Is the Stock Market Rational?
A Money Manager’s Guide to 25 Years of Efficient Markets Research

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I. INTRODUCTION

What are the determinants of share prices? Do correct forecasts of company profits lead to investment success? Do traders pay too much or too little attention to market psychology? What returns may be expected? What are the risks involved? These and related questions have been asked ever since the emergence of capital markets and stock exchanges. After decades of academic research, some puzzles have been resolved but many have not. Until recently the general consensus was that, at all times, markets are rational and that stocks are priced efficiently, i.e., with proper regard for economic fundamentals and the available information. Furthermore, because traders compete, new information was thought to be reflected in prices very quickly. “Beating the market” again and again was considered a near impossibility except perhaps with the help of inside information or large doses of luck. It was further accepted that the Sharpe-Lintner capital asset pricing model (C.A.P.M.) - with its emphasis on the role of the market portfolio - captured most, if not all, of the variation in expected returns. All of the above argued against active portfolio management since a buy-and-hold strategy applied to all listed firms (“indexing”) or to a random selection of stocks

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market conditions - low transaction costs, competitive "price-taking" behavior, etc. - assumed by finance theory. However, most empirical regularities are confirmed in other countries as well.

The paper is organized as follows. Section II reviews the early pre-1980 research on stock valuation. Section III lists the anomalies. Section IV provides an interpretation and suggests future directions in the development of asset pricing theory. Section V concludes with a discussion of the central issue, i.e., the practical consequences for investment strategy and risk management in the 1990s. As it turns out, we believe that, while much has been learned during the last 25 years, the basic contribution of efficient market theory remains correct. Conceptually, it is difficult to tell whether the apparent "excess" profits are earned through careful security analysis, "excess" risk-bearing, or astute investing. But no one will deny that the profits, if they exist, require considerable sophistication and much work. Therefore, they are not free. We conclude that, for the foreseeable future, easy "wealth without risk" - like alchemy - remains an illusion.  

II. THE EARLY RESEARCH

The two main building blocks of virtually all empirical research in stock valuation until the late 1970s were stock market efficiency and the C.A.P.M. Market efficiency means that security prices at all times fully reflect the available information. However, this definition is so general that it has no easily testable implications. In order to decide whether (on average) actual prices differ from "correct" prices, one needs a theory of value and price formation. It is obviously difficult to develop such a theory. The C.A.P.M. addresses an "easier" question. It is a theory of the equilibrium risk-return tradeoff. It asks how stocks are priced relative to one another. Assuming, among other things, utility maximization, risk aversion, normal return distributions, homogeneous investor expectations, and liquid markets with zero transaction costs, the C.A.P.M. states that the expected rate of return on stock $j (\mu = E(R_j))$ satisfies the following relationship: $E(R_j) = \mu_j + \beta_j [E(R_m) - \mu_j]$. In this equation, $\mu_j$ represents the risk-free borrowing and lending rate, $E(R_m)$ is the expected rate of return on a market-wide index of all traded assets, and $\beta_j$ is the beta-coefficient.

The C.A.P.M. captures the intuition that, in a world where everyone has the same information, it is sound policy to diversify one's
holdings. In fact, in equilibrium, only the risk of general market movements is remunerated. On the other hand, unique risk is diversifiable and is not compensated. The expected market risk premium is \( \text{E}(R_m) - R_f \). It is the average risk premium earned by individual stocks. For each security, beta measures the non-diversifiable risk that a particular asset adds to the market portfolio as a whole. It also measures the asset's return sensitivity to market-wide movements.

Since transactions involve out-of-pocket costs, the optimal trading strategy does not only involve diversification but also a minimization of the number of transactions. The main difficulty to using the C.A.P.M. in practice is that expected returns are not observable; hence the return \( \text{E}(R_i) \) has to be estimated. This is done by rewriting the C.A.P.M. into the so-called market model format:

\[
R_{jt} = \alpha_j + \beta_j \times R_{mt} + e_{jt}
\]

with: \( R_{jt}, R_{mt} \) = realized return of security \( j \), respectively the market return over period \( t \);

\[
\alpha_j = R_j \times (1 - \beta_j);
\]

\[
e_{jt} = \text{an error term reflecting the deviation of realized return from expected return},
\]

i.e. \( e_{jt} = \text{E}(R_j) - R_{jt}; \) on average \( e_{jt} \) is zero; \( \alpha_j, \beta_j \), and \( e_{jt} \) in preceding equation can be estimated by regression.

Given a realized rate of return on the market over some period \( t \) (i.e. \( R_{mt} \)), the expected return on stock \( j \) is estimated to be equal to \( \alpha_j + \beta_j \times R_{mt} \). The efficiency tests of earlier work involve a comparison of realized rates of return generated by active investment strategies with the rates of return estimated from the market model. Thus, the market model estimates function as a benchmark \( 4 \). When significant excess returns are observed, these returns are not necessarily anomalous. It may simply be that the market model estimates are not the correct benchmark. In fact, assuming efficiency, systematic deviations from the market model's predicted return do not even imply that the C.A.P.M. is wrong. The market model, for instance, presumes that risk premia remain constant over time, or at least for the time period under investigation. Unfortunately time variation in \( \text{E}(R_m) - R_f \) and/or \( \beta_j \) (not inconsistent with the C.A.P.M.) may ruin the empirical estimates from the market model. All studies of market efficiency suffer at least to some extent from this joint hypothesis problem.

Besides the joint hypothesis problem, a second issue in tests of market efficiency is the distinction between costly and costless information. Costly private information is possessed by insiders (e.g., corporate executives) or produced by specialized intermediaries (e.g., security analysts or investment bankers). From the perspective of market rationality, it is a less troubling if insiders can "beat the market" than if price changes are predictable on the basis of publicly available - say, historical - information. That insiders and some financial analysts earn excess profits perhaps justifies their survival. After all, it is important to ask: Who pays for financial research and how can market prices ever be rational without it?

On the other hand, if market prices show simple time-series patterns, or if the market ignores and/or systematically misinterprets wide-available public news stories, the verdict must be less friendly. Thus, we make a distinction between classes of information and levels of efficiency. Fama (1970) defines three forms of efficiency: weak-form, semi-strong form, and strong form.

A. Weak form efficiency

Weak form efficiency tests check whether one can improve upon a passive buy-and-hold investment strategy by actively trading on the basis of past stock price changes. Technical analysts and chartists claim that market prices "have a memory", i.e. future movements are to some extent predictable from past movements. Early tests (that adjust for CAPM beta-risk) suggest that, at best, chartist methods do only slightly better than buy-and-hold portfolios\( 5 \). But active trading is costly in terms of brokerage commissions, spent time and effort, etc. Controlling for these costs, no "economic" profits are to be made. In his review of the evidence, Fama concluded that he would "advise any reader who is interested only in practical results, and who is not a floor trader and so must pay commissions, to turn to other sources on how to beat buy and hold" (Fama(1976), p.395).
B. Semi-strong form efficiency

Like weak-form tests, semi-strong form tests also examine the speed and accuracy with which information gets reflected in prices. But here the main focus of the tests is on the impact of the public release of firm specific information. This research asks how the stock market reacts to the public announcement of accounting earnings, dividends, stock splits, changes in accounting policies, changes in the financing of the corporations, takeover attempts, and other news items relevant to corporate finance. The tests rely on "event study methodology". This involves calculating a time-series of the realized stock returns for the period before, on, and after a particular announcement. This series is compared with a corresponding series of expected returns, as predicted by the market model, i.e., $\alpha + \beta R_{mv}$. Past studies overwhelmingly show that on the announcement date the stock price immediately jumps up or down, depending on whether the information is "good" news or "bad" news for shareholders' wealth. Hence stock prices react quickly.

It is more difficult to judge whether the magnitude of the price reaction is "correct". However, there is only limited evidence of immediate systematic reversals or runs in price changes after news announcements. This aspect of the data argues against persistent over- or underreaction.

It is interesting to note that for most studies the joint hypothesis problem discussed above is relatively unimportant. In particular, event studies usually are concerned with daily returns. In many instances the one day price jump at announcement is so large relative to realized returns on other days that the specification of the model that rules risk premia has no effect on the interpretation of the empirical results.

C. Strong-form efficiency

This branch of the efficiency literature investigates whether access to insider private information offers opportunities to earn excess returns. The general result is that profits can be made but perhaps only by corporate executives and board members (see, e.g., Seyhun (1986)). Professional investment managers and brokers' recommendations generally do not offer excess returns. Of course, the inability to beat a simple buy and hold strategy is bad news for money management companies.

III. EMERGING CRITICISMS: THE ANOMALY LITERATURE OF THE 1980S

As discussed in Section II, the early work largely supported the notion of efficient markets and the correctness of the market model as a description of the real world risk-return tradeoff. Since the late 1970s continued research has discovered an increasing number of anomalies. In fact, during the 1980s, so many stock market anomalies were discovered that, in retrospect, it is a substantial task just to list them. Table 1 provides an overview with selected references to the literature. We make a distinction between pricing anomalies and broader puzzles of investor rationality. As the anomalies are consistent with the following two hypotheses: either (1) the market is efficient but it is influenced by rational time-varying risk premia and/or return effects related to institutional factors or (2) the market reacts in an "irrational" way. Presently the academic profession is only at the beginning of a debate about the true role of investor psychology and the quality of judgment.

Research on time-varying expected returns has replaced the classical weak-form efficiency tests. The early work has been broadened to include the forecast power of variables such as dividend yield, earnings-price ratios, and term structure variables. Furthermore, instead of studying short-run returns only, the new research includes predictability tests for returns over a long horizon.

In general, the anomaly research with short-term returns is based on much larger data sets than the early studies. Nevertheless, while the observed return patterns definitely deserve our attention, the findings may be hampered by measurement error. For example, Lo and MacKinlay (1988) and Conrad and Kaul (1990) find that weekly returns on portfolios of small stocks show statistically significant and economically important positive auto-correlations, i.e. positive (negative) returns tend to be followed by more positive (negative) returns. The results are at least partly due to "nonsynchronous trading."
### TABLE 1
An overview of stock market anomalies

<table>
<thead>
<tr>
<th>Category</th>
<th>Selected References</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>A. ASSET PRICING ANOMALIES</strong></td>
<td></td>
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<tr>
<td>Seasonalities in returns</td>
<td></td>
</tr>
<tr>
<td>January effect</td>
<td>Keim (1983)</td>
</tr>
<tr>
<td>Monday/Friday effect</td>
<td>French (1980)</td>
</tr>
<tr>
<td>Intra-day price patterns</td>
<td>Harris (1986)</td>
</tr>
<tr>
<td>Cross-sectional predictors of returns (other than past returns)</td>
<td></td>
</tr>
<tr>
<td>Small firm (size) effect</td>
<td>Banz (1981); Schwert (1983)</td>
</tr>
<tr>
<td>Price-earnings ratio effect</td>
<td>Basu (1977); Reinganum (1981); Jaffe et al. (1989)</td>
</tr>
<tr>
<td>Dividend yield</td>
<td>Shiller (1981); Fama and French (1988)</td>
</tr>
<tr>
<td>Market value/Book value</td>
<td>De Bondt and Thaler (1987); Fama and French (1991)</td>
</tr>
<tr>
<td>Ratio effect</td>
<td></td>
</tr>
<tr>
<td>Past returns as predictors of future returns</td>
<td></td>
</tr>
<tr>
<td>Short-term price reversals</td>
<td>Lehmann (1990); Lo and MacKinlay (1990);</td>
</tr>
<tr>
<td></td>
<td>Jegadeesh (1990); Jegadeesh and Titman (1992); De Bondt and Thaler (1989)</td>
</tr>
<tr>
<td>Nonlinear price dependencies</td>
<td>Brock et al. (1992);</td>
</tr>
<tr>
<td>Technical analysis</td>
<td></td>
</tr>
<tr>
<td>&quot;Underreaction&quot; to earnings announcements</td>
<td>Latane and Jones (1980); Bernard and Thomas (1990);</td>
</tr>
<tr>
<td>&quot;Excess&quot; return volatility</td>
<td>Shiller (1981); Le Roy and Porter (1981);</td>
</tr>
<tr>
<td></td>
<td>West (1988)</td>
</tr>
<tr>
<td>Price pressure effects</td>
<td>Shleifer (1986); Harris and Gurel (1988)</td>
</tr>
<tr>
<td><strong>B. OTHER PUZZLES OF INVESTOR RATIONALITY</strong></td>
<td></td>
</tr>
<tr>
<td>Volume of trading</td>
<td>Harris and Raviv (1992)</td>
</tr>
<tr>
<td>Active portfolio management</td>
<td>Jensen (1968)</td>
</tr>
<tr>
<td>Investor enthusiasm for initial public offerings, closed-end funds, commodity funds, etc</td>
<td>Ritter (1991); Lee et al. (1990)</td>
</tr>
</tbody>
</table>

Nonsynchronous trading may produce the appearance of positive autocorrelation in returns, even if the true autocorrelations are zero. So far the effects of this measurement problem have not been disentangled from possible real autocorrelation. French and Roll (1986) study daily returns and find small but positive autocorrelations for the portfolios of larger firms and small negative autocorrelations for smaller firms. One explanation consistent with the negative autocorrelation is that, for small companies, uninformed traders influence prices and at times drive them up too high or push them down too much. Informed traders recognize this and jump into the market, thereby forcing the price back to its fundamentals. Hence price increases tend to be followed by price decreases and vice versa. One consequence of this uninformed trader or "noise trader" hypothesis is that the variation of daily returns is larger than the variation that may be expected from a rational price process. French and Roll conclude that the variability of short-term returns is indeed too large.

However, what remains unclear at this point is the degree to which this effect is driven by institutional features of the New York market, e.g., the bid-ask spread. Related to this research are other papers that document day-of-the-week effects (i.e. on specific days of the week returns are higher than on other days), week-of-the-month effects, holiday effects, intraday effects etc. (Harris (1986)). Again, some of these patterns may be caused by bid-ask spread bias or other institutional factors.

Probably the most controversial efficiency tests at this moment have to do with the predictability of long-term returns on the basis of past returns. The data systematically indicate strong negative autocorrelation between realized returns on the market index over successive time periods of 2 to 8 years (e.g., see Poterba and Summers (1988), or Fama and French (1988)). But there is a controversy about the statistical reliability of the findings. Even with data reaching back to the 1870s, the long-term return tests are based on small sample sizes and hence suffer from low statistical power. In addition, Fama and French (1988) report that the negative autocorrelation is concentrated in the period 1926-1940.

However these latter findings about the movement of market indexes certainly do not settle the matter. For, in two articles that use data for individual companies, De Bondt and Thaler (1985) (1987) find persistent negative autocorrelation in long run returns. This test
design suffers much less from a statistical power problem. In particular, for each year since the early 1990s, De Bondt and Thaler rank all major companies listed on the NYSE based on their performance over the previous 2 to 5 years. In subsequent years, prior “losers” earn about 8% per year more than prior “winners”. The results of this contrarian investment strategy are consistent with stock market overreaction. One interpretation of De Bondt and Thaler is that, at times, investors naively extrapolate past earnings growth into the future. This excessive optimism or pessimism (and its magnifying effect on prices) only gets reversed as reality contradicts expectations. Many researchers disagree with this behavioral interpretation. They argue that the tests cannot distinguish psychological overreaction from market efficiency combined with rational time-varying risk premia.

Next to research of return predictability of returns on the basis of past returns, there is presently also a vast body of findings concerning return predictability on the basis of other variables. For instance, Fama and French (1988) find that past dividend yields have low predictive power for short-term returns; however, for long-term returns, the predictive ability becomes significant. Similarly, Campbell and Shiller (1988) show that past earnings/price ratios have forecast power for long-term returns.

As so much of academic understanding of stock pricing turns around distinguishing rational time-varying risk premia from irrational bubbles, it is not surprising to see the emergence of many papers trying to find distinctive evidence favouring one of the two hypotheses. So far there is only circumstantial evidence. Fama and French (1989) suggest that if time varying risk premia (maybe related to changing macroeconomic conditions) are at the heart of the empirical regularities, then it is likely that these same forces are also at work in other asset markets, say, the government and industrial bond markets and non-U.S. equity markets. As it happens, Fama and French (1989), Keim and Stambaugh (1986), Harvey (1991) and others indeed find that this is the case. Note that these results, which consistent with rationality, do not rule out speculative bubbles. Bubble fans would suggest that investor sentiment is correlated across assets and markets.

A closely related literature is the one on “excess” volatility. Shiller (1981) pioneered “variance-bounds tests” by examining whether the variability in stock prices is in line with the variation in “intrinsic values”. He studies annual observations since 1889 of the inflation-adjusted value of the Standard & Poor’s Index (including dividends). Intrinsic value (p*) is measured by a dividend discount model, ex post. In efficient markets, the level of the market index (p) may be seen as a rational forecast of p*. It can be shown that under those circumstances the observed volatility in p should be less than the volatility in p*. Shiller finds that this “variance bound” on p* is dramatically violated: the variability in p is too large relative to subsequent movements in dividends.

However, this conclusion assumes that p* is a correct estimate of intrinsic value. In addition, the volatility in stock prices may be explained by changes in discount factors. Shiller points out that the risk premium movements that are required to keep p equal to p* seem implausibly large. Hence he concludes that fashion is the most likely explanation. But again, not all researchers agree. Some refer to estimation errors in p*. Others propose that the forces driving dividend patterns may be responsible. For example, Fatemi and Rad (1992) examine the relationship between stock return variation and expected dividends for six European countries. They find that in the U.K. market, more than half of the observed variability of quarterly returns is explained by variations in expected dividends. In contrast, in continental European markets, variation in expected dividends explains only a small part of observed return variability (e.g. in Germany only 18%). Fatemi and Rad argue that the concentrated ownership structure of continental European firm and the typical tailor-made dividend patterns force dividends to be nearly void of informational content. In other words, variation in expected dividends and return volatility need not be strongly related.

Another challenging set of results has to do with the stock price adjustment to quarterly earnings announcements. Bernard and Thomas (1990) find a positive correlation between this quarter’s stock price reactions and earnings announcements up to three quarters back. This suggests that the news content in what is arguably the most important of all announcements, takes three quarters or more to be fully reflected in prices. This is especially true for small firms with extreme earnings changes. On the other hand, measured returns on small stock portfolios may be quite sensitive to the exact methods that are used to form the portfolios.

Probably the best documented anomaly concerns the small firm or “size” effect and the January effect. Extensive empirical evidence indicates that on average, over past decades, stocks of small firms
have realized a rate of return beyond that predicted by the C.A.P.M.; simultaneously large firms have realised too small a return. The surplus returns earned by small firms are concentrated in January (e.g. see Keim (1983)). Several possible explanations have been put forward.

Within the efficient markets approach the apparent superior performance of small company stocks may simply mean that conventional risk-adjusted-performance measures are inadequate. For instance, they do not reflect the liquidity risk of those stocks. Taxes have been linked to the January effect. In some tax systems (e.g., the U.S.) investors can reduce their tax bill by selling losing stocks before the end of the calendar year. This may cause selling pressure at year-end and positive abnormal returns at the beginning of the new tax year as the prices of affected stocks rebound. But this immediately raises the question: why do these pressure effects survive the profit-motive and the efforts of rational arbitrageurs?

An anomaly that has attracted widespread interest from the investment community concerns mutual funds that have outperformed the market over many years. One extraordinary example is the Magellan fund that beat the Standard and Poors (New York) market index in eleven of thirteen years ending in 1989; more importantly, its average yearly return of 28% was quite far above the 17.5% yearly return of the Standard and Poors index. However two caveats should be raised.

The first concerns a statistical selection problem. The Magellan fund receives attention exactly because it is the best from a population of thousands of funds. The following table, based on a simulated coin flipping test illustrates what it means to select the winner from populations as small as 50 funds. There proves to be a chance of 9.2% that the best portfolio outperforms the market in 9 out of 13 years. Furthermore, considering only the performance of the winner in each simulation run, the winner has a chance of 43.3% that he outperforms the market in 11, 12 or 13 out of 13 years.

What makes it unlikely, however, that the Magellan fund beat the market by mere chance is the size of outperformance (28% - 17.5%): the tests in Marcus (1990) indicate that this is far beyond the bounds of luck.

### Table 2

**Probability Distribution of Number of Successful Years out of Thirteen for the Best-Performing Money Manager in a coin-flipping based contest**

<table>
<thead>
<tr>
<th>Winning years</th>
<th>50</th>
<th>100</th>
<th>250</th>
<th>500</th>
</tr>
</thead>
<tbody>
<tr>
<td>8</td>
<td>0.1%</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>9</td>
<td>9.2</td>
<td>0.9</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>10</td>
<td>47.4</td>
<td>31.9</td>
<td>5.7</td>
<td>0.2</td>
</tr>
<tr>
<td>11</td>
<td>34.8</td>
<td>51.3</td>
<td>59.7</td>
<td>42.3</td>
</tr>
<tr>
<td>12</td>
<td>7.7</td>
<td>43.3</td>
<td>14.6</td>
<td>31.8</td>
</tr>
<tr>
<td>13</td>
<td>0.8</td>
<td>1.2</td>
<td>2.8</td>
<td>5.9</td>
</tr>
<tr>
<td>Mean winning years of best performer</td>
<td>10.43</td>
<td>10.83</td>
<td>11.32</td>
<td>11.63</td>
</tr>
</tbody>
</table>

Subsequent to this statistical observation, a further refinement in fund performance evaluation is offered by a decomposition technique based on CAPM and already proposed by Fama in 1972. Essentially overall performance is decomposed into two components: security selection or net selectivity and diversification.

Since for a perfectly or completely diversified portfolio

$$\sigma_p = \beta_p \cdot \sigma_m$$

it follows that if $\beta_p \neq \sigma_p/\sigma_m$ the fund portfolio contains some diversifiable risk as measured by $(\sigma_p/\sigma_m)$ requiring an additional risk premium of $E(R_p) - E(R_m)$. The amount $R_p - E(R_p)$ is defined as the net selectivity. If net selectivity is positive, then the investor succeeded in picking stocks that outperformed the market even after compensating for the additional risk resulting from imperfect diversification. Notwithstanding this refinement Fidelity's Magellan Fund outperformed the market in the 1980's.

Finally, there is the phenomenon of the closed-end mutual fund pricing and the long run performance of initial public offerings (IPO's). The latter is discussed in detail in the IPO-paper in this issue. Lee, Shleifer and Thaler (1991), Weiss (1989), Peavy (1988) and others observe that new closed-end mutual funds tend to get started when the existing funds are selling at premia or small discounts. In fact when they first start to trade, the funds usually trade at a small premium. However, after a few months of trading these
funds typically sell at a substantial discount. Hence the question: why does anyone buy these funds when they are first issued?

Several rational explanations have been proposed (e.g., taxes, agency costs, investments in illiquid stocks) but none is satisfactory. Interestingly, the discount on closed-end funds moves parallel to the performance of small companies, suggesting that both anomalies are driven by changes in investor sentiment.

IV. THE FUTURE OF ASSET PRICING THEORY

Do the empirical findings of the 1980s imply that the EMH is wrong and that we should try to forget about it? Probably not. On balance, we agree with Fama (1991) that, at this stage, the data appear just as consistent with (augmented) rational as with psychological theories. The problem is that it is difficult to distinguish between these hypotheses without better theories of asset valuation. From this perspective, the anomaly literature may be seen as both good news and bad news. The good news is that, over the last 25 years, a lot has been learned about the actual behavior of stock prices. The bad news is that finance looks more and more like:

- a collection of theories for which there is no completely convincing evidence, and
- a collection of empirical observations for which there is no solid theory. In sum, the efficient markets debate is probably a good starting point for empirical research in finance but it is not where the research should end.

Future work in asset pricing theory is likely to fall into one of four categories:

- rational models that explain the predictability of returns by changes in macro-economic conditions;
- rational models that give more weight to institutional details (e.g., portfolio insurance, the introduction of option markets; models with alternative specifications of the real data generating process);
- statistical models based on more precise descriptions of the return generating process;
- noise trader models with both rational and non-rational actors.

The first three approaches are the logical continuation of modern finance. They rely on the concept of *homo economics*, and possibly market imperfections. The fourth approach introduces market psychology and imperfections in people. In other words, it addresses
the quality of judgment and assumes that rational arbitrage cannot costlessly offset the consequences of uninformed trading.

The strength of modern finance has always been that it is based on rational behavior and mathematical logic. However, even Milton Friedman would admit that, as descriptions of individual behavior, the rationality assumptions are only an approximation. Nevertheless, modern asset pricing theory remains a simple, beautiful, and important starting point in understanding the behavior of stock prices. For instance, the C.A.P.M. motivates concepts (e.g., systematic risk, diversification, arbitrage) that are actually used by portfolio managers and other finance practitioners.

In this tradition the first category of possible future work would try to remedy our present lack of knowledge about the links between general economic conditions, individual firm behavior and stock prices. The second would extend our knowledge about the consequences of regulation, financial innovation, tax policy, etc.

Models of the third category have recently received a great deal of attention, especially chaos theory and nonlinear dynamics (see also Table 1). The attractiveness of nonlinear and chaotic dynamics is its ability to generate large movements which "look" random, with greater frequency than the classical (linear) models. Not surprisingly, after the market crash of 1987, there has been a explosion of papers searching for nonlinear dynamics in financial time series. The rationale for a possible existence of nonlinear dynamics could be described as follows. The classical economic models assume a system that is globally stable but subject to exogenous random shocks. If the exogenous shocks are small, and the underlying dynamics are "smooth", the dynamics can be approximated by a linear model like the C.A.P.M. However, if the system dynamics are not smooth, or the shocks to the system are systematically large, the dynamics show non-linearity. Even without random shocks, the system is very sensitive to small perturbations and large effects may result. This makes prediction difficult and gives the appearance of random behavior, even though the system evolves deterministically. As a rule, financial economists are skeptical about tests for chaos because no one can seriously believe that asset prices would be perfectly deterministic. However, recently Liu, Granger and Heller (1992) have extended these models by considering non-linear structure with significant stochastic shocks. It is possible that such models could reconcile market efficiency and some of the observed anomalies by specifying a non-linear asset pricing model as a benchmark.

Finally, also the fourth type of models have recently undergone tremendous development. Currently there is a whole literature of noise trader models that typically present two kinds of investors: rational traders and noise traders, who make their investment decisions partly on irrational factors (see Black (1986) and Shleifer and Summers (1990) for overviews). In fact, behavioral theories predict some of the observed anomalies. For example, the overreaction hypothesis (i.e. the typical representative investor gives too much weight to recent information) predicts both excess volatility and the long run return patterns reported by De Bondt and Thaler (1985). Consequently, noise trader models should develop to better incorporate these behavioral theories. One of the important paradoxes such models should come to grips with is the fact that during the 1980s stock markets simultaneously showed increased volatility and increased sophistication - at least as measured by the rising importance of professional money managers. In particular, current "noise trader" explanations typically blame small uninformed investors (see, e.g., De Bondt (1993)) for excess volatility and apparent irrationality in stock price movements. However, over the last twenty years, increasing institutionalization (and trading volume) has gone hand in hand with increasing volatility.

V. PRACTICAL IMPLICATIONS FOR MONEY MANAGERS

What does all of this mean for finance practitioners? Probably two things. First, the evidence is consistent with the notion that financial markets are a very competitive business environment; and just as in any competitive industry, actors doing their "home-work" (i.e. fundamental and/or technical analysis) may benefit from those who don't. It would be a serious mistake, though, to expect that many investors can consistently beat the averages. For if such "surplus" returns would be there for the taking, we would see many wealthy analysts and even more happy clients. In fact, as our readers can confirm for themselves, the bulk of analysts still go to work every day and some of them even get fired by their clients. Surely, it is extremely difficult to beat the S & P or any other market index. Thus, in this sense, the efficient markets view remains a solid first approximation to stock price behavior.
Second, the data tell us that there are exceptions and that they can be spotted. Analysts with access to high-powered computers and large data bases may be able to do what most of us can't. To earn abnormal returns, one probably also needs deep pockets - to be able to withstand market setbacks - and a lot of patience.

The more successful (contrarian) strategies appear to be long-term and they do not involve much trading. Regrettably, much of the money management business is not set up in a way that well-informed portfolio managers can take advantage of these opportunities. For example, money managers are evaluated at regular (quarterly and annual) intervals. Thus, there are strong incentives to abandon strategies that do not pay off in the near term. Of course, at a different level, this arrangement makes sense since clients have a hard time knowing whether their agents are skillful or merely lucky. Thus, in the end, agency problems, the inherent ambiguity of stock value, and the presence of noise traders imply that some of the investment opportunities documented in the 1980s are hard to reap in practice.

Finally, there is the problem of selecting winning strategies, post (i.e., after the fact). Although this paper is about anomalies, the bulk of the literature suggests that anomalous behavior is the exception rather than the rule. We already know from the coin flipping experiment in Table 2 that if the number of strategies that is tried is large enough, the best performer is bound to look "phenomenal". Hence, before putting down his clients' money and betting on an "anomaly", a cautious money manager better asks: why does the "opportunity" occur? Can I believe this? Is the anomaly real?

NOTES

1. Of course, this is not to deny the importance of theory and correct statistical methods. We direct the interested reader to other specialized surveys, e.g., Fama (1970, 1991), Lehmann (1991), Le Roy (1991), and Granger (1992).

2. This point of view may be less relevant to a small trader who has inadequate access to public information, pays high brokerage costs as well as taxes, but on the positive side (?!) is blessed with some gambling instinct and a taste for money.

3. As an appetizer to our paper, the reader may consider the results of the "Investment Dartboard Stock-Picking Contest" run by the Wall Street Journal. In a series of 37 six-month contests between professional stock analysts and darts, "the forces of chance have won 45% of the time. The pros have captured 20 contests and the darts 17" (Wall Street Journal, July 8, 1993, p. C1).

4. In recent empirical work, the expected return estimates are sometimes based on the arbitrage pricing theory (A.P.T.) of Ross (1976) rather than the C.A.P.M. and the market model.

5. i.e. the chartist's return is compared to \( (\alpha_i + \beta_{i} \times R_{m}) \) with \( \beta_{i} = \beta \) the beta of the chartist's portfolio.

6. Note that the two hypotheses are not mutually exclusive: the human factor as well as knowledge of economic fundamentals may matter to stock price valuation.

7. Two stocks are subject to nonsynchronous trading if on a certain trading day the last trade in both stocks does not take place at the same moment in time. Nonsynchronous trading causes positive autocorrelation in realized returns, and especially in a market with continuous trading over several hours (like the New York stock exchange on which Lo and MacKinlay's and Conrad and Kaul's have conducted their research) this can be a problem. To see this suppose that the last trade in stock A on day 1 takes place at 11 a.m. and the last trade in stock B at 3 p.m. Also assume that a new piece of positive information hits the stock market at 1 p.m. Then the day's closing price of stock A does not reflect this good news yet; it will only lead to an increase in the next day's price. Stock B's price however has already jumped up on day 1. If stocks A and B are used to form a portfolio, both on day 1 (through stock B) and day 2 (through stock A) there will be a price rise and hence the portfolio shows a positively autocorrelated return.

8. Chan (1988), Ball and Kothari (1989), and other claim that the returns may be compensation for changing beta-risk. De Bondt and Thaler (1987) and Chopra et al. (1991) dispute this. Evidence of market overreaction is also found on European exchanges. (See, e.g. Alonso and Rubie (1990) or Bos (1991)).

REFERENCES


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