack of adequate disclosure. It has been suggested that increasing the value of identifiable intangibles acquired is one way in which companies have sought to avoid writing up substantial amounts of goodwill [Curran (1989)].

27 It would appear from their discussion that Anderson and Zimmer include as "purchased goodwill" any upward revaluation of assets acquired during a takeover.
approximates economic goodwill since the purchase is based on a market transaction. Anderson and Zimmer reason that because the purchased goodwill is, in effect, "the market's assessment of the firm's expected incremental income, firms are reluctant to amortise goodwill unless the value of economic goodwill is falling. In contrast, the amortisation provisions of the goodwill regulations operate with the maintained assumption that once acquired, the value of purchased goodwill continuously diminishes and the diminution occurs in linear fashion" (p. 5).

It should be noted that firms' reluctance to amortise purchased goodwill other than in line with falls in economic goodwill is not based on pedantry. The requirement imposed by AAS 18 (and ASRB 1013), to capitalise purchased goodwill and systematically amortise it against operating profit over a period not exceeding 20 years, is costly to firms. Many claimholder contracts with a firm are based on reported earnings. The reporting of a lower earnings figure because of a mandatory expensing which does not reflect the economic substance of a transaction lowers the quality of the earnings information produced by the reporting system. Anderson and Zimmer hypothesise that the mandatory treatment of purchased goodwill imposes "costs on firms in renegotiating accounting based descriptions in existing contracts and [in] 'undoing' the regulatory numbers in prospective contracts" (p. 5).

In their research, Anderson and Zimmer focus more on the costs of renegotiating the contracts-in-place but it is probable that greater costs are imposed on firms by the need to "undo" the regulatory imposed numbers in prospective contracts. Herz and Abahoonie (1990) document the exotic (and costly) methods used by U.S. firms to minimise goodwill on acquisition. The U.S. has, since the early 1970s, closely regulated accounting for acquisitions and one would expect that by now most firms would have re-negotiated their contracts to take into account the change in earnings caused by the regulations. Herz and Abahoonie's evidence, that firms continue to attempt to avoid the effects of the regulations, suggests that the mandatory accounting procedures for the treatment of goodwill impose permanent, on-going and significant costs on the firms affected. Other evidence consistent with this proposition includes the extensive and, at times, heated debate which accompanied the introduction of AAS 18 and AAS 1013; the grudging reluctance with which Australian companies complied with the standards on goodwill [Carnegie and Gibson (1987), Kirkness (1987)]; and the continuing efforts of some companies to 28 One example: debt agreements generally contain interest coverage constraints based on earnings. Significantly, it is common in Australia for the agreements to leave the method of calculation of earnings undefined [Anderson and Zimmer (1991)]. 29 The exposure draft for AAS 18, released in March 1983, attracted 91 submissions commenting on it. This is almost twice the number of submissions usually received on such drafts. Whittred and Zimmer (1988, p. 221) suggest that this possibly indicates the sensitivity of the issue.
avoid the provisions of AAS 18 and ASRB 1013.\(^{30}\)

Given Anderson and Zimmer's thesis, the pertinent implication is that over the period analysed in this study there was no systematic (downward) bias in the accounting earnings of merged firms. In the absence of a specific accounting standard, firms accounted for their acquisitions using methods which ensured that their purchased goodwill did not fall below their economic goodwill. In the majority of instances, this meant goodwill was either not amortised at all in many years or amortised unsystematically. Reported earnings did not differ significantly from the amounts that would have been recorded had the pooling method been used to account for the takeovers.

Evidence confirming the above proposition is found in Gibson and Francis (1975), Goodwin (1986), Carnegie and Gibson (1987), Kirkness (1987) and Wines and Ferguson (1993). Gibson and Francis (1975) describe the accounting treatment of goodwill on consolidation adopted by 196 firms listed on the Melbourne Stock Exchange.\(^{31}\) After recording goodwill, 108 (i.e., 55\%) of the 196 firms made no subsequent adjustment to goodwill. Only 14 of the remaining 88 firms indicated that they systematically wrote-off goodwill against consolidated profit (i.e., the treatment recommended by AAS 18). Twenty four firms wrote off goodwill against consolidated profit either in a lump sum or at Directors' discretion.\(^{32}\) Fifty firms wrote off goodwill against consolidated reserves.\(^{33}\)

The results of Gibson and Francis's survey support the proposition that, given the choice, firms prefer to adopt accounting treatments which minimise the impact of purchased goodwill on operating earnings. Gibson and Francis express surprise that very few (21) companies chose to minimise goodwill recorded by revaluing goodwill.

\(^{30}\) Easton (1985) asserts that some firms went so far as to forsake or reduce acquisition activity while others restructured arrangements to ensure that acquired firms were not consolidated, thus avoiding having to record goodwill. It is difficult to assess the prevalence of this type of action (or inaction). However, Anderson and Zimmer report that their results confirm their hypothesis. The higher the level of purchased goodwill (i.e., economic goodwill) in their sample firms, the more likely it was that the firms undertook actions aimed at avoiding or diluting the effects of AAS 18 (and ASRB1013) on their reported earnings.

\(^{31}\) All 525 non-mining companies listed on the Melbourne Stock Exchange were invited to participate in the survey. Replies were received from 277 firms. Only 196 of the 277 respondents indicated that they prepared consolidated statements.

\(^{32}\) Carnegie and Gibson (1987) observe that "writing off goodwill at the discretion of the directors is consistent with the view that consolidated goodwill should not be written off unless a permanent diminution in its value becomes apparent" (p. 4). This was the stance adopted by companies which used this method. The argument supports Anderson and Zimmer's thesis.

\(^{33}\) Writing goodwill off against consolidated reserves lowers the equity and asset base of the merged entity without affecting earnings [Carnegie and Gibson (1987)]. Given that this practice was widespread over the period analysed, it potentially imparts a systematic bias in favour of the merger sample. The bias is manifest if either book value of equity or total assets are used to calculate returns. The research design adopted in this thesis does not involve the use of book value of either equity or total assets to estimate the returns to the firms analysed.
(upwards) the acquired firms' assets. However, this procedure usually results in a higher depreciation charge against operating profit, i.e., it effectively results in a systematic amortisation of economic goodwill. For this reason one would not expect it to be very popular.

Goodwin (1986) and Carnegie and Gibson (1987) contain extensions of Gibson and Francis's (1975) survey. The results of both studies confirm that the large variety of accounting treatments of goodwill found by Gibson and Francis persisted at least up to the release in March 1983 of the AAS 18 exposure draft. Goodwin sums up the situation over the 1980 through 1983 period as follows: "Overall, the methods required in AAS 18 were not widely used. The majority of companies surveyed did not treat goodwill on consolidation as an asset and most of those that did so did not amortise it" (p. 20).

Given that the period analysed in this thesis extends to June 1985, the impact of AAS 18 on the accounting practices adopted by firms over the period 1983 through 1985 is of interest. AAS 18 was made operative on March 31, 1985 but an exposure draft had been released in May, 1983. It is possible that companies changed their accounting procedures to bring them into line with, initially, the exposure draft and then the standard. Carnegie and Gibson (1987) and Kirkness (1987) investigate this issue. The results of both studies indicate that the exposure draft and standard were partially successfully in their stated aim of reducing the observed variety of treatments of purchased goodwill.

Kirkness finds that one effect of the exposure draft was to significantly increase the number of firms electing to write-off goodwill immediately. The percentage of sample firms in this category was 18.2% in 1982 and increased to 44.2% in 1984. The probable reason for the popularity of this approach is that it only affects the earnings figures for one year (assuming it is written off against current year profits) and avoids the AAS 18 requirement to systematically amortise goodwill against operating profits. Carnegie and Gibson find that the major impact of AAS 18 on firms reporting in 1985 was to increase the prevalence of firms revaluing assets on acquisition. Revaluing assets upwards on acquisition decreases the amount of goodwill.

34 Goodwin (1986) obtained her data by examining the annual reports of the 133 companies whose successful takeover offers were published on Mondays in The Australian Financial Review during the years 1980 through 1983. Carnegie and Gibson (1987) invited the 619 companies listed on the Australian Stock Exchange in January 1986 to take part in their survey of the methods used to account for purchased goodwill. They received useable responses from 189 (i.e., 30.5%) of the companies invited.

35 Kirkness' sample comprises 100 industrial and commercial firms randomly selected from the Sydney Stock Exchange listings as at March 20, 1985. Data on their accounting policies with respect to goodwill are taken from the annual reports of the 100 firms. The years covered are 1980 through 1985. Because an average of 49 firms in each year either do not show any goodwill or show only negative goodwill, the effective sample size averages 51 in each year.
goodwill on consolidation. If the assets revalued upwards can be amortised over an indefinite period, e.g., identifiable intangibles, then the impact on earnings of depreciation charges is diluted. The findings of both Kirkness, and Carnegie and Gibson are consistent with firms wishing to minimise the impact of mandatory, systematic write-offs of goodwill against operating profit.

Although in both studies the sample firms may have sought to minimise the impact of AAS 18 on earnings, it is clear that the introduction of AAS 18 did affect firms' accounting for goodwill over the years 1983 through 1985. However, a number of considerations suggest that there is no significant systematic (downwards) bias against merger firms in those years. Kirkness, and Carnegie and Gibson agree that as of June 1985 substantial variety in treatment of purchased goodwill existed.36 Given that the costs of non-compliance with the standard were relatively low up to June 1985, it is probable that the firms with significant amounts of goodwill chose to delay their compliance. Conversely, the firms which did comply can be expected to comprise mostly those firms with relatively minor amounts of purchased goodwill [Anderson and Zimmer (1991)].

Wines and Ferguson (1993) examined the accounting policies adopted for goodwill and for identifiable intangible assets by a sample of 150 ASX listed companies over the period 1985 to 1989 inclusive. Their findings, in tandem with the earlier studies, strongly support Anderson and Zimmer's proposition that firms perceive significant costs in complying with the AAS 18 and ASRB 1013 requirement to systematically amortise goodwill over a period not exceeding 20 years. Wines and Ferguson find that it took the introduction of ASRB 1013 in April 1988 for compliance to be more effectively enforced. However, companies sought to minimise the impact of the amortisation requirement by recognising identifiable intangible assets, thereby reducing the amount that would otherwise have to be recorded as goodwill. This stratagem was manifest through an increase in the number of companies recognising identifiable intangible assets over the period 1985 to 1989 and an increase in the diversity of accounting policies adopted for those identifiable intangibles. The diversity in policies, which on the whole reflected a widespread reluctance to write-off the identifiable intangibles against operating profit, continued despite the issue of Accounting Guidance Release No. 5 in 1985 by the AARF, which "drew attention to the fact these intangibles, in accordance with AAS 4, Depreciation of Non-Current Assets, are required to be written off by systematic charges to the profit and loss account over the period of time during which benefits are expected" (p. 92).

36 Kirkness reports that less than half the firms he surveyed changed their goodwill accounting method to adopt the standard. Carnegie and Gibson report that 46% of their surveyed firms did not comply with the provision of AAS 18.
In summary, there are two reasons why this thesis does not adjust for systematic biases when using accounting earnings to evaluate the post-merger performance of the experimental sample. One reason is that the information disclosed in the annual reports of the sample firms is not sufficient to permit an informed adjustment of any bias. The second is that a review of the economic incentives and regulatory environment prevailing during the period studied suggests that no significant systematic bias exists. There is strong empirical evidence to support this proposition.

Finally, there is one pertinent implication of the above argument. MR's finding that their sample of merged firms experienced only indifferent (as opposed to improved) performance relative to both their own performance in the pre-merger period and to the performance of the matched control firms in the post-merger period cannot be attributable to their failure to adjust for systematic biases traceable to accounting for goodwill. No adjustment is necessary because there is no systematic bias against the merged firms over the period studied.

5.2 McDOUGALL AND ROUND (1986): SUMMARY AND CRITIQUE

Research studies are rarely strictly comparable. Differences in aims, research methods used and resources available constrain the scope of comparison. It is therefore appropriate to preface this critique of MR's (1986) study with an outline of their aims, reasons for choice of research method and constraints placed on the depth and breadth of research. An appreciation of these issues is essential in order to place the critique below in appropriate context.

In August 1985, McDougall and Round were commissioned by the National Companies and Securities Commission (NCSC) and the Australian Institute of Management - Victoria (AIM) to undertake a study into the determinants and effects of corporate takeovers in Australia. The study was published seven months later in April 1986. The speed with which the study was completed was prompted, in part, by the NCSC's need to respond quickly to the then heated debate over the impact of corporate takeovers. This consideration also influenced the choice of a research method which could be applied relatively quickly. "The NCSC consulted with various individuals and organisations as to the appropriate methodology, and finally selected the approach used by Professor Dennis Mueller and co-researchers in an international study of takeovers sponsored by the International Institute of Management Science Centre, Berlin (Mueller, 1980)" [McDougall (1987), p. 118]. Significantly, McDougall (1987) confirms that the study "was not expected to provide the complete study on the topic [of the determinants and effects of
takeovers] - years of investigation would be needed for this. A methodology [sic] was adopted that had been successfully applied to the analysis of takeovers in a number of overseas countries, and that would permit Australian results to be placed in an international perspective" (ibid., p. 118).

MR analyse both the share market effects of corporate takeovers and the effects of takeovers on firms' accounting numbers. This critique focuses solely on their accounting based analysis. One reason for not reviewing their share market analysis is that Bishop, Dodd and Officer's (1987) study of the share market effects of takeovers is more comprehensive and complete. Another reason is that MR's sample of firms has been analysed in Chapter Four using share market data. Further, MR place far more weight on their accounting based results relative to their share market based results. In any event, MR's share market based results are similar to those obtained by Bishop, Dodd and Officer. MR find that shareholders in the target firms gained the most, in terms of percentage increase in share price, from takeovers. The returns to the acquirers' shareholders did not differ significantly from the returns to the shareholders in the matched, control firms over both the pre- and post-takeover periods.

The critique is structured as follows.\(^{37}\) Sample selection procedures and research method are reviewed first. The major findings are then summarised and the validity of some conclusions drawn from them by MR is examined. A key point made is that, given the sample firms selected by MR, their results are consistent with those obtained by Bishop, Dodd and Officer in their share market study. The divergences in the two studies lie more in the conclusions drawn by the respective authors than in their findings.

5.2.1 MR's Sample Selection Procedures and Research Method

Following Mueller (1980), MR analyse the "major corporate performance characteristics" of their sample of merged firms in the pre- and post-takeover period. The performance characteristics analysed are profitability, risk, leverage and growth. Performance is assessed by "comparing the pre-takeover performance of the acquiring and target firms, and by comparing the post-takeover performance of the enlarged entity with the pre-takeover performance of the acquiring firms and with the pre-takeover performance of a composite of the acquiring and target firms" [McDougall (1987), p. 118, emphasis in original].

The pre- and post-takeover performance of the merging firms are also compared

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\(^{37}\) McDougall (1987) offers a succinct summary of the McDougall and Round study. This critique is based on the material in both McDougall (1987) and the official report.
with the pre- and post-takeover performance of a control group of firms. The control firms comprise firms which have been specifically matched with each target and bidding firm. The matching criteria include "that the matched firm should operate in the same industry as the acquiring firm, be of about the same size, and not have been involved in any takeovers for at least a minimum six year period centred on the date of the takeover offer made by the acquiring firm" [MR (1986), p. 131]. If it is not possible to obtain a matched firm using the above criteria, MR match their sample firm with a control firm of similar size operating in an industry with similar economic conditions. If this criterion proves impossible to satisfy, a composite control firm is developed using industry averages. The industry average is derived from firms in the pertinent industry which have not been involved in takeovers at all over the period under investigation.

MR's experimental sample of 88 takeovers is drawn from their identified population of 235 full takeovers involving listed firms in the retail, transport and industrial sectors over the years 1970 through 1981. MR do not study the full population of 235 takeovers in part because "in order properly to analyse the effects of a takeover, at least a three-year period of data following the takeover is desirable, in order to allow the post-takeover entity time to 'settle down' as reflected in various financial variables" [MR (1986), p. 105]. They note that a minimum five year period is preferable but "to have insisted on this would have resulted in a very small final sample, given the 'urge to merge' which appears to have been endemic in the Australian business community in the last two decades" [ibid., p. 105]. MR rule out of consideration any of their identified sample of 235 takeovers in which one of the parties had been involved in another takeover in the previous three years or if the merged entity was involved in another takeover within three years after the original takeover. This is done so that the effects of the takeovers could be isolated unambiguously.

The stringent sample selection criteria imposed by MR raise the question whether their final sample is representative of the population of firms involved in takeover activity. MR note that the firms which were heavily involved in takeover activity over the period they study are not represented in their sample (e.g., Adsteam, Bell Group, Burns Phillip, Elders-IXL and Industrial Equity). They also warn that "any inferences about takeover behaviour generally which are drawn from our results apply, strictly speaking, only to the 88 takeovers which have been investigated".

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38 McDougall and Round note that they are "forced to assume here that if the firms selected in the final sample were involved in any takeovers of private firms, the influence of such events would have been negligible" [ibid., footnote 1, p. 106]. A check of the annual reports issued during the year of the takeover under investigation reveals that a number of the experimental sample firms took over private firms during the period studied. A list of the more prominent acquirers includes Brambles, Carlton Brewery, Edwards Dunlop, Siddons Industry, McPhersons and Wattyl.
Notwithstanding this caveat, MR go on to assert that "there is no doubt that our sample is representative, by any standards, of takeover activity in Australia between 1970 and 1981" [ibid., p. 107].

There is some argument, noted earlier in this thesis, in support of the view that MR's sample is unrepresentative of the population of firms involved in takeover activity over the period 1970 through 1981. Early criticism of the study drew attention to the inadvertent selection bias against firms which showed good performance from takeover.39 MR's criterion that their sample excludes firms which were involved in another takeover within a period of three years before and after the specific takeover investigated systematically favours firms which did not have a comparative advantage in managing a takeover. These firms are the ones most likely to have made a takeover, achieved indifferent or below par results from the takeover and decided not to pursue other targets. Firms with a comparative and recognised advantage in managing corporate takeovers are systematically excluded from MR's experimental sample.

One defence open to MR against the above criticism is that their stated aim is to isolate unambiguously the effects of particular takeovers on firm performance. This aim is made more difficult, if not impossible to achieve, if the sample firms were involved in more than one takeover over the period studied. Given that the sample selection criteria are consistent with the stated objectives, it is inappropriate to criticise the MR study on this score.40 However, this issue does highlight a difference in emphasis between this thesis' concerns and those of the MR study. The focus in this thesis is on the viability of takeovers per se as a form of investment. The hypothesis to be tested is the neo-classical view that, at the margin, the returns from internal and external investments are expected to be the same. If it is the case that the returns from takeover are, at the margin, lower than the returns from internal investment then one would expect that firms which are more active in the takeover market would have lower average returns relative to firms which expand primarily via internal investment. It is not necessary to isolate the effects of particular takeovers in order to test this hypothesis. MR's research design does not permit a strong test of this hypothesis because it excludes firms most active in the takeover market. The discussion below summarises, explains and critiques MR's findings in relation to their sample. To repeat, the performance characteristics they analyse are profitability, risk, leverage and growth.

40 Although one might continue to argue that the unrepresentative nature of the sample selected limits the scope of the conclusions that can be drawn from the study.
5.2.1.1 Profitability

Two measures of profitability are used: (i) the return on shareholders' equity, defined as net profit after tax to shareholders' funds\textsuperscript{41} (EAT/EQUITY); and (ii) the return on total assets, defined as the ratio of net profit before interest and tax to a firm's total assets (OPPROF/ASSETS).

MR argue that if takeovers are motivated by the acquiring firms' desire to maximise profits, they should lead to a higher profit rate for the merged entity relative to the weighted average of the profit rates for the two firms had they not combined (p. 96). A weakness in this argument lies in that a profit maximiser invests up to the point where the net present value of the marginal investment is zero [Brealey and Myers (1988), p. 20]. This implies that even a decline in the average rate of return of a company can be consistent with profit maximisation. Further, the higher the rate of return to a company the more likely it is that future investments will result in a lower average rate of return because better than average investment opportunities are relatively scarce. This consideration is particularly significant when comparing pre-takeover rates of return with post-takeover rates of return. If, as BDO suggest, bidding firms are typically above average performers then it is likely that any new investments they make (including takeovers) are likely to lead to a decline in their average profitability. This is despite the new investments being, on average, value increasing investments.

A similar point can be made in connection with the comparison of the merged entity's profitability rate with the weighted average profitability rate of two matched control firms in the post-merger period. Given profit maximisation as the goal, one would not expect the merging firms' post-takeover rate of return to fall below the weighted average rate of return of the matched control firms. On the other hand, there is no reason to expect the reverse either. The alternative to growth by takeover is growth by internal investment. At the margin, the returns from the two types of investment are expected to be equal. Given that the matched control firms grew by internal investment, there is no economic reason to expect their average profitability in the post-takeover period to differ significantly from that of the firms which grew, in part, via takeovers.

There is a striking consistency between the neo-classical economic expectations from takeover and the results reported by MR. With regard to the EAT/EQUITY

\textsuperscript{41} McDougall and Round exclude extraordinary items from net profit. The three reasons given for their exclusion are: (i) "the diversity of practice in their determination and measurement in the context of a takeover" (p. 90, footnote 4); (ii) disposal of assets would be reflected in the profitability measures used in the study; and (iii) that their major concern is to measure profits arising from normal operations, i.e., long term or sustainable profits.
ratio, MR find that, on average, over all takeovers in their sample the profitability of the acquiring firms fell slightly in the post-takeover period. "This fall, however, was not as great as that experienced by the matching firms, although the difference in the rates of decline ... was not statistically significant. Firms involved in conglomerate takeovers, and in small takeovers, exhibited a different pattern to the overall situation. Their post-takeover profitability fell to a greater extent than that of their control firms although, again the difference was small and not statistically significant ... [F]irms involved in large takeovers performed better than their matching firms, by a margin of 2.5 percentage points (which was significant at the one per cent level). This is interesting when it is considered that the target firms in large takeovers had significantly lower values, on average, for the EAT/EQUITY ratio in the pre-takeover period than did their matching firms" (p. 149).

The results from the analysis of the profitability ratios provide the most obvious illustration that to a large extent the divergences in MR and BDO's studies are attributable more to differences in interpretation rather than in results. Where BDO would consider average earnings to be an expected outcome of efficiently operating markets MR view this finding as suggesting "that there were few real gains to acquiring firms, [which] leads to the conclusion that acquiring firms and their shareholders (and perhaps society generally) may not have benefited directly from takeovers" (p. 165). Differences in sample selection criteria and research method have been posited as reasons for the divergences in the two studies. While these factors have influenced the studies' outcomes, their impact has been less significant than the analytical framework of the authors.

5.2.1.2 Profit Variability

In investigating whether takeovers stemmed from "a managerial desire to reduce risk" (p. 94), MR measure the relative profit variability of each firm. That is, MR investigate "whether firms which were involved in takeovers experienced greater variability in profits before the takeovers, than was the case for non-merging firms" (p. 94). "Variation is measured by the coefficient of variation (a relative measure of variability), calculated by dividing the intertemporal standard deviation of the profit figures by the mean profit level for the period" (p. 91).

The results reported by MR indicate that there was no significant difference in the variability of the acquiring firms' profit before and after takeover\(^{42}\). However, the

\(^{42}\) Oddly, MR in their discussion observe that "the post-takeover profit variability of the acquiring firms increased significantly [sic] when compared to their pre-takeover experience, and was significantly [sic] higher compared to that experienced by the matching non-acquiring and target firms together in the pre-takeover period" (p. 154). However, in footnote 16 on p. 154 they then note that "strictly, according to the classical principles of hypothesis testing, we should conclude that there
raw figures did display an increase in profit variability for acquirers involved in horizontal takeovers and small takeovers. Acquirers in "large" takeovers "appeared to have suffered less from increased risk exposure than those acquirers in the other sub-groups" (p. 156). Given that the differences in levels of profit variability were insignificant, speculation as to the possible reasons for the observed differences in levels of profit variability is pointless. However, MR's results do rule out lower profit variability as a benefit accruing to their sample of firms.

5.2.1.3 Leverage

MR measure leverage as the ratio of total debt to total funds. It is the only ratio which provides conclusive results. "A simple investigation of the ratio of the average leverage levels of the acquiring firms after the takeover to that of the non-merging firms before the takeover, showed a significant consistent increase in all takeover-type and size-groups of takeovers, of around 10-11 per cent" (p. 160). An increase in leverage is consistent with the acquiring firms financing their acquisitions through the issue of debt and thereby increasing their debt/equity ratio. MR posit that the increase in leverage is "most likely, the reason for the observed superior performance of the merging firms in some groups with respect to the EAT/EQUITY ratio, as well as being the cause of the increased variability in after-tax earnings" (p. 161). MR's hypothesis is consistent with their results although no additional empirical evidence is provided in support. Another equally consistent hypothesis is that the superior returns were due, at least in part, to improved operating performance.

5.2.1.4 Growth

One criticism of unbridled takeover activity is that managers of bidding firms engage in acquisitions as a means of empire building. It is a criticism consistent with one of MR's conclusions, from their results, that "takeovers, on balance, appear to have been caused by so-called managerial motives" (p. 182). Managers may be presumed to act in their own self-interest and there is evidence that management compensation is positively related to the size of the firm. The issue here is whether firms' contracts in place serve to link effectively their managers' self-interested behaviour with the broader interests of shareholders; i.e., whether managers' self-interested behaviour also furthers the interest of shareholders by making positive net present value investments. The issue is not restricted to takeover activity since other forms of investment are also possible. The point is, takeover activity is not the only form of

is no significant difference in the pre- and post-takeover levels of profit variability, as the positive calculated t-ratios lie in the accept region of the one-tailed (lower tail) significance test. MR do not discuss their basis for applying or not applying the classical principles of hypothesis testing.
investment in which non-value-maximising management behaviour may manifest itself and this argument provides an insufficient basis for closer regulation of takeovers alone. However, it might be that in some firms the corporate takeover route is the least costly means for management to engage in non-value-increasing empire building. If this is the case and if managers do systematically adopt this strategy then we would expect that the asset growth rate of firms engaged in takeovers would be greater than the growth rate of other firms.43

MR's results do not confirm the above expectation. While their sample group of acquirers registered higher growth in the post-takeover period relative to the pre-takeover period this was also true of the control non-merging firms. Although the "acquiring firms improved their growth rates to a greater extent than did the non-merging firms ... the differences were not significant" (p. 158).

In sum, close scrutiny of MR's results indicates that their results are not inconsistent with the neo-classical model of takeovers. Despite the strong argument in support of the view that MR's sample of bidder firms is not representative of the population of bidder firms involved in takeovers, an argument which stated that MR's selection criteria were biased towards the inclusion of firms with little or no comparative advantage in managing takeovers, MR's sample of firms, on close interpretation, fared as well from the takeover experience as the neo-classical theory would lead one to expect. The results from MR's own analysis of their sample firms' performance are consistent with the view that takeovers are a form of corporate investment which, at the margin, yields returns similar to other forms of corporate investment. In the next section, the neo-classical view is subjected to another test.

5.3 RESEARCH METHOD

5.3.1 Aims and Justification of Method

Given the joint aims of this thesis, detailed in Chapter One - firstly, to reconcile the divergent results reported in Australian studies of takeovers, and secondly, to test the neo-classical hypothesis that the returns from takeovers are equal, at the margin, to the returns from other forms of corporate investment - the research design differs from that adopted by MR in their accounting based study. One significant difference is that the research design in this thesis incorporates a portfolio investment framework that complements the share market event study research method. An appealing feature of the event study research method is that it yields performance

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43 This alone, of course, is not sufficient to discredit takeovers as being, on average, non-value increasing investments.
measures that may be given a straight-forward economic interpretation. This quality facilitates comparison of results across different samples, studies and research methods. As stated earlier, an economic interpretation of a firm performance indicator requires that it be couched in terms of a returns metric, that is, performance is assessed relative to an investment outlay. Conventional accounting ratios of firm performance (e.g., as in Mueller et al. (1980) and MR) typically do not specify the investment strategy required to mimic their results and this makes it difficult to assess their practical significance.

In this thesis the above problem is overcome through the use of a research design that estimates the aggregate pro-rata net income attributable to an equally weighted buy-and-hold investment in each sample bidder (target) firm. Apart from the advantages outlined in Section 5.1, this research design has the additional attraction of facilitating comparison of the accounting based results with share market based results, since both methods estimate returns achieved by explicit investment strategies that are consistent across all sample firms.

5.3.2 Research Design

The following protocol describes the accounting numbers based research method. Adaptations of the method are described further on when implemented. Comment on the research method directly follows the description.

Protocol

Let \( \{X\} \) be the set of bidder (target) firms in the experimental sample. \( X_1, X_2, X_3, \ldots, X_n \) identify each firm in \( \{X\} \).

(i) The experimental time window for \( X_1 \) is defined as the eleven year period, \([-5,+5]\) years, centred on the bid announcement year. The end of each year is defined as June 30.

(ii) A $100,000 investment is assumed to be made in \( X_1 \) at the beginning of year \([-5]\) and the value of all rights and dividends accruing to this investment during year \([-5]\) are assumed to be immediately reinvested in \( X_1 \). Appendix C details and illustrates the reinvestment procedure.

(iii) The proportional equity in \( X_1 \) at the end of year \([-5]\) that results from the $100,000 investment is identified. Proportional equity is defined as the fraction obtained by dividing the year-end value of the initial $100,000 investment in \( X_1 \) plus the value of all re-invested rights or dividends by the year-end market capitalisation.
of X1. Year-end market capitalisation for sample firm X is defined as the share price of firm X multiplied by the number of firm X's shares outstanding on June 30 of the year [-5].

(iii) For year [-5], the pro-rata level of net income attributable to the proportional equity in X1 is identified. Pro-rata net income is defined as the product of X1's reported net income for year [-5] and the proportional equity ownership in X1.

(iv) Steps (i) to (iii) are repeated for firms X2 to Xn. The sum of the pro-rata net income across all X1 to Xn is divided by n, i.e., the total number of sample firms. This calculation yields the average pro-rata net income in year [-5] of the experimental sample firms that is attributable to the $100,000 investment in each firm.44

(v) Steps (i) to (iv) are repeated for each succeeding year in the eleven year window. One significant difference is that the beginning investment value in firm X1 in each succeeding year is equal to the ending investment of the previous year, e.g., the investment in X1 at the beginning of year [-4] is equal to the value of investment in X1 at June 30 of year [-5].

Comment on Protocol

Step (i) It would be preferable to centre the window on the first year that the merged firms' accounting data were consolidated since this would yield a more accurate estimate of pre- and post-merger performance by the sample firms, but these data are unavailable from the sources accessed by this thesis. However, as the discussion of results indicates, it is unlikely that this deficiency significantly affects the conclusions drawn from the analysis.

The decision to restrict the pre- and post-merger performance evaluation periods to not more than five years is based mainly on data availability. However, the restriction does lend additional power to any test of improved post-merger performance since a proportion of merging companies would achieve their expected gains from merger after the expiry of the five year period, i.e., the test will yield a downward biased estimate of the gains from takeover.

The definition of June 30 as the year-end of each of the eleven years in the window is based on the consideration that the most of the companies included in the CRIF Annual Report database have June 30 as their financial year-end. However, this

44 Using the mean pro-rata net income rather than the aggregate net income figure facilitates the comparison of returns across sample portfolios of different size.
convention is not universal and some companies nominate December 31 as the end
of their financial year, that is, their reported profit figures are for the year ending
December 31. Nevertheless, the proportional equity in these firms is still calculated
on the basis of their June 30 capitalisation. There is no obvious reason to expect the
results to be biased in any particular direction in consequence.

Step (ii) One hundred thousand dollars is selected as the initial investment
amount mainly to limit the impact of rounding errors on the results. It has become
conventional to express the outcomes of such theoretical investment strategies in
terms of the returns to one dollar. The results reported in this thesis may be divided
by $100,000 to yield figures compatible with the convention.

Lev (1974), in discussion of the appropriateness of earnings per share (EPS) as an
indicator of firms' long term performance, notes that a differential earnings retention
rate in two firms will cause their EPS values to differ systematically even though
relative performance remains unchanged. A similar potential bias exist in the
performance metric adopted in this thesis. Companies that pay out a higher
proportion of their earnings as dividends or make rights issues more frequently will
over a period of years record different dollar returns to the initial $100,000
investment. In order to ensure that dividend payments and rights issues do not bias
the results [Lev (1974)], the proportional ownership in each firm is calculated as if
the value of all dividends and rights issues attributable to our stake in each firm are
reinvested in the distributing firm.45

5.3.3 Benchmark Comparisons

MR, following Mueller et al. (1980), assume that the distributional properties of
their ratios conform to those assumed in Student's t-tests of statistical significance.
While such an approach is, or has been, customary, the consistent finding by a
number of studies that ratio analysis adequately controls for size only under highly
restrictive conditions and that the distribution of ratio values across industry sectors
is non-normally distributed [cf., Lev and Sunder (1979), McLeay and Fieldsend
questions about the statistical validity of their findings.

The performance metric analysed in this thesis is not a ratio since it measures the
absolute income attributable to a $100,000 investment in each sample firm.
However, it is an implausible assumption that net profits reported by firms are
normally or even symmetrically distributed. Further, it is not obvious, ex ante, what

45 Ikenberry and Lakonishok (1991) do not control for this bias. To date, the author is unaware
of other accounting studies which have controlled for the bias.
statistical distribution best describes the actual distribution of net profits reported by Australian firms.

Given that standard statistical tests may be inappropriate, this thesis compares the total net income achieved by a $100,000 investment in each of the sample firms with an empirical distribution of the values of aggregate net income achieved by 1000 randomly selected portfolios that were assumed to adopt a similar investment procedure. The empirical distribution is constructed as follows. (i) For each sample firm, the experimental time window is defined as the period \([-5,+5]\) years centred on the bid announcement year. (ii) For each sample firm, the set \(\{Y\}\) of firms with earnings data available over the same time period as the experimental time window is identified. (iii) An equal investment of $100,000 dollars is assumed to be invested at the beginning of year \([-5]\) in each of the firms in set \(\{Y\}\) and the pro-rata earnings attributable to that investment is calculated for each firm, for each year over the period \([-5,+5]\) years. (iv) A firm is selected at random, with replacement, from the set \(\{Y\}\) and the pro-rata earnings to the selected firm in each of the years from -5 to + 5 are allocated to the first control portfolio. Step (iv) is repeated 999 times, each time placing the set of 11 pro-rata earnings to the randomly selected firm into the next control portfolio in the sequence of 1000 portfolios. (v) After completing steps (i) through (iv) for all experimental sample firms, we calculate (a) the average earnings for each year over the period \([-5,+5]\) years achieved by each of the 1000 control portfolios and (b) calculate the frequency with which the 1000 control portfolios' metric exceeded the experimental portfolio's metric in each of the 11 years of the experimental time window. Note that the average earnings for, say, the year \([-3]\) to a given control portfolio is defined as the simple average of the pro-rata earnings achieved in year \([-3]\) by the firms included that portfolio.

The advantage of constructing an empirical distribution of results in the above manner is that we are not forced to assume a possibly erroneous distribution of performance metric values when assessing the significance of our results. A further advantage is that irregularities in the data that are unrelated to the effect of interest are captured in the empirical distribution thus permitting an unbiased evaluation of results. As discussed further on, this is a particularly pertinent consideration.

5.3.4 Sample Characteristics and Data Sources

Given the aim of this thesis, the sample firms analysed consist of firms included in MR's and BDO's respective studies. BDO's sample is more comprehensive than MR's sample as it comprises all firms involved in takeover bid offers over the period January 1972 to June 1986 for which data are available. Not all bids were successful; some bids were either withdrawn or failed to attract a sufficient number
of shares. This thesis analyses the accounting numbers based performance of bidders involved in 369 successful bids and of target firms that were subject to 367 successful bid offers. As noted earlier, apart from the requirement that their sample firms be publicly listed companies with available share market data, no other selection criteria is employed by BDO. Thus it is likely that their sample is broadly representative of the population of firms involved in takeover bids.

The data for the accounting numbers based analysis were obtained from the Centre for Research in Finance (CRIF) database compiled by the Australian Graduate School of Management (AGSM) at the University of New South Wales. The annual report data in the CRIF database covers listed firms over the period 1957-1985.

The window over which the sample firms' performance is analysed is [-5,+5] years centred on the year in which each successful takeover bid is made. However, the period over which BDO's sample of firms is drawn extends from 1972 to 1985. Given that the CRIF data are available only through 1985, it follows that we are not able to assess all the sample firms' performance over a complete window of 11 years. The results reported for each year relative to the bid announcement year relate only to mergers for which data are available, e.g., not all bidding firms had accounting data available for the year [+5], since their takeover offers were made within the five year period leading to 1985 (the last year for which data are available). A total of 1335 firms have some data available in both the CRIF Annual Report database and the CRIF SPPR database.

5.4 RESULTS

5.4.1 Bidding Firms

Column 3 in Table 5.1 reports the average pro-rata income attributable to a (buy-and-hold) investment of $100,000 in each sample bidder firm in the fifth year prior to the bid announcement year. Column 2, in the same table, reports the number of randomly selected control portfolios (out of 1000) whose average pro-rata income exceeds that of the bidder firms. The figures appear largely consistent with the results obtained from the share market analysis of the returns from takeover. In the four years leading up to and including the year of the bid announcement, bidding firms, on average, achieve relatively good returns. In year [-1] only 17 out 1000 control portfolios achieve a higher average dollar return than that achieved by the bidder firms. In year [0], none of the 1000 control portfolios outperform the portfolio comprising the bidder firms.

The relatively strong performance of the bidding firms continues over the period

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46 Annual report data which goes beyond 1985 are available from the STATEX database. However, at the time the test was conducted, the relevant STATEX data were inaccessible.
Table 5.1

Relative Performance of the Bidding Firms:
Average Incomes Attributable to a Buy-and-Hold $100,000 Investment in Each Firm
Made in Year [-5].

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<td>5</td>
<td>2</td>
<td>44902.1</td>
<td>29968.4</td>
</tr>
</tbody>
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Legend

Column 1: Year relative to the bid announcement year.

Column 2: No. of control portfolios (out of 1000) whose average income exceeds the average income achieved by the experimental sample firms' portfolio.

Column 3: Average income achieved by the experimental sample firms.

Column 4: Mean of the average incomes achieved by the 1000 control portfolios.
In every year of the post-bid period the average pro-rata income achieved by the bidder portfolio is higher than the pro-rata average income achieved by very large majorities of the 1000 control portfolios. The relatively good performance of the bidder firms decreases slightly in year [+2]. One possible explanation is that there is a systematic downward bias in post-merger earnings caused by the methods typically used to account for a takeover; however, as discussed earlier, this is unlikely for the sample of firms selected. An alternative possibility is that the decrease is a function of "noise" in the data. More on this later.

Adopting a buy-and-hold investment strategy in the accounting research design facilitates comparison with the share market based results which reflect the outcomes of a similar investment assumption. However, accounting earnings are not independent over time in the same way as share market returns. Given that bidding firms typically display above average earnings in the pre-bid period, their continued strong relative performance in the post-merger period might be a function of positive correlation in their earnings figures rather than being reflective of the benefits of merger. This potential bias is investigated through the adoption of a different investment strategy.

Table 5.2 reports the performance of the bidding firms relative to the performance of 1000 control portfolios comprising firms matched on income with the bidding firms. The procedure used to compile the control portfolios is as follows:

(1) For the first bidder firm with available data in year [-6], the set of firms \{S\} with the same return to equity [±10%] is identified. (2) A random selection, with replacement, is done from the set \{S\} and its pro-rata net income for year [-5], accruing from a $100,000 investment made at the end of year [-6], is placed into the first control portfolio. Step 2 is repeated 999 times, each time placing the randomly selected return into the next control portfolio in the sequence of 1000 portfolios. (3) After completing steps 1 and 2 for each experimental bidder firm, we (a) calculate the average pro-rata net income achieved by each of the 1000 control portfolios in year [-5] and (b) calculate the frequency with which the 1000 control portfolios' metric exceeded the experimental portfolio's metric. Steps 1 through 3 are then repeated for each of the remaining 10 years of the experimental time window.

Significantly, compared with Table 5.1, the results in Table 5.2 are more (but not wholly) consistent with MR's finding that "merged firms do worse post-takeover relative to both the pre-merger period and to matched non-merging firms" (p. 198). Although year [+2] is the only year in which the bidder firms are out-performed by a

---

Table 5.2

Relative Performance of the Bidding Firms:
Average Incomes Attributable to Separate Investments of $100,000 in Each Firm in Each Year Immediately Prior to the Year of Interest. Control Portfolios Comprise Firms Matched on Income With the Experimental Firms in the Year of Investment.

<table>
<thead>
<tr>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>-5</td>
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<td>20301.5</td>
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<tr>
<td>-4</td>
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<td>4</td>
<td>18</td>
<td>21438.1</td>
<td>17435.2</td>
</tr>
<tr>
<td>5</td>
<td>428</td>
<td>17783.4</td>
<td>17351.6</td>
</tr>
</tbody>
</table>

Legend

Column 1: Year relative to the bid announcement year.
2: No. of control portfolios (out of 1000) whose average income exceeds the average income achieved by the experimental sample firms' portfolio.
3: Average income achieved by the experimental sample firms.
4: Mean of the average incomes achieved by the 1000 control portfolios.
majority of the 1000 control portfolios comprising matched firms, there is a marked
decrease in the relatively good performance of the bidder firms in the post-bid
period. In four of the years over the period [+1,+5], the difference between the mean
return to the bidder firms and the mean of the mean returns to the 1000 control
portfolios is about $500. In contrast, over the period [-5, 0] years, the difference in
mean returns, which consistently favours the bidder firms, is never less than $2000.
The apparent good performance of the bidder firms in year [+4], when only 18
control portfolios outperform the sample bidder portfolio, is disquieting. It is
sandwiched between two years when the bidder portfolio achieves a relatively
indifferent return. It is difficult to formulate a merger related explanation why the
sole relatively outstanding performance by the sample bidder firms in the post-bid
period is achieved in year [+4] and it is likely that this anomalous result is due to
"noise" in the accounting data. We return to this issue after considering the earnings
based returns to the target firms.

5.4.2 Target Firms' Returns

Table 5.3 reports mean returns achieved by the target firms over the period [-5,+5]
assuming a buy-and-hold investment strategy. The mean earnings figures reported
after the bid announcement can be treated as unrepresentative of the target firms’
performance since the vast majority of the targets are formally taken over and
delisted by year [+2].

In line with the share market results, the mean earnings return for the target firms
indicate that the average target firm typically achieves relatively mediocre to bad
performance in the years leading to the successful takeover bid. In two of the years
over the period [-5,0], the target firms were outperformed by a substantial majority
of the 1000 control portfolios while in the other three years the performance of the
target portfolios was relatively indifferent.

The results in Table 5.4 below, which were obtained after matching the firms in the
control portfolios with target firms having approximately the same return to equity in
the year prior to the year of interest, diverge from the results reported in Table 5.3.
The target firms’ relative performance improves when compared with firms matched
on return to equity. However, this relative improvement is more apparent than real.
A significant number of the target firms reported net losses in the pre-bid period and
most of these firms could not be included in the calculation of the target portfolio’s
mean returns because no matching control firm could be found for them. The net
effect of excluding firms which reported losses is an upward bias in the mean return
to the target portfolios.
Table 5.3
Relative Performance of the Target Firms
Average Incomes Attributable to a Buy-and-Hold $100,000 Investment in Each Firm
Made in Year [-5].

<table>
<thead>
<tr>
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<th>(3)</th>
<th>(4)</th>
</tr>
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<td>-5</td>
<td>528</td>
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<td>-4</td>
<td>498</td>
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<td>790</td>
<td>19889.4</td>
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<td>-2</td>
<td>401</td>
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<td>802</td>
<td>22708.5</td>
<td>25260.8</td>
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<tr>
<td>0</td>
<td>869</td>
<td>21875.8</td>
<td>25802.3</td>
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<td>6691.26</td>
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Legend

<table>
<thead>
<tr>
<th>Column</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>1:</td>
<td>Year relative to the bid announcement year.</td>
</tr>
<tr>
<td>2:</td>
<td>No. of control portfolios (out of 1000) whose average income exceeds the average income achieved by the experimental sample firms' portfolio.</td>
</tr>
<tr>
<td>3:</td>
<td>Average income achieved by the experimental sample firms.</td>
</tr>
<tr>
<td>4:</td>
<td>Mean of the average incomes achieved by the 1000 control portfolios.</td>
</tr>
</tbody>
</table>
Table 5.4

Relative Performance of the Target Firms:
Average Incomes Attributable to Separate Investments of $100,000 in Each Firm in Each Year Immediately Prior to the Year of Interest. Control Portfolios Comprise Firms Matched on Income With the Experimental Firms in the Year of Investment.

<table>
<thead>
<tr>
<th>(1)</th>
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<th>(3)</th>
<th>(4)</th>
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<tbody>
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<td>149</td>
<td>21030.7</td>
<td>19352.2</td>
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<tr>
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<td>302</td>
<td>15316.7</td>
<td>12116.4</td>
</tr>
</tbody>
</table>

Legend

Column 1: Year relative to the bid announcement year.
Column 2: No. of control portfolios (out of 1000) whose average income exceeds the average income achieved by the experimental sample firms' portfolio.
Column 3: Average income achieved by the experimental sample firms.
Column 4: Mean of the average incomes achieved by the 1000 control portfolios.
5.4.3 Significance of the Accounting Based Results

One reason for constructing the empirical distributions described above is high positive skewness and the existence of "many extreme or outlying values" in the frequency distribution of financial ratios [McLeay (1986), p. 209]. As discussed earlier, this implies it would be inappropriate to apply standard parametric tests of significance to our results. Notwithstanding the preceding, it still remains to be answered whether the mean returns achieved by 1000 portfolios each comprising randomly selected firms provide an adequate benchmark for comparing the mean earnings returns achieved by the experimental sample firms.

We investigate this issue by (1) selecting at random, with replacement, the set \( \{E\} \) comprising 250 firms from the population of firms with available earnings data. (2) Each firm in \( \{E\} \) is assigned a year, at random with replacement, from the set \( \{Y\} \) which comprises the bid announcement years of the set of experimental sample firms. (3) The set \( \{E\} \) is assumed to be the experimental sample firms and the mean pro-rata earnings return achieved by each firm in the set in each year over the experimental window [-5, +5] years is compared against the mean pro-rata earnings return achieved by each of 1000 control portfolios. (4) Steps 1 through 3 are repeated 100 times and the relative performances of the randomly selected "experimental firms" in each year over the experimental time window are reviewed. Ideally, the 100 replications of \( \{E\} \) would tend to register indifferent relative performance in each year over the window [-5, +5] years since they are selected at random from the same population as the firms comprising the control portfolios.

Figure 5.1 displays the histogram of the relative performances of the 100 replications of \( \{E\} \) in the bid announcement year. The number of sets of \( \{E\} \) which outperform over 900 of their assigned 1000 control portfolios fall into the first decile, the number of sets of \( \{E\} \) which outperform over 800 of their assigned 1000 control portfolios fall into the second decile and so on.

In its irregularity, Figure 5.1 is typical of the pattern of similar histograms constructed for every year of the experimental window period of [-5, +5] years. The histograms do not display the ideal pattern of a narrow, symmetrical, single peaked distribution centred on the 5th and 6th deciles. Typically, the sets \( \{E\} \) cluster in the first, sixth and ninth deciles with less pronounced clustering in the 3rd and 5th size deciles. One possible explanation for these results is that the population of firms from which the control portfolios and sets of \( \{E\} \) are selected is small relative to the size of the portfolios. Given also that the frequency distribution of the earnings to equity ratio is characterised by many extreme values [McLeay (1986)], this could
Distribution of the Relative Performances of the 100 Sets of \{E\} in Year [0]

Performance Decile [Sets of \{E\} falling in the first decile outperform over 900 of their control portfolios]

Figure 5.1
account for the disproportionate clustering in the extreme deciles.

In relation to the present study, the above results markedly reduce confidence in the belief that the accounting based performance metric used can allow us to assess accurately the relative impact of takeover activity on firm performance. Although the accounting based results are broadly consistent with the share market analysis of the returns from takeover, the noise in the data does not allow us to endorse this finding with a reasonable degree of confidence.

5.5 SUMMARY AND CONCLUSIONS

The results presented in this chapter summarise the accounting earnings returns achieved by Australian firms involved in corporate takeovers over the period 1974 through 1985. The sample firms were drawn from two independent Australian studies which reached divergent conclusions on the average wealth effects of takeovers.

The accounting data based results are less clear-cut than the share market based results. The results from a research design which assumes a buy-and-hold investment strategy similar to that assumed in the share market event studies are consistent with the share market based results. Bidder firms typically achieve relatively strong performances in the pre-bid and more or less maintain this relatively good performance in the period [+1,+5] years. When the bidder firms are matched on returns to earnings with the control firms, their relative performance is less strong and only weakly consistent with the share market based results. The accounting results for the target firms are also consistent with the share market based results. Target firms typically achieve, at best, only mediocre performance in the years leading to the bid announcement year.

Notwithstanding the above, the extreme noise in the accounting data implies that little confidence can be placed on the finding that there is some correlation between the accounting based and share market based assessments of the effects of takeovers. Given that share market based event studies of takeover do not enjoy the confidence of a significant proportion of people interested in the effects of takeovers, the accounting results summarised in this paper continue to leave ample scope for divergent views on the effects of takeovers. At a minimum, further investigation of the accounting numbers reviewed in this section needs to be carried out before we can hope to gain a substantial appreciation of the relative impact of takeovers on accounting rates of returns.
APPENDIX B: ARR BIASES ESTIMATION MODEL

Appendix B describes a formula that may be used to estimate the downward bias in the observed ARR of merged firms. The downward bias in ARR manifests in the post-takeover period and is attributable to the amortisation of goodwill arising from the takeover premium paid by the acquiring firm. The formula assumes that there are no merger related differences in the operating performance of the merged firms, ie, pre-merger cash-flows equal the post-merger cash-flows.

Let

\[ a = \text{acquiring firm}, \]
\[ b = \text{acquired firm}, \]
\[ IIa = \text{A's reported profit}, \]
\[ IIb = \text{B's reported profit}, \]
\[ II = IIa + IIb, \]
\[ ka = \text{depreciated book value of total assets of A prior to merger}, \]
\[ kb = \text{depreciated book value of total assets of B prior to merger}, \]
\[ K = ka + kb \]
\[ Vb = \text{market value of B's assets (including goodwill) and}, \]
\[ k*b = \text{book value of net assets of B after merger (excluding goodwill). Immediately after the merger the book value will equal the market value of the assets}. \]

Typically, \( Vb > kb \) and \( Vb - kb \) comprises

\[ k*b - kb \] (which is the excess of the market value of the assets acquired over their net book value) and

\[ Vb - k*b \] (the goodwill paid for the assets as a going concern).

Finally, let

\[ \emptyset = Vb - kb \] and
\[ d = \text{the fraction of } \emptyset \text{ that is depreciated each year and so} \]
\[ d\emptyset = \text{extra expense to the merged entity due to amortisation of goodwill on acquisition and depreciation of the (upwardly) revalued portion of assets acquired}. \]

Define

\[ R = \frac{(IIa + IIb)}{(ka + kb)} = \frac{II}{K} = (\text{weighted average}) \text{ ARR} \] (1)
The concern is whether in the event of a takeover, the observed ARR of the new firm, denoted $R^*$, is equal to $R$ (assuming no change in real income to the merged entity).

$$R^* = \frac{(IIa + IIb - d\bar{\theta})}{([ka + Vb] - d\bar{\theta})} \quad (2)$$

Assume $IIa$ and $IIb$ remain unchanged after the merger and $Vb > kb$. Equation number 3, derived below, enables one to obtain an indication of the extent to which $R^*$ is a downward biased estimate of $R$. The indication is given by substituting some typical values for $R$, $d$, $K$ and $\bar{\theta}$ and deriving the corresponding $R$ values from the equation.

In restating $R$ in terms of $R^*$, note that $II = RK$ and, of course, $K = II/R$.

From (2) it follows

$$R^* = \frac{(II - d\bar{\theta})}{(K + [Vb - kb] - d\bar{\theta})}$$

$$= \frac{(RK - d\bar{\theta})}{(K + [Vb - kb] - d[Vb - kb])}$$

Upon rearrangement:

$$R = \frac{(R*K + Vb - kb - d[Vb - Kb])/K + d\bar{\theta}/K}{K + d\bar{\theta}/K}$$

$$= \frac{R* + (R*[Vb - kd][1 - d])/K + d\bar{\theta}/K}{K + d\bar{\theta}/K} \quad (3)$$

The last column, $R^*$ minus $R$, in Table A below shows the extent to which $R^*$ is downward biased relative to $R$. $R^*$ is the ARR of the merged firms in the post-merger period. $R$ is the weighted average ARR of the merging firms prior to merger. The estimates of the downward bias are obtained by inputting a range of values for $R$, $d$, $K$, and $\bar{\theta}$ in equation (3) above.

As an aid to interpreting Table A, the first scenario assumes a post-merger ARR of 10%, a total premium over the target firm's net book value of $100, an amortisation/depreciation charge on $\bar{\theta}$ of 10% and a merged entity's total asset value of $1000.$
Note that the extent to which $R^*$ is a downward biased estimate of $R$ increases as $R$ increases. That is, the higher performing firms are the ones most likely to record in a decrease in their observed ARR in the post-merger period purely as a result of the amortisation of goodwill. The examples above show that the bias does not affect all firms equally. Merging firms that display exceptionally high weighted average ARRs in the pre-merger will, on this measure, appear to be the most adversely affected by the merger if the downward bias in the post-merger ARR is not controlled.

Part (ii), above, of Table A demonstrates that as $K$, the combined total assets of $a$ and $b$, increase in proportion to $\phi$, the excess of the market value of $b$ over $b$'s pre-merger net book value, the extent to which $R^*$ is a downwards biased estimate of $R$ decreases. That is, the bias in $R^*$ is a particular concern when the premium over book value paid by the acquiring firm for the target firm is large relative to the total book value of the merged entity's assets.

Another way of demonstrating the point made in part (ii) of Table A is by showing that as $\phi$, the total premium over book-value paid by the acquiring firm, increases,
\( R^* \) yields an increasingly downward biased estimate of \( R \). We may note in this context that sharemarket takeover premiums have increased in the 1980s and this may be expected to go in tandem with an increase in the premium over book-values paid by acquiring firms. One implication is that ARR based assessments of the gains from takeover will display increasingly poor returns from takeover in the 1980s relative to earlier periods.

(iv)

<table>
<thead>
<tr>
<th></th>
<th>( R^* )</th>
<th>( \emptyset )</th>
<th>( d )</th>
<th>( K )</th>
<th>( R )</th>
<th>( R^* - R )</th>
</tr>
</thead>
<tbody>
<tr>
<td>10%</td>
<td>100</td>
<td>0.05</td>
<td>1000</td>
<td>11.45%</td>
<td>1.45%</td>
<td></td>
</tr>
</tbody>
</table>

Part (iv) of Table A above shows that given the plausible scenario where the pre-merger weighted average ARR of two firms is 11.45%, a merger of the two firms which results in the takeover book premium equalling 10% of the merged entity's total book value will bring about a decrease in the post-merger ARR of 1.45%. This assumes that the takeover premium is amortised at the rate of 5% per year. Mueller et al (1980) and MR report negative changes in pre and post-merger ARR that, on average, fall within the range of 1.45% for firms in their samples.
APPENDIX C: PROPORTIONATE DOLLAR PROFIT FORMULA

Formula for arriving at the proportionate profit attributable to a fixed dollar investment in a firm.

Assumptions

(i) The value of any rights issues and dividends issued are re-invested in the firm.

(ii) Profits are earned proportionally through the year.

Let $I = \text{market value of a number of shares resulting from the same initial dollar investment in each firm.}$

$MVE = \text{total dollar value of firm A's equity at the end of the year.}$

$\pi = \text{firm A's reported profit in dollars}$

$MV_x = \text{total dollar value of firm A's equity at month x.}$

$\text{$ value of } \pi \text{ attributable to investment } I \text{ in firm } A =}$

$I \times \frac{\pi}{(MVE/MV_1) + (MVE/MV_2) + \ldots + (MVE/MV_{12})}$

Equation (1) above weights firm A's reported profit figure to ensure that the effects of distribution of profit during the year or rights issue do not result in a downwards biased estimate of the proportional dollar returns from investing a fixed dollar amount in firm A's equity.

Illustrations

Assumptions

(i) All firms earn 20% per annum on their assets.

(ii) All firms are 100% equity financed (introducing debt complicates the examples without altering the point).

(iii) $100 is invested in each firm.

The illustrations below show the proportional dollar value of each firm's reported profit that accrues to the $100 investment in each firm. The firms differ with respect to the number of shares on issue and/or rights issues made during the year. The initial $100 investment is maintained at all times, that is, no further investment is made in the firm other than the re-investment of the value of all rights issues and dividend payments accruing to the $100 investment. Two issues are important:
ensuring that the dollar returns from firms which pay out dividend are not downwards biased and that the timing of the rights issue is adjusted correctly. Applying equation (1) ensures that dividend payments or rights issues do not bias the proportional dollar return accruing to the initial investment in the firm.

<table>
<thead>
<tr>
<th>Firm</th>
<th>No of Shares on Issue</th>
<th>Divs/Rights per share</th>
<th>Time weight</th>
<th>Share of year's profit</th>
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<tbody>
<tr>
<td>A</td>
<td>1000 @$1</td>
<td></td>
<td></td>
<td>100/1000*200</td>
</tr>
<tr>
<td></td>
<td>1000 @$1</td>
<td></td>
<td></td>
<td>= $20</td>
</tr>
<tr>
<td>B</td>
<td>2000 @$1</td>
<td>Rights 1:1 12/12 @$1</td>
<td></td>
<td>100/2000*400</td>
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<tr>
<td></td>
<td>2000 @$1</td>
<td></td>
<td></td>
<td>= $20</td>
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<tr>
<td>C</td>
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<tr>
<td></td>
<td>2000 @$1</td>
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<tr>
<td>D</td>
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<td></td>
<td>2000 @$1</td>
<td></td>
<td></td>
<td>*1.6 = $20</td>
</tr>
<tr>
<td>F</td>
<td>1000 @$1</td>
<td>Rights 1:1 1.5/12 @$1</td>
<td></td>
<td>100/2000*225</td>
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<tr>
<td></td>
<td>2000 $1</td>
<td></td>
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<td>*1.77 = $20</td>
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6.0 SUMMARY

Corporate takeover bids are a frequent event in the Australian share market. The high incidence of bids made for companies and the big proportion of all ASX delistings attributable to takeovers (over 40%) suggest that the market for corporate control exerts a real discipline on management.

Walter (1984) and Bishop, Dodd and Officer (1987) find from their share market analyses that takeovers benefit, on average, both target and bidder shareholders and conclude that takeovers help allocate capital to more valued uses. McDougall and Round (1987) analyse the performance of merged firms using accounting numbers. They find that merged firms perform worse that they did pre-merger and worse relative to matched non-merging firms. They conclude that takeovers are motivated primarily by managerial hubris and aggrandisement.

This thesis finds that the divergent conclusions derived from the share market and accounting studies are attributable primarily to differences in their respective sample firms. A reanalysis of the share market performance of BDO's sample firms after controlling for the systematic negative association of firm size and return, survivorship, positive asymmetry in returns and market return shows that their findings and conclusions are robust. The shareholders of both bidder and target firms benefit, on average, from being involved in takeover activity.

Target firms typically display abnormally poor performance in the period leading up to six months before the news of a takeover bid is publicised. News of an impending takeover bid causes a sharp positive revaluation of target firms' shares. The conclusion that the shareholders of target firms benefit unequivocally from takeover bids is confirmed by the typically large increase in share price over the period the takeover bid is announced. The targets of unsuccessful bids do not lose their gains. This is consistent with these firms being flagged as likely recipients of another takeover bid in the near future and with the target firms' managements' implementing value-increasing changes in an effort to stave off further bids.

The evidence indicates that shareholders of bidding firms benefit as well. The usually well above average share market performance of bidding firms in the two years leading up to the takeover bid is consistent with these firms being profitable.
entities that seek to maximise gains by taking over relatively poor performers and implementing value-increasing strategies. On average the share market agrees with the bidding firms' assessment of the expected gains from takeover. Over the period the takeover bid is announced the bidding firms continue to register above average performance. Importantly, the bidding firms do not lose their gains in the post-bid period. The share market does not systematically revise downwards the share price of successful bidding firms in the post-bid period.

The preceding summary describes the share market performance of BDO's sample of firms which were involved in over 1000 takeover bids made between January 1974 and June 1985. The bidding firms involved MR's sample of 88 full takeovers made between 1970 and 1982 experienced a different pattern of returns. Over the four year period centred on the bid announcement month MR's bidding firms achieved, on average, indifferent returns. When compared against the strongly positive returns earned, on average, by BDO's much larger sample of bidder firms the indifferent to poor share market performance of MR's sample of bidding firms supports the argument that their selection criteria systematically excluded those firms with a comparative advantage in managing takeovers.

The differences in sample selection criteria are the most significant factors in explaining the divergence in the results and conclusions drawn in the share market studies and the accounting numbers based study. One point stressed in Chapter Five is that there are no sound methodological grounds for presuming that accounting numbers based evaluations of firm performance and share market based evaluations of firm performance diverge inevitably or that one method of evaluation is always to be preferred over the other. The evidence discussed in Chapters Three to Five supports strongly the conclusion that both methods are subject to biases which some earlier studies have either not recognised or controlled.

6.1 CONTRIBUTIONS

The preceding section summarises the conclusions drawn from the empirical evidence documented in this thesis on the economic consequences of corporate takeovers. This section summarises the methodological empirical contributions of this thesis. It begins with a review of the share market based methodological contributions.

Given the requirement that the share market based event study research design controls for all systematic variation in share prices, an important first step in this thesis was to establish the robustness over time of the anomalous regularities in
share returns documented by earlier researchers. The results of a survey of Australian monthly share returns over the period January 1974 to December 1990 confirm the existence of a robust systematic relationship between firm size and return; after controlling for the variance in return that is related to the market portfolio, small firms (in terms of market capitalisation) persistently earn positive abnormal returns. The seasonalities in monthly returns reported by, among others, Brown, Keim, Kleidon and Marsh (1993) are less regular in appearance and salience.

An important and novel contribution of this thesis is the presentation of evidence that documents the extent and direction of survivorship bias on calculations of market index returns and on estimated abnormal returns. Inadvertent survivorship bias is introduced in event-studies when the returns to the sample firms are adjusted by the return to a market portfolio comprising firms that have not necessarily survived over the full event-window. The extent and direction of survivorship bias is an empirical issue. The analysis and evidence in Chapter Four is consistent with the proposition that a market portfolio confined to firms that have survived over a any given period of month records a lower [sic] return, on average, than a market portfolio that does not require firms to survive over the complete period of month as a criterion for inclusion. The evidence suggests that the firms which delist from the ASX experience, on average, strongly positive abnormal returns. The positive returns are driven largely by the high return recorded, on average, by the firms which exit from the ASX boards as a result of a takeover or merger. Takeovers and mergers account for over 40% of ASX delistings.

This thesis is the first to take account of the impact of positive asymmetry in the distribution of firms' returns. The extent of positive asymmetry is such that deleting all returns that lie beyond three standard deviations of the mean (equally weighted) return to all firms listed over a given period does not entirely eliminate the asymmetry. One consequence of positive asymmetry in returns that has not always been appreciated in earlier studies is that the median return to the firms in the market portfolio is, on average, less than the mean return. Comparing the sample portfolio's positive adjusted return (PAR) ratio against the empirical distribution of PAR ratio values to 1001 control portfolios is one method, developed in this thesis, to obtain an unbiased assessment of relative performance in the presence of positive asymmetry in equity returns.

The two important accounting numbers related methodological contributions of this thesis are the development of a model which demonstrates the downward bias in the observed accounting rate of return (ARR) of merged firms and the development of an empirical test to assess the reliability of accounting numbers based measures of
performance in a portfolio context. The model, exposed in Appendix B, demonstrates that the downward bias in post-merger ARR attributable to the amortisation of goodwill arising from the takeover premium paid by the acquiring firm is directly proportional to the amount of goodwill paid and to the ratio of the size of the target firm over the size of acquiring firm. The empirical test of the efficacy of accounting numbers in assessing performance indicated that aggregating different firms' accounting based measures of performance yields unreliable measures of portfolio performance.

6.2 FUTURE RESEARCH

Corporate takeovers are complex undertakings (so to speak). The extensive share market based literature has, to date, focused largely on the average outcome of takeovers to, typically, large and disparate samples of bidder and target firms. One reason for doing so is that the question has till now been straightforward: are takeovers, on average, value increasing investments?

Given that the measurement problems have been solved (i.e., given that biases such as the firm size effect have been controlled), the share market evidence presented in this thesis suggests that the answer is "yes". Future research should concentrate on developing a more sophisticated model of takeover behaviour or the market for corporate control. Notwithstanding the evidence presented in this thesis, the hubris hypothesis remains a plausible explanation for a significant proportion of takeovers. One aim in future research should be to sharpen our analysis by identifying specific variables which might tell us whether hubris is the prime motive for the bid. Possible variables include the acquiring firm's previous takeover experience and the degree of connection between the acquiring firm's business and that of the target firm.
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