

Para 83

CAN YOU BEAT THE STOCK MARKET?

■ Most people interested in the stock market fall into one of three categories: (1) academic scholars who doubt that anybody really knows how to beat the market; (2) professional investors who indignantly reject this view of the matter; and (3) amateur investors who also believe that you can beat the market but don't realize how controversial this assumption is. I have long been a partisan of the first group, and until the last year or so had assumed that its case was airtight.

The professors seemed to have built an overwhelming case for the so-called efficient market hypothesis (EMH). If you think of the hypothesis as a literal description of the real world, the stock market cannot be beaten by mere mortals. Question: how close to reality is EMH? Having now resurveyed the basic case made for it in the business schools, and also looked at some recent findings that seem inconsistent with it, I find myself still answering that EMH is extremely useful for understanding the stock market—but doubting that it's as close to reality as I had previously assumed. It seems fairly clear that some superior investors are out there beating the market systematically.

THE EFFICIENT MARKET hypothesis says that stock prices always tend to reflect everything known about the prospects of individual companies and the economy as a whole. This simple-sounding academic proposition has some staggering implications. It implies, first of all, that stock prices cannot be predicted: if all cur-

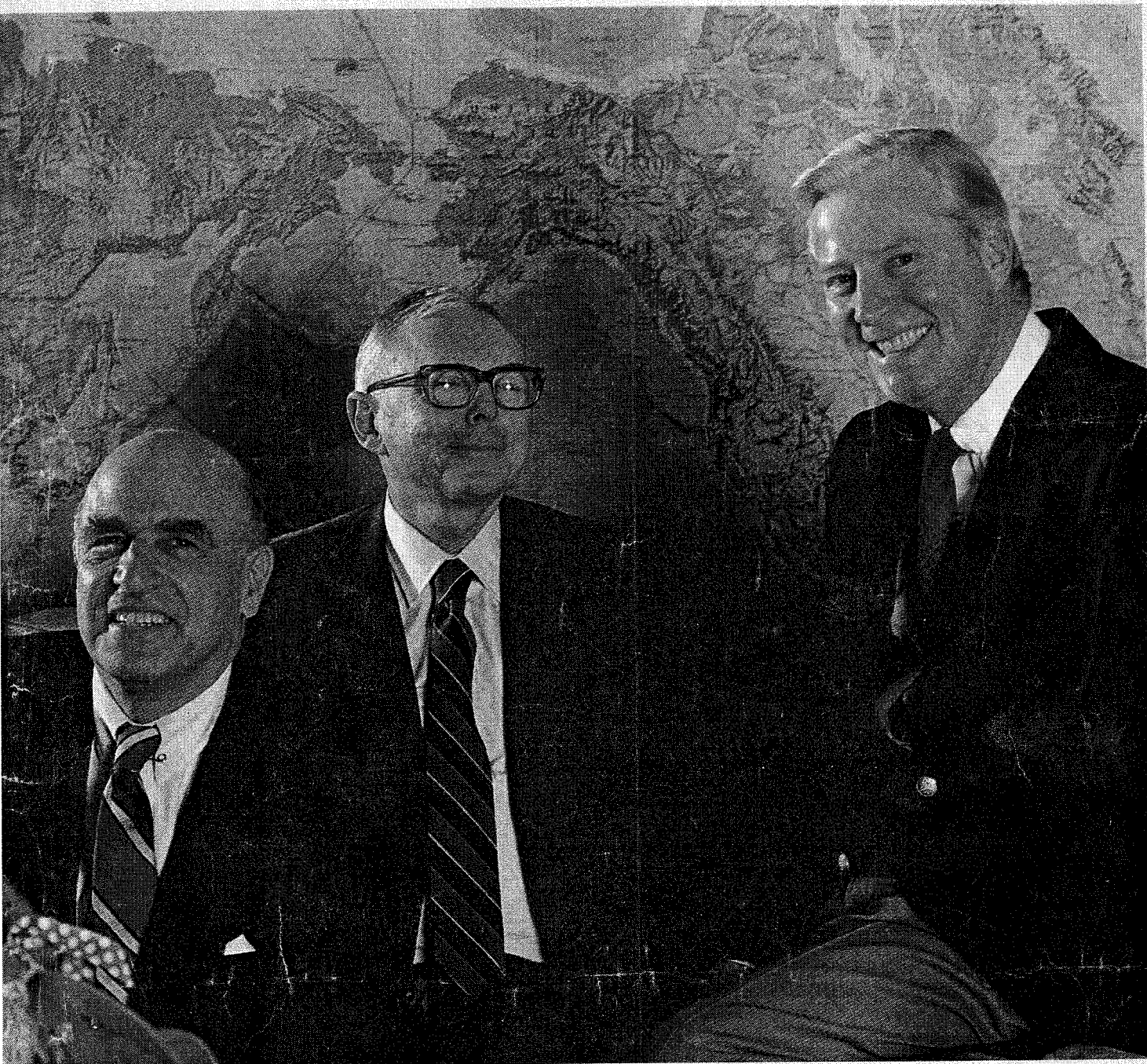
RESEARCH ASSOCIATE Robert Steyer

rent information is already embedded in the prices, then they will be moved only by events not now foreseen—which are, by definition, unpredictable. This means in turn that all “technical analysis,” and especially efforts to discern future stock price trends by examining past trends, are futile. EMH also implies that no amount of fundamental research, including the exhaustive and high-priced studies done by Wall Street for big institutional investors, will give investors an edge. It implies that if you're in the stock market, you should buy and hold rather than trade a lot; trading increases your brokerage costs without increasing your expected return. It tells you to assume that professionals, or indeed any investors, who have outperformed the market in past periods were probably just lucky, and that we have no reason to believe they will have superior results in the future. In his textbook *Foundations of Finance*, economist Eugene F. Fama of the University of Chicago asks “whether there are individuals or groups—for example, managers of mutual funds—who are adept at investment selection in the sense that their



choices reliably provide higher returns than comparable choices by other investors.” Answer: “If prices always fully reflect available information, this sort of investment adeptness is ruled out.”

In the mid-Sixties, Fama probably did more than anyone to develop the efficient market hypothesis. (Nobel laureate economist Paul Samuelson of MIT was also among those who elaborated the concept then.) Asked recently how well he thought EMH has stood up over the years, Fama replied genially: “It's done pretty well. Most economic models barely make it to the next set



of data." His perspective is that publicly available information (but not all inside information) is almost certainly reflected in stock prices. This means that Fama, like most other academics, believes in the "semi-strong" form of EMH. In the so-called strong form, *all information known to anybody* is said to be built into prices; however, I never did succeed in finding anyone who accepted this proposition as literally true.

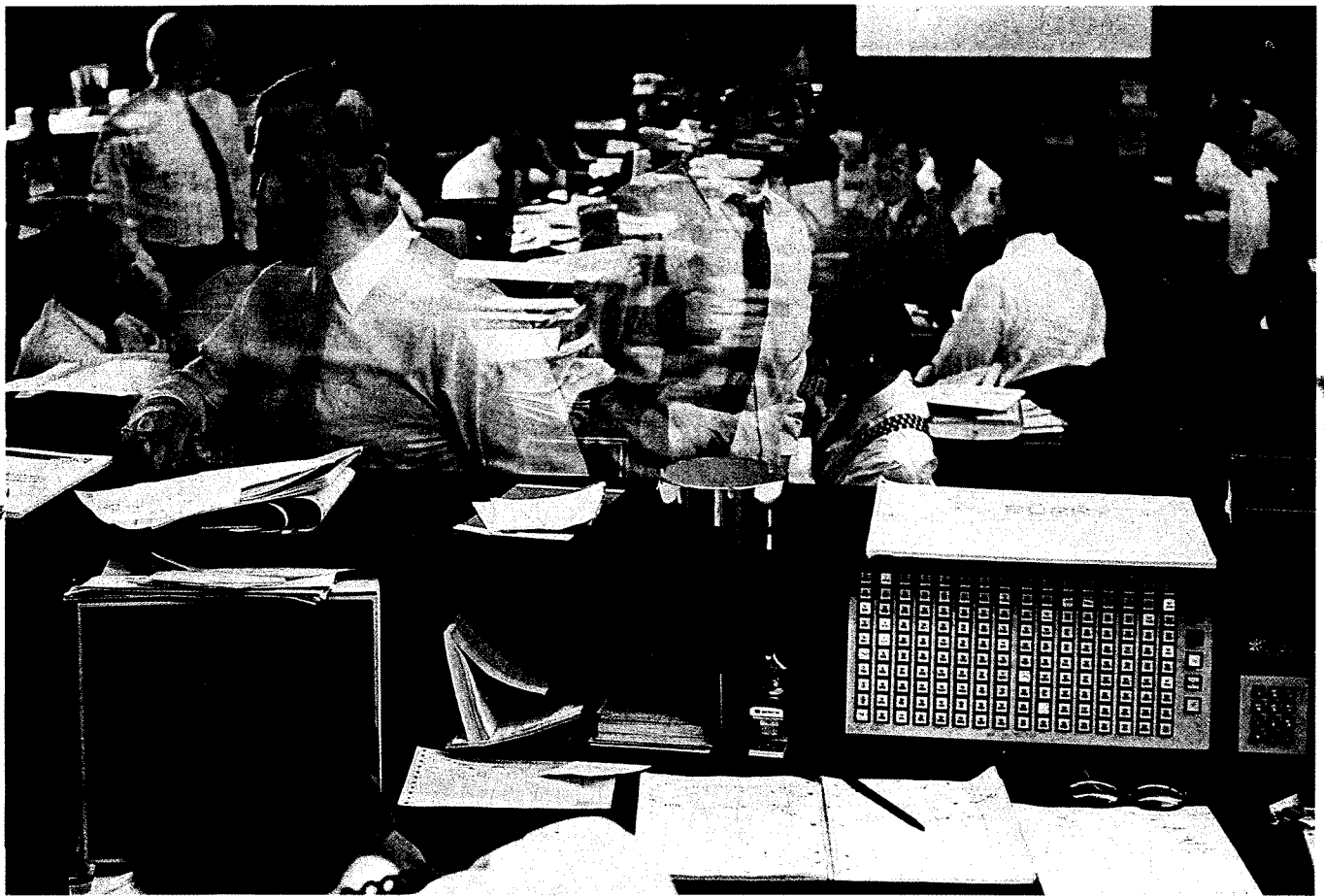
Even in the semi-strong form, however, EMH is hard for Wall Street to swallow. It implies that much of what investors hear around them every day is nonsense. Fast ex-

ample: Standard & Poor's was presumably talking nonsense last June, when (like a lot of other advisory services) it said the stock market would rise further because profits were improving; the efficient market hypothesis tells us that prices in June would already be reflecting whatever was knowable about future earnings.

Having somewhat different material interests from members of the investment community, I have always found the efficient market hypothesis intuitively appealing and told myself that it had tremendous explanatory power. Indeed, it explains the single

These super-investors beat the market quite regularly and claim that others could too if they followed the precepts of the late Benjamin Graham. Shown aboard the QE2 en route to England recently are (from left) Warren Buffett, Walter Schloss, Charles Munger, and William Ruane. They meet with a small group of other stock market aficionados to exchange ideas every two years, usually on land.

most obvious mystery about the securities business: how can it be that thousands of professional stock-pickers, including many who are plainly intelligent and industrious, are endlessly confounded by the market and



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embarrassed by their selections? Security analysis is one of the very few lines of work in which we take for granted that the recommendations of respected professionals will be wrong half the time or more.

EMH is intuitively appealing on several other grounds. In a world where hundreds of thousands of investors are endlessly scratching around in search of some advantageous risk-return relationship, and where professional arbitrageurs on exchange floors stand ready to pounce on any security that offers a marginal advantage, and where, furthermore, computers have enormously multiplied the number of investors with access to instantaneous price quotations, it would be hard to explain how market inefficiencies *could* last more than a few minutes or even seconds. These armchair arguments have been buttressed by an avalanche of empirical studies that have made EMH a solidly settled question on the campuses. Indeed, some scholars are concerned that it may be excessively settled. Michael C. Jensen of the University of Rochester, who has no doubts at all about market efficiency, nevertheless worried recently about the battle having

been won so thoroughly, and added: "It's dangerously close to the point where no graduate student would dare send off a paper criticizing the hypothesis."

Among the empirical findings that make EMH noncontroversial on the campuses are any number showing that markets either anticipate or adjust instantaneously to published information and that they repeatedly see through misleading accounting practices. Studies of mutual fund managers disagree only about a minor matter: whether the managers are (a) unable to outperform the market or (b) able to do so but not by enough to give shareholders an edge after subtracting commissions and other costs. The studies agree that funds as a group do not enable investors as a group to achieve returns higher than those of the market. In the course of researching this article, I sat in on a lecture on EMH by Dean Burton G. Malkiel at the Yale Graduate School of Management. Malkiel, a lucid and witty lecturer who is a former member of the Council of Economic Advisers, had the students in stitches describing a study whose principal finding was an utter lack of correlation between mu-

tual fund rankings from one year to the next. In this context he mentioned the Mates Investment Fund, which was ranked No. 1 among mutual funds in 1968 but never got above No. 300 in subsequent years. Malkiel's throwaway line was that Fred Mates eventually got out of the mutual fund business and took to running a singles bar in New York called, appropriately, Mates. Evidently assuming that this was a made-up detail, the Yalies groaned and hissed at the line; however, it happens to be true.

NATURALLY not wishing to give up on a theory that helps to explain life's mysteries, I have been distressed by some signs in recent years that the efficient market hypothesis might be in trouble, or at least in need of some updating. Like many other EMH fans, I have been shaken by the proliferation of "anomalies"—this being the professors' preferred term for stock market news that seems to confound the hypothesis. News of this kind is taken very seriously indeed on the campuses. Professor Stephen A. Ross of Yale's management school commented jo-

vially the other day that papers on the anomalies have become "a major growth sector of the academic world."

One disturbing anomaly centers on the extraordinary records compiled by certain high-visibility investors. The records of one tightly knit group of investors, of whom Warren Buffett is the best known, are laid out in the box below. Buffett, chairman of Berkshire Hathaway Inc. and the subject of a recent FORTUNE article ("Letters from Chairman Buffett," August 22), is very much aware of the extent to which his investment record constitutes a challenge to the efficient market hypothesis. He believes that there are exploitable "pockets of inefficiency" in the market, and he has several times argued his case in appearances at the Stanford business school, on whose advisory council he serves. Speaking to the council, Professor William F. Sharpe of Stanford, one of the school's academic stars and the author of a popular textbook solidly endorsing EMH,

once referred to Buffett as "a five-sigma event." In business school lingo, this superlative signifies that you should think of his investment performance as being five standard deviations above the mean; if literally true—no one claims that it is—this would tell us that there was only about one chance in 3.5 million of compiling an investment record like Buffett's by chance.

THE APPARENTLY superior long-run performance of the *Value Line Investment Survey* is another anomaly that efficient-market fans must come to terms with. The professors have, in fact, been worrying about the survey since 1970, when Value Line Chairman Arnold Bernhard made a presentation about its record at the University of Chicago.

Every week Value Line ranks about 1,700 stocks on a scale of 1 (most favorable) to 5. Bernhard's most compelling detail: in the five-year period beginning in April 1965, the

returns to investors had corresponded precisely to the rankings. In every one of the five years, Rank 1 returns had been highest, Rank 2 returns had been next highest, and so on. For the five years as a whole, Rank 1 was up 129% and Rank 5 was down 41%; this was a period in which stocks were rising about as often as they were falling.

Bernhard's methodology in getting to those figures was challenged by Professor Fischer Black, now of MIT, a leading advocate of the efficient market hypothesis. However, Black ultimately concluded—reluctantly, I have to assume—that Value Line had indeed done well over the five years. His own figures showed annual risk-adjusted returns averaging 10% for Rank 1 during the period and -10% for Rank 5. (A risk-adjusted return is one from which you have subtracted the return expected on a randomly selected portfolio of comparable risk.) Reviewing matters three years later, Black wrote in the *Financial Analysts Journal*: "If anything, the

YOU ONLY SWING ON 3 AND 0

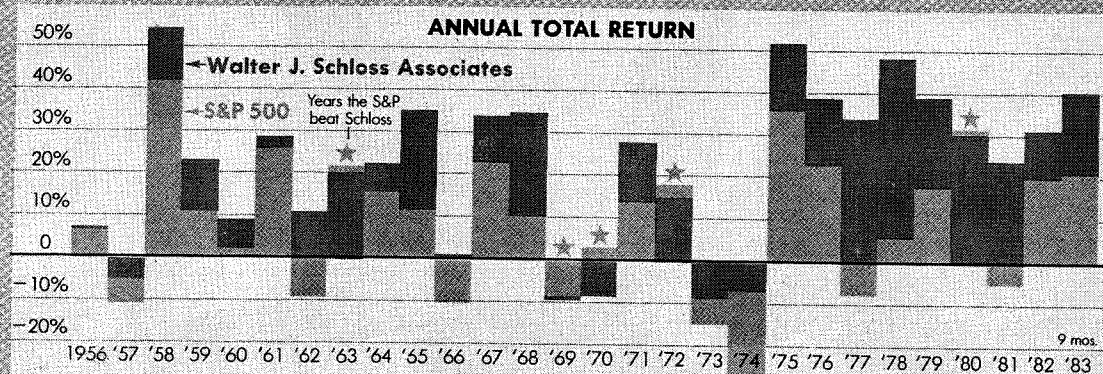
■ In principle, says the efficient market hypothesis, no one can systematically beat the stock market. In applying EMH to Warren Buffett, Charles Munger, William Ruane, and Walter Schloss, the investors pictured on pages 82 and 83, you would have three difficulties: (1) all have outperformed the market over long periods; (2) they have generally done so in both bullish and bearish environments, so it's hard to argue that their higher returns simply reflect greater risk-taking; and (3) all are pursuing strategies that reflect the ideas of the late Benjamin Graham, so it's hard to view their performance as a random event. Buffett, Ruane, and Schloss studied under Graham, and all four have been influenced by his classic *Security Analysis*, written with David L. Dodd and first published in 1934. Graham's core idea: look for companies that for some reason are undervalued and hold the stocks for as long as it takes the market to see the values. Obviously such companies are hard to find. Says Buffett: "You wait for the 3 and 0 pitch." Prize example of what you can get by waiting: the Washington Post Co. in 1974, when its market value was \$80 million and its TV stations alone were worth more than that. Now its market value is about

\$1 billion, and Buffett's Berkshire Hathaway Inc. owns 13%.

Buffett himself has not formally been in the money management business since 1969, when he dissolved the Buffett Partnership Ltd. after nearly 14 years of operation. One reason for the dissolution: Buffett had stopped finding undervalued securities. During its lifetime the partnership had an average annual return of 29.5%, vs. 8.2% for the S&P 500. (Like the figures below, these assume annual reinvestment of dividends.)

Charles Munger's partnership (Wheeler Munger & Co.) operated from 1962 to 1975. It had an average return of 19.8%, vs. 5.3% for the S&P 500. Bill Ruane runs the immensely successful Sequoia Fund, which was first offered to the public in July 1970 and since then has had an average return of 18.6%, vs. 10.6% for the S&P. Sequoia now has \$333 million of assets and has suspended sales to new investors because, says Ruane, "the money was coming in faster than I had ideas."

The private partnership run by Walter Schloss has had an average return of 21.3%, vs. 8.7% for the S&P. In a recent sentimental letter to his partners, Schloss saluted Graham and *Security Analysis*, which "helped many of us along a rocky road."



Sources: Standard & Poor's; Walter J. Schloss Associates

Could This Be Luck?

Like his fellow Grahamites, Walter Schloss has a 28-year record that's hard to attribute to luck. His partnership's total return (unrealized gains plus distributions) exceeded that of the S&P 500 in all but five years.

results have been better since 1970."

However, Value Line was not yet home free. During the past decade or so, further refinements in performance evaluation techniques have led to still more pulling apart of the organization's results; the latest edition of *Financial Theory and Corporate Policy*, a textbook by Thomas E. Copeland and J. Fred Weston, offers a "partial listing" of seven serious academic papers on Value Line. The central issue in the most recent of these papers—as in much other literature about

traded, sold for years at huge discounts from their underlying net asset values. After several of the heavily discounted closed-end funds went open-end a decade or so ago, the discounts began to shrink; however, no one has satisfactorily explained why they were so wide in the first place. "I've heard a hundred convoluted explanations of the discount," says Stephen Ross of Yale, "and not one that makes any sense."

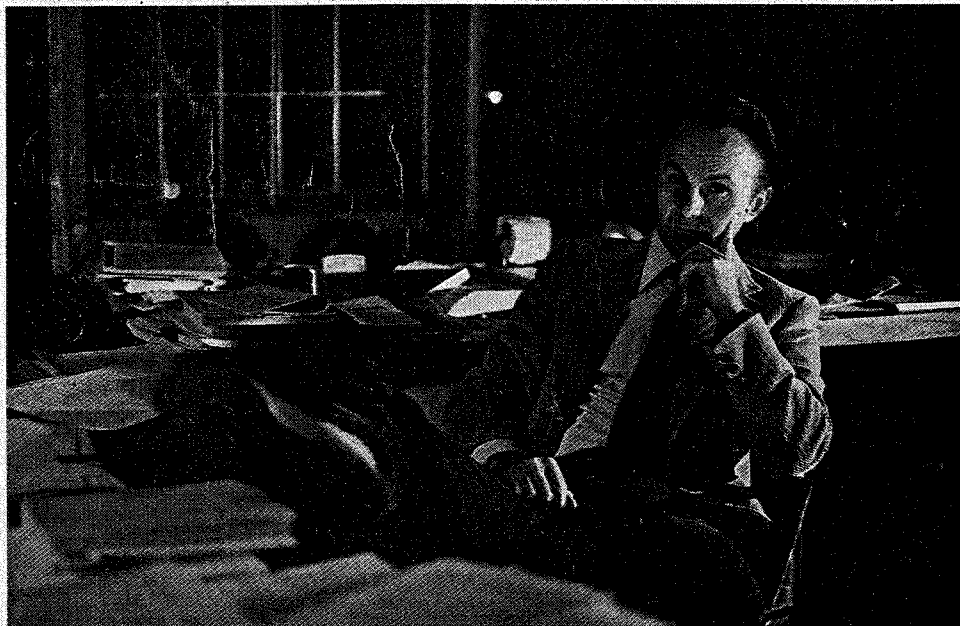
In retrospect the discounts look especially disconcerting because an investor who had

investors during that month, with the gains heavily concentrated during the first five trading days of the month; in addition, the gains are concentrated in the stocks of small companies. Such seasonal happenings are supposed to be ruled out by EMH.

The other calendar-based anomaly seems even more bizarre. It is the "weekend effect," a phenomenon on which Kenneth R. French of the University of Chicago appears to be the world's leading authority. Analyzing daily returns from 1953 through 1977, French found a persistent tendency for returns on Monday to be negative even though returns for the period as a whole were positive. The data suggested the possibility of a profitable trading rule: load up on stocks on Monday, just before the close, and then sell just before Friday's close. French's data said that if you applied this rule to Standard & Poor's composite index of 500 stocks during 1953-77, you would have had an average annual return of 13.4% (before transaction costs), vs. 5.5% for the S&P.

THE WEEKEND EFFECT seems especially mind-blowing when you focus on another detail. Why should Monday be a sick day on Wall Street? Instinct suggests that the below-average returns must have something to do with the fact that the market had been closed during the two previous days. For example, you might wonder whether companies weren't more likely to release bad news on days the market is closed. But this thought doesn't hold up. French found that when the market is closed because of a holiday (and not just a weekend), the day after the holiday is *not* sick. In other words, the weekend effect really does have something to do with weekends, and not closed markets in general.

The discovery of these calendar effects is most ironic. Over the years the academics who have developed EMH have been at pains to shoot down various Wall Street superstitions rooted in the calendar, the principal ones being belief in a summer rally and a year-end rally. In the lecture I attended at Yale, Malkiel explained why it was absurd to believe that the week between Christmas and New Year's Day tends to be bullish. If there were any foundation to the belief, he said, then investors would obviously load up on stocks just before Christmas and sell just before the New Year. If they did that, the bullish period would be moved back a day or so, the investors would then have to buy and sell still earlier, and they would be caught in an infinite regress. Against this background, it is bothersome to the academics to be in the position of discovering calendar effects that Wall Street hadn't heard about.



Eugene F. Fama, 44, expounded the efficient market hypothesis in his 1965 doctoral dissertation.

EMH anomalies—is risk adjustment. Obviously, you cannot get agreement on Value Line's providing superior risk-adjusted returns unless you first get agreement on how to measure risk.

In examining Value Line's record, a 1982 paper by Copeland and David Mayers of UCLA applied a measure of risk different from that used by Fischer Black; in addition, they extended the period under examination out to 1978. The upshot: Value Line's edge now looked much smaller, and a strategy of going long on Rank 1 and short on Rank 5 would have yielded only 6.8% a year in risk-adjusted returns, at which level profits would apparently have been wiped out by brokerage commissions. In a final zinger, Copeland and Mayers noted that the abnormal returns appeared to be sinking toward the end of the period. Still, any abnormal returns at all represent a challenge to EMH. The Copeland-Weston textbook concludes a detailed passage on Value Line by proclaiming that it "remains an enigma."

Several heavily studied anomalies concern the well-known discount on closed-end funds: many of the funds, which are publicly

naively told himself that he was outfoxing the market by buying his portfolio at a discount *was* outfoxing the market, or at least outperforming it. Professor Rex Thompson of the University of British Columbia has shown that during the 32 years beginning in 1940 an investor could have consistently earned abnormal returns with a simple trading rule. The rule: maintain a portfolio of discount funds, and weight the portfolio so as to emphasize those with the largest discounts. The trading rule would have rather consistently given you a risk-adjusted rate of return of over 4%. Conversely, an investor who had maintained portfolios of the funds selling at a premium (which a few of the funds always did) would have racked up risk-adjusted losses of 7.9% a year—also an affront to EMH. Or, rather, an apparent affront. Thompson himself is inclined to think that he was looking not at a market inefficiency but at still another situation in which risk wasn't being measured properly.

Two other heavily studied anomalies pertain to the calendar. One, the "January effect," refers to a distinct, statistically significant pattern of above-average returns to

Furthermore, the calendar anomalies are not of the class that can be attributed to uncertainty about measuring risk. Professor G. William Schwert of the University of Rochester, who is one of the editors of the *Journal of Financial Economics*—which can claim to have published more articles about efficient market anomalies than any other periodical in the world—commented wryly the other day: “I’m willing to believe that we’re mis-specifying risk in some serious way, but I have trouble believing that we only do

fect, the academics have come up with another startling finding: the small-firm effect is in part a reflection of the January effect. Donald B. Keim of the Wharton School has demonstrated that about half of that outsize return to small-company stocks is accounted for by their superior performance during January, especially during the first few days of the month. Whether this seasonal news reduces or magnifies the mystery of the small-firm effect is somewhat unclear, but the finding has evidently encouraged academics to look

toward his findings is reflected in the article’s Teutonic title: “*Vas ist das?*”) This left him having to explain why traders didn’t bid up the prices of small-company stocks in December and sell them in January. The likeliest explanation, he decided, was high transaction costs, especially the huge bid-asked spreads on many small stocks. Although Roll is one of the heavyweights of the efficient market fraternity, his explanation of the small-firm-cum-January phenomenon has not been accepted by all his colleagues.

Even if it were universally accepted, we would still need an explanation for the half or so of the outsize small-company returns that are not attributable to happenings in January. And we still need explanations for the other anomalies—investments that seem to have consistently offered superior risk-adjusted returns in defiance of EMH. Hopes about resolving these mysteries center on two possibilities: (a) that we will find new and better measures of risk that make the anomalies go away, and (b) that we will find quirks in the marketplace that explain the anomalies without requiring us to throw out EMH. Nobody in the academic world, so far as I know, is responding to the anomalies by saying that maybe they mean markets are much less efficient than previously supposed.



Stephen A. Ross, 39, has pioneered in developing new measures of stock market risk.

it on certain days of the week.”

The leading entry in the anomaly sweepstakes these days is none of the above. It is, instead, the “small-firm effect.” Schwert contributed an article on the small-firm effect to the June 1983 issue of his journal, which also carried six other articles on the subject. Since small-company stocks are in general riskier than blue chips, you would, of course, expect them to have higher returns on average. The news on the small-firm effect is that even after adjusting for risk, small-company stocks yield outsize returns. Marc R. Rein-ganum of the University of Southern California has found that for “very small capitalization firms,” the risk-adjusted annual return has been running at an incredible average rate of more than 20%.

How can this be? If small-company stocks—or any other class of stocks, for that matter—are clearly identified as superior investments, you would expect the market to bid up their prices until they reached a level at which risk-adjusted returns to future investors would be merely normal. Why doesn’t this happen?

In attempting to fathom the small-firm ef-

closely at some tax-related and other institutional explanations of the anomaly.

For example, what about the possibility that small-company stocks do well early in January because they are rebounding from year-end tax-loss selling? Presumably they are especially vulnerable to such selling because, as high-risk stocks, they are more likely than blue chips to represent losses to investors. “This argument is ridiculous, of course,” wrote Richard W. Roll of UCLA in a now famous article in the winter 1983 *Journal of Portfolio Management*, and he explained why: “If investors realized that such a pattern were persistent, they would bid up prices before the end of the year and there would be no significant positive returns after January 1.” Roll nevertheless went ahead and ran some empirical tests of the ridiculous argument; for example, he checked to see whether the stocks that did best early in January were in fact most likely to be those with poor performances in the prior year. It turned out that they were, and for this and other reasons Roll surprised himself and concluded that the tax-selling hypothesis had something to it. (His mind-boggled attitude

FOR THE PAST 20 years or so, the preferred risk measure, both in the business schools and among portfolio managers, has been the famous beta. The logic of beta derives from the Capital Asset Pricing Model (CAPM), developed in the early Sixties by William F. Sharpe of Stanford and others. The central insight of the CAPM is that not all kinds of risk affect return. The market does not compensate you for bearing a risk associated with a given company—the risk of a strike, say, or a product failure—because you can effectively eliminate such risks by diversifying. What you cannot eliminate is the “systematic risk” that all investors share by virtue of the fact that stocks tend to rise and fall together in bull and bear markets. Beta therefore expresses only the systematic risk of a given stock and endeavors to do so by measuring the extent to which returns on the stock have been more, or less, variable than those for the market as a whole. With the market’s own beta pegged at 1.0, a highly volatile stock that has historically offered extremely variable returns might have a beta of 2.0. A defensive stock with minimum swings and not too much dispersion in its returns might have a beta of 0.5. In principle, high-beta stocks have correspondingly high rates of return. So if beta works as intended, and if markets are efficient as believed, it should be im-

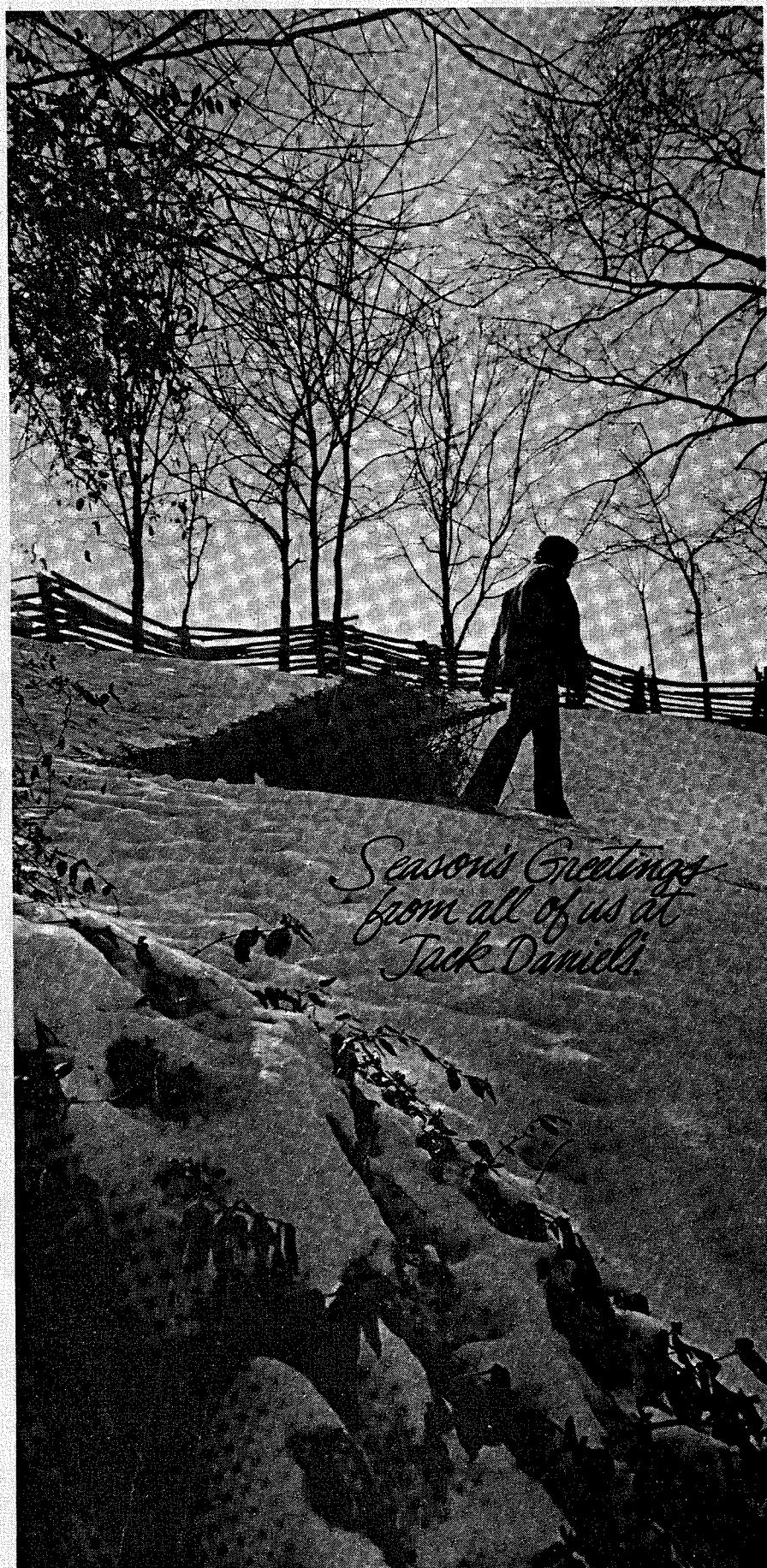
possible for scholars to find anomalies like the small-firm effect.

The fact that the anomalies nevertheless keep turning up is one reason why beta looks somewhat shaky these days. Fama's crisp judgment on beta: "It's not adequate—that's for sure." This risk measure is not in trouble just, or even mainly, because of the anomalies. It has also been hurt by (a) evidence that betas are not as stable over time as long assumed, (b) evidence that any individual stock's beta will vary considerably, depending on which index one uses as a proxy for the market as a whole, and (c) the bothersome finding of a decade ago that over one extremely long period (1931-65) the risk-return relationship wasn't quite right: actual returns were somewhat higher than predicted for low-beta stocks and lower than predicted for high-beta stocks.

The hottest current candidate to replace beta is a measure developed by Ross of Yale and Roll of UCLA. They propose to replace the whole CAPM with their APT, which stands for Arbitrage Pricing Theory. Like the CAPM, the APT assumes that only systematic risk—the kind that can't be diversified away—needs to be measured. However, it also assumes that systematic risk cannot be captured adequately in a unitary measure like beta. The research thus far tells Ross and Roll that systematic risk needs to reflect several separate factors. The three mentioned most often: unanticipated changes in inflation, in industrial production, and in interest rates. Efforts to determine whether, and to what extent, the APT will get rid of the anomalies are still preliminary, but Ross believes that his model will wipe out most of them.

IN ANY CASE, the APT clearly sends out messages about various stocks' riskiness quite different from what beta tells us. The beta approach casts utilities, for example, as low-risk defensive stocks, while APT shows them to be extremely risky in periods of unexpected inflation. One side effect of APT's emergence has been to give utilities a useful new tool in arguing with rate commissions about their appropriate returns. Revealing a utopian side to his character, Ross says he hopes that utilities will use APT because it is a better model and not just because it happens to serve their interests.

What about the possibility that some anomalies will ultimately be explained by quirks of the marketplace—by arrangements that might sometimes lead investors to be less rational than EMH assumes them to be? Some proposed explanations look plausible. Dean LeBaron, president of the rapidly growing Batterymarch financial management



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group in Boston, observes that institutional investors in general are apt to be unreasonably leery of high-risk situations because the corporate treasurer, say, doesn't want to have to explain how come the pension fund portfolio he okayed had a company in it that went bankrupt. LeBaron says that some of his institutional clients were "not enthusiastic" about a high-risk portfolio that he was assembling early in 1982 and that included such subsequent big winners as Chrysler and International Harvester.

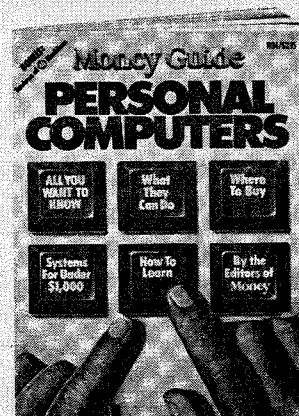
Another cluster of quirks has to do with the sizable transaction costs (mainly in the form of outsize bid-asked spreads) and liquidity problems associated with companies that have small capitalizations. A portfolio manager would have trouble loading up on such stocks without sending their prices through the roof. Dimensional Fund Advisors, a hot new fund group, is trying to solve this problem by assembling small-company portfolios and offering them to pension funds and other institutional investors. Among DFA's advisers: Gene Fama.

ON BALANCE it seems likely that all or most of the anomalies will ultimately yield to further academic findings about risk and institutional quirks. It seems most unlikely that the anomalies will end up vindicating Wall Street. You have to keep reminding yourself that the Street would never have heard of most of the anomalies but for business school research, and that the puzzles still to be solved are puzzling only in relation to academic models that have long been scorned on the Street. Anyway, if the market actually was inefficient in some fundamental way, the professional stock-pickers of America wouldn't be in such chronic disarray.

The hard part is explaining the existence of such apparently superior investors as Buffett and Value Line. An EMH hard-liner would argue that it's possible such investors are just being lucky year after year after year; and since nobody knows how to run a controlled experiment decisively differentiating between luck and skill in stock selection, that argument cannot be totally excluded. But Buffett's view—that in a basically efficient market it is nevertheless possible to find occasional, exploitable pockets of inefficiency—seems much more plausible.

■ What Buffett's view implies for the average investor is another matter. The professors make a persuasive case for just buying and holding a diversified portfolio. And even if you accept that superior investors do exist, it could be a mistake to act as though you and your broker are among them. **E**

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