EMPIRICAL RESEARCH ON THE EFFICIENT STOCK MARKETS HYPOTHESIS: THE STATE OF AFFAIRS

1. INTRODUCTION

The efficient markets hypothesis (henceforth EMH), which in its most stringent form claims that security prices always fully reflect all available information, has been a dominant paradigm in finance for more than three decades. During that period, literally thousands of articles have been devoted to empirical examinations of the validity of the EMH. In this working paper, I try to give a structured overview of this vast literature with a view to creating an overall image of our present-day knowledge regarding the hypothesis' descriptive validity with respect to stock markets[1].

The reasons underlying the impressive academic interest that the EMH has elicited are legion. Firstly, one must never forget that the EMH is just a hypothesis. Although many of its proponents have virtually taken the EMH as axiomatic, it is important to emphasize that stock markets are not efficient by definition. Admittedly, the EMH is founded on a whole of most appealing theoretical considerations and no alternative hypothesis has earned comparable status, but market efficiency is merely a *possible* outcome of the real-world stock price formation process. As such, assessment of the descriptive validity of the EMH has almost naturally called for empirical testing. However, and that is a second reason for examining the EMH, empirical research to date has been inconclusive to say the least. The overview of the literature provided in this paper will clearly indicate that although our understanding of stock price formation has been considerably enhanced over the years, it has remained fairly limited. Empirical research has

[1] Clearly, the descriptive validity of the EMH has also been investigated for securities markets other than stock markets. However, given that the amount of research pertaining to stock market efficiency constitutes the bulk of the EMH literature and is already very impressive in itself, I have limited this overview to studies on the EMH's validity with respect to stock markets.

been particularly hampered by the joint-hypothesis problem[2], which has made it extremely difficult to discriminate between risk-based explanations and market inefficiency-based explanations for the obtained results. This has led to an ongoing controversy. Thirdly, the EMH has been subjected to extensive empirical testing as a consequence of its role as a maintained assumption in many lines of research. The convenience of the EMH stems from the fact that some most interesting properties are attributed to security prices by assuming that they always fully reflect the available information. For instance, if markets are efficient, prices are unbiased estimates of fundamental values enabling researchers to assess the usefulness of various valuation models by examining the extent to which they are able to explain the cross-section of prices. Also, under the assumption of market efficiency a realized return over a certain period can be interpreted as a direct measure for the economic earnings during that period. Furthermore, the EMH has a very specific and useful implication as far as subsequent abnormal returns are concerned: under the null hypothesis, neither systematic positive nor negative abnormal returns are predicted. Among other things, this property permits examination of the descriptive validity of asset pricing models as well as measurement of the flow of new information to the market. But as I mentioned above, the EMH is not valid by definition and so one might wonder whether the conclusions drawn in those other lines of research have been distorted by systematic deviations from market efficiency. Clearly, this has constituted another reason for testing the validity of the EMH. Fourthly, attaining and ensuring market efficiency belong (or at least should belong) to the main goals of securities regulation. After all, the quality of a financial market and the extent to which it provides for an efficient allocation of resources are largely determined by its degree of informational efficiency. Consequently, research aimed at assessing the extent to which the EMH is descriptively valid has also been warranted by policy considerations. Finally, empirical tests of the EMH are obviously useful in the context of portfolio management. The most far-reaching practical implication of market efficiency is probably that the development of active investment strategies is

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[2] The joint-hypothesis problem takes root in the fact that nearly all empirical tests of the EMH have one way or another relied on the use of abnormal returns. Since estimation of the latter in turn requires using a particular expected returns model to determine the magnitude of the returns that may be considered as normal, virtually all empirical examinations of the EMH have in fact tested the combined hypothesis of market efficiency *and* validity of the expected returns model concerned. Consequently, for basically all apparent rejections of the EMH, it has been impossible to determine whether the evidence is actually inconsistent with the EMH or whether the expected returns model is misspecified (or both).

pointless. Therefore, examination of the descriptiveness of the EMH should be helpful to assess the usefulness of active investment styles. In addition, the EMH's clean benchmark quality has proved to be very valuable with respect to the identification of variables that are related to subsequent stock returns, which, apart from the positive influence on our understanding of price formation, is obviously of great importance to portfolio management in general. To the extent that the predictive variables concerned appear to constitute real inefficiencies, they also contain suggestions for potentially profitable active investment strategies.

Clearly, all of these properties of the EMH have contributed to it having become one of the most intensively studied topics in (financial) economics. The unremitting academic interest in the subject makes that the number of related publications continues to grow rapidly. As a consequence, it is far from evident to keep up with the developments in this line of research. As such, I have written this paper in the first place to remedy this difficulty. By providing a general overview of the extant EMH literature with respect to stock markets, I aim to give the reader an image of our current understanding of the degree of rationality inherent in the stock price formation process. By including and in fact dedicating special attention to the most recent findings in this area, I try to make a contribution beyond existing reviews of the EMH literature.

This does surely not imply that I pack in those earlier review articles. As a matter of fact, as far as the structuring of the literature is concerned I heavily rely on the two most renowned publications in this respect, namely Fama (1970) and Fama (1991). Concretely, I have chosen to use the classification of the EMH literature as presented in Fama (1991) to structure the massive amount of empirical work at hand. The more traditional subdivision of the EMH literature described in Fama (1970) serves as an introductory guideline. Although this traditional subdivision has slightly fallen into disuse in recent years, there are at least three reasons because of which one must not underestimate its present-day significance: firstly, it should be acknowledged that Fama's 1991 classification system is in fact nothing but an adapted version of the traditional subdivision. Secondly, the traditional subdivision is in my opinion in at least one respect somewhat superior to its contemporary counterpart in that it is based on an unequivocal and well-defined classification criterion, whereas the 1991 structuring simply distinguishes three

groups of studies without specifying such a classification criterion. Moreover, in view of the extreme character of the null hypothesis inherent in the general definition of market efficiency ("prices fully reflect *all available information*"), the classification criterion employed in the traditional subdivision is very useful to determine and describe at which level of information subset the EMH appears to break down. Finally, in many recent textbooks (e.g. Elton and Gruber (1995) and Bodie, Kane and Marcus (1999)) authors have held on to the traditional subdivision as a starting point to explain the concept of market efficiency. It is in view of the above considerations that I have decided to use both the traditional and the more contemporary classification scheme to create an overall image of the existing literature on the EMH.

It is important that I stress beforehand that I have tried to provide an as objective overview as possible of the empirical EMH literature. Although the major advantages and disadvantages of the various approaches to empirically testing the EMH will now and again be highlighted and although some degree of subjectivity in interpreting the research results is virtually inevitable, I have seen to it that I take a fairly neutral position in all instances. In my opinion, the need for such an objective overview of the EMH literature has been left unmet way too long. Especially now that the debate on the degree of real-world stock market efficiency has largely decayed into an at first sight dead-end discussion in which both the hypothesis' advocates and opponents tend to turn a deaf ear to each other's arguments, neutrality is in my view called for when reviewing the evidence.

The structure of this paper is as follows: in section two, I describe the traditional subdivision of the empirical EMH literature. The far more detailed discussion of the evidence to date follows in section three, in which I depart from the structure proposed by Fama (1991). Finally, I present my conclusions and a number of suggestions for future research in this area.

2. THE TRADITIONAL SUBDIVISION OF THE EMPIRICAL EMH LITERATURE

The theoretical concept of market efficiency was actually developed in response to the need for rationalization of the (at the time unexplained) empirical evidence of randomness in asset price changes. So before Samuelson (1965) and Mandelbrot (1966) laid the theoretical foundations of the EMH, most empirical work in this field focused on the unpredictability of returns on the basis of historical price information. However, the concept of market efficiency as it followed from Samuelson's and Mandelbrot's analyses was much broader in that it postulated that rational investor behaviour should lead to the incorporation of *all* available information (and not just historical price data). As a consequence, the theoretical contributions by Samuelson and Mandelbrot considerably widened the scope of EMH studies and brought on a barrage of new academic efforts to determine the validity of this more comprehensive hypothesis.

As such, an almost natural segregation within the EMH research was created founded on the extensiveness of the subset of information with respect to which efficiency was defined. In developing the traditional subdivision of the empirical literature on the EMH, Fama (1970) clearly built on this segregation, although he did further refine it somewhat.

The traditional classification distinguishes 3 versions of the EMH and on the basis thereof it discerns 3 sorts of studies: weak form, semi-strong form and strong form. The **weak version** of the EMH states that all the information embedded in the *history of past prices* is fully reflected in security prices. So it is this version that most of the early empirical tests were mainly concerned with. The **semi-strong version** of the EMH is more stringent in that it asserts that security prices fully reflect *all publicly available information*. In addition to the history of past prices, such information includes, among other things, accounting data, the quality of a firm's management, fundamental data on its product line and information with respect to the geographical and product market(s) in which the company operates. Finally, the **strong version** of the EMH postulates that simply *all available information* is reflected in security prices. This implies the impossibility to earn

abnormal returns not only on the basis of publicly available information, but also on the basis of *private information*, i.e. information available only to some investors (e.g. company insiders). The strong version of the EMH is obviously quite extreme. I presume that most of us would agree that at least company insiders have access to value-relevant information long enough before the public does to enable them to earn abnormal returns by trading on that information. As I shall describe in more detail in section 3.1., it is therefore hardly surprising that this version of the EMH has generally lacked empirical support. As such, it constitutes the only branch of the empirical EMH literature with respect to which a widespread consensus has been reached among academics regarding the implications for the (in)validity of the hypothesis under consideration.

3. THE CONTEMPORARY SUBDIVISION OF THE EMPIRICAL EMH LITERATURE

I now turn to the more contemporary categorization of the empirical market efficiency literature, as proposed by Fama in his 1991 review article. In my overview I shall, however, reverse the order in which the three types of studies that Fama discerns, are discussed. I do so because the reversal permits me to begin with the similarities between the traditional and the contemporary subdivisions and work my way to the distinctions between the two classifications.

The three categories that Fama distinguishes in his 1991 subdivision are labelled as follows:

1. Tests for private information
2. Event studies
3. Tests for return predictability

The subset of **tests for private information** obviously corresponds to the group of studies that were described as tests of the strong version of the EMH in the traditional terminology. Contrary to what Fama states, I feel that there is no such identity for the second category, the so-called **event studies**. According to Fama, for this category also, there is only a change in title, but not in coverage. I disagree:

the subset of tests of the semi-strong version of the EMH in the traditional classification is much broader than the category of event studies. As a matter of fact, the latter does not include the vast area of academic efforts that have used methodologies other than event study methodology to assess the degree of market efficiency with respect to publicly available information. Under the contemporary structuring, these studies are classified among the **tests for return predictability**. Consequently, this last subset of empirical EMH research in the contemporary classification system is, in turn, considerably more comprehensive than the group of tests of weak version market efficiency in the traditional subdivision. I shall now discuss the contents of each of the three contemporary categories in more detail.

3.1. TESTS FOR PRIVATE INFORMATION

Within this category of the empirical EMH literature, two major streams of research can be discerned. The first one has been concerned with whether *company insiders* tend to have above-normal expected trading returns thanks to their monopolistic access to some information. The central goal of the second stream of academic efforts has been to examine whether certain other members from the investment community, i.e. *company outsiders*, appear to systematically have information at their disposal before it is reflected in stock prices.

3.1.1. Company insiders

Not surprisingly, the empirical evidence has generally provided the question whether company insiders have above-normal expected returns with an affirmative answer. Scholes (1969) was among the first to demonstrate that sales of large blocks of common stock by company insiders are on average followed by negative abnormal returns for those stocks. Since he found the magnitude of the stock price adjustments to be independent of the sizes of the insider sales, Scholes ruled out the "price pressure"-based alternative explanation for his findings and concluded that the subsequent relatively poor performance of the stocks concerned indicated that insider sales convey bad news to the rest of the investment community. Scholes' conclusion obviously implies that insiders tend to have information at their disposal that has not been priced yet by the market.

In the following years, the sensitivity of the early findings to differences in methodology was examined. Particularly much attention was thereby given to the procedure used for estimating "normal" returns, i.e. the joint-hypothesis problem. Whereas the earliest studies in this area used rather ad hoc and stochastic-based estimation procedures such as the market model or historical average returns, subsequent research (e.g. Jaffe (1974), Givoly and Palmon (1985) and Seyhun (1986)) started making use of theoretically well-substantiated cross-sectional expected returns models like the capital asset pricing model (henceforth CAPM). The finding that company insiders tend to have information that is not reflected in prices appeared to be very robust. In fact, over the years, hardly any contradictory evidence has been published. That explains why, as I already mentioned in section 2, the academic community considers this issue as largely resolved and has reached agreement on insiders having the opportunity[3] to systematically earn abnormal returns on the basis of the private information that they have at their disposal.

Having more or less established that company insiders may profit from their monopolistic access to value-relevant information that is not yet fully reflected in stock prices, research in this area has been refined in recent years. Concretely, academics have begun to empirically investigate both the sources of insider gains and the mechanisms and/or trading strategies through which they are obtained. Aboody and Lev (2000) have demonstrated, for instance, that a major source of private information leading to information asymmetries and insider gains lies in the extent to which firms engage in research and development activities. With respect to the nature of the trading strategies employed by company insiders, a very insightful article is Noe (1999). Previous research (e.g. Penman (1982), Hirschey and Zaima (1989), Seyhun (1990), Karpov and Lee (1991), John and Lang (1991) and Lee, Mikkleson and Partch (1992)) had already established that insider transactions appear to be based upon knowledge regarding firms' long-term prospects rather than on information concerning short-term performance. However, those studies all failed to come up with convincing evidence in support of the commonly proposed hypothesis that insiders mainly earn abnormal returns by

[3] It should be noted that most industrialized countries have regulations aimed at preventing insiders from exploiting their privileged situation. Among others, the studies mentioned above have nevertheless demonstrated

carrying out transactions in anticipation of shortly-upcoming corporate news events. Noe has therefore made a major contribution by providing evidence that is consistent with his quite remarkable hypothesis that insiders tend to make transactions when information asymmetry with the public is actually relatively *low*. More specifically, Noe has demonstrated that managers appear to time insider sales (purchases) *after* "good news" ("bad news") management earnings forecasts. Such a strategy grants insiders the opportunity to earn abnormal returns, while at the same time shielding themselves against profiteering allegations. Although his results suggest causality between companies' voluntary disclosure decisions and insider transactions, Noe acknowledges that alternative explanations for the apparent relationship cannot be ruled out at this stage. That is why, in my opinion, Noe's following conclusion sums up the contribution of his work pretty well (p. 307):

> *"Whichever the case, this paper shows that the interaction between managers' insider transaction decisions and firms' information disclosure activities is more complex than previously thought."*

Apart from providing a solid (albeit extremely general) summary of Noe's findings, the above quotation is in my view also indicative of the avenue that empirical research in this particular sub-area of the EMH literature is likely to follow. After all, since academics have more or less come to a consensus regarding the inefficiency of stock markets with respect to insiders' private information, it is most probable that future research efforts on this specific matter will mainly be concerned with on the one hand identifying the sources of those private information elements and on the other hand unravelling the strategies used by insiders to exploit their privileged situation.

3.1.2. Company outsiders

Mainly due to data availability restrictions and with the exception of a mere handful of studies (e.g. Barber and Odean (1998)), research aimed at examining whether certain members of the investment community appear to have some sort of private

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that, on average, insider purchases (sales) tend to be followed by abnormal price increases (decreases). Without going into the overall desirability of such regulations, their general efficacy may therefore be questioned.

information at their disposal, has focused on *market professionals*. A first group of professionals that has received considerable academic attention are *financial analysts and investment advisory services*. Womack (1996), for instance, has investigated rating changes pertaining to about 800 different stocks and made by various brokers that are taken up in the First Call database. He concludes that these advices do have predictive value with respect to the subsequent price performance of the stocks concerned. The investment advisory service that has probably elicited the most academic interest is the Value Line Investment Survey. The latter weekly ranks common stocks into five groups on the basis of earnings and price information: group one supposedly having the best return prospects and group five the worst. Over the years, quite substantial evidence (e.g. Black (1973), Copeland and Mayers (1982) and Stickel (1985)) has been presented documenting that group one stocks on average earn higher risk-adjusted returns than group five stocks, indicating that Value Line's analysts may have access to information before it is fully reflected in stock prices.

There are, however, at least four reasons due to which one should be very cautious in making inferences on the basis of the above findings: first, the apparent access of some analysts to private value-relevant information must not be generalized. After all, there is also considerable empirical evidence of analysts' recommendations having very little or no predictive power with respect to subsequent returns (e.g. Wijmenga (1990), Graham and Harvey (1996) and Hulbert (1997)). Second, even as far as the aforementioned successful investment advisory services are concerned, the abnormal returns accruing to the recommended stocks have generally been quite modest (about one to three percent in the days surrounding the announcement date). This makes that the implications of these results for the validity of the EMH are contingent upon the way in which the concept of market efficiency is defined. Using the strict definition by Fama (1970), even such small abnormal returns constitute evidence of markets not being fully efficient in the sense that not all available information is always reflected in prices. However, taking the frictions into account that are surely present in real-world financial markets, those small "abnormal" returns may in fact merely constitute fair compensation for the costs that the investment advisory services concerned have to incur to generate the private value-relevant information. The evidence is therefore not necessarily inconsistent with Jensen's (1978) less stringent definition of market

efficiency[4]. Third, as long as the sources of the apparently private pieces of information are not properly identified, it is difficult to reliably rule out alternative explanations. It has, for instance, been claimed (e.g. Affleck-Graves and Mendenhall (1992) and Lacey and Phillips (1992)) that the Value Line enigma is merely the reflection of other cross-sectional anomalies[5], for which it has not been established with certainty either whether they constitute market inefficiencies or not. And finally, it is important that a distinction be made between the theoretical and the real returns on the recommended stocks. Hulbert (1990) and Salomon (1998), for example, have highlighted the large disparity between the returns on theoretical investments in Value Line's group one stocks and the returns earned by Value Line's mutual funds that invest in those stocks in practice, the former being considerably larger than the latter.

This last reason probably partly explains why researchers have also devoted a lot of attention to a second group of market professionals, namely *professional portfolio managers*. Examining whether they systematically earn abnormal returns on their portfolios obviously sidesteps the problem of discrepancy between paper and real-world profits and forms a more direct test for private information. Unfortunately, this so-called performance evaluation literature has found itself confronted with another major obstacle under the form of the joint-hypothesis problem. Since raw returns obviously do not permit proper assessment of portfolio managers' performance, adjustment for risk is called for. In the context of performance evaluation, risk adjustment may intuitively be thought of as a comparison of the returns on the actively managed portfolio under consideration with the returns on a passive portfolio with the same level of risk. However, explicit or implicit construction of such a passive portfolio requires reliance on the validity of a particular expected returns model and that is where the joint-hypothesis problem surfaces.

The controversy resulting from the joint-hypothesis issue has been intensified by the fact that research findings in this area have proved to be extremely sensitive

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[4] In response to the issue that real-world investors are confronted with positive information and transaction costs so that Fama's extreme version of the EMH is surely false, Jensen (1978) has put forward a weaker and more realistic version of the EMH, stating that "prices reflect information to the point where the marginal benefits of acting on information do not exceed the marginal costs".

[5] For an overview and a discussion of the major cross-sectional anomalies, I refer to section 3.3.2.

both quantitatively and qualitatively to the methodology used for risk adjustment. For instance, with the exception of the early work by Jensen (1968 and 1969) and McDonald (1974), studies that have used CAPM-based benchmark portfolios to evaluate the performance of mutual fund managers (e.g. Mains (1977), Henriksson (1984), Chang and Lewellen (1984) and Ippolito (1989)) have generally concluded that the latter have access to sufficient private information to cover the expenses and fees they charge investors. In recent years, however, the validity of this conclusion has become most questionable. Following the publication of considerable evidence anomalous to the predictions of the traditional one-factor CAPM[6], researchers active in the area of performance evaluation have begun to use multiple portfolio benchmarks and in doing so, the formerly as abnormal considered returns earned by mutual fund managers have generally vanished into thin air. This does not imply that agreement has been reached, however. Depending upon the exact procedure used to construct the various benchmark portfolios, some studies have found no evidence of abnormal performance (e.g. Grinblatt and Titman (1989 and 1994), Connor and Korajzcyk (1991) and Elton, Gruber, Das and Hlavka (1993)), while others (e.g. Lehmann and Modest (1987)) have suggested that mutual fund managers actually earn significantly *negative* abnormal returns.

An excellent overview of the performance evaluation literature as well as directions for future research are provided in Grinblatt and Titman (1995). Their main conclusion, and I fully concur with them, is that research in this area has progressed considerably over the years, but that a lot of work remains to be done. From the various contradictory results we have learned the importance of the choice of a benchmark portfolio, yet due to this sensitivity to methodology academics have so far failed to satisfactorily answer the question whether professional portfolio managers add value or not. Clearly, a lot more research is called for, but on the basis of the more recent evidence it is probably safe to say that it is quite doubtful that access to a significant amount of private information is pervasive among professional portfolio managers.

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[6] Again, I refer to section 3.3.2. for an overview of this anomalous evidence.

3.2. EVENT STUDIES

Since its introduction by Fama et al. (1969), event study methodology has become increasingly popular and has been adopted in diverse lines of research. Within the context of accounting and finance, the term "event study" refers to a type of research design aimed at investigating the way in which security market behaviour is influenced by specific financial or economic events. Examples of such events include changes in dividend policy (e.g. Charest (1978), Asquith and Mullins (1983), Michaely, Thaler and Womack (1995) and McCaffrey and Hamill (2000)), seasoned equity offerings (e.g. Asquith and Mullins (1986) and Masulis and Korwar (1986)), stock repurchases (e.g. Dann (1981), Vermaelen (1981), Ikenberry, Lakonishok and Vermaelen (1994 and 1998) and Liu and Ziebart (1997)) and mergers (e.g. Mandelker (1974), Franks, Broyles and Hecht (1977), Dodd and Ruback (1977), Dodd (1980), Elgers and Clark (1980) and Asquith (1983)). Some studies have examined the volatility of security returns and patterns of trading volume surrounding events (for a review, see Yadav (1992)), but most of them have focused on security prices. Using the occurrence of an event as the sampling criterion, academics using this type of methodology have typically investigated whether abnormal returns occur in the days surrounding the event under consideration, i.e. the so-called "event window".

In the context of EMH testing, event study methodology has been adopted to examine the speed with which new information is incorporated into stock prices. Since market efficiency requires that all available information always be fully reflected in prices, new elements of information should almost immediately and unbiasedly affect market values. In his overview, Fama (1991) contends that apart from a few exceptions, event study-based tests of the EMH have generally been supportive of the hypothesis. Moreover, as do most proponents of the EMH, Fama claims that, within the whole empirical EMH literature, event studies have provided the "cleanest" evidence on market efficiency. The latter contention follows from the fact that event studies traditionally focus on the days surrounding the occurrence of the event concerned. For such short periods of time expected returns are so close to zero that inferences regarding the abnormality of observed raw returns are virtually irrespective of the procedure used to estimate expected returns. As such,

contrary to most other empirical research designs used to test the validity of the EMH, event studies are hardly susceptible to the joint-hypothesis problem.

Personally, I disagree with Fama on both empirical and conceptual grounds. From an empirical point of view, I do not believe it is fair to say that the event study-based evidence has generally corroborated the EMH. Over the years, a considerable number of event studies has produced results that may be interpreted as evidence of stock prices failing to adjust in a timely fashion when new information reaches the marketplace. A very robust and thoroughly examined example is the so-called "post-earnings announcement drift"[7]. Conceptually, I do not concur with Fama's claim that event studies produce the cleanest evidence on market efficiency. On the contrary, I would be inclined to contend that studies with short event windows (i.e. the ones Fama's remark pertains to) are virtually unable to provide us with *any* useful direct evidence regarding the descriptive validity of the EMH. Such studies can merely demonstrate whether a certain event elicits *a* market reaction or not. However, to learn whether the observed reaction in the days surrounding the announcement of the event is complete (i.e. unbiased), the event window needs to be lengthened. But since lengthening of the event window entails the need to estimate expected returns, it is my opinion that those event studies that may constitute useful tests of the EMH are just as prone to the joint-hypothesis problem as the rest of the market efficiency literature[8].

On the other hand, I feel that event studies with short event windows may be of indirect value to the empirical EMH literature. After all, they do allow that events with a significant impact on stock prices be distinguished from others. As such, they may provide suggestions with respect to the events of which the occurrence can be used as the dependent variable in tests for return predictability[9].

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[7] This phenomenon is discussed in more detail in section 3.3.2.2.
[8] Admittedly, Fama (1991) does acknowledge that event studies become subject to the joint-hypothesis problem when part of the response of prices to information seems to occur slowly. However, this does not keep him from emphasizing what he calls the main point, namely that event studies provide the cleanest evidence on market efficiency.
[9] Examples of such tests are discussed in section 3.3.2.2.

3.3. TESTS FOR RETURN PREDICTABILITY

The last group of empirical EMH studies is definitely the one with the most diversity. In order to gain an insight in the structure of this so-called "predictability literature", it is therefore absolutely necessary to distinguish a few sub-areas of research. I shall thereby not use the complete same sub-classification as Fama (1991), although there is considerable overlap between his structuring of the predictability literature and mine. In fact, the main difference lies in the classification of what has become known as the "contrarian"[10] evidence. Fama classifies the contrarian studies under what he calls "research on the time-series predictability of stock returns", rather than under "research on cross-sectional return predictability". I feel, however, that although the contrarian evidence is based on autocorrelations in returns, it constitutes more of a cross-sectional anomaly and therefore belongs in the latter category. In turn, this reclassification compels me to rename the former category. This makes that the two sub-areas of literature that I distinguish are "market timing research" on the one hand and "research on cross-sectional return predictability" on the other hand.

3.3.1. Market timing research

In section 2, I already mentioned how the EMH was initially conceived as a rationalization for the apparent random behaviour of stock prices. Also due to the often unjust interchange of the terms "fair game", "(sub)martingale" and "random walk", most of the early empirical work on the EMH assumed that the concept of market efficiency does not just imply that abnormal returns are unpredictable, but also that expected returns are constant through time. Over the years, however, quite considerable evidence has been presented that expected returns are not constant, but time-varying in a to some extent predictable manner. Unfortunately, academics have so far failed to agree on the implications of this evidence: are stock market returns predictable due to systematic inefficiencies or is the time-varying nature of expected returns merely a rational reflection of varying economic conditions? Based upon the variables and methodology used to examine the predictability of stock market returns, four kinds of evidence can be discerned: (1)

[10] See section 3.3.2.1.

return autocorrelations, (2) economic forecasting variables, (3) volatility tests and (4) seasonal patterns.

3.3.1.1. Return autocorrelations

The early work on return autocorrelations mainly used *individual* stock returns. Fama (1965), for instance, found that three out of four Dow Jones 30 Industrial stocks had significantly positive autocorrelations in *daily* returns during the 1957-1962 period. His findings were only partly corroborated by subsequent research, however. Specifically, French and Roll (1986) have demonstrated that daily autocorrelations are indeed positive as far as large stocks are concerned, but that for small stocks they are actually negative on average. These findings have generally led academics to the conclusion that there is an underlying positive serial dependency in daily stock returns, which is, however, not observed for small stocks because their estimated autocorrelations are relatively more biased by the so-called "bid-ask bounce". This is the microstructure-related effect that successive trades tend to occur alternatively at the bid and the ask, resulting in negative serial autocorrelation in returns for short intervals (Niederhoffer and Osborne (1966)). With respect to *weekly* returns, Lo and MacKinlay (1988) have found negative autocorrelations for individual stock returns, albeit both statistically and economically insignificant ones. For that matter, doubtful economic (and to some extent even statistical) significance may be considered a more or less common characteristic of the evidence that has been obtained on the basis of individual stock return autocorrelations. Moreover, the explained portion of total return variability has typically been trivial in these studies (i.e. generally less than one percent).

With a view to reducing the variance in returns and increasing statistical power, researchers have therefore turned more and more often to the examination of *portfolio* return autocorrelations. Lo and MacKinlay (1988), for example, have demonstrated that there is reliable positive autocorrelation in *weekly* returns on size-based portfolios. However, since the positive autocorrelations documented by Lo and MacKinlay were clearly most pronounced for small stocks, it has been suggested that their results were driven by nonsynchronous trading effects (Fisher (1966)). Admittedly, *this* argument has at least partly been countered by

subsequent evidence in Keim and Stambaugh (1986) and Conrad and Kaul (1988), but one cannot ignore the fact that although the observed short-horizon portfolio return autocorrelations have generally been statistically significant, their economic significance has remained most questionable.

In response, Shiller (1984) and Summers (1986) have argued that tests pertaining to short-horizon returns lack statistical power to reject the joint null hypothesis of market efficiency and constant expected returns. More specifically, they have developed models featuring irrational market participants leading in aggregate to a strongly inefficient stock market in which prices take large swings away from intrinsic values. According to Shiller and Summers, such irrationalities do not necessarily show up in short-horizon tests, however. Subsequently, Stambaugh (1986) has pointed out that in order for the Shiller-Summers model to be descriptive, significant negative autocorrelations should be observed at longer return horizons. Apart from Stambaugh, also Fama and French (1988a) and Poterba and Summers (1988) have presented empirical evidence of such long-horizon negative return autocorrelations. This apparent empirical support for the Shiller-Summers model has been counterbalanced, though, by Richardson (1991), who has shown that the type of U-shaped pattern observed for long-term returns is naturally induced by a data series that follows a random walk. Furthermore, the statistical power of the empirical tests with respect to long-horizon autocorrelations has been low, due to the relatively small sample sizes available for such tests.

In short, the studies with a focus on return autocorrelations have hardly produced convincing evidence to reliably reject the joint hypothesis of market efficiency and constant expected returns. The autocorrelations found for short-horizon returns have been statistically significant at best, but their economic significance has generally remained dubious. On top of that, those results have been clouded by microstructure-related biases. In that respect, the evidence on longer horizon autocorrelations should be considered superior and more interesting. Unfortunately, this evidence has in turn been criticized because of the low statistical power of the tests. And moreover, one should, again, not lose sight of the joint nature of the hypothesis that is being examined. Even if the apparent negative autocorrelations in long-horizon returns are considered to be real, they do not, as Fama and French (1988a) emphasize, constitute irrefutable evidence of stock

markets being inefficient. Although they are consistent with the irrational investor behaviour described by Shiller (1984) and Summers (1986), they may also be interpreted as evidence of rational time-variation in expected returns. As long as this ambiguity is not resolved or at least mitigated, time-series tests of market efficiency will probably remain largely fruitless.

3.3.1.2. Economic forecasting variables

To a certain extent, studies that have tried to predict returns on the basis of predetermined economic variables have also suffered from this ambiguity. This line of research has produced slightly more conclusive evidence, though, that the partial predictability of returns ought to be attributed to rational time-variation in expected returns.

Within this sub-area of academic work also, the earliest efforts nevertheless failed to explain an appreciable portion of the total return variability. The negative relation between expected inflation and monthly stock returns that was documented in those early studies (e.g. Bodie (1976), Jaffe and Mandelker (1976), Nelson (1976), Fama and Schwert (1977) and Fama (1981)), did therefore not pose a real challenge to the joint hypothesis that markets are efficient and that expected returns are constant through time.

However, subsequent research *has* cast doubts on the validity of this joint hypothesis. Following work by Rozeff (1984) and Shiller (1984), Fama and French (1988b) have demonstrated that the fraction of total return variability that is explained by dividend yields (henceforth, D/P) increases with the length of the return horizon and rises to as much as 25% for two- to four-year returns. Comparable evidence has been presented regarding the predictive ability of earnings-to-price ratios (henceforth E/P ratio; e.g. Campbell and Shiller (1988)) and book-to-market ratios (henceforth B/M ratio; e.g. Kothari and Shanken (1997) and Pontiff and Schall (1998)). Especially combined, these results obviously constitute a rather reliable rejection of the aforementioned joint hypothesis. Following this evidence, a more probing research question has risen naturally: do the predictive variables provide indications that stock markets are over- or undervalued? Or do

they merely proxy for economic conditions that induce rational time-variation in expected returns?

In this respect, important new insights have been generated by Fama and French (1989), who have tackled the joint-hypothesis issue in an original manner. Building on Keim and Stambaugh (1986) and Campbell (1987), Fama and French have shown that dividend yields also have predictive power for bond returns and, similarly, that term-structure variables may also be helpful in forecasting stock returns. The reasoning underlying Fama and French's approach is that the common nature of the variation in returns across security classes indicates that the predictability of stock returns probably results from rational variation in investor tastes and investment opportunities. The findings of Fama and French have been formally confirmed and extended in Ferson and Harvey (1991), Campbell and Hamao (1992) and Fama and French (1993). Harvey (1991) has even shown that the common variation in expected returns is international, in that U.S. dividend yield and term-structure variables predict returns on foreign common stock portfolios.

Still, even a fervent proponent of the EMH like Fama recognizes that the common variation in expected returns may in fact just as well be explained as evidence of correlated over- or undervaluation across assets and markets. One should therefore remain healthily critical of the rationality-based interpretation of the results. Personally, though, I consider the evidence of common variation to be a convincing indication that expected returns vary rationally through time. There are two elements in the common expected returns hypothesis that I find particularly appealing: first, the link that has been established between the forecasting variables and business conditions strongly suggests that the predictable component in expected return variability stems from investors adapting rationally to changes in the economic environment. And second, the rationality-based explanation is supported by the fact that both Keim and Stambaugh (1986) and Fama and French (1989) have demonstrated that the regression coefficients on the common factors increase in accordance with intuition about generally increasing risk across asset categories, i.e. from government bonds to corporate bonds, from high-grade bonds to low-grade bonds, from bonds to stocks and from large stocks to small stocks.

That is why I feel inclined to agree with the conjecture that the predictability of stock returns on the basis of economic forecasting variables is risk-related.

3.3.1.3. Volatility tests

Conceptually, volatility tests are closely related to the research on return autocorrelations (see section 3.3.1.1.), yet they follow an entirely different methodological approach. They are based on Samuelson's (1965) demonstration that the traditional null hypothesis of no predictability in returns tests is equivalent to the so-called "present-value relation". According to the latter, the actual stock price in an efficient market is the best predictor of the ex-post rational price, i.e. the discounted value of future actual dividends. Stated otherwise, the current price should be equal to the mathematical expectation of ex-post rational price conditional upon the information set that investors have at their disposal.

This present-value relation inspired LeRoy and Porter (1981) and Shiller (1981) to propose an alternative empirical test of market efficiency. They reasoned that if stock prices equal the discounted stream of expected future dividends, prices should only change when expected dividends change. Based thereupon, they derived that in order for markets to be efficient the volatility of stock prices should be bounded above by the volatility of actual dividends, the latter being a proxy for expected dividends. Since LeRoy and Porter as well as Shiller found that these bounds were severely violated, volatility tests were initially considered to provide quite persuasive evidence of irrational stock market behaviour.

The main reason for the development of the volatility test methodology was that the tests were claimed to have greater statistical power than returns tests. Ironically, the early bounds tests were shown to actually have *lower* power (e.g. Flavin (1983) and Kleidon (1986)) due to econometric difficulties[11]. In spite of the fact that these difficulties have at least partly been remedied in later work, the volatility tests have lost out on academic interest to the predictability studies using economic forecasting variables (see the previous section). The reason is obvious: both lines of approach have produced more or less reliable evidence against the joint hypothesis

[11] For a more detailed review of the literature pertaining to volatility tests and the econometric problems involved, see, for example, LeRoy (1989), Gilles and LeRoy (1991) and LeRoy and Steigerwald (1995).

of constant expected returns and market efficiency, but the volatility tests have had the major disadvantage that, contrary to the studies using economic variables, they have failed to provide any indication as to the nature of the variation in expected returns.

3.3.1.4. Seasonal patterns

The validity of the joint hypothesis of constant expected returns and efficient markets has also been questioned on the basis of the apparent presence of certain "seasonal patterns" in returns. I shall focus on the most renowned and most thoroughly examined of these patterns, namely the so-called *"January effect"*. Rozeff and Kinney (1976) were the first to demonstrate that stocks listed on the New York Stock Exchange on average earn significantly higher returns in January than in other months. The effect is not specific to the United States and has been shown to exist in a considerable number of stock markets around the world (e.g. Gultekin and Gultekin (1983) and Hawawini and Keim (1995)). The magnitude of the January seasonal is not the same for all firms, though. Keim (1983) and Blume and Stambaugh (1983) have provided evidence that the effect is clearly most pronounced for small firms. Apparently, the month of January even accounts for nearly the entire cross-sectional size anomaly[12].

As far as explaining the January effect is concerned, market efficiency proponents have generally held on to arguments relating to market microstructure. They feel confirmed in their view by the fact that the pattern predominantly manifests itself for smaller stocks. Specifically, it has been suggested that the January seasonal is merely an artefact of the data, in the sense that it is a reflection of the bid-ask spread caused by the use of closing transaction prices in the calculation of returns. Hawawini and Keim (1995) aptly describe the reasoning as follows (p. 527-528):

> *"These closing prices may be equivalent to dealer (specialist) bid or ask prices depending on whether the trade was seller- or buyer-initiated. Now, as a case in point, if there is a preponderance of seller-initiated transactions at the end of the year (...) and an abundance of buyer-initiated transactions in the*

> *beginning of the new year (...), and this behaviour tended to be particularly pronounced for small stocks (...), then the large returns we tend to see at the turn of the year could be due to a systematic movement from transactions occurring closer to the bid toward transactions that are closer to the ask."*

This argument has, however, not received full support from the empirical evidence. Keim (1989), for instance, has shown that such a bias in calculated returns does seem to exist, but that it is too small to explain the January effect. Apart from that, I find the microstructure-related explanation in itself most unsatisfactory because it fails to offer a reason for the predominance of seller-initiated (buyer-initiated) transactions at the end (beginning) of the year.

Additional hypotheses are, therefore, required and these have been formulated in alternative efforts to explain the January seasonal. Probably the most popular of these explanations is the tax-loss selling hypothesis. According to the latter, the observed higher returns in January are merely the result of prices rebounding to equilibrium levels after having been depressed by serious selling pressure at the end of the previous year. This selling pressure, in turn, is caused by the fact that the sale of securities that have experienced recent price declines is encouraged so that capital loss can be offset against taxable income. Due to their typical higher price variances, small stocks are deemed more likely to have suffered such price decreases.

Apart from the fact that there are a number of a priori reasons for questioning the validity of the tax-loss selling hypothesis[13], the empirical evidence pertaining to it has been inconclusive to say the least. Both Reinganum (1983) and Roll (1983a) have reported that the observed January effect is closely associated with tax-loss selling induced by negative returns over the previous year. However, Reinganum acknowledges that adopting a trading rule designed to exploit this phenomenon

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[12] The size anomaly is discussed in section 3.3.2.1.

[13] Brown et al. (1983) advance three such reasons: firstly, since it is generally recognized that the demand curve for securities is essentially horizontal, any tax-related selling at the end of the year does not necessarily imply price decreases. Secondly, they state that the mechanics of the tax-loss selling hypothesis are unclear. They thereby quote Sharpe (1981), who has documented that although tax-driven end-of-year sales are rather common, no major fall in prices appears to result from that pressure. Finally, Brown et al. wonder why capital losses are not realized throughout the year and why they should be concentrated at the end of December.

would lead to nothing but fairly modest profits and that the tax-loss selling hypothesis cannot explain the entire January pattern. Evidence in support of the tax-loss selling theory has been documented by Schultz (1985), who has shown that there was no sign of a January effect prior to 1917, i.e. the year in which the United States adopted the War Revenue Act and an incentive for tax-motivated selling was created.

On the other hand, the tax-loss selling hypothesis has often been tested by examining the behaviour of returns in countries with either different tax codes or different tax year-ends than the United States. On balance, these studies have produced evidence that is difficult to reconcile with the tax-loss selling theory. For instance, Brown et al. (1983) have found significantly higher stock returns in Australia for the months December, January, July and August, while Australia's tax year ends in June. Berges, Mc Connell and Schlarbaum (1984) have found evidence of a January effect in Canada prior to 1972, although Canada had no capital gains tax at the time. Finally, Kato and Schallheim (1985) have documented a January effect in Japan similar to the one noted in the United States, in spite of fundamental differences in the tax regimes of the two countries.

In view of the inconsistent evidence with respect to the tax-loss selling hypothesis, other potential explanations have been advanced. Haugen and Lakonishok (1987) have suggested that the January effect may stem from "window dressing" by institutional investors. There is some evidence that institutions indeed tend to sell "loser" stocks at year-end with a view to keeping them from appearing on the year-end statements sent to constituent shareholders, but it is hard to distinguish this phenomenon's impact from potential tax-loss selling. Yet another hypothesis ascribes the January effect to periodic infusions of cash into the stock market as a result of, for example, proceeds from bonuses. Kato and Schallheim (1985) and Hawawini (1991) have provided some support for this hypothesis by demonstrating that in Japan seasonals occur in both January and June, thus coinciding with the times at which bonuses are traditionally paid in Japan. Ariel's (1987) turn-of-the-month effect (see below) may also be interpreted as evidence of stock prices being pressured by periodic cash infusions. This hypothesis cannot, however, be easily reconciled with the common finding that the January effect is typical of smaller stocks.

Overall, I feel that none of the above hypotheses is able to fully explain the January effect by itself. For the market microstructure-related theories, this has even been formally demonstrated. One may therefore be inclined to accept one of the alternative theories such as tax-loss selling or window dressing. However, in turn, these hypotheses fail to rationalize the fact that a well-known and amply exposed pattern like the January effect has not ceased to exist as a result of the actions of arbitrageurs. This suggests to me that a complete explanation of the January seasonal is to be sought in a combination of the various hypotheses: first of all, I think that the presence of a January effect in many stock markets around the world makes it quite safe to say that the seasonal is real and not the spurious result of data mining. It is also more than likely that the pattern is caused by price pressure effects induced by a combination of tax-loss selling, window dressing and cash infusions. But why has it not been arbitraged away then? One possibility of course is that markets are simply inefficient. Another is that many, if not most, studies have overestimated the magnitude of the January effect (cf. the microstructure-related biases) and that it is actually too small to cover arbitrageurs' transaction costs, in which case Jensen's (1978) version of the EMH would not be violated. To date, academics have largely failed to distinguish between these two possibilities. This is hardly surprising, given the fact that the joint-hypothesis issue has seriously clouded all attempts to assess whether the abnormal returns that can be earned during the month of January exceed transaction costs or not. On top of that, matters are made more complex by the January effect's apparent interrelation with cross-sectional phenomena that are not yet fully understood either, namely the size anomaly and the value-versus-growth effect[14].

For reasons of completeness, it should be noted that a number of other seasonal patterns in stock returns have been detected. Examples are the *"turn-of-the-month effect"*[15], the *"holiday effect"*[16] and the *"weekend effect"*[17]. However, these patterns have by far not received as much academic attention as the January seasonal.

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[14] See section 3.3.2.1. and 3.3.2.2., respectively.
[15] Ariel (1987) was the first to demonstrate that the average returns for common stocks are positive only for the last trading day of the month and for the trading days during the first half of the month.
[16] It was first shown by Ariel (1990) that stock returns are significantly higher on the trading days preceding holidays than on other trading days.
[17] This refers to the regularity first documented by Cross (1973) that stock returns are on average lower during the first days of the trading week and higher as of Wednesday.

Given that even the latter has not been fully resolved yet, it does not come as a surprise that these other seasonal patterns have not been satisfactorily explained either. For each of them, a number of potential explanations have been brought forward, though. An overview is provided in Hawawini and Keim (1995).

3.3.2. Research on cross-sectional return predictability

Having given an overview of the academic efforts with respect to the predictability of returns on the stock market as a whole, I now turn to the research on the cross-sectional predictability of stock returns. This is the field that has become commonly known as *"the anomalies literature"*, whereby the term "anomaly" was taken from Kuhn (1970), whose definition was paraphrased by Ball (1992) as follows (p. 321):

> *"Systematic evidence that appears scientifically precise but is inconsistent with the tenets of basic theory."*

In the context of cross-sectional EMH research, the phrase "basic theory" obviously refers to the joint paradigm of market efficiency and asset pricing consistent with the predictions of, originally, the CAPM. In spite of the fact that the second part of this paradigm has been made flexible in the sense that several alternative equilibrium expected returns models have been introduced, an impressive number of such anomalies has been revealed over the years.

Fama (1991) structures the studies in this line of research on the basis of the asset pricing model used. Personally, I do not feel comfortable with such a classification in a text aimed at drawing a picture of where we stand when it comes to assessing whether stock markets are efficient or not. After all, Fama's review comes down to an evaluation of the descriptive validity of various asset pricing models using the EMH as *a maintained hypothesis*. Admittedly, tests of market efficiency are inextricably bound up with equilibrium models of asset pricing (otherwise there would not be a joint-hypothesis problem). With a view to creating a general image of the degree to which the various regularities constitute important challenges to the EMH, I consider it more appropriate, though, to discuss the evidence on an anomaly-per-anomaly basis and to indicate, where possible, the extent to which the

anomalies have proved to be robust to the use of different expected returns models. I shall thereby make a distinction between two kinds of anomalies, the classification criterion being whether an anomaly makes use of currently observable *accounting* information to predict future abnormal returns or whether it makes use of *non-accounting* information. To some, this categorization may appear somewhat artificial and arbitrary, but it allows me to introduce additional structure into the impressive number of potential anomalies that has been uncovered over the years. Furthermore, this extra distinction highlights how the subset of financial statement information has proved to be a particularly fruitful source of challenges to the cross-sectional validity of the EMH.

3.3.2.1. Anomalies based on non-accounting information

The regularity that is generally considered to be the premier anomaly is the so-called **size effect**[18]. The seminal articles with regard to the size effect are Banz (1981) and Reinganum (1981). Banz was the first to present empirical evidence of the fact that, on average, small firms seem to have higher risk-adjusted returns than large firms. He used an asset pricing model that allowed the expected stock return to be a function not only of the traditional market beta risk, but also of a measure of the company's relative market capitalization (i.e. size). Banz found that the size term had roughly the same statistical significance as beta, which led him to believe that an additional factor relevant for asset pricing had been identified. In order to illustrate that what he had discovered was of more than just academic interest, Banz demonstrated that the annualised average excess return on an arbitrage portfolio with a long position in the smallest stocks and a short position in the largest stocks amounted to 19.8% for the 1936-1975 period. Almost simultaneously, Reinganum presented corroborating evidence. In fact, due to the inclusion of the on average smaller stocks from the American Stock Exchange (henceforth AMEX) in his sample, the size premium found by Reinganum was even more impressive and equalled about 30% on an annual basis.

With a view to understanding the causes underlying the market value effect, the existence and the dimensions of the size premium have been examined for various stock markets around the world. Although the magnitude of the anomaly has been

shown to vary considerably across markets, the size effect is commonly considered to be an internationally widespread phenomenon that is not specific to the American stock market. As I already mentioned in section 3.3.1.4., it has also been demonstrated that the size anomaly is closely related to the January effect. According to Keim (1983) and Cook and Rozeff (1984), for instance, approximately half of the size premium is earned during the first month of the year. Blume and Stambaugh (1983) and Jaffe, Keim and Westerfield (1989) have even argued that the market value effect is only significant in January. Another well-documented characteristic of the size anomaly is its instability through time. The fact that there are sub-periods during which small firms are clearly outperformed by their large counterparts was already indicated by Banz (1981) and was more formally noted by Brown, Kleidon and Marsh (1983), among others. More recently, it has been shown that the variability in the size effect is not entirely random. Reinganum (1992) has demonstrated that over longer horizons (i.e. 5 years) the size premium appears to be negatively autocorrelated and Jensen, Johnson and Mercer (1997 and 1998) have linked the variability in the size premium to fundamental macro-economic conditions in general and monetary policy in particular.

The number of potential explanations for the size anomaly that has been brought forward is impressive. Remarkably, all of these explanations have basically held on to the EMH as a maintained hypothesis. Academics have, nevertheless, taken quite divergent positions in attempts to rationalize the market value effect. From a statistical point of view, the size anomaly has been attributed to improper measurement of small firms' market betas (e.g. Roll (1981), Chan and Chen (1988) and Handa, Kothari and Wasley (1989)), flaws in the methods used to calculate returns (e.g. Blume and Stambaugh (1983) and Roll (1983b)) and sensitivity to outlier observations (e.g. Knez and Ready (1997)). Economic explanations include the tax-loss selling hypothesis (see section 3.3.1.4.), the transaction costs hypothesis (e.g. Stoll and Whaley (1983) and Roll (1983a)), the differential information hypothesis (e.g. Arbel and Strebel (1983) and Barry and Brown (1984)) and the marginal firm/relative distress hypothesis (e.g. Chan and Chen (1991) and Fama and French (1995)). Still others have ascribed the size anomaly to a lack of descriptive validity on the part of the CAPM and have generally used multifactor

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[18] For a detailed literature review of the size effect, I refer to Van Uytbergen (1999).

alternatives (e.g. Chan, Chen and Hsieh (1985), Chen (1988), Fama and French (1992, 1993, 1994, 1995 and 1996), Jagannathan and Wang (1996) and Brennan, Chordia and Subrahmanyam (1998)).

Notwithstanding the abundance of potential explanations, the size effect has remained somewhat of a mystery. Recently, Horowitz, Loughran and Savin (2000) have even contended that the widespread attention that has been given to size in the context of asset pricing is unwarranted and that there is no such thing as a size effect! Still, this should obviously not be considered a reflection of the current general attitude of academics towards the market value variable. In fact, although the ultimate source of the anomaly has not been satisfactorily identified yet, it has become more or less common practice to regard size as a proxy for some (additional) systematic risk factor. In recent years, virtually all tests of cross-sectional return predictability have, therefore, used some sort of size adjustment in calculating abnormal returns. If not to properly adjust for risk, then at least to make sure that the cross-sectional predictability being investigated is not just a concealed version of the size effect.

A second non-accounting-based anomaly is the **overreaction effect**, also referred to as the **contrarian strategy**. De Bondt and Thaler (1985 and 1987) were the first to show that the stocks identified as the biggest losers (winners) over a three- to five-year period on average earn the highest (lowest) excess returns over the following period. Since both De Bondt and Thaler are fervent proponents of the behavioural finance paradigm, it is not surprising that they interpreted their findings as evidence of market inefficiency. More specifically, De Bondt and Thaler attributed the observed predictability to market overreaction in the sense that stock prices take large swings away from intrinsic values due to irrational waves of optimism or pessimism before returning eventually to fundamental values. De Bondt and Thaler's results were not sample-specific: evidence of an overreaction effect (or at least negative autocorrelation in long-term individual stock returns) has been found for stock markets outside the United States as well (e.g. Mun, Vasconcellos and Kish (1999 and 2000)). Chopra, Lakonishok and Ritter (1992) have demonstrated that, although the overreaction effect exhibits a pronounced January seasonal and is substantially stronger for smaller firms, it remains economically important even after adjusting for size. Zarowin (1989), on the other

hand, has claimed that contrarian profits disappear when the size effect is controlled for.

Apart from the issue whether the overreaction effect is merely a reflection of the size anomaly, it has been questioned whether contrarian strategies are really systematically associated with abnormal returns. Although De Bondt and Thaler (1987) explicitly ruled out risk-based explanations for the overreaction anomaly, Chan (1988) and Ball and Kothari (1989) have argued that the apparent abnormal profits accruing to contrarian investment strategies actually stem from inadequate adjustment for risk. Specifically, Chan and Ball and Kothari claim that loser (winner) firms as defined by De Bondt and Thaler have seen their market capitalization decline (increase) and, ceteris paribus, have therefore become more (less) leveraged and more (less) risky. To the extent that traditional risk-adjustment methods fail to fully capture this leverage-related source of priced risk[19], abnormal returns associated with contrarian investment strategies may be seriously misestimated. On top of that, Ball, Kothari and Shanken (1995) have contended that as a consequence of the typically low prices of loser stocks and the concomitant high sensitivity to market microstructure effects, problems might even arise in measuring the practically feasible *raw* returns accruing to such stocks.

In short, the overreaction effect has received a lot of academic attention, because it was in fact the first cross-sectional anomaly that was revealed by proponents of behavioural finance theory. As such, the anomaly has constituted a fierce challenge to the EMH as of its introduction. Regrettably, the joint-hypothesis problem as well as possible microstructure-related biases have prevented researchers from conclusively deciding whether the observed price reversals should be interpreted as evidence of irrational investor behaviour.

Another anomaly is the **short-term underreaction effect** or **momentum anomaly** originally described by Jegadeesh and Titman (1993). They have found that strategies of buying stocks that have recently performed well and selling stocks that have performed poorly generate significant positive returns over three- to twelve-month holding periods. At first sight, the momentum anomaly may appear to be

[19] The "leverage anomaly" is discussed in section 3.3.2.2.

inconsistent with the contrarian anomaly, but it is crucial to see that the former pertains to short-term investment horizons and the latter to longer-term holding periods. In fact, short-term underreaction and long-term overreaction have been reconciled in a number of behavioural models. Barberis, Shleifer and Vishny (1998) and Daniel, Hirshleifer and Subrahmanyam (1998), for example, have attributed the observed anomalous patterns in stock returns to biases in the way in which investors process information. Hong and Stein (1999) have claimed that short-term momentum and long-term overreaction stem from the existence of two groups of investors, each trading on the basis of only part of the available information set. Conrad and Kaul (1998), on the other hand, have argued that the apparent momentum in stock returns does not constitute evidence of market inefficiency, but simply results from cross-sectional variation in expected returns. That is, stocks that have recently outperformed the market have done so because they are more risky and it is therefore merely logical that they continue to earn higher returns. In an attempt to evaluate the various explanations for the profitability of momentum strategies, Jegadeesh and Titman (2001) have examined the returns of the winner and loser stocks over the period following the initial holding period. Their finding of negative post-holding period returns provides support for the behavioural theories and involves a rejection of the Conrad and Kaul hypothesis. Mindful of the lack of robustness of this result, Jegadeesh and Titman remain cautious, but it is clear that the behavioural models appear to be closer to the truth than their EMH-based counterpart when it comes to explaining the momentum anomaly.

A fourth type of non-accounting based anomalies are **post-event drifts**. It has been shown for a number of events that prices continue to drift long after the occurrence of the events concerned, thus suggesting that prices react sluggishly to new information. Examples of such events are initial public offerings and seasoned equity offerings (e.g. Loughran and Ritter (1995)), mergers (e.g. Asquith (1983) and Agrawal, Jaffe and Mandelker (1992)), stock splits (e.g. Ikenberry, Rankine and Stice (1996) and Desai and Jain (1997)) and share repurchases (e.g. Mitchell and Stafford (1997)). However, as emphasized by Fama (1998), these post-event drifts have proved to be quite sensitive to changes in methodology or have been found to

be manifestations of other and more fundamental anomalies such as the size effect and the value-versus-growth effect[20].

The last non-accounting-based anomaly is the **value-to-price effect** (henceforth V/P) revealed in Frankel and Lee (1998). I understand that some may find it surprising that I classify this effect as non-accounting-based. Admittedly, Frankel and Lee use an accounting-based valuation framework to estimate firms' intrinsic values. Specifically, they employ the residual income valuation model under which a company's fundamental equity value is estimated as the sum of its current book value of equity and the discounted stream of future residual accounting earnings. However, for the prediction of future earnings, Frankel and Lee appeal to consensus analysts' forecasts. As such, the anomaly that they reveal is not based on currently observable accounting information, but on analysts' information[21].

The results obtained by Frankel and Lee (1998) are absolutely striking. For a three-year investment horizon, for example, they report an average return of 30.6% on a zero cash-investment portfolio with a long (short) position in the highest (lowest) V/P stocks. None of the traditional risk measures (e.g. market beta, size, B/M ratio) can explain this large spread in returns between the long and short positions. On top of that, Herzberg (1998) has demonstrated that Frankel and Lee's results may even be biased downwards due to conservatism with respect to the timing of the construction of their portfolios. Frankel and Lee themselves remain cautious, nevertheless, and acknowledge that their findings may stem from some yet unidentified source of difference in risk between high and low V/P companies. Personally, though, I feel that the V/P anomaly constitutes one of the most serious challenges to the market efficiency paradigm. Not just because of the magnitude of the returns that appear to be associated with it or the fact that traditional risk measures cannot account for it at all, but because of the effect's comprehensive use of a well-articulated valuation theory and the inherent direct comparison of fundamental values to market values.

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[20] The value-versus-growth effect is discussed in section 3.3.2.2.

[21] Frankel and Lee do of course use current book values in the estimation of residual income-based firm values, but the emphasis in their study is clearly on the prediction of future earnings and, as argued above, for that they make use of analysts' forecasts.

3.3.2.2. Anomalies based on accounting information

With respect to the subset of accounting-based anomalies, I suggest that an additional distinction be made. Specifically, I shall distinguish between **direct** and **indirect accounting-based anomalies**. The former are the ones that directly relate currently observable accounting(-based) information to subsequent stock returns, while the latter make use of an intermediate variable or event.

The major **direct accounting-based anomaly** is undoubtedly the so-called **value-versus-growth** (or **glamour**) **anomaly**. The term value stocks is used to refer to companies trading at relatively low multiples of certain accounting variables, while growth stocks are the ones with high prices relative to accounting measures of value. There is overwhelming evidence that the former type of stocks systematically outperforms the latter on a risk-adjusted basis. Although there is no theoretical reason to expect this to be true, Graham and Dodd (1934) already argued that value strategies are profitable. Over the years, several variables have been used to distinguish between value and growth stocks. The most important ones are the **price-to-earnings ratio** (e.g. Nicholson (1960), Basu (1977), Reinganum (1981) and Jaffe, Keim and Westerfield (1989)), the **price-to-book ratio** (henceforth P/B ratio; e.g. Rosenberg, Reid and Lanstein (1985), De Bondt and Thaler (1987), Keim (1988) and Fama and French (1992)), the **price-to-cash-flow ratio** (henceforth P/CF ratio; e.g. Chan, Hamao and Lakonishok (1991)) and the **price-to-sales ratio** (henceforth P/S ratio; e.g. Senchack and Martin (1987) and Jacobs and Levy (1988))[22].

Over the years, ample evidence has been presented (e.g. Aggarwal, Hiraki and Rao (1988), Chou and Johnson (1990) and Capaul, Rowley and Sharpe (1993)) that there is a premium associated with value-versus-growth investing on stock markets outside the United States as well. On the basis of an extensive international study, Fama and French (1998) have recently confirmed that the outperformance of growth stocks by value stocks is a worldwide phenomenon. The pervasiveness of the anomaly has generally led researchers to the conclusion that the observed premium is real and not the result of data mining. However, as to the source of the value

[22] It should be noted that many studies have not used the variables as defined here, but rather their inverses. I use the variables and their inverses interchangeably. In view of the qualitative similarity, this should not pose any problems, although it is obviously important that one be wary of the change in sign with respect to the relation with subsequent returns.

premium, serious disagreement continues to exist. Following Ball (1978 and 1992), Fama and French (1992 and 1995) have claimed that the premium is risk-related and that the accounting-based price multiples proxy for companies' loadings on a priced risk factor that is not identified in the CAPM. Specifically, Fama and French argue that the systematic risk factor concerned is related to profitability and they label it "relative distress". In contrast, several researchers have contended that the value premium cannot be accounted for by risk differences and should be considered evidence of inefficient stock markets. According to Lakonishok, Shleifer and Vishny (1994), for instance, the profitability of value-versus-growth investment strategies stems from the fact that investors naïvely extrapolate past earnings behaviour and insufficiently take into account that the growth in earnings tends to converge for value stocks and growth stocks. Bauman and Dowen (1988), La Porta (1996) and Dechow and Sloan (1997) have suggested that the anomaly is caused by investors' naïve reliance on analysts' long-term earnings growth forecasts, despite the fact that these forecasts are systematically biased. Daniel and Titman (1997), on their part, do not offer a full explanation for the value-versus-growth effect, but they do argue that the return premium on value stocks does not arise because of the co-movements of these stocks with pervasive factors. Thus, they rule out that the accounting-based price multiples proxy for non-diversifiable factor risk.

It is evident that the implications of this anomaly with respect to market efficiency are not quite clear yet. Still, the Fama and French (1992) study[23] has been so influential that virtually all recent work involving empirical tests of the EMH has somehow taken not only a size factor but also a value-versus-growth-related factor (most often the B/M ratio) into consideration when calculating abnormal returns.

Another direct accounting-based anomaly is the **leverage anomaly** described by Bhandari (1988). Bhandari showed that the debt-to-equity ratio (henceforth, D/E ratio) is positively related to expected stock returns. According to him, the observed relationship could not be fully explained by traditional risk measures. In addition, the results of a number of sensitivity tests led Bhandari to the conclusion that the leverage variable did not just have explanatory power for subsequent returns due to inappropriate risk adjustment. Thus, without providing a fully fledged explanation

as to its source, Bhandari implicitly suggested that what he discovered ought to be considered evidence of some market inefficiency.

Nevertheless, very little attention has been given to the leverage anomaly since. One potential reason is that in spite of Bhandari's contentions many academics have almost naturally regarded the D/E ratio as a risk proxy. After all, financial theory predicts that, holding operational risk constant, a higher degree of leverage implies higher equity risk. Probably more important, though, is the fact that not so long after the publication of Bhandari's article, the renowned Fama and French (1992) study was published. In this effort to reconcile some of the major cross-sectional anomalies, Fama and French have shown that the leverage anomaly disappears when a three-factor asset pricing model is used including a market risk factor, a size factor and a B/M factor. This probably explains why leverage has rarely been considered a potential separate factor in asset pricing since.

The last direct accounting-based anomaly I discuss is the **Holthausen and Larcker anomaly**. The article by Holthausen and Larcker (1992) was primarily written in reaction to earlier evidence documented in Ou and Penman (1989a)[24], who used purely statistical analysis methods to derive a summary financial statement measure (i.e. the Pr-measure) aimed at predicting the probability of the sign of one-year-ahead earnings changes. Furthermore, Ou and Penman showed that the thus obtained summary measure can be used to develop apparently profitable investment strategies. Holthausen and Larcker have presented empirical evidence that questions the robustness of Ou and Penman's results and suggests that the profitability of the latter's trading strategy may have been sample-specific.

In response, Holthausen and Larcker (1992) have developed an investment strategy of their own that is based on a research design largely identical to Ou and Penman's. Holthausen and Larcker too, start out with a large set of financial descriptors to which (stepwise) LOGIT analysis is applied in order to select the accounting-based variables with forecasting abilities. However, the two studies differ fundamentally with respect to the nature of their dependent variables:

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[23] The Fama and French (1992) study is discussed in more detail in section 3.3.2.3. on the reconciliation of the various anomalies.
[24] The Ou and Penman anomaly is discussed further in this section.

whereas Ou and Penman constructed a measure to predict the sign of one-year-ahead earnings changes, Holthausen and Larcker directly focus on the sign of future abnormal returns. Ou and Penman themselves had rejected such a direct approach, claiming that it would yield a model that is prone to simply detecting misspecification in the abnormal return measures. To a certain extent, Holthausen and Larcker acknowledge this remark and to alleviate the problem they make use of three alternative abnormal return calculations, namely market-adjusted returns, Jensen's alphas (i.e. returns in excess of the CAPM prediction) and size-adjusted returns. The investment strategy proposed by Holthausen and Larcker then consists of going long (short) in the stocks with the highest (lowest) estimated probability of earning positive abnormal returns during the subsequent period. Using data for the major American stock markets for the 1978-1988 period, they find that for each of the three abnormal return measures their strategy clearly outperforms Ou and Penman's and earns abnormal returns in a more consistent manner.

Holthausen and Larcker (1992) themselves acknowledge that they were slightly surprised to find that a purely statistical model appears to make it possible to earn abnormal returns of quite impressive magnitude (on average, between seven and ten percent for a 24-month holding period). In view of the lack of economic foundations underlying their approach, it is not surprising that Holthausen and Larcker fail to offer an explanation for their anomalous results. Since the number of follow-up studies has also been very limited, it is not clear yet whether the Holthausen and Larcker anomaly is real and if it is, what causes it.

I now turn to the **indirect accounting-based anomalies**. As I mentioned in the introduction of this section, they differ from the direct anomalies in that they are not based on a direct supposed relationship between the currently observable accounting variable(s) and subsequent abnormal stock returns, but rather on the accounting-based predictability of some event or variable that is alleged to be associated with future abnormal returns. Most of these indirect predictability studies have focused on forecasting earnings. One strand of research forms a notable exception, though, namely the **acquisition probability** literature. One of the most representative studies in this particular area is Wansley, Roenfeldt and Cooley (1983). Their approach was founded on earlier evidence (e.g. Mandelker

(1974), Franks, Broyles and Hecht (1977) and Elgers and Clark (1980)) of abnormal returns accruing to shareholders of acquired firms in the period prior to merger. Following Simkowitz and Monroe (1971) and Stevens (1973) who reported reasonable success in predicting mergers from publicly available information, Wansley, Roenfeldt and Cooley have evidenced the possibility to earn abnormal returns by investing in firms with financial profiles indicating a high potential for merger. In spite of the interesting rationale underlying the acquisition probability anomaly, it has received relatively little academic attention. The reason probably lies in the fact that some of the variables included in the merger prediction models show great resemblance to variables upon which other anomalies are based. Many academics have, therefore, considered the acquisition probability anomaly a distorted manifestation of more fundamental anomalies like the size effect and the value-to-growth effect.

The most thoroughly studied anomaly among the indirect accounting-based anomalies relating to the prediction of future earnings is the **post-earnings announcement drift** (henceforth PEAD), surely. PEAD refers to the empirical regularity that companies which announce positive (negative) unexpected earnings tend to earn higher (lower) *subsequent* returns than the market on a risk-adjusted basis. The first formal documentation of a drift in stock returns following earnings announcements is attributed to Ball and Brown (1968). It is probably quite telling and indicative of PEAD being inherent in stock price formation that evidence of this anomaly was already found in the article that is commonly viewed as the starting point of capital markets research in accounting. It is important to note, though, that Ball and Brown's evidence of PEAD pertained to *annual* earnings announcements. This particular finding has rarely if ever been corroborated since. In contrast, the PEAD pattern has proved to be very robust as far as *quarterly* earnings announcements are concerned (e.g. Jones and Litzenberger (1970), Joy, Litzenberger and McEnally (1977), Latané and Jones (1977 and 1979), Watts (1978) and Rendleman, Jones and Latané (1982)).

Some readers may find it surprising and possibly even disturbing that I classify PEAD as an *indirect* accounting-based anomaly. Admittedly, the term was initially conceived to refer to a *direct* relationship between unexpected earnings and subsequent abnormal returns. Early efforts to rationalize PEAD (e.g. Ball (1978)

and Foster, Olsen and Shevlin (1984)) mainly focused on possible specification errors and problems in estimating (abnormal) returns, but failed to satisfactorily explain the anomalous pattern. However, during the past 15 years or so, our understanding of the causes of PEAD has improved considerably, changing the perceived nature of the anomaly in the process. The articles by Rendleman, Jones and Latané (1987), Bernard and Thomas (1989 and 1990) and Freeman and Tse (1989) have been seminal in this respect. They have suggested the so-called "naïve expectations hypothesis" as the main cause underlying PEAD. This hypothesis claims that investors, being unaware that firms' seasonally-differenced quarterly earnings are serially correlated, mistakenly use a seasonal random walk model in setting their expectations with respect to future quarterly earnings. As a result, investors supposedly make or imply inferior forecasts of future earnings changes, making it possible that the market reaction to subsequent earnings announcements be partly predicted. Subsequent research has further refined our understanding of the descriptive validity of the naïve expectations hypothesis. In conjunction, Ball and Bartov (1996) and Soffer and Lys (1999) have shown, for instance, that immediately after a quarterly earnings announcement none of its implications for future earnings is reflected in stock prices. As the quarter progresses, this is partly remedied and by the time of the next earnings announcement about half of the information in the prior announcement is incorporated into earnings expectations. It has also been shown that the lack of understanding with respect to the time-series properties of quarterly earnings is not homogeneous across firms. Specifically, it is more pronounced for smaller companies (e.g. Bernard and Thomas (1989 and 1990)), stocks involving relatively larger transaction costs (e.g. Bhushan (1994)) and firms for which only a small proportion of the shares is held by institutional investors (e.g. Bartov, Radhakrishnan and Krinsky (2000)). In spite of these extensions and refinements, the essential claim of the naïve expectations hypothesis has basically been confirmed in all instances: investors do not fully use past quarterly earnings information when setting their expectations regarding future quarterly earnings and that is what causes PEAD. In fact, the evidence consistent with the predictions of the naïve expectations hypothesis has been so overwhelming that even the most fervent proponents of the EMH (e.g. Fama (1998)) have found it hard to reject the hypothesis as the most likely explanation for PEAD. That is why I think it is fair to say that our increased understanding of PEAD has

turned it from a direct into an indirect accounting-based anomaly that uses future earnings as the intermediate variable(s).

It is important that the impact of the strand of PEAD research be emphasized. In my opinion, it is extremely damaging to the concept of market efficiency. After all, of all cross-sectional anomalies, PEAD has proved to be one of the most robust ones. In addition, it is probably safe to state that PEAD is the anomaly that is currently best understood by academics. It is therefore absolutely daunting to the case of EMH proponents that this particular regularity apparently stems from persistent irrational investor behaviour.

The **Ou and Penman anomaly** is another indirect accounting-based anomaly. As mentioned earlier, there are quite a lot of similarities between the investment strategy developed in Ou and Penman (1989a) and the Holthausen and Larcker anomaly. Both trading strategies are founded on summary financial statement measures calculated from LOGIT models. The latter are thereby estimated to select on purely statistical grounds the accounting variables with forecasting abilities. Contrary to the major similarities between Ou and Penman's work on the one hand and Holthausen and Larcker's (1992) on the other, the two studies differ fundamentally with respect to the nature of the future event of which the probability is estimated: whereas Holthausen and Larcker focus directly on the sign of abnormal returns, Ou and Penman concentrate on the direction of earnings changes. Concretely, they construct zero investment portfolios consisting of long positions in companies with high estimated probabilities of experiencing positive one-year-ahead earnings changes and short positions in firms with low estimated probabilities. Ou and Penman report that, after controlling for risk, their zero investment portfolios earn mean 24-month buy-and-hold returns of about seven percent. In a follow-up study, Ou and Penman (1989b) have claimed that their summary financial statement measure actually distinguishes transitory components in annual earnings. In combination, these findings have led Ou and Penman to the conclusion that investors recognize the future earnings-related information that is present in financial statements with a lag and hence that stock markets are not efficient.

However, subsequent research has cast serious doubts on whether the Ou and Penman anomaly is real. As I have already mentioned earlier, Holthausen and Larcker (1992) have produced evidence suggesting that the abnormal returns accruing to Ou and Penman's (1989a) trading strategy were sample-specific. In addition, Stober (1992) has contended that the summary financial statement measure derived by Ou and Penman should probably be regarded as a proxy for an omitted risk factor, rather than an indicator of over- or undervaluation. He reaches this conclusion on the basis of the fact that Ou and Penman's hedge portfolios continue to earn apparently abnormal returns for several years into the future, making it unlikely that their summary measure captures mispricing that is allegedly rooted in investors' failure to use all available information when predicting *one-year-ahead* earnings. In a similar vein, Greig (1992) has demonstrated that the anomalous profitability of Ou and Penman's strategy disappears when the size effect is appropriately controlled for. As a whole, the aforementioned replications of Ou and Penman's approach have clearly produced results constituting major challenges to the findings of the original study. Therefore, it is probably fair to say that Ou and Penman's "market inefficiency" conclusion may have been somewhat premature.

Probably more troublesome for the case of market efficiency is the **accrual-based anomaly**. Building on the arguments brought forward by some financial analysts that investors insufficiently take the differential implications of the components of earnings into account, Sloan (1996) has formally investigated this regularity. Specifically, Sloan has examined whether stock prices fully reflect the information about future earnings that is contained in the accrual and cash flow components of current earnings. Apparently, investors tend to overestimate the time series persistence of the accrual component of earnings and to underestimate that of the cash flow component. Moreover, the misweightings seem to be also economically significant, as evidenced by the large mean returns (i.e. about ten percent for a twelve-month holding period) earned by the hedge portfolios constructed by Sloan. In drawing conclusions from his findings, Sloan remains very cautious. As such, he emphasizes that his results do not necessarily imply investor irrationality, because the investment strategy examined in his research design may be unfeasible in practice.

Personally, though, I feel that the accrual-based anomaly constitutes a major challenge for the EMH. The abnormal returns associated with it are too large to be rationalized by transaction costs and the fact that the anomaly is not limited to small stocks makes it unlikely that the exploitability of Sloan's (1996) investment strategy is severely constrained by price pressure effects. In addition, Xie (2001) has recently refined Sloan's findings by demonstrating that investors particularly tend to overprice *discretionary* accruals (i.e. accruals induced by earnings management). In my opinion, this has considerably increased the likelihood that the strong and consequent profitability of accrual-based trading strategies stems from investors' incapability to "see through" the accounting process underlying the reported figures.

The last indirect accounting-based anomaly I discuss is the **Abarbanell and Bushee anomaly**. Abarbanell and Bushee (1998) have examined whether the application of true fundamental analysis can yield significant abnormal returns. Their approach is similar to Ou and Penman's (1989a) in that their trading strategy is also based on multivariate accounting-based prediction of one-year-ahead earnings. However, unlike Ou and Penman, Abarbanell and Bushee's selection of the variables upon which their fundamental strategy is based, is directly motivated by economic arguments. Abarbanell and Bushee find that their portfolios earn an impressive average twelve-month abnormal return of about 13%.

Hardly any follow-up studies have investigated the robustness of Abarbanell and Bushee's (1998) findings. Nevertheless, I feel that also this anomaly should be considered a meaningful indication that stock markets may not fully use the information embedded in financial statements. After all, Abarbanell and Bushee themselves conduct an overwhelming number of sensitivity checks and supplemental tests, which basically all suggest that the abnormal returns earned by their investment strategy stem from mispricing rather than inadequate adjustment for risk.

3.3.2.3. Efforts to reconcile the various cross-sectional anomalies

The number of cross-sectional anomalies that I have discussed in the preceding sections is impressive. If the four variables that have been used to distinguish

value stocks from growth stocks are counted separately, no fewer than 15 cross-sectional anomalies have been reviewed. This number is all the more impressive considering that, basically, real anomalies (i.e. not sample-specific and not brought on by market microstructure effects or market frictions) can only be caused by either the fact that priced risk factors are left unidentified in the asset pricing models used or the fact that investors over- or underreact to certain items of information. Admittedly, there may be several such risk factors and investors may react inappropriately to a number of information elements, but one can hardly deny that it is very likely that at least some of the anomalies proxy for similar underlying phenomena.

The earliest reconciliation efforts mainly focused on the interaction between the size effect and the value-versus-growth effect. They were induced by the observation that both effects make use of the variable "price per share" and by the findings that the two effects exhibit within-year seasonalities and common long-term variation through time. Those early reconciliation studies produced contradictory results. For example, Reinganum (1981) found that the size effect subsumes the E/P effect, while Basu (1983) argued just the opposite. Cook and Rozeff (1984) concluded that no one effect dominates the other.

As the number of cross-sectional anomalies grew over time, researchers began to widen the scope of their reconciliation efforts. By far the most renowned and most influential article in this area has been Fama and French (1992). Their extensive study demonstrated that size and the B/M ratio combine to capture the cross-sectional variation in average stock returns, thus leaving no role for market beta, leverage and the E/P ratio. Fama and French (1993) have further refined this result, showing that the size and B/M factors can explain the differences in average returns across stocks, while the market factor is needed to explain why stock returns are on average above the risk-free rate. In spite of its somewhat ad hoc nature, the three-factor asset pricing model developed by Fama and French has more or less become the standard tool to calculate benchmark expected returns in empirical tests of the EMH. However, the three-factor model has certainly not provided all the answers. For example, Fama and French (1996) have shown that although the model is apparently quite successful in explaining cross-sectional anomalies, it fails to subsume the short-term underreaction effect. In addition,

Smith Raedy (2000) has produced evidence that casts serious doubts on the robustness of Fama and French's findings. She has contended, for instance, that the three-factor model cannot explain the CF/P anomaly and the overreaction effect. Finally, since the development of the three-factor model some new anomalies that I find particularly interesting (e.g. the accrual-based anomaly and the Abarbanell and Bushee anomaly) have been revealed and it has not been established yet how the model deals with those recently discovered regularities. Overall, it is therefore probably fair to say that the efforts that have been made to reconcile the many cross-sectional anomalies have come up with results that are anything but conclusive. The way in which the various challenges to the EMH are related to one another is still not well understood.

4. CONCLUDING REMARKS

Where do we stand when it comes to assessing whether stock markets are efficient? The way in which this question is answered is possibly largely dependent on the nature of whoever is answering. An optimist would probably emphasize how we have learned that markets are not strong-form efficient, how it has been convincingly demonstrated that expected returns tend to vary through time and how it has been shown that the CAPM (or at least the way in which it has been given empirical content) does not provide an adequate description of equilibrium stock pricing. A pessimist's attention, on the other hand, is likely to be drawn by the methodological problems that certain lines of research (e.g. return autocorrelation tests and volatility tests) have encountered, the omnipresence of the joint-hypothesis issue and the impressive number of seasonalities and cross-sectional anomalies for which no adequate explanation has been formulated yet.

My personal opinion lies somewhere between these two extremes. I do feel that considerable progress has been made since the theoretical conception of the EMH. Indeed, the access of company insiders to value-relevant information before it is reflected in stock prices, for example, has become a stylised fact. Academics have also generally agreed upon the lack of real-world descriptiveness of the models (i.e. the constant expected returns model and the CAPM) that were commonly maintained in early empirical tests of the EMH. As such, I agree with Fama (1991) when he argues that the combination of the models concerned and the EMH have

basically lived up to expectations in that they have enhanced our understanding of stock market behaviour by providing a benchmark (joint) hypothesis against which reality could be compared. However, one can hardly deny that although our understanding of stock price formation has been enhanced, it has remained fairly limited. It is important to note that the truly well-established facts virtually only consist of *rejections* of pricing models: expected returns are apparently *not* constant through time, the CAPM does *not* satisfyingly explain the cross-section of average stock returns, etc. In contrast, when it comes to describing then how stock markets *do* work, a lot of disagreement continues to exist among academics. A good example are the many cross-sectional anomalies for which the actual sources are yet to be identified. Do they stem from flaws in the way in which expected returns models are given empirical content? Do they proxy for priced risk factors and if so, which are those factors? Are they evidence of market inefficiency and if so, what causes those inefficiencies? Or is a combination of the above potential reasons responsible for the anomalies? To be honest, in many cases, we do not have a clue! Then again, the stock price formation process is extremely complex and, in addition, the joint-hypothesis problem has made nearly all empirical test results most ambiguous.

Overall, though, I feel that especially in recent years researchers have begun to adopt approaches that have helped to overcome at least partially the main pitfalls present in earlier studies. One example is the link that has been established between the variation in expected returns and changes in the economic environment. Another example are the cross-sectional anomalies that have linked abnormal returns to predictable earnings forecasting errors. On the whole, I am therefore inclined to summarize our understanding of the descriptive validity of the EMH as follows:

- markets are not efficient with respect to insider information;
- most investment professionals probably do not have access to a significant amount of private information;
- expected returns vary through time and the variation is likely to be rational. As such, pricing of the stock market as a whole is generally consistent with the predictions of the EMH;

- although it is highly unlikely that all of the cross-sectional anomalies are real (let alone constitute rejections of the EMH), there may be important cross-sectional market inefficiencies that are possibly rooted in investors' failure to fully and rationally use the publicly available information when forecasting firms' future performance.

It is obvious, though, that still an awful lot of work remains to be done with a view to improving our understanding of the behaviour of stock markets. For example, as far as the rather well-established inefficiency with respect to insider information is concerned, considerable additional research is required to gain an insight into the specific sources of insider information and the strategies used by insiders to exploit their information advantages. In my opinion, academics should also try to link those studies with the line of research that aims to assess whether investment professionals have access to private information or not. Given that the latter stream of literature has constantly been struggling with the virtually insurmountable joint-hypothesis problem, I feel it is perhaps time for researchers active in the field of performance evaluation to change their tack. In view of the fact that any *real* differences in performance (either with respect to prediction accuracy or with respect to investments) are likely to stem from different degrees of access to non-public information, examinations of the sources of insider information and of insider strategies may also be helpful to explain the variation in performance of company *outsiders*.

With respect to the research on the variation of expected returns through time, it is logical to further explore the path that has been taken, i.e. to further investigate the macro-economic factors and conditions underlying the observed variation. Substituting more theory-based modelling efforts for the currently popular ad hoc approaches would be extremely laudable in this respect.

Finally, there is the research on the cross-sectional predictability of stock returns. This is probably the subset of empirical EMH tests that is still most in crisis due to the joint-hypothesis issue. Nevertheless, I feel that important progress has been made in recent years. In my opinion, it is not a coincidence that our understanding of the cross-sectional descriptive validity of the EMH has particularly been enhanced by what I have labelled indirect tests. Studies that examine the relation

between currently observable variables and subsequent abnormal returns through the use of intermediary variables are in my view considerably less prone to the joint-hypothesis problem. After all, abnormal returns are ultimately attributable to either (1) changes in the expectations regarding future positive value drivers (e.g. cash flows or earnings), or (2) unexpected changes in the riskiness of the equity, or (3) ex ante misestimation of equity risk. The joint-hypothesis issue thereby takes root, of course, in the latter possibility. However, the likelihood that this third possible cause can fully account for any "abnormal" returns accruing to a certain trading strategy is in my opinion seriously mitigated if one can show that those abnormal returns are systematically associated with biases in market expectations of future positive value drivers or in market perceptions of risk. The methodologies used in recent studies on post-earnings announcement drift, the accrual-based anomaly and the Holthausen and Larcker anomaly should serve as examples in this respect.

Clearly, the aforementioned suggestions for future research are somewhat subjective, whereas I contended in the introduction to this paper to take an as objective position as possible with this literature review. Therefore, it is perhaps a good idea to end on a more neutral note, that is with a conclusion that everyone can agree upon. Unfortunately, the ongoing debate about the EMH's descriptive validity makes that such a conclusion is necessarily extremely general: there is still an awful lot to be learned about the real-world stock price formation process and so empirically-oriented researchers taking an interest in the concept of market efficiency need not immediately shift their focus to keep busy.

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