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# Efficient Capital Markets and Martingales

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## I. Overview

AT ITS MOST GENERAL LEVEL, the theory of efficient capital markets is just the theory of competitive equilibrium applied to asset markets. An important idea in the theory of competitive equilibrium is the Ricardian principle of comparative advantage: England exported cloth to Portugal and imported wine from Portugal not because England necessarily had an absolute advantage over Portugal in producing cloth, but because England produced cloth comparatively more cheaply than wine relative to Portugal.<sup>1</sup> The same idea applies in analyzing equilibrium in financial markets, except that

<sup>1</sup> Ricardo gracefully headed off a criticism of chauvinism by specifying that Portugal had an absolute advantage over England in producing both cloth and wine ([1817], 1960, p. 82), rather than England over Portugal.

comparative advantage is conferred by differences in information held by investors, rather than differences in productivity among producers. The analogue in financial markets of Ricardo's assertion that absolute advantage is irrelevant is the proposition that information that is universally available cannot provide the basis for profitable trading rules. Thus if it is generally known that a firm has favorable earnings prospects, the theory of efficient capital markets says that the price of the firm's stock will be bid to the point where no extranormal capital gain on the stock will occur when the high earnings actually materialize. Therefore knowledge that earnings will rise in the future does not imply that the stock should be bought now. It is only differences in information—information that is not "fully reflected" in

prices—that confer comparative advantage, and that therefore can form the basis for profitable trading rules.

Most of the lessons of market efficiency are direct consequences of thinking about financial asset prices as determined by the conditions of equilibrium in competitive markets populated by rational agents. Some of these lessons are obvious. For example, the decision by a company to split its stock (i.e., issue two or three new shares in exchange for each old share) should have no effect on the rate of return on this stock. This proposition is a direct corollary of the fact that in any economic equilibrium the choice of numeraire is arbitrary. Other lessons of market efficiency, however, while apparently equally direct consequences of the nature of competitive equilibrium, go deeply against the grain of finance practitioners and financial journalists. For example, during the era of conglomerate formation in the 1960s and 1970s (which, of course, has since been succeeded by the current wave of leveraged buyouts accompanied by conglomerate breakups), firms routinely justified acquiring other firms in unrelated lines of business on the grounds that the acquisition served to diversify their activities, thereby reducing risks to stockholders. In an efficient market, this justification makes no sense at all. Firms have no comparative advantage over individuals in diversifying risk because individuals can diversify risk simply by buying the stock of several firms or the shares of a mutual fund that holds many firms' stocks. This example indicates that in finance, as everywhere else in economics, economists risk offending entrenched opinion to the extent that they insist on taking seriously even elementary conclusions drawn from equilibrium analysis.

When economists defend some statement as being a consequence of the fact (or assumption) that capital markets are

efficient, they are signaling that at a minimum they want to think of asset prices as being determined by the interaction of rational agents—that is, as being determined as an economic equilibrium—and that they see the proposed statement as following from this fact. Frequently, however, the term *efficient capital markets* carries in addition the presumption that the amount of information which is publicly available, and which for this reason cannot be used to construct profitable trading rules, is large. In the limit, the doctrine of capital market efficiency contains the assertion that individuals do not in fact have different comparative advantages in information acquisition. In such a world there are no profitable trading rules. This extended meaning of capital market efficiency underlies statements such as the following: In an efficient capital market, agents should have no investment goals other than to diversify to the maximum extent possible so as to minimize idiosyncratic risk, and to hold the amount of risk appropriate to their risk tolerance.

The importance of the topic of capital market efficiency is evident. Investors have no choice but to base their investment decisions on information. In evaluating their information, investors must consider not only whether it is accurate, but also whether it is generally known—in practitioners' parlance, whether it has already been discounted in the market price. Because the value of information depends on the extent of its dispersion, investors' decisions about what information to acquire depend on whether they think capital markets are efficient; to the extent that markets are informationally efficient, acquisition of information is a waste of time.

Suppose that capital markets are efficient with respect to some information set  $\Phi$ . Then by definition an individual investor who acquires information  $\Phi$

does not gain comparative advantage over his rivals, this information being already fully reflected in prices. The earliest empirical investigations of capital market efficiency tested this postulated failure of information to confer comparative advantage by constructing hypothetical trading rules based on particular information sets and testing their profitability under actual securities returns. Buying and selling stocks according to some prescribed formula based on  $\Phi$  should not result in systematic success if capital markets are efficient with respect to  $\Phi$ , but might do so otherwise.

Although it was insufficiently realized at first, these empirical tests of whether asset prices fully reflect available information also presume the validity of a particular equilibrium model specifying precisely how information is reflected in prices: the martingale model. Martingales will be defined and described below. The fact that the empirical literature on capital market efficiency is inextricably linked to the martingale model justifies our taking the martingale model as the unifying theme for this survey. It is true, however, that there exist branches of the literature on efficient capital markets that are unrelated to martingales but that nonetheless are important to a full understanding of market efficiency. These are sketched in the following two paragraphs. Coverage of these subliterations in addition to the martingale literature would result in a survey that is disjointed and superficial. Accordingly, topics are emphasized and deleted here primarily according to how closely they are linked to the martingale model.

The principal omission that is justified on grounds of unrelatedness to the martingale topic is the large and important literature on rational expectations equilibria under asymmetric information. In the asymmetric information literature the focus is on how agents who are

rational (and who have rational expectations) interact when it is common knowledge that they have different information. There is no question that mastery of the asymmetric information literature is indispensable to a deep understanding of capital market efficiency. However, this area, besides having no close connection with martingales, does not bear directly on the empirical work on market efficiency.

Another important literature that bears on capital market efficiency as defined above, but which is not discussed in this paper, is that on portfolio separation. Contrary to the implication in the first paragraph, it is not generally true that only differences in information give agents reason to trade securities. If futures markets are incomplete, changes in wealth and conditional distributions of future returns will in general interact with agents' risk aversion so as to induce them to trade even when there is no disagreement about the conditional distribution of returns. However, under certain restrictions on preferences and return distributions it can be shown that identically informed agents will hold identical, or virtually identical, portfolios. Under these restrictions it is true that all, or virtually all, differences in comparative advantage in holding securities can be traced to differential information. The theory of portfolio separation, which derives these restrictions on agents' optimal portfolios from assumptions about preferences and return distributions, is discussed in introductory graduate texts in finance (e.g., Jonathan Ingersoll, Jr. 1987; Chi-fu Huang and Robert Litzenberger 1988).

If the origin of the efficient capital markets literature is dated in the 1930s, as is reasonable, the martingale model appeared on the scene after the chronological midpoint of the literature (1965). Up to the mid-1960s, market efficiency was

associated with the random walk model. The literature on the random walk model is reviewed in Section II. After this background material is presented, the martingale model is presented in Section III. It is shown there that, as just indicated, empirical tests of market efficiency are in fact tests of a joint hypothesis which includes the martingale specification. Further, it is shown that, despite being a descendant of the random walk model, the martingale is closely related to the fundamentalist model which had earlier been thought to be diametrically opposed to the random walk. In Section IV Eugene Fama's influential analyses of capital market efficiency are discussed.

Section V begins the presentation of empirical developments in the analysis of efficient capital markets over the past two decades. It was realized around 1975 that the martingale model implied that asset prices should be less volatile than they apparently are. An extended debate, not yet concluded, then began over whether the observed volatility of asset prices in fact exceeds that which capital market efficiency implies, or whether instead the apparent violations reflect nothing more than statistical problems in the (purported) demonstrations of excess volatility. A closely related literature, that on mean reversion in asset prices, is then reviewed. The discussion of the latter topic is abbreviated because the literature on mean reversion, being still at a very early stage in its development, has not yet arrived at any kind of consensus. Section VI turns to examination of alternatives to the martingale model. It is easy to show that relaxation of the strong restrictions on preferences and return distributions required for the martingale model could in principle reconcile observed asset price volatility with that implied by market efficiency. However, it turns out that empirically these generalizations of the martingale model

do not succeed well. Continuing, the large finance literature on anomalies in asset pricing is reviewed in Section VII. Finally, conclusions are presented in Section VIII.

## II. *The Prehistory of Efficient Capital Markets*

Early works that were directly related to securities analysis as it is now practiced were J. B. Williams' *The Theory of Investment Value* (1938) and Benjamin Graham and David Dodd's *Security Analysis* (1934), upon which a generation of financial analysts was educated. These put forth the idea that the "intrinsic" or "fundamental" value of any security equals the discounted cash flow which that security gives title to, and that actual prices fluctuate around fundamental values. Accordingly, analysts were instructed to recommend buying (selling) securities that are priced below (above) fundamental value so as to realize trading profits when the disparity is eliminated. Because calculating present values is analytically trivial—particularly so inasmuch as the theory gave little practical guidance as to what discount rate to use—"fundamental analysis" consisted in practice mostly of forming projections of future cash flow. This involved analyzing demand for the product, possible future development of substitutes, the probability of recession, changes in the regulatory environment; in short, all information relevant to future profitability.

The only problem with fundamental analysis was that it appeared not to work. Alfred Cowles (1933) demonstrated that the recommendations of major brokerage houses, presumably based at least partly on fundamental analysis, did not outperform the market. The implication was that investors who paid for these recommendations were wasting their money. Other clouds shortly began appearing on

the horizon. In (1934) Holbrook Working argued that random walks—cumulated series of probabilistically independent shocks—characteristically developed patterns that look like those commonly ascribed by market analysts to stock prices. Was it possible that stock prices follow a random walk? In his (1960) paper, Working provided additional evidence in favor of purely random stock prices by showing that, if data generated by a random walk were averaged over time, spurious correlation between successive changes would result. Thus existence of such correlations did not necessarily constitute evidence against the random walk model.

The “random walk hypothesis”—forerunner of the efficient capital markets model—was inaugurated in earnest with a major statistical study by M. G. Kendall (1953) which examined seriously the proposition that stock prices follow a random walk. Kendall found that they do, as Working’s results had suggested. Clive Granger and Oskar Morgenstern (1963) followed up Kendall’s result with an econometric study using spectral analysis that supported the same conclusion. As it turned out, however, the results of Kendall and Granger and Morgenstern had been anticipated in a remarkable PhD dissertation written in 1900 by Louis Bachelier, a French mathematician. Bachelier conducted an empirical study of French government bonds, finding that their prices were consistent with a random walk model. Besides anticipating the empirical work that was to come more than a half a century later, Bachelier also developed many of the mathematical properties of Brownian motion (the continuous-time analogue of the random walk) which had been thought to have been first derived later in the physical sciences. In particular, Bachelier had anticipated many of the mathematical results developed in Albert Einstein’s 1905

paper. Bachelier’s study is excerpted in Paul Cootner’s (1964) collection of papers on the random walk model.

At first the random walk model seemed flatly to contradict not only the received orthodoxy of fundamental analysis, but also the very idea of rational securities pricing.<sup>2</sup> If stock prices were patternless, was there any point to fundamental analysis? The random walk model seemed to imply that stock prices are exempt from the laws of supply and demand that determine other prices, and instead look more like the casino or musical chairs game that John Maynard Keynes (1936) chose as metaphors for the stock market. However, economists immediately realized that such a conclusion was premature. Harry Roberts (1959) pointed out that in the economist’s idealized market of rational individuals one would expect exactly the instantaneous adjustment of prices to new information that the random walk model implies. A pattern of systematic slow adjustment to new information, on the other hand, would imply the existence of readily available and profitable trading opportunities that were not being exploited.

These considerations raised awkward questions for proponents of fundamental analysis: If fundamental analysis worked, why did not new entrants into the business of fundamental analysis, realizing this fact and planning to participate in the trading gains, compete these gains away? That is what happens in every other competitive industry in which profits exceed costs—why not in financial analysis? Alfred Cowles’ (1933) results

<sup>2</sup> “Adam Smith” (1968) expressed the skepticism about the random walk model that was characteristic of market professionals, and also the sense that the random walk model is diametrically opposed to the fundamentalist model: “I suspect that even if the random walkers announced a perfect mathematic proof of randomness, I would go on believing that in the long run future earnings influence present value . . .” (pp. 157–58).

suggested that in fact this was exactly what did happen. Fundamentalists had no good answers to these questions.

However, the random walk model left as many questions unanswered as it resolved, and its ablest proponents, such as Roberts, fully realized this. It was embarrassing for economists to have to shelve the competitive theory of price—surely the jewel in their professional crown—when it came to analyzing stock market prices, instead making do with informal and qualitative remarks such as if stock prices did not follow a random walk there must exist unexploited profit opportunities. If stock prices had nothing to do with preferences and technology, what about the prices of the machines that firms use? What about the wheat the farmer produces and the baker uses, but which is also traded on organized exchanges just like stock? Where does Marshall's *Principles* stop and the random walk start? Plainly there must be more to be said.

There is another problem with the random walk model. Critics of the random walk model can turn the random walkers' own method of argument back on them: Huge sums of money are spent every year on an activity—securities analysis—which, if the random walk model is correct, is entirely unproductive. Random walkers, the critics observe, expect us to believe at once: (1) that unexploited patterns in securities prices cannot persist because for them to do so would imply that investors are irrationally passing up profit opportunities, but also (2) that investors are nonetheless irrationally wasting their money year after year employing useless securities analysts. If the argument that no behavior inconsistent with rationality and rational expectations can persist in equilibrium is employed it must be employed consistently, and this the random walkers were not doing. Thus the continuing existence of large

incomes based on generating investment advice is as much a thorn in the side of the random walkers as the failure of this advice to generate extranormal trading returns is a thorn in the side of fundamentalists.

### III. *Martingale Models*

Resolutions to the puzzles pointed out in the preceding section required situating the random walk model within the framework of economic equilibrium. Such an account was not forthcoming within the random walk literature. A quarter-century later, it is easy to see why: By requiring probabilistic independence between successive price increments, the random walk model is simply too restrictive to be generated within a reasonably broad class of optimizing models. However, a weaker restriction on asset prices that still captures the flavor of the random walk arguments—the martingale<sup>3</sup> model—turned out to be more tractable. Paul Samuelson's (1965) paper was the first to develop the link between capital market efficiency and martingales. The simplicity of Samuelson's argument led some (for example, Mark Rubinstein 1975) to dismiss the result as obvious. Perhaps it is, particularly with hindsight. However that may be, when the dust had cleared and the implications of Samuelson's argument were fully assimilated, the random walk model had been jettisoned and replaced with the martingale model. Most analysts now consider Samuelson's to be the most im-

<sup>3</sup> The word *martingale* refers in French to a betting system designed to make a sure franc. Ironically, this meaning is close to that for which the English language appropriated the French word *arbitrage*. The French word *martingale* refers to Martigues, a city in Provence. Inhabitants of Martigues were reputed to favor a betting strategy consisting of doubling the stakes after each loss so as to assure a favorable outcome with arbitrarily high probability.

I am indebted to Christian Gilles for supplying this background.

portant paper in the efficient capital markets literature because of its role in effecting this shift from the random walk to the martingale model. The martingale model does not resolve all the puzzles that accompany the random walk, but it does resolve many of them. Unlike the random walk, the martingale model does constitute a bona fide economic model of asset prices, in the sense that it can be linked with primitive assumptions on preferences and returns which, although restrictive, are not so restrictive as to trivialize the claim to economic justification.

A stochastic process  $x_t$  is a *martingale* with respect to a sequence of information sets  $\Phi_t$  if  $x_t$  has the property

$$E(x_{t+1} | \Phi_t) = x_t \quad (3.1)$$

and a stochastic process  $y_t$  is a *fair game* if it has the property

$$E(y_{t+1} | \Phi_t) = 0. \quad (3.2)$$

Here (3.1) says that if  $x_t$  is a martingale, the best forecast of  $x_{t+1}$  that could be constructed based on current information  $\Phi_t$  would just equal  $x_t$  (it is assumed that  $x_t$  is in  $\Phi_t$ ).<sup>4</sup> This is true for any possible value of the information  $\Phi_t$ . Similarly, (3.2) says that if  $y_t$  is a fair game the corresponding forecast would be zero for any possible value of  $\Phi_t$ . It is obvious that  $x_t$  is a martingale if and only if  $x_{t+1} - x_t$  is a fair game.<sup>5</sup>

The martingale and fair game models are two names for the same characterization of equilibrium in financial markets;

<sup>4</sup> The exposition to follow comes with apologies to Donald McCloskey who, in instructing writers of economics to avoid "prefabricated and predictable" prose—boilerplate—wrote: "Explaining a model of efficient capital markets by writing for the thousandth time 'P<sub>t</sub> given I<sub>t</sub>, where I<sub>t</sub> is all the information' does not advance understanding. If it didn't much help to make Eugene Fama's work clear when he first uttered it, why suppose it will enlighten someone now?" (McCloskey 1987, p. 24).

<sup>5</sup> Fair games are for this reason sometimes called *martingale differences*.

rates of return are a fair game if and only if a series closely related to prices—that is, prices plus cumulated dividends, discounted back to the present—is a martingale. To prove this, let  $r_t$  be the rate of return on stock (for example)<sup>6</sup> from  $t - 1$  to  $t$ , and suppose that  $r_t$ , less a constant  $\rho$ , is a fair game. Using the definition of the rate of return as the sum of dividend yield plus capital gain, less one, it follows from the fair game assumption that stock price  $p_t$  is given by

$$p_t = (1 + \rho)^{-1} E(p_{t+1} + d_{t+1} | \Phi_t), \quad (3.3)$$

where  $d$  is dividends. Equation (3.3) says that the stock price today equals the sum of the expected future price and dividends, discounted back to the present at rate  $\rho$ . When there is no ambiguity about the information set, as here, it is convenient to rewrite (3.3) more compactly as

$$p_t = (1 + \rho)^{-1} E_t(p_{t+1} + d_{t+1}). \quad (3.4)$$

None of the variables defined so far is a martingale. The variable that is a martingale is the discounted value of a mutual fund that holds stock the price of which follows (3.4). The mutual fund is assumed to reinvest received dividends in further share purchases. To see that the discounted value of this mutual fund follows a martingale, let  $v_t = (1 + \rho)^{-t} p_t h_t$  be the value of the mutual fund discounted back to date zero, where  $h_t$  is the number of shares of stock the mutual fund holds at  $t$ . The assumption that the mutual fund plows back its dividend income implies that  $h_{t+1}$  satisfies

$$p_{t+1} h_{t+1} = (p_{t+1} + d_{t+1}) h_t. \quad (3.5)$$

Now consider  $E_t(v_{t+1})$ . We have

<sup>6</sup> Stock prices will be the principal source of examples throughout this paper. Justification for martingale models for other sorts of financial prices—for example, futures prices—is sometimes different (Danthine 1977; LeRoy 1982; Gilles and LeRoy 1986).

$$\begin{aligned}
E_t(v_{t+1}) &= E_t[(1 + \rho)^{-(t+1)} p_{t+1} h_{t+1}] \\
&= E_t[(1 + \rho)^{-(t+1)} (p_{t+1} + d_{t+1}) h_t] \\
&= (1 + \rho)^{-t} p_t h_t = v_t. \quad (3.6)
\end{aligned}$$

Here the second equality uses (3.5) and the third uses (3.4). Hence  $v_t$  is a martingale.

It is worth emphasizing that (3.4) implies that the price itself, without dividends added in, is not generally a martingale in the class of models just set out: If the dividend-price ratio changes over time because of fluctuations in current dividends relative to the variables that predict future dividends, as it generally will, the fair game model implies that the conditionally expected rate of capital gain must vary in an offsetting manner so as to maintain the nonrandomness of the conditionally expected rate of return. Such variation in expected capital gain conflicts with the martingale definition (3.1) (where  $p_t$  and  $p_{t+1}$  are substituted for  $x_t$  and  $x_{t+1}$ ). Nevertheless, the practice in the efficient capital markets literature is to speak of stock prices as following a martingale; in such cases "price" should be understood to include reinvested dividends. We will follow this imprecise but convenient usage.

The most direct empirical tests of the martingale model attempt to determine whether some variable in agents' information set is a predictor of future returns. If so, the martingale model is violated. For example, if agents know past returns and are able to use these to predict future returns, returns cannot follow a fair game. Of course, this result points to a fundamental ambiguity in the simplest tests of the martingale model: Finding some variable that predicts future returns could mean either that the capital market is inefficient—that is, does not satisfy the martingale property—or that that variable is not in agents' information sets. However, some more sophisticated tests of the martingale model do not suf-

fer from this ambiguity. For example, rejection of the variance-bounds inequality (discussed in Section V) implies rejection of the martingale model for any specification of agents' information sets.

The specification that a stochastic process  $x_t$  follows a random walk (coupled with the additional assumption that the increments have zero mean) is more restrictive than the requirement that  $x_t$  follows a martingale. The martingale rules out any dependence of the conditional expectation of  $x_{t+1} - x_t$  on the information available at  $t$ , whereas the random walk rules out this and also dependence involving the higher conditional moments of  $x_{t+1}$ . The importance of the distinction between the martingale and the random walk is evident: Securities prices are known to go through protracted quiet periods and equally protracted turbulent periods. Formally, one might represent this behavior using a model in which successive conditional variances of stock prices (but not their successive levels) are positively autocorrelated. Such a specification is consistent with a martingale, but not with the more restrictive random walk.

Samuelson (1965) proved a result—more precisely, pointed out the relevance of a well-known result from probability theory, the rule of iterated expectations—which put the theory of efficient capital markets on a firm footing for the first time. Similar results were presented by Benoit Mandelbrot (1966) at about the same time. Samuelson cast his original statement in terms of futures prices. However, continuity of exposition is best maintained here if his result is restated in terms of stock prices; in fact, Samuelson (1973) himself provided such a restatement. Samuelson's result was that the fair game model (3.4) implies that stock prices equal the expected present value of future dividends:

$$p_t = \sum_{i=1}^{\infty} (1 + \rho)^{-i} E_t(d_{t+i}). \quad (3.7)$$

To derive (3.7), replace  $t$  by  $t + 1$  in (3.4) and use the resulting equation to substitute out  $p_{t+1}$  in (3.4) as written. There results

$$p_t = (1 + \rho)^{-1} E_t[(1 + \rho)^{-1} E_{t+1}(p_{t+2} + d_{t+2}) + d_{t+1}]. \quad (3.8)$$

If it is assumed that agents never forget the past, so that  $\Phi_{t+1}$  is more informative than  $\Phi_t$ , the rule of iterated expectations guarantees that  $E_t[E_{t+1}(p_{t+2})]$  equals  $E_t(p_{t+2})$ , and similarly for dividends. Therefore (3.8) becomes

$$p_t = (1 + \rho)^{-1} E_t(d_{t+1}) + (1 + \rho)^{-2} E_t(p_{t+2} + d_{t+2}). \quad (3.9)$$

Proceeding similarly  $n$  times and assuming that  $(1 + \rho)^{-n} E_t(p_{t+n})$  converges to zero so as to rule out speculative bubbles,<sup>7</sup> (3.7) results. Also, the reverse implication obtains: The expected present-value model (3.7) implies that rates of return are a fair game.

Samuelson's result implies that the appearance noted in Section II of diametric opposition between the fundamentalist model and the efficient capital markets model of asset prices—with the former (latter) apparently implying that asset prices are completely systematic (unsystematic)—is entirely illusory. In fact, Samuelson's result implies that if fundamentalists are correct in viewing stock prices as equal to discounted expected cash flows, then it follows that future returns are unpredictable, just as the martingale model postulates. The fundamentalists, in focusing on the predictable part of asset prices, are asserting that the glass is half full, while the martingale model contends that the glass is half empty. As

the analogy implies, there is no contradiction even though the focus is different.

To be sure, in arguing for the similarity between the fundamentalists' model and the martingale model we have implicitly redefined the fundamentalist theory of asset valuation in a subtle but critically important way. Instead of assuming that price fluctuates around fundamental value (discounted expected cash flow), Samuelson assumed (or proved, depending on which direction of implication is being considered) that price actually equals fundamental value. The importance of this change is evident: If price always equals fundamental value, then no profit can be earned by trading on a discrepancy between the two, contrary to the fundamentalists' assertion. This observation implies that it would be no more correct to regard the fundamentalist model as originally formulated as identical to the martingale model than it would be to view the two as diametrically opposed. Contrary to both of these, it is best to regard the martingale model as an extreme version of the fundamentalists' model: If we start with the fundamentalists' model and modify it by assuming that a large majority of traders are conducting fundamental analysis, are arriving at the same estimates of fundamental value, and are trading appropriately, then price will be bid to equality with fundamental value and trading profits will disappear.

Under what assumptions regarding preferences is the martingale model satisfied? Samuelson pointed out that it would be satisfied if agents have common and constant time preference, have common probabilities, and are risk-neutral. If these conditions are satisfied, investors will always prefer to hold whichever asset generates the highest expected return, completely ignoring differences in risk. If all assets are to be held willingly, as must be the case in equilibrium, all must

<sup>7</sup> See Gilles (forthcoming) or Gilles and LeRoy (1988b) for a statement of conditions under which this convergence is guaranteed.

therefore earn the same expected rate of return, equal to the real interest rate. The interest rate, being equal to the constant discount factor, is itself constant over time. Therefore returns follow the fair game model (3.2), or, equivalently, prices plus reinvested dividends follow a martingale.

Risk neutrality implies the martingale (3.1), but not the more restrictive random walk. If agents do not care what the higher moments of their return distributions are, as risk neutrality implies, they will do nothing to bid away serial dependence in the higher conditional moments of returns. Therefore risk neutrality is consistent with nonzero serial correlation in conditional variances: The fact that future conditional variances are partly forecastable is irrelevant because risk neutrality implies that no one cares about these variances. Following Samuelson's paper, analysts realized that the theoretical underpinnings for efficient-markets models in fact point toward the martingale rather than the random walk. Once aware of the distinction between random walks and martingales, they also realized that most (but not all; see the following section) of the empirical tests for randomness were in fact tests of the weaker martingale model or, for example, the still weaker specification that rates of return are uncorrelated.

#### IV. Fama's Definitions and Evidence

The dividing line between the "prehistory" of efficient capital markets, associated with the random walk model, and the modern literature is Fama's (1970) survey. This influential paper brought the term *efficient capital markets* into general use and is widely interpreted as associating market efficiency with the martingale model, although it will be seen that this interpretation reflects a misreading of the paper.

Fama's paper, like the literature it surveyed, was devoted almost exclusively to empirical work. However, some preliminary theoretical discussion was also included, and Fama's (1970) definition of capital market efficiency became the industry standard, reproduced in innumerable subsequent papers, until it was supplanted by his equally influential (1976a) definition. In Fama's (1970) usage, a capital market is efficient if all the information in some information set  $\Phi$  is "fully reflected" in securities prices. Fama, crediting Harry Roberts with the original statement, then distinguished three versions of the efficient markets model depending on the specification of the information set  $\Phi$ . Capital markets are "weak-form efficient" if  $\Phi$  comprises just historical prices. Weak-form efficiency implies that no trading rule based on historical prices alone can succeed on average. Capital markets are "semistrong-form efficient" if  $\Phi$  is broadened to include all information that is publicly available. Finally, capital markets are "strong-form efficient" if  $\Phi$  is broadened still further to include even insider information.

In light of the discussion in the preceding section of the martingale model, it would seem natural to identify market efficiency with the specification that returns follow a fair game, with (1) weak-form, (2) semistrong-form, and (3) strong-form efficiency obtaining depending on whether the information set includes (1) past prices and returns alone, (2) all public information, or (3) private as well as public information. An attractive feature of this specification is that, from a mathematical property of conditional expectations, strong-form efficiency implies semistrong-form efficiency, which in turn implies weak-form efficiency, just as Fama's choice of terminology suggests.

However, Fama explicitly rejected this specification. Instead, he identified mar-

ket efficiency with the assumption that  $y_t$  is a fair game:

$$E(y_{t+1}|\Phi_t) = 0, \quad (4.1)$$

where  $y_{t+1}$  is defined to equal the price of some security at  $t + 1$  less its conditional expectation:

$$y_{t+1} = p_{t+1} - E(p_{t+1}|\Phi_t). \quad (4.2)$$

Fama correctly observed that the fair game model so defined

does not necessarily imply that the serial covariances of *one-period returns* are zero. . . . In the 'fair game' efficient markets model [as defined by (4.1) and (4.2)], the deviation of the return for  $t + 1$  from its conditional expectation is a 'fair game' variable, but the conditional expectation itself can depend on the return observed for  $t$ . (p. 392)

Here Fama is explicitly rejecting the identification of capital market efficiency with the requirement that rates of return themselves be a fair game variable—if they were, the serial covariances of one-period returns would in fact necessarily equal zero (because past returns are assumed to be in agents' information sets under all three forms of market efficiency). In Fama's definition, however, it is only the deviation of price from its conditional expectation that is a fair game.

The problem with Fama's characterization of market efficiency is that (4.1) follows tautologically from the definition (4.2) of  $y_{t+1}$ —just take expectations conditional on  $\Phi_t$ , on both sides of (4.2). Therefore the characterization of  $y_{t+1}$  as defined in (4.2) as a fair game variable does not restrict the stochastic process for price in any way. On Fama's definition, any capital market is efficient, and no empirical evidence can possibly bear on the question of market efficiency. The passage quoted in the preceding paragraph was not an isolated slip. In his theoretical discussion Fama observed that most empirical tests of market efficiency

are based on the assumption that "the conditions of market equilibrium can (somehow) be stated in terms of expected returns . . . , described notationally as follows:

$$E(p_{t+1}|\Phi_t) = [1 + E(r_{t+1}|\Phi_t)]p_t" \quad (4.3)$$

(p. 384). Again we have a tautology: (4.3) is obtained by applying a conditional expectations operator to the identity defining the rate of return as equal to the price relative  $p_{t+1}/p_t$  (less one). The tautologous nature of Fama's characterization of capital market efficiency was pointed out in LeRoy's (1976) comment; in his (1976b) reply, however, Fama rejected the argument, explicitly denying the existence of any tautologous elements in his definition.

In a subsequent section continuing his gloss on what it would mean for prices to "fully reflect" available information, Fama proposed the *submartingale* model

$$E(p_{t+1}|\Phi_t) \geq p_t, \quad (4.4)$$

so that (neglecting dividends) conditionally expected rates of return are nonnegative. This submartingale characterization of market efficiency is, of course, not tautologous. Fama asserted that if stock prices follow a submartingale, then no trading rule based on  $\Phi$  can outperform buy-and-hold. No support was given for this claim, and it is easy to produce examples of economies in which the prices of all primitive securities follow submartingales but in which there exist trading rules that outperform buy-and-hold in terms of expected return. In any case, the capital asset pricing model (CAPM) implies that equilibrium asset returns will not necessarily follow a submartingale: A stock that covaries negatively and sufficiently strongly with the market might well be priced to yield a negative expected return. Despite the negative expected return, risk-averse investors would be willing to include the stock in

question in their portfolios because its negative correlation with the market implies that it helps to insure the returns on the other stocks held, thereby reducing overall risk.

The ambiguity in Fama's theoretical discussion of capital market efficiency carried over to his interpretation of the empirical evidence. Fama generally interpreted the near-zero autocorrelations of successive stock price changes as favoring market efficiency, suggesting that he in fact identified efficiency with the characterization of returns as a fair game, contrary to his formal statement. Evidence that mechanical trading rules do not outperform buy-and-hold (Sidney Alexander 1961, 1964; Fama and Blume 1966) was similarly interpreted as favoring weak-form efficiency, providing further support for this reading. However, Fama's interpretation of Victor Niederhoffer and M. F. M. Osborne's (1966) evidence on runs—successive price changes of the same sign—is difficult to square with the fair game interpretation. Niederhoffer and Osborne found that reversals (pairs of successive price changes of opposite sign) occurred two to three times as frequently as continuations. Such systematic patterns are inconsistent with the fair game model. Despite this, Fama concluded and emphasized that such patterns, even though statistically significant, do not imply market inefficiency (p. 398).<sup>8</sup> Fama apparently based this conclusion on the fact that a plausible explanation for the predominance of reversals over continuations, reflecting the way limit orders are executed on the organized stock exchanges, can be constructed (see Niederhoffer and Osborne).

<sup>8</sup> Strictly, Niederhoffer and Osborne's evidence contradicts the more restrictive random walk, not the martingale. However, this distinction does not appear to be what Fama had in mind in denying that Niederhoffer and Osborne's evidence was inconsistent with market efficiency.

Generally, the implication of Fama's discussion of Niederhoffer and Osborne seems to be that markets are to be interpreted as efficient either if price changes are serially independent or if they are serially dependent but a convincing economic explanation can be found for the dependence. This is very different from the fair game interpretation of market efficiency, according to which departures from the fair game per se are identified with inefficiency.

Also arguing against Fama's identification of market efficiency with the martingale model is the fact that several of the studies he interpreted as bearing on market efficiency use the CAPM to remove the risk-premium component of asset returns (Michael Jensen 1968, 1969, for example). In the CAPM (strictly, in the intertemporal extension of the CAPM discussed in Section VII), prices do not generally follow a martingale.

Fama acknowledged the existence of some evidence against efficiency, particularly against the implausibly restrictive strong-form version, which requires that the information set with respect to which the market is efficient include even inside information. For example, Niederhoffer and Osborne documented the fact that market makers on organized exchanges have no difficulty converting their monopolistic knowledge of supply and demand functions for stock, as embodied in limit orders, into extranormal trading gains. This example is somewhat isolated, however; Fama reported that, surprisingly, the evidence against even strong-form efficiency is sparse. Mutual fund managers, who presumably have access to expert securities analysis, are apparently unable to acquire portfolios that systematically outperform the market (Jensen 1968, 1969). With regard to semi-strong-form efficiency, Fama et al. (1969) demonstrated that the information contained in stock splits is accurately re-

flected in stock prices at the time of the split, implying that stock splits cannot be used to construct profitable trading rules (unless, of course, one can find out about forthcoming splits before they become public knowledge).

In sum, Fama (1970) concluded that the evidence strongly but not unanimously supported market efficiency.

Fama proposed a different definition of capital market efficiency in his (1976a) finance text. A capital market is efficient if (1) it does not neglect any information relevant to the determination of securities prices, and (2) it (acts as if it) has rational expectations. The assumption of rational expectations means that investors use their information to make those inferences about future events that are justified by objective correlations between the information variables and the future events, and only those inferences. In other words, rational expectations models treat the agents being modeled as knowing the structure of the model and the values of its parameters. Putting these ideas together, Fama defined capital markets as efficient if the market uses all relevant information to determine securities prices, and uses the information correctly. Fama emphasized that efficiency can be tested only jointly with some particular model of market equilibrium, the nature of which depends on endowments and preferences, but which is not implied by market efficiency. Although his (1976a) definition has a major drawback, it is a great improvement over the (1970) definition. Most important, by clearly and unambiguously defining capital market efficiency in a way that is logically independent of particular market models, Fama resolved many of the ambiguities in his (1970) treatment of market efficiency. The drawback lies in his anthropomorphic characterization of "the market": One can speak unambiguously of "the market's" information only if all

agents have the same information, in which case informational efficiency is satisfied trivially.

The term *efficient capital markets* is seen to have several possible meanings, even if one ignores definitions proposed in the asymmetric information literature (Sanford Grossman 1978; Grossman and Joseph Stiglitz 1976, 1980; James Jordan 1983), as we do here. Nonetheless, the practice in the empirical finance literature is to speak of *tests of market efficiency* as if this phrase had unambiguous meaning. For the most part, in the empirical literature market efficiency is in practice equated with rational expectations plus the martingale model, and we will follow this convention.

#### V. *Empirical Evidence: Variance Bounds and Mean Reversion*

Fama's (1970) survey marked a high point for capital market efficiency; most of the evidence accumulated in the nearly 20 years since then has been contradictory rather than supportive. In this section the discussion will concentrate on the variance-bounds violations and the literature on mean reversion that grew out of it. These topics are chosen because they are directly related to martingales. Also, other types of evidence, such as the calendar-based "anomalies" explored in the finance literature, have recently been surveyed elsewhere. This other evidence will be acknowledged very briefly in Section VII.

Beginning in the mid-1970s, analysts came to realize that the same models which imply that returns should be unforecastable also imply that asset prices should have volatility which is, in a precise sense, low relative to the volatility of dividends. Results of tests of these volatility implications of market efficiency were circulated in 1975 in the paper by LeRoy and Richard Porter (published

1981). Robert Shiller, working independently, reported the results of tests of similar volatility relations in his (1979) and (1981b) papers. In both cases the outcome was the same: Asset prices appear to be more volatile than is consistent with the efficient-markets model. In setting out the “variance-bounds” theorems (as LeRoy and Porter called them) here, I present the version that we developed rather than Shiller’s version because, as will be seen shortly, much of the subsequent original (as opposed to critical) work on the variance-bounds theorems turned out to be more closely related to our paper than to Shiller’s.

To begin, note that the fair game assumption (3.4) plus the definition of the rate of return imply that  $p_t$  can be written as

$$p_t = (1 + \rho)^{-1}(d_{t+1} + p_{t+1}) - (1 + \rho)^{-1}e_{t+1}, \quad (5.1)$$

where  $e_{t+1}$  is the unexpected component of the one-period return on stock:

$$e_{t+i} \equiv p_{t+i} + d_{t+i} - E_{t+i-1}(p_{t+i} + d_{t+i}) \quad (5.2)$$

(it is assumed throughout that all variables have finite means and variances). Now replace  $t$  by  $t + i$  in (5.1) and multiply both sides by  $(1 + \rho)^{-i}$ :

$$(1 + \rho)^{-i}p_{t+i} = (1 + \rho)^{-(i+1)}(d_{t+i+1} + p_{t+i+1}) - (1 + \rho)^{-(i+1)}e_{t+i+1}. \quad (5.3)$$

Summing (5.3) over  $i$  from zero to infinity and assuming convergence, there results

$$p_t^* = p_t + x_t, \quad (5.4)$$

where

$$p_t^* = \sum_{i=1}^{\infty} (1 + \rho)^{-i} d_{t+i} \quad (5.5)$$

$$\text{and } x_t = \sum_{i=1}^{\infty} (1 + \rho)^{-i} e_{t+i}.$$

Here  $p_t^*$  is the price of stock that would obtain if future realizations of dividends were perfectly forecastable. Following

Shiller, I call  $p_t^*$  the “ex post rational” stock price. The difference between ex post rational and actual price,  $x_t$ , is seen to equal the discounted sum of the unexpected component of future returns.

Taking conditional expectations, (5.4) yields

$$p_t = E(p_t^* | \Phi_t), \quad (5.6)$$

so that  $p_t$  is a forecast of  $p_t^*$  given agents’ information  $\Phi_t$ . Given (5.6), (5.4) says that  $p_t^*$  can be expressed as the sum of a forecast ( $p_t$ ) and a forecast error ( $x_t$ ). Optimal forecasting implies that forecasts and forecast errors are uncorrelated. Uncorrelatedness in turn implies that

$$V(p_t^*) = V(p_t) + V(x_t). \quad (5.7)$$

Because variances—specifically,  $V(x_t)$ —are always nonnegative,  $V(p_t^*)$  is an upper bound for  $V(p_t)$ .

The implied variance inequality,

$$V(p_t) \leq V(p_t^*), \quad (5.8)$$

is attractive because the upper bound depends only on the dividends model and the discount factor, but not on agents’ information sets. Thus, econometric problems aside, rejection of (5.8) unambiguously implies rejection of the martingale model for any specification of agents’ information sets. It will be recalled that, in contrast, under conventional tests rejection could mean either that markets are inefficient or that whatever variable allows prediction of future returns is not in agents’ information sets.

The variance of the (unobservable) forecast error  $x_t$  turns out to be proportional to the variance of the (observable) unexpected component of returns, where the factor of proportionality depends on the discount factor alone. To prove this, begin with the second equation of (5.5), which says that the forecast error  $x_t$  is a discounted sum of the unexpected components of future returns. Taking variances and evaluating an infinite sum,

$V(x_t)$  is seen to be related to  $V(e_t)$  according to

$$V(x_t) = \frac{V(e_t)}{2\rho + \rho^2}, \quad (5.9)$$

assuming that  $V(e_t)$  is constant.<sup>9</sup> This equation will prove useful later.

As LeRoy and Porter observed, equation (5.7) may be shown to imply that the more information agents have, the greater will be the variance of price and the lower will be the variance of discounted returns. To see the first implication, consider some information set  $H_t$  which is less informative than agents' actual information set  $\Phi_t$ . Define  $\hat{p}_t$  to be the price of stock that would obtain under the information set  $H_t$ :

$$\hat{p}_t = E(p_t^* | H_t). \quad (5.10)$$

Here  $\hat{p}_t$ , like  $p_t^*$ , is a fictional stock price series that would obtain if investors had different information than they actually do. In a sense,  $\hat{p}_t$  and  $p_t^*$  are on opposite sides of  $p_t$ : The former is the price that would prevail if agents had less information than they do, while the latter would prevail if they had perfect information. Because of this, one would expect that the relation of  $\hat{p}_t$  to  $p_t$  would be qualitatively similar (in some sense) to the relation of  $p_t$  to  $p_t^*$ . Specifically, one might guess that, just as  $V(p_t^*)$  is an upper bound for  $V(p_t)$ , it might also be true that  $V(p_t)$  is an upper bound for  $V(\hat{p}_t)$ , or, equivalently, that  $V(\hat{p}_t)$  is a lower bound for  $V(p_t)$ . This guess turns out to be correct. The rule of iterated expectations<sup>10</sup> implies that

<sup>9</sup> Derivation of (5.9) makes use of the fact that because the  $e_t$  are serially uncorrelated, the variance of the sum of the  $(1 + \rho)^{-i}e_{t+i}$  terms equals the sum of the variances (the covariance terms drop out). Also used is the fact that the variance of a constant times a random variable equals the constant squared times the variance of the random variable.

<sup>10</sup> Formally, the rule of iterated expectations is used in exactly the same way here as in passing from (3.8) to (3.9) in the derivation of the present-value relation. Its use may be easier to understand intuitively there than here.

$$\hat{p}_t = E[E(p_t^* | \Phi_t) | H_t] \quad (5.11)$$

or

$$\hat{p}_t = E(p_t | H_t), \quad (5.12)$$

using (5.6). Repeating the reasoning presented in the derivation of (5.8) from (5.6), but substituting  $\hat{p}_t$  for  $p_t$ ,  $H_t$  for  $\Phi_t$ ,  $p_t$  for  $p_t^*$ , and  $z_t$  for  $x_t$ , where  $z_t = p_t - \hat{p}_t$ , it follows from the fact that  $V(z_t) \geq 0$  that

$$V(\hat{p}_t) \leq V(p_t). \quad (5.13)$$

Proving the second implication—that the more information agents have, the lower will be the variance of discounted returns—amounts to showing that returns are more volatile under information  $H_t$  than under  $\Phi_t$ . This demonstration is direct. Defining  $\hat{x}_t$  as  $p_t^* - \hat{p}_t$  and observing that (5.7) continues to hold with  $\hat{p}_t$  and  $\hat{x}_t$  replacing  $p_t$  and  $x_t$ , it follows from (5.13) that

$$V(\hat{x}_t) \geq V(x_t) \quad (5.14)$$

which, in light of (5.9), implies

$$V(\hat{e}_t) \geq V(e_t), \quad (5.15)$$

where  $\hat{e}_t$  is the forecast error for returns under the information set  $H_t$ .

This completes the statement of LeRoy and Porter's theoretical results. It is useful to summarize what has been proven. Two basic facts about the martingale model are that the variance of stock price and the variance of returns (multiplied by a constant) add up to the variance of ex post rational price (5.7 and 5.9), and that the variance of the ex post rational price does not depend on how much information agents have. These facts imply that hypothetical variations in agents' information induce a negative relation between the variance of price and the variance of returns: That is, the more information agents have, the higher is the variance of price and the lower is the variance of returns. Thus if agents

have very little information, stock prices are usually not much different from the discounted sum of unconditional expected dividends, a constant. Therefore stock prices have low volatility. In this case realizations of actual dividends come as near-complete surprises, inducing high volatility in actual returns. However, if agents have a great deal of information about future dividends, stock prices have almost as much volatility as discounted actual dividends, the two being highly correlated. In this case significant surprises occur very seldom, implying that returns will usually be nearly equal to their unconditional expectation.

Given that the volatilities of price and returns depend monotonically on how much information agents have, it follows that if we can place bounds on agents' information, these will induce bounds on the variances of price and returns. The obvious choice for the upper bound on agents' information is perfect information, implying that  $V(p_t^*)$  is an upper bound for  $V(p_t)$  and, unhelpfully, that zero is a lower bound for  $V(e_t)$ . Given Fama's definition of weak-form efficiency, the obvious choice of a lower bound on agents' information is that agents know past returns, but nothing else. It follows that  $V(\hat{p}_t)$  is a lower bound for  $V(p_t)$ , and  $V(\hat{e}_t)$  is an upper bound for  $V(e_t)$ . Of the four variance bounds, two are interesting empirically:  $V(p_t^*)$  as an upper bound for  $V(p_t)$ , and  $V(\hat{e}_t)$  as an upper bound for  $V(e_t)$ .

LeRoy and Porter reported the results of two types of tests: bounds tests and orthogonality tests. The null hypothesis in a bounds test is satisfied if the variance of price (or returns) is less than its theoretical upper bound. An orthogonality test, on the other hand, is a test of the implications for variances of the equality restrictions on parameters implied by the orthogonality of forecasts and forecast errors. The null hypothesis of a bounds test

is thus an inequality restriction on parameters, whereas the null hypothesis of an orthogonality test is an equality restriction. We constructed both tests using the estimated parameters of a bivariate autoregression model for prices and dividends (i.e., two regressions in which price and dividends, respectively, were regressed on their own and the other's lagged values). This model, together with the estimated discount factor, implies an estimate of the upper bound  $V(p_t^*)$ . The bounds test compared the estimate of  $V(p_t)$  implied by the bivariate model with the estimated upper bound. The inequality (5.8) was reversed empirically, contradicting the martingale model. Shiller (1981a) reported rejection of a similar inequality.

LeRoy and Porter's orthogonality test was conducted by constructing an estimate of each term of (5.7) from the estimated bivariate model for price and dividends: Instead of using only the information that  $V(x_t)$  is nonnegative, as in the bounds test, the orthogonality test used the fact that  $V(x_t)$  is related to the variance of one-period returns according to (5.9). The test then consists of evaluating the null hypothesis

$$H_0: V(p_t) = V(p_t^*) - \frac{V(e_t)}{2\rho + \rho^2} \quad (5.16)$$

against the alternative

$$H_1: V(p_t) > V(p_t^*) - \frac{V(e_t)}{2\rho + \rho^2} \quad (5.17)$$

Again the martingale model was rejected, although the confidence interval for the null hypothesis turned out to be extremely large.

A major difference between Shiller's and LeRoy and Porter's interpretations of the variance-bounds violations was that Shiller saw them as constituting evidence against efficiency and in favor of the existence of elements of irrationality

in securities pricing, whereas LeRoy and Porter characterized the violations merely as an anomaly requiring explanation (LeRoy 1984). At first it appeared that LeRoy and Porter's reluctance to draw any but the weakest conclusions from the variance-bounds violations was better justified than Shiller's willingness to base strong conclusions on the finding of excess volatility: Shortly after publication of the original studies it became clear that at least some of the variance-bounds tests were subject to severe econometric problems. Focusing on Shiller's tests, Marjorie Flavin (1983) demonstrated that small-sample problems led to bias against acceptance of efficiency. She did this by showing that the estimated variances of both  $p^*$  and  $p$  were biased downward, with the bias in the former estimate exceeding that in the latter. The reason for the downward bias in estimating the variances of  $p^*$  and  $p$  is that the sample means of both  $p^*$  and  $p$  must be estimated, and the usual fixup (reduce degrees of freedom by one) gives an inadequate correction for the induced downward bias in the sample variance to the extent that the underlying series is autocorrelated. Because  $p^*$  is more highly autocorrelated than  $p$ , the downward bias is greater in estimating the variance of  $p^*$  than of  $p$ , which is why the net effect is to bias the test toward rejection.<sup>11</sup> Allan Kleidon (1986a) focused on the econometric consequences of violation of a stationarity assumption. He showed that, if dividends have unit roots, problems similar to Flavin's could persist even in arbitrarily large samples.<sup>12</sup>

<sup>11</sup> Incidentally, Flavin noted a potential remedy (suggested to her by Porter; see Flavin 1983, p. 950) for this problem: estimate variances around zero rather than around the sample mean. It is easy to verify that, under the null hypothesis, the noncentral variances of  $p^*$  and  $p$  obey the same inequality as the variances around their common mean.

<sup>12</sup> Other papers making similar points as Flavin and Kleidon in different ways were Kleidon (1986b)

Flavin and Kleidon's papers gave proponents of market efficiency reason to hope that the apparent evidence of excess volatility was entirely a consequence of flawed econometric procedures. However, the next round of variance-bounds papers made it evident that the variance-bounds violations were here to stay, and that Shiller's willingness to draw far-reaching conclusions based on these violations (and other evidence) may in fact have been justified. Shiller (1988), responding to Kleidon (1986), contended that under realistic parameter values the bias which Kleidon had pointed out was insufficient to explain the magnitude of the violations (in turn, Kleidon, 1988a, took issue with Shiller's criticism).<sup>13</sup> More important, a new round of "second-generation" variance-bounds tests, allegedly free of the biases that had been pointed out in Shiller's original tests, led to the same conclusion of excess volatility. These are surveyed by Gilles and LeRoy (1988a) and West (1988b). N. Gregory Mankiw, David Romer, and Matthew Shapiro (1985), following Porter's suggestion to Flavin (see Footnote 11), tested the variance-bounds inequality using second moments around zero

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and Marsh and Merton (1986). For brief summaries of these papers see LeRoy (1984) or Kenneth West (1988b); for a fairly detailed exposition and evaluation of these papers see Gilles and LeRoy (1988a).

<sup>13</sup> Also, Gilles and LeRoy (1988a) showed that the criticisms leveled at Shiller by Kleidon and Flavin do not extend to LeRoy and Porter. Because LeRoy and Porter used a different trend correction than Shiller did, Kleidon's demonstration that Shiller's tests are invalid if the underlying data are nonstationary does not apply to LeRoy and Porter (however, see LeRoy and William Parke 1988). With regard to Flavin's criticism, Gilles and LeRoy showed that in addition to Flavin's bias toward rejection of efficiency, there exists another bias which skews the test toward acceptance. It is not known which bias is stronger. In establishing that LeRoy and Porter's test has a bias of indeterminate sign, however, we have said no more than would in any case follow immediately from the fact that the variance of expected rational price is a nonlinear function of the underlying parameters.

rather than around the sample means so as to avoid the bias Flavin had pointed out. The particular form of their test was ingenious. Suppose that  $p_t^0$  is any "naive" forecast of  $p_t^*$ —that is, any function of investors' information, however inaccurate as a forecast of  $p_t^*$ . Subtracting and adding  $p_t$ , we have the identity

$$p_t^* - p_t^0 \equiv (p_t^* - p_t) + (p_t - p_t^0). \quad (5.18)$$

If  $p_t$  is an optimal estimator of  $p_t^*$ , the difference between the two will be uncorrelated with investors' information variables, and therefore also with  $p_t - p_t^0$ . Accordingly, we have

$$E(p_t^* - p_t^0)^2 = E(p_t^* - p_t)^2 + E(p_t - p_t^0)^2, \quad (5.19)$$

implying in turn

$$E(p_t^* - p_t^0)^2 \geq E(p_t^* - p_t)^2 \quad (5.20)$$

and

$$E(p_t^* - p_t^0)^2 \geq E(p_t - p_t^0)^2. \quad (5.21)$$

Mankiw, Romer, and Shapiro constructed the sample counterparts of the population parameters in (5.20) and (5.21) and checked the associated inequalities empirically. They found that both were reversed, implying excess volatility of  $p_t$ . They characterized this exercise as an unbiased test of the variance-bounds inequality, although they proved only that the expectation of the sample statistic has the same positive sign as the corresponding population parameter, not that it necessarily has the same magnitude. Further discussion of Mankiw, Romer, and Shapiro is found in Gilles and LeRoy (1988a).<sup>14</sup>

<sup>14</sup> West's (1988a) variance-bounds test is essentially the same as the upper-bound test on return variances (5.15), which LeRoy and Porter derived but did not conduct. However, there are minor differences. West defined the inequality on the variances of innovations in  $p_t$  and  $\hat{p}_t$  rather than on the forecast errors as in (5.15), which turned out greatly to complicate the derivation of the bound. West's innovations version, unlike LeRoy and Porter's, does not hold for all infor-

Finally, John Campbell and Shiller (1988a) reported a variety of tests, including what are effectively variance-bounds tests, of a class of models that include the martingale. Campbell and Shiller's paper shows much greater similarity to LeRoy and Porter's (1981) paper than to Shiller's earlier papers.<sup>15</sup> Unlike Shiller's earlier papers, Campbell and Shiller's is an orthogonality test rather than a bounds test. Also like LeRoy and Porter, Campbell and Shiller tested the martingale model by constructing a bivariate time-series model for stock prices and dividends and determining whether the restrictions on the coefficients of the model implied by the martingale model are satisfied. Specifically, Campbell and Shiller noted that if current stock price is used to construct forecasts of dividends, and if these forecast dividends are discounted back to the present, the result should equal current price. This equality implies testable restrictions on the parameters of the bivariate process for dividends and stock price. Campbell and Shiller found that these restrictions are not satis-

mation sets  $H_t$  and  $I_t$  obeying  $I_t \supset H_t$  (for example, West noted in a footnote that if  $H_t$  contains no information at all, the innovations in  $p_t^*$  will be zero identically, implying that the purported upper bound will be zero). For practical purposes this limitation is inconsequential, however, because if  $H_t$  contains at least past returns, as is always assumed in efficiency tests, then innovations in  $p_t^*$  do in fact coincide with returns, implying that West's upper-bound test on innovations coincides with LeRoy and Porter's upper-bound test on returns.

West, like LeRoy and Porter, Shiller, and Mankiw, Romer, and Shapiro, found empirically that the volatility of returns exceeds its theoretical upper bound, indicating rejection of the martingale model.

<sup>15</sup> However, Campbell and Shiller's tests were superior to LeRoy and Porter's for several reasons. Most important, by postulating an underlying log-linear process and then linearizing, they eliminated the need for trend correction, therefore avoiding any error introduced by faulty trend removal (see Gilles and LeRoy 1988a for exposition of LeRoy and Porter's trend removal algorithm; LeRoy and Parke 1988 showed that this algorithm induces a downward trend in the supposedly trend-adjusted data).

fied, actual prices having about twice the standard deviation of the constructed price series.

The second-generation variance-bounds tests, like the first-generation tests, found excess volatility.<sup>16</sup> This outcome conflicted with the early work reviewed by Fama (1970): How can it be that, if stock price volatility is excessive, successive daily or weekly stock returns are uncorrelated? This discrepancy posed a major analytical problem. Several explanations resting on sophisticated econometric arguments were proposed before it was recognized that there is a simple answer. The central point, inadequately recognized at first, is that the variance-bounds inequalities are implications of return orthogonality conditions just as conventional efficiency tests are. To see this, write (5.4) and (5.5) as

$$\begin{aligned} p_t^* &= p_t + x_t \\ &= p_t + \sum_{i=1}^{\infty} (1 + \rho)^{-i} e_{t+i}, \end{aligned} \quad (5.22)$$

so that the restriction on which the variance-bounds theorems are based—orthogonality of  $p_t$  and  $x_t$ —says that a particular weighted average of past returns (which is all that  $p_t$  is) must be uncorrelated with a different weighted average of future returns. Excess volatility means that, empirically, these weighted averages of returns are negatively correlated—otherwise  $V(p_t)$  could not exceed  $V(p_t^*)$ . The crucial difference between conventional efficiency tests and variance-bounds tests is this: The former tests the orthogonality of returns over

short intervals (for example, successive daily or weekly returns), whereas the variance-bounds theorems test the orthogonality of a smooth average of past returns over a period of years and a similar smooth average of future returns.

The obvious way to evaluate this explanation for the differing results of variance-bounds tests and the conventional return autocorrelation tests is to estimate directly the correlation between average returns over the interval from  $t - T$  to  $t$ —call this  $r_{t-T,t}$ —with  $r_{t,t+T}$  for various values of  $T$ . Fama and French (1988a) conducted exactly this exercise. They found a U-shaped pattern: For  $T$  of one year the correlation was essentially zero. For  $T$  on the order of three to five years about 35 percent of the variation of  $r_{t,t+T}$  is explained by  $r_{t-T,t}$  with the correlation being negative as expected. For  $T$  of ten years the correlation reverts to approximately zero. Fama and French's finding that five-year returns have a large forecastable component is exactly what the variance-bounds violations would lead one to expect. The simplicity of Fama and French's test and its outcome provide independent corroboration of the econometric soundness of the variance-bounds tests.

The question becomes: What sort of model would generate the U-shaped pattern in the return autocorrelations that Fama and French reported? Shiller (1981a, 1984) and Lawrence Summers (1986) proposed that instead of modeling stock price (with dividends added in) as a martingale, analysts should consider assuming that price comprises a random walk plus a fad variable, where the latter is modeled as a slowly mean-reverting stationary series. This specification, simple as it is, generates exactly the forecastability pattern required. That returns over short intervals are approximately uncorrelated is a basic, but not adequately known, fact about (a wide class

<sup>16</sup> LeRoy and Parke's (1988) paper is an exception. Our purpose was to construct a bounds test of the inequality (5.8) that is valid if dividends follow a geometric random walk. We found that the variance of stock prices is lower than the theoretical upper bound, conforming to the variance-bounds inequality. However, LeRoy and Parke concluded that this evidence in favor of the martingale model is extremely weak. This is so because bounds tests inherently have lower power than orthogonality tests.

of) stochastic processes (Christopher Sims 1984). Intuitively, the reason the return from  $t - T$  to  $t$  is for small  $T$  approximately uncorrelated with the return from  $t$  to  $t + T$  is that the contribution of the drift term to the variation of price is proportional to  $T^2$ , whereas the contribution of the dispersion term is proportional to  $T$  (recall from econometrics the analogous fact that mean square error is the sum of variance plus bias squared). For small  $T$  the dispersion dominates the drift, implying that the return autocorrelations for (almost) any stationary stochastic process look like those of a fair game (zero). Similarly, return autocorrelations over long horizons approach zero because the random walk term dominates the mean-reverting component of price. In between, however, a negative correlation is to be expected. This occurs because for intermediate values of  $T$  high returns from  $t - T$  to  $t$  imply a positive value (on average) for the fad variable at  $t$ . Mean reversion implies that the fad will probably have diminished by  $t + T$ , implying an abnormally low return from  $t$  to  $t + T$ . The extent of the induced negative correlation between  $r_{t-T,t}$  and  $r_{t,t+T}$  depends on how quickly fads die out and on the respective error variances.

The preceding discussion exaggerated the similarity between the variance-bounds and return autocorrelation tests. The problem lies with the assertion following equation (5.22) that current price is a weighted average of past returns, so that variance-bounds tests (which are based on the orthogonality of price and future returns) and return autocorrelation tests (which are based on the orthogonality of past and future returns) are essentially equivalent. In fact, price is a nonlinear function of past returns; even if the function relating current price to past returns is linearized, the weights depend on dividends, which are random

and correlated with returns. It is not yet known whether this qualification to the assertion above that variance-bounds and return autocorrelation tests are essentially similar is important empirically.

A third type of test, which can be interpreted as a hybrid of variance-bounds and return autocorrelation tests, determines directly whether price, or some variable closely related to price such as the dividends-price ratio, predicts future returns. These tests usually lead to strong rejection of the martingale model (Fama and French 1988b; Campbell and Shiller 1988a, 1988b).

The variance-bounds, return autocorrelation, and price-return orthogonality tests constitute three ways to test the martingale model. A fourth way to test for mean reversion is to use variance ratios (John Cochrane 1988).<sup>17</sup> The variance ratio is defined as the variance of  $k$ -period returns divided by the variance of one-period returns, and also by  $k$ . Under a random walk the variance ratio should equal unity for any value of  $k$ . However, James Poterba and Summers (1988) showed that the variance ratios declined with  $k$ , indicating the presence of a mean-reverting component.

The presence of a mean-reverting component in stock prices implies substantial forecastability of intermediate-term returns, and therefore also (by the variance-bounds theorem) substantial differences between price and "fundamentals," meaning by the latter the (rational) expectation of ex post rational price. Thus there is no inconsistency between essentially unforecastable short-term returns and wide discrepancies

<sup>17</sup> The material under discussion was anticipated by Holbrook Working. In his (1949) paper, Working proposed that statistical series be modeled as the sum of a random walk and a stationary series, and explicitly proposed the use of variance ratios to determine the relative importance of each component.

I am indebted to Frank Diebold for this reference. See also Diebold (1988).

between price and fundamental value. This result is best seen as documenting a pronounced bias in our psychological metric—even though there is no question that complete unpredictability of short-term returns implies exact equality between price and fundamental value (speculative bubbles aside), the result here is that a “surprisingly small” degree of forecastability of short-term returns is consistent with a “surprisingly large” discrepancy between price and fundamental value (Shiller 1984; Summers 1986).

Shiller’s suggestion that asset prices be modeled as the sum of a random walk and a mean-reverting process is seen to give a parsimonious model that predicts (1) near-zero autocorrelations for daily and weekly returns as reported in the early efficient markets literature, (2) negative autocorrelations for returns over holding periods of several years, and (3) variance-bounds violations. Unfortunately for this tidy story, however, several recent studies have raised questions about the validity of the purported facts for which the mean-reversion model gives a unified explanation. Andrew Lo and A. Craig MacKinlay (1988) found that weekly and monthly stock returns had positive autocorrelation coefficients on the order of 30 percent, contradicting both the finding of approximately zero autocorrelation reported in the early efficient markets literature and the prediction of approximately zero autocorrelation from the mean-reversion model. Moreover, several studies have questioned Fama and French’s conclusion that returns are significantly negatively autocorrelated over three- to five-year holding periods. Myung Jig Kim, Charles Nelson, and Richard Startz (1988), for example, found evidence of mean reversion only in data sets that include the 1930s—for the post-World War II period they found no evidence of negative return au-

tocorrelation. This finding, together with the fact that the most recent studies continue to conclude that the variance-bounds inequalities are violated empirically, raises further questions about whether the variance-bounds violations are empirically the same thing as mean reversion. At this writing these questions remain unresolved.

## VI. *Nonmartingale Models*

Documenting the existence of systematic empirical departures from the martingale model may seem to be entirely beside the point. After all, Samuelson’s derivation of the martingale model assumed risk neutrality, whereas in fact people are risk-averse. So why should one be surprised when the martingale model does not work empirically? Aware of this point, analysts were led to look for an analogue to the martingale model that would remain valid if agents were risk-averse. It has not proved difficult to formulate such extensions theoretically, but, as will be reported in this section, it has turned out to be very difficult to correlate the departures from the martingale that these theories lead one to expect with the departures that one sees in the data. Therefore allowing for risk aversion does not in practice go far toward resolving the empirical puzzles that attend the martingale model. Consequently, not much is lost empirically by ignoring risk aversion, which is why that was done in the preceding sections.

Samuelson was not aware that his derivation of the martingale model depended critically on the assumption of risk neutrality: He conjectured that risk aversion could be handled simply by including a risk premium in the discount factor used to calculate present values. However, it is easy to see why asset returns will not generally be a fair game if agents are risk-averse. Suppose that

risk covaries positively over time, so that big price changes (positive or negative) are likely to be followed by big changes and small changes by small changes.<sup>18</sup> If agents are risk-averse, they will hold risky assets only if expected returns vary so as to compensate them for these changes in risk. One would expect that returns therefore will in general be partly forecastable: If the current realization of  $\Phi_t$  implies high risk over the near future, should it not also imply high expected return?

To formalize this reasoning, one would like to have in hand a model that allows for risk-averse agents and that can generate an intertemporal sequence of equilibrium prices and returns. The problem in incorporating risk aversion into efficient-markets theory was that as of about 20 years ago the only equilibrium asset pricing model extant in which risk and risk aversion were adequately handled was the equilibrium version of the CAPM of William Sharpe (1964), John Lintner (1965), and Jan Mossin (1966). (General analytical frameworks like the Arrow-Debreu contingent claims setup are, of course, capable in principle of dealing with risk aversion, but unless suitably restricted, are too general to be of much use in applied work.) The CAPM takes the mean and variance of next-period price as exogenous and determines current asset prices as those prices that just induce agents to bear existing risk willingly. Price, in other words, equals discounted expected return less a correction that reflects risk and risk aversion. Now, the fact that next-period expected price and the variance of next-period return are given exogenously in the CAPM means that even though the CAPM determines the current risk premium endogenously, it does not give a complete

general equilibrium determination of returns on multiperiod assets such as stock. In multiperiod models it makes little sense to determine current risk premia endogenously while taking future risk premia, as embodied in expected next-period price, as exogenous.

What was needed was a model that would generate price from the probability distribution of next-period returns, and that would simultaneously characterize agents' probability distribution of next-period returns in a manner that is consistent with agents' expectations that price will be determined in a similar fashion when the next period arrives. This required a new concept of equilibrium. In my (1971) dissertation and (1973) paper, equilibrium was defined to consist of a single function simultaneously mapping current dividends into current price and next-period dividends into next-period price such that if agents have rational expectations about future dividends and optimize, then markets clear for any level of dividends.<sup>19</sup> The solution method then was to specify a general class of price functions and derive the appropriate equilibrium condition under the assumption that both current and next-period price conform to this function. The equilibrium price function was that for which this equilibrium condition is satisfied *as an identity in dividends* (it would contradict the exogeneity of dividends if markets failed to clear for some values

<sup>18</sup> Empirical evidence supports this specification (for example, Poterba and Summers 1986).

<sup>19</sup> Merton (1973) reported an intertemporal extension of the CAPM at the same time. Neither LeRoy (1973) nor Merton (1973) provided a full general equilibrium analysis. For example, in both models the risk-free interest rate is taken as exogenous rather than determined from preferences and technology. However, the former paper pointed more clearly in the direction of general equilibrium by contributing the idea that equilibrium can be characterized as a stable function linking exogenous state variables to asset prices, an idea not found in the latter paper. General equilibrium versions of LeRoy's and Merton's models were provided by Lucas (1978) and Cox, Ingersoll, and Stephen Ross (1985), respectively.

of dividends, hence the need for the equilibrium condition to hold as an identity).

As it turned out, the identical concept, which came to be called *rational expectations equilibrium*, was being developed at the same time in the macroeconomics literature. Further, exactly the same “undetermined coefficients” solution method—seeking coefficients such that the equilibrium condition holds as an identity—came into use in linear rational expectations macroeconomic models (Robert Lucas 1973).

In the intertemporal version of CAPM just described, the conditional expected return per dollar fluctuates over time as dividends change. Because dividends are autocorrelated, conditional expected returns are autocorrelated as well, implying that actual returns are partly forecastable. This forecastability goes contrary to the martingale model. It is, however, consistent with equilibrium because equilibrium stock prices are such that the fluctuations in risk per dollar invested induced by dividends fluctuations correlate with the fluctuations in expected returns so as to leave agents just willing to hold existing assets. In other words, even though the existence of serial dependence in conditional expected returns implies that different formulas for trading bonds and stock will generate different expected returns, because of risk, these alternative trading rules are utility-decreasing relative to the optimal buy-and-hold strategy. Of course, if as a special case it is assumed that agents are risk-neutral, these effects disappear and the martingale model obtains.

These considerations made clear that, in general, risk aversion will lead to departures from the martingale model. It does not follow from this that risk neutrality is the only case in which conditionally expected returns will be constant. In his (1977) comment on my (1973) pa-

per, James Ohlson showed that if dividend growth rates are serially independent and agents have constant (but not necessarily zero) relative risk aversion, then the conditional expected rate of return on stock will be constant and returns will be unforecastable. In a sense Ohlson’s case was very specialized because if agents are risk-averse the martingale requires restrictions both on return distributions and risk aversion, rather than just the latter as in the risk-neutrality case. However, neither of Ohlson’s assumptions is as wildly at odds with reality as the assumption of risk neutrality. The practical implication of Ohlson’s result is that even though the conditions under which he derived an exact martingale are restrictive, the assumption that these conditions are satisfied to a tolerable approximation may not be so implausible.

The foregoing discussion has concerned asset prices that are or are not martingales with respect to the probabilities that agents actually have—more precisely, with respect to the probabilities that, under the axioms of choice under uncertainty, are implicit in agents’ orderings over portfolios. Suppose, however, that we start from the other end by assuming that asset prices always follow martingales with respect to some probabilities. It is easy to show that there always exist such probabilities: They are readily derived by repackaging the Arrow-Debreu prices that underlie any equilibrium (Stephen Ross, 1977, was the first clearly to appreciate this point; see also Ross 1978; J. M. Harrison and D. M. Kreps 1979; Harrison and S. R. Pliska 1981).<sup>20</sup> These probabilities are

<sup>20</sup> As a sidelight, it is interesting to note that in the finance literature Ross’ (1977) paper is almost universally incorrectly referred to as having been published in 1976. This practice was started by Ross, who deliberately misdated references to this paper in order to encourage readers interested in the arbitrage pricing theory to read it before taking on his more difficult, less intuitive, and more rigorous (1976)

called *risk-neutral probabilities* in the finance literature because asset prices can always be expressed as discounted expected returns—as would be appropriate if agents were risk-neutral—if the expectation is taken with respect to these probabilities rather than the probabilities implicit in agents' orderings of portfolios. In other words, asset prices can be analyzed as if agents are risk-neutral, but take expectations with respect to the risk-neutral probabilities rather than their actual probabilities. The risk-neutral probabilities coincide with actual subjective probabilities if agents are in fact risk-neutral—otherwise they contain in addition adjustments for risk aversion.

The fact that there always exist martingale representations of asset prices is very convenient for theoretical work. It is also useful in such applied work as the pricing of redundant assets, the central problem of applied finance.<sup>21</sup> For the study of capital market efficiency, however, this line of research is not directly relevant. Given that market efficiency includes rational expectations (Fama 1976a), the subjective probabilities implied by agents' orderings over portfolios must be identifiable with the objective probabilities specified to obtain in the model under discussion. In the present setting it is therefore of no help to know that returns are always fair games with

respect to some fictional probability measure that has no directly observable counterpart.

Both my (1973) paper and Ohlson's (1977) comment were essentially counterexamples, the former to the proposition that capital market efficiency necessarily implies martingales, and the latter to the proposition that risk neutrality is required for martingales. As such, there is nothing wrong with the fact that they are highly specialized. For general analysis, however, more powerful methods are needed so as to derive equilibria in more general settings. These were supplied in Lucas' (1978) paper. (Related material, developed independently, was presented in Douglas Breeden, 1979, and in John Cox, Jonathan Ingersoll, and Stephen Ross' 1985 paper, which was circulating as a working paper in the mid-1970s.) Lucas assumed that identical infinitely lived agents maximize the utility function  $\sum (1 + \rho)^{-t} U(c_{t+\tau})$ , which allows for risk aversion ( $U$  strictly concave) as well as risk neutrality ( $U$  linear). Using dynamic programming, Lucas demonstrated the existence and uniqueness of a pricing function similar to that of my (1973) paper. Even though the equilibrium pricing function is nonlinear in Lucas' model and is usually not amenable to closed-form representation, many of its properties can be derived analytically.

In Lucas' model equilibrium prices satisfy the stochastic Euler equation

$$p_t U'_t = (1 + \rho)^{-1} E_t(p_{t+1} + d_{t+1}) U'_{t+1} \quad (6.1)$$

Here the marginal utilities  $U'_t$  and  $U'_{t+1}$  are evaluated at the endowment, reflecting the equilibrium condition that consumption must equal the endowment in an exchange economy. To understand the Euler equation (6.1), suppose that an investor is considering selling one share of stock and consuming the proceeds. The utility gain is  $p_t U'_t$ . Assuming

treatment of the arbitrage pricing theory. The (1977) paper was actually written in 1971. Most subsequent writers on the arbitrage pricing theory followed Ross' lead in dating the (1977) paper as 1976. An exception is Ingersoll (1987), who, going Ross one better, referred to the publication date of Ross' (1977) paper as 1975.

<sup>21</sup> For example, Cox, Ross, and Rubinstein (1979) presented an intuitive derivation of the Black-Scholes model of option pricing using martingale representations. Specifically, they derived risk-neutral probabilities from the assumed price of stock and the interest rate, and then calculated the price of an option on the stock by discounting its expected return, where the expectation was calculated using the risk-neutral probabilities.

that consumption at dates other than  $t$  and  $t+1$  remains unchanged, the budget constraint implies a drop in consumption at  $t+1$  of  $p_{t+1} + d_{t+1}$ . The right-hand side of (6.1) gives the expected utility cost of the decline in consumption, discounted back to  $t$ . If the investor is at an optimum, the utility gain at  $t$  must just equal the expected utility loss at  $t+1$ .

Equation (6.1) agrees with the martingale model

$$p_t = (1+\rho)^{-1} E_t(p_{t+1} + d_{t+1}) \quad (3.4)$$

except that in (6.1) price at  $t$  is weighted by current marginal utility and next-period price by next-period marginal utility. Under risk neutrality  $U'_t$  and  $U'_{t+1}$  are equal to a common constant, so (6.1) and (3.4) agree. Lucas therefore again pointed out that martingales generally would obtain only under risk neutrality. Also, Lucas' work made clear that the connection between risk neutrality and martingales obtains without qualification only in exchange economies. In production economies in which corner solutions are possible, prices will reflect the technology as well as preferences whenever corner solutions occur, so risk neutrality by itself is insufficient to generate the martingale. This qualification was not stated in Samuelson's paper or mine. In production economies like that of William A. Brock (1982) in which the technology excludes corner solutions, on the other hand, risk neutrality is sufficient for the martingale model without qualification.

An immediate payoff of Lucas' model was that it provided an analytical framework in which to determine whether the violations of the variance-bounds theorems reflect the unrealism of the underlying risk-neutrality assumption. It was shown by LeRoy and C. J. LaCivita (1981), Grossman and Shiller (1981), and Ronald Michener (1982) that in Lucas'

model there is a general presumption that the more risk-averse agents are, the more volatile asset prices will be. The argument is very simple. In an economy with no production, agents must consume their randomly fluctuating endowment (taking account of capital and intertemporal production would complicate the story, but would not alter its fundamentals). The price system must induce them to do so willingly. Highly risk-averse agents, however, will want very much to smooth their consumption streams over time. This they cannot do in the aggregate. To induce them not to save (by buying stock) in periods of prosperity, and not to dissave (by selling stock) in periods of shortage, stock prices must be very high in periods of prosperity and very low in periods of shortage. Thus the more risk-averse agents are, the more volatile equilibrium stock prices will be. However, this argument is not completely general. As Ohlson (1977) showed, if dividend growth rates are independently distributed, then prices will follow a martingale for any degree of (constant relative) risk aversion.<sup>22</sup> In such settings risk aversion cannot be the explanation for asset price volatility in excess of that implied by the martingale model. See Kevin Salyer (1988) for a general discussion of price volatility in models like Ohlson's.

These theoretical developments raised the possibility that the variance-bounds violations (or, equivalently, the partial forecastability of intermediate-term returns) reflected departures from the mar-

<sup>22</sup> Ohlson's model is not a special case of Lucas' because dividend levels are nonstationary in the former. However, Mehra and Prescott (1985) formulated a general framework analogous to Lucas' except that dividend growth rates rather than levels are stationary. Ohlson's model is a special case of Mehra and Prescott's. In Mehra and Prescott's setting there is no simple connection between risk aversion and asset price volatility (Salyer 1988).

tingale model induced by risk-aversion. Grossman and Shiller (1981) and Lars Hansen and Kenneth Singleton (1982, 1983), among others, attempted to determine whether asset price fluctuations could be interpreted as reflecting risk-averse agents' attempts to smooth consumption over time. Results to date have been disappointing (see Singleton 1987 for a survey of this literature). The problem is that consumption-based models of asset pricing, at least in their simplest form, imply that stock returns will be positively and strongly correlated with consumption growth, and this turns out not to be true empirically. Therefore, introducing risk aversion does not generally improve the performance of the predicted price series much in tracking actual prices relative to the martingale model. However, this pessimistic evaluation is not universally shared: Kleidon (1988b), for example, expressed doubt that the variance-bounds violations reflect anything deeper than an unjustified assumption of a constant rate of time discount (and perhaps, given the econometric problems, not even that). Also, more sophisticated representations of risk aversion (for example, George Constantinides' 1988 non-time separable utilities) may improve the results.

In fact, rather than resolving the difficulties attending the martingale model, passing to the consumption-based asset pricing model has given rise to new problems. Rajnish Mehra and Edward Prescott (1985), studying a representative-agent model, showed that no reasonable specification of agents' rates of time preference and risk aversion was able to generate real returns on bonds as low as those measured, while at the same time generating real returns on stock as high as those measured. It is true that Mehra and Prescott's model is highly simplified, but the dramatic failure of the consumption-based model of asset prices to ex-

plain the equity premium cannot be easily dismissed.<sup>23</sup>

There remains the fads model proposed by Shiller (1981a, 1984). Here, of course, we are dealing with an alternative to the efficient capital markets model, not with a modification of it. Most economists are extremely reluctant to resort to fads models because doing so would involve relaxing the stable-preferences assumption that many economists regard as an indispensable part of their outlook (George Stigler and Gary Becker 1977). In any case, pending a theory of what causes fads to come and go or a specification of potential phenomena that would be inconsistent with a fads model, it is not clear that anything is gained by characterizing an unexplained variation in asset prices as a fad. One is reminded of Robert Solow's (1957) labeling as technological change the unexplained residual in output growth after allowing for increase in inputs: Precisely because the residual is unobserved, one is free to accept or reject the interpretation; nothing is at stake either way. Advocacy of a fads model is perhaps best interpreted as a statement of belief that the most fruitful avenues of future research will involve social or cognitive psychology, rather than as referring to any well-formed model that is now available.

## VII. *Other Evidence*

The discussion of empirical evidence in the preceding two sections was narrowly concentrated on the time structure of asset returns and such closely related topics as variance bounds. This restricted focus was adopted to avoid spreading the discussion too thin. But there is no point in basing conclusions on only a small sub-

<sup>23</sup> Proposed resolutions to Mehra and Prescott's equity premium puzzle have been suggested by Rietz (1988), Constantinides (1988), and Nason (1988).

set of the available evidence. This section briefly acknowledges the existence of other types of evidence that bear on the question of capital market efficiency. For more extensive reviews, see the papers in Elroy Dimson (1988) (especially Donald Keim 1988), G. William Schwert (1983), Richard Thaler (1987a, 1987b, 1988), Josef Lakonishok and Seymour Smidt (1988), and Ross Clark and William Ziemba (1987).

There always existed a subculture within the finance profession that rejected the majority conclusion in favor of efficiency. These heretics pointed to the "P-E anomaly": stock with low price-earnings ratios appeared systematically to outperform those with high price-earnings ratios (Francis Nicholson 1968; Sanjoy Basu 1977, 1983; Marc Reinganum 1981; David Dreman 1982). Recently Werner DeBondt and Thaler (1985, 1987) documented the related proposition that "losers"—stocks that had recently undergone large drops—appear systematically to generate higher returns than winners. Another similar result is that the ratio of price to book value is a predictor of returns (Barr Rosenberg, Kenneth Reid, and Ronald Lanstein 1985). This evidence of systematic overreaction to current information may be related to the excess volatility documented in the variance-bounds literature. Also, the apparent success of some investors—Warren Buffett—and some investment services—Value Line—in outperforming the market is difficult to reconcile with capital market efficiency. Proponents of market efficiency have always minimized such evidence. It is true that the correspondence of the Value Line stock rankings with subsequent performance appears too strong to have occurred by chance if Value Line is thought of as a single prespecified observation. But suppose that Value Line is thought of as the best performing of  $n$  investment

advisory services. If  $n$  is large, one would expect the best of  $n$  services to perform extremely well purely by chance. And surely the population of investment services is large, especially if, as is appropriate, one counts the services that drop out because of a poor track record.<sup>24</sup>

The advent of cheap computing and large financial data bases brought new anomalies. The consensus now is that the anomalies pose a serious problem which cannot be shrugged off, as had been presumed earlier. The best known of these is the "January effect" (see Thaler 1987a or Clark and Ziemba 1987 for surveys). Michael Rozeff and William Kinney (1976) found that stock returns averaged 3.5 percent in January, while other months averaged 0.5 percent, a pattern which, being nonstationary, is inconsistent with a martingale. Subsequent studies (for example, Reinganum 1981, 1982, 1983, and Richard Roll 1983) replicated and refined the January effect. Rolf Banz (1981) found that small firms have higher returns than is consistent with their riskiness. Keim (1983) showed that the small-firm effect and the January effect may be the same thing: The January effect appears only in samples that include and give equal weight to small and large firms (see also Lakonishok and Smidt 1988 and

<sup>24</sup> One is reminded of the story about an entrepreneur who wanted to sell recommendations to football bettors. He divided a list of 16,000 potential customers into two sublists of 8,000 names each. He informed the first sublist of his prediction that the Redskins would beat the 49ers on Sunday, while the second sublist was given the reverse prediction. When the Redskins did beat the 49ers, he threw out the second list. The next week he divided the first list into two new sublists of 4,000 names each. He reminded both that he had correctly predicted the outcome of last week's game. For the first sublist he picked the Giants over the Eagles; the second sublist received the reverse prediction. After four weeks he was left with 1,000 names. He then wrote to these reminding them that he had correctly called the past four games, and expressed a willingness to tell them the outcome of the next game in exchange for \$10,000.

Mustafa Gultekin and N. Bulent Gultekin 1987), as opposed to samples that weight firms by value.

Not only is the January effect an anomaly in its own right, but it contaminates the one regularity that finance theory (specifically the CAPM) predicts should be found in the data: the relation between risk and expected return. Fama and James MacBeth (1973) and others had earlier confirmed the CAPM (or commonsense) prediction that riskier stocks should earn higher average returns. Seha Tinic and Richard West (1984) were motivated by the findings just summarized to analyze the monthly patterns in the risk-return relation. Incredibly, they found that the risk-return trade-off occurs entirely in January: They could not reject the hypothesis that during the other eleven months investors are not compensated at all for bearing risk (however, see also Tinic and West 1986).

The January effect is only one of several calendar-based anomalies that have been unearthed in recent years. Another is the "weekend effect" (Frank Cross 1973; French 1980; Keim and Robert Stambaugh 1984; Lakonishok and Maurice Levi 1982; R. Rogalski 1984; Jeffrey Jaffe and Randolph Westerfield 1985; Lawrence Harris 1986), which finds that stock returns are on average negative from the close of trading on Fridays to the opening of trading on Mondays. A similar effect exists for bonds (Michael Gibbons and Patrick Hess 1981). Further, we have the "Wednesday effect": In 1968 the New York Stock Exchange was closed on Wednesdays in order to ease the paperwork backlog at brokerage houses. French and Roll (1986) found that the volatility of prices from Tuesday to Thursday was lower than over other two-day intervals, suggesting that prices fluctuate more when markets are open than when they are closed. Because, presumably, as

much news about fundamentals is generated on Wednesdays as other weekdays, this "Wednesday effect" suggests that it is the trading process itself rather than news about fundamentals that generates price changes.<sup>25</sup> The Wednesday effect, like the January effect and assorted other calendar effects, appears difficult to reconcile with the martingale model. Finally, Robert Ariel (1987) showed that returns are positive on average only in the first half of the calendar month.

It is difficult to know how seriously to take these asset pricing anomalies. As Robert Merton (1987) and many others have noted, there is a problem of selection bias in these results. An analyst who conducts an empirical study investigating a purported correlation between stock returns and the stage of the moon, for example, and finds no correlation is unlikely to succeed in reporting this result in the journals. Therefore the published literature is skewed toward interesting, that is, anomalous, results, and away from boring confirmations of the absence of anomaly. A related problem is that anomalies are typically tested on the same data on which they are discovered, and analysts frequently construct their classifications so as to maximize the anomalous nature of the finding. For example, Ariel (1987) included the last day of the preceding month along with the first half of the current month because returns on the last day of the month are very high, implying an increased reported disparity between returns in the first half of the month and returns in the second half (see Lakonishok and Smidt 1988 for discussion).

Different types of evidence bear more directly on the assumptions of rationality and rational expectations that underlie

<sup>25</sup> However, see Slezak (1988) for an alternative explanation for the Wednesday effect which is consistent with (a sophisticated version of) the efficient markets model.

market efficiency (and, consequently, are less closely related to martingales). For example, there is some evidence that asset prices are subject to "winner's curse" (Edward Miller 1977; Stuart Theil 1988; Kenneth Hendricks and Robert Porter 1988; S. Michael Giliberto and Nikhil Varaiya 1989). If agents have different opinions about the value of some asset to be sold at auction, and if their bids are naively based on these opinions, the winner will be the bidder with the most inflated estimate of the asset's value. On average, winners will overpay. Winner's curse is inconsistent with full rationality: Each bidder's strategy should make allowance for the possibly biased nature of his own appraisal of value (R. Preston McAfee and John McMillan 1987). Richard Thaler (1988) interpreted the finding of Walter Mead, Asbjorn Moseidjord, and Philip Sorensen (1983, 1984) that winning bidders on wildcat offshore oil leases overpay on average as evidence of winner's curse.<sup>26</sup>

A very striking piece of evidence conflicting with market efficiency is the high volume of trade on organized securities exchanges. For some reason this is seldom listed in the finance literature as

one of the major anomalies of efficient capital markets. Paul Milgrom and Nancy Stokey's (1982) paper and Jean Tirole's (1982) paper (see also Harrison and Kreps 1978) showed that rational agents with asymmetric information will not offer to trade securities based on a naive interpretation of their private information. Rather, they will take account of the fact that if they are able to consummate a trade, that will occur because some other agent with different but perhaps equally accurate information is willing to take the other side of the trade. Such transactions, being a zero-sum (or negative-sum, if brokerage charges and costs of information acquisition are included) game, are pure risk uncompensated by positive expected gain. Risk-averse agents will reject such trades. Contrary to the prediction of Milgrom, Stokey, and Tirole's model, large numbers of investors forsake the buy-and-hold strategy that efficient-markets theory dictates in favor of actively betting their information against other investors' information. Of course, it is not the fact that the volume of trade is positive that causes the problem: Milgrom, Stokey, and Tirole's theorem depends on assumptions that are not even approximately satisfied empirically—for example, that agents have common priors (see Hal Varian 1985, 1989 for analyses of models in which agents have heterogeneous priors) and that the pretrade allocation of securities is Pareto-optimal. Given market incompleteness, rational investors will want to buy or sell securities to provide for or finance large expenditures or adjust risk exposure. However, it is clear that only a small percentage of stock market trades can be rationalized in this way. The majority of trades appear to reflect belief on the part of each investor that he can outwit other investors, which is inconsistent with common knowledge of rationality.

The Milgrom, Stokey, and Tirole re-

<sup>26</sup> However, Thaler did not note that these authors suggested an explanation different from winner's curse for the low returns to successful bidders on wildcat leases. The successful bidder on a wildcat lease—a lease for which there exists no drilling data that would indicate potential productivity—acquires valuable proprietary information about oil reserves in neighboring tracts. When drainage leases—leases on tracts adjoining tracts from which oil is already being extracted—on these neighboring tracts come up for auction, the holder of the wildcat lease can modify his bid in light of this privileged information. Mead, Moseidjord, and Sorensen (1983, 1984) showed that, as this argument leads one to expect, returns to successful bidders on drainage leases were exceptionally high when these bidders were those who had already leased neighboring tracts. Mead, Moseidjord, and Sorensen suggested that the low returns to successful bidders on wildcat tracts are consistent with market efficiency when allowance is made for the value of the information gained by the successful bidder about neighboring tracts.

sult poses a problem: Either analysts of financial markets must ignore the existence of high volumes of securities trading or they must incorporate irrationality into their models, at least when analyzing complete-market environments. Given the traditional hostility toward irrationality as manifested, for example, in Shiller's fad variables, neither alternative is attractive. Fortunately, Fischer Black (1986) came to the rescue. By renaming irrational trading "noise trading" Black avoided the I-word, thereby sanitizing irrationality and rendering it palatable to many analysts who in other settings would not be receptive to such a specification. The economic effects of noise traders is now an active research area (Campbell and Albert Kyle 1986).

Inasmuch as efficient-markets theory attributes asset price changes exclusively to information about fundamentals, it implies that returns should be explainable *ex post* by fundamentals. Curiously, financial economists have until recently displayed a marked lack of interest in testing this implication of market efficiency, strongly preferring instead to concentrate their attention on testing the martingale implication that returns should not be explainable by fundamentals *ex ante* (see Summers 1985 for discussion). However, two recent studies by Roll are distinguished exceptions. After persuasively arguing that information on weather in Florida—specifically, information bearing on the probability of a freeze, which would adversely affect the orange crop—should be the dominant influence on orange juice futures prices, Roll (1984) showed that weather information could explain empirically only a small fraction of the variation in these prices. He could not identify any variable that explained the remainder of the variation. In his presidential address to the American Finance Association, Roll (1988) continued along the same lines, showing empirically that it is difficult to

explain *ex post* more than a small fraction of the variation in individual stock prices, even using data like industry average prices and market price indices as explanatory variables.

It would seem almost self-evident that the recent wave of leveraged buyouts provides strong evidence against market efficiency: The astronomical fees to investment bankers that these mergers generate are difficult to reconcile with any nontautologous version of market efficiency, as are the stock price gyrations that accompany leveraged buyouts. Mergers themselves, of course, are consistent with efficiency; indeed, they are implied by efficiency if they result in synergies in operations or serve to remove bad management. However, most students of corporate takeovers believe that such effects are of secondary importance. On Roll's (1986) account, takeovers may be consistent with market efficiency even if motivated solely by the "hubris" of the acquiring group. Roll interpreted the stock price declines that typically follow takeovers as validating the pretakeover valuation of the firm on the part of the large majority of investors, and as invalidating the runup that occurs upon takeover. The majority of traders, then, value the firm correctly; only the acquirer is led by "hubris" to overpay. Roll argued from this that "the market," which he identified with the majority of traders, is efficient. This argument will not do at all. The simplest efficient-markets reasoning implies that no systematic pattern of price decline should occur in the wake of a publicly known event like a successful takeover. Further, as proponents of market efficiency themselves insist in other contexts, the market price of a company is the price that the firm trades at, no more and no less. Even (in fact, especially) within the logic of efficient capital market theory, which rejects any distinction between market price and "true value," no case whatever can be made

for discounting the price runup on the grounds that only a minority of traders are involved.

Finally, we have the October 19, 1987, stock market selloff. As readers are well aware, stock values dropped half a trillion dollars on that single day in the complete absence of news that can plausibly be related to market fundamentals. The undeniable and spectacular presence of nonfundamental factors affecting stock prices on Black Monday renders more credible the presence, and perhaps dominance, of similar factors when the stock market is functioning normally.

### VIII. *Conclusion*

The central idea of efficient capital market theory is that securities prices are determined by the interaction of self-interested rational agents. At this most basic level, the assertion that capital markets are efficient therefore reduces to the assertion that it is economic theory rather than any other discipline that provides the analytical tools appropriate for understanding securities pricing. The intuitive presentation of efficient capital market theory in the introduction was intended to convey its essential identity with economic theory. Empirical tests of capital market efficiency, however, are in practice usually tests of the martingale model. This survey should by now have made amply clear that the transition between the intuitive idea of market efficiency and the martingale model is far from direct. Few financial economists, surprisingly, have taken direct issue with the prevailing practice in the finance literature of identifying market efficiency with the validity of a particular specialized model of equilibrium in financial markets.<sup>27</sup>

<sup>27</sup> Ross (1987) is an exception. Ross proposed as if it were self-evident that the intuition of market efficiency is essentially that of no arbitrage, rather than the martingale model or rational expectations. Because (loosely) any equilibrium price system implies satisfaction of the no-arbitrage condition, and satisfac-

The failure of many financial economists to appreciate the extent of the gulf separating market efficiency interpreted as economic equilibrium and market efficiency interpreted as the martingale model has led them to vacillate between viewing market efficiency, on one hand, as hard-wired into their intellectual capital and unfalsifiable and, on the other hand, as consisting of a specific class of falsifiable models of asset prices. In abstract discussions, financial economists almost always characterize market efficiency as a specific theory which in principle is falsifiable, but which in practice turns out not to be falsified empirically. At an applied level, however, they frequently find it difficult to specify concretely what evidence would in principle contradict the theory. This is most evident in Fama's (1970) discussion, where market efficiency was described as a substantive theory generating falsifiable predictions, but where at the same time the mathematical formulation of the market efficiency was tautologous. Further, it was noted in Section IV that several pieces of evidence that seemed to contradict market efficiency were dismissed by Fama for reasons that were not made clear.

There is no shortage of other examples of lack of clarity and consistency in discussions of capital market efficiency.

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tion of the no-arbitrage condition implies the existence of a consistent equilibrium price system, Ross' identification of market efficiency with the absence of arbitrage opportunities is essentially equivalent to our identification in the introduction of market efficiency with economic equilibrium. Now, most economists regard the proposition that the data they observe were generated by some, as opposed to a particular, equilibrium model as an untestable expression of a preferred research method. If so, Ross' definition implies that market efficiency is untestable, and that therefore the entire empirical literature on market efficiency is beside the point. Despite the considerable merits of Ross' characterization of market efficiency, it is seen to be at odds with the received practice, which emphasizes the testability of market efficiency.

Merton (1987) went out of his way to emphasize that the hypothesis of stock market rationality is not tautologous: Market efficiency is "not consistent with models or empirical facts which imply that either stock prices depend in an important way on factors other than the fundamentals . . . or that . . . investors can systematically identify significant differences between stock prices and fundamental value." Yet Terry Marsh and Merton (1986) interpreted the variance-bounds violations, which would seem to raise questions about the empirical validity of both these attributes of market efficiency, as constituting evidence against the assumed stationarity of dividends rather than as conflicting with market efficiency. Apparently when the evidence is favorable, market efficiency is supported, but when the evidence is unfavorable, market efficiency is treated as part of the maintained hypothesis, insulated from falsification. Another example of the extreme reluctance, bordering on inability, of proponents of efficient capital markets to acknowledge contrary evidence is Roll's (1986) "hubris" hypothesis of corporate takeovers, discussed in the preceding section.

Several attributes of financial economists' outlook help explain the extraordinary durability of the widely held opinion that the bulk of the empirical evidence favors capital market efficiency. As observed in Section VII, financial economists at once insist on the central importance of their contention that asset prices are determined exclusively by fundamentals, and at the same time have been unreceptive to attempts to determine empirically whether price changes are in fact traceable to fundamentals, at least until recently. Accordingly, it has been only recently that they have come to appreciate that fundamentals appear to explain *ex post* only a small portion of price changes. Further, financial economists

have always displayed a strong preference for empirical tests in which market efficiency implies the absence of a pattern, such as return autocorrelation tests, over tests that do not have such a characterization, such as variance-bounds tests. Therefore they have been led to dismiss out of hand some of the most important evidence bearing on market efficiency. Finally, financial economists' preference for arbitrage-based over equilibrium-based arguments (together with the predilection noted above for tautologous formulations of market efficiency) has diverted them from attempting to specify intellectually coherent alternatives to market efficiency, and from analyzing the econometric properties of these alternatives relative to the null hypothesis of market efficiency. Thus they have not seriously considered the possibility that many of the econometric tests that favor market efficiency have little power to reject reasonable alternative hypotheses.

The foregoing discussion suggests that financial economists have not always succeeded in applying to efficient capital market theory the same high standards of rigor and consistency that they have exhibited in other areas of finance. As a result, they have for the most part been able to avoid confronting the conclusion that is warranted by the evidence: However attractive (to economists) capital market efficiency is on methodological grounds, it is extraordinarily difficult to formulate nontrivial and falsifiable implications of capital market efficiency that are not in fact falsified.

Empirical rejection of the martingale model suggests that there exist trading rules that increase expected returns relative to buy-and-hold. It is this implication that advocates of market efficiency have always found implausible: Even if it is conceded that some or most traders act irrationally, why would rational traders not exploit the patterns, and in so doing

bid them away?<sup>28</sup> The simplest answer to this question is that optimal trading by rational agents will completely reverse the effects of irrational trades on prices only if the rational agents are well financed and risk-neutral. The need for substantial wealth on the part of rational agents is obvious: The existence of a lower bound (zero) on any agent's consumption in any state implies the existence of bounds on that agent's security purchases and sales.<sup>28</sup> Existence of these bounds is consistent with rational agents completely offsetting the effect on prices of irrational agents' trades only if the bounds are not binding, which will occur only if the rational agents' wealth is large. The need for risk neutrality is equally obvious: If rational agents are risk-averse, they will find that the portfolio they would have to acquire in order completely to reverse the effects of irrational trades imply excessive risk.

A related argument for full rationality is sometimes put in sociobiological terms. Traders who act irrationally will, it is suggested, lose wealth on average. Like any group of individuals whose ability to survive and reproduce is impaired by a dysfunctional genetic mutation, the irrational agents will eventually disappear from the population. In a series of recent papers, Bradford De Long et al. (1988a, 1988b, 1989a, 1989b), however, questioned this reasoning. If the irrational behavior of the nonoptimizers consists of taking risks based on unrealistically optimistic appraisals of possible outcomes, this irrationality may have effects that are indistinguishable from low risk aversion. Because in a population of risk-averse agents the average rewards to risk takers exceed those to risk avoiders, the law of large numbers implies (in

some settings) that the risk takers as a whole do better than the risk avoiders, even though individual risk takers will suffer bad outcomes with higher frequency than individuals who are fully rational. Thus irrationality may actually be rewarded in the aggregate.

Market efficiency is a complex joint hypothesis. Some elements of this joint hypothesis are central to economists' way of thinking, like rationality and rational expectations, while others are no more than convenient auxiliary assumptions, like the martingale model. Rejection of market efficiency requires that one or more of these elements of the joint hypothesis be replaced. Understandably, economists have focused their critical attention on those elements that can be discarded with the least damage to their research programs. We have already seen that the effort to generalize the martingale model to allow for risk aversion has not succeeded empirically so far. While it is possible that this work will succeed better in the future than it has in the past, several considerations suggest that the problems with market efficiency go deeper.

The high volume of trade on organized securities markets poses a serious problem; no minor tinkering with efficient-markets models seems likely to provide an intelligible reason why rational agents would exchange securities as much as real-world market participants do. The willingness of investors to pay for information is equally problematic: As noted in the introduction, if the purchased information makes profitable trades possible, securities markets cannot be informationally efficient, while if it does, agents are irrationally wasting their money. Neither is consistent with efficiency. These considerations suggest that a large number of market participants act as if they do not believe that the market is efficient. While there may be some

<sup>28</sup> It is true that by borrowing agents can acquire investments that exceed their total assets in value, but insofar as borrowings are secured by future wealth, their amount remains bounded.

sense in which securities markets can be efficient even though most agents act as if they believe them to be inefficient, the argument is far from transparent, to say the least. Regrettably, it appears as if it is the assumptions of rationality and rational expectations that require reformation.

The recent literature on cognitive psychology (e.g., Kenneth Arrow 1982; Daniel Kahneman, Paul Slovic, and Amos Tversky 1982; Robin Hogarth and Melvin Reder 1987; Mark Machina 1987) provides a promising avenue for future research. Cognitive psychologists have documented systematic biases in the way people use information and make decisions. Some of these biases are easy to connect, at least informally, with securities market behavior. For example, agents allow their decisions to be distorted by the presence of points of reference that should be irrelevant ("anchoring"). Further, they systematically overweight current information and underweight background information relative to what Bayes' theorem implies. To be sure, most of the evidence for these biases comes from experiments and questionnaires. Economists have in the past confidently assumed that these biases would disappear in settings where the stakes are high, as in real-world securities markets. However, this line is beginning to wear thin, particularly in light of economists' continuing inability to explain asset prices using models that assume away cognitive biases.

The problem with the cognitive psychology literature is that it is more successful in providing after-the-fact explanations for observed behavior than in generating testable predictions. Economists require from their theories a clear statement of what observed phenomena would be inconsistent with these theories, and so far this has not been forthcoming from the psychologists. Models

that make indiscriminate use of irrationality and nonrational expectations cannot impose discipline on economists' thinking about securities markets. Nonetheless, there is no reason in principle to believe that these objections cannot be met. It is a task to which economists, working with psychologists, would do well to turn their attention.

If it is accepted that successful models of securities prices will require a broader analytical framework than has been adopted up to now, it follows that the routine use of efficient-markets reasoning will require reassessment. Some arguments based on appeal to market efficiency will remain valid, while others will have to be discarded. The most fundamental insight of market efficiency—the reminder that asset prices reflect the interaction of self-interested agents—will remain. However, the contention that no successful trading rule can be based on publicly available information may have to go; it is this strict version of market efficiency that produces the empirical implications that the evidence contradicts.

The most radical revision in efficient-markets reasoning will involve those implications of market efficiency that depend on asset prices equaling or closely approximating fundamental values. The evidence suggests that, contrary to the assertion of this version of efficient markets theory, such large discrepancies between price and fundamental value regularly occur.<sup>29</sup> The implication is that there may be a constructive role for government in altering or regulating the operation of securities markets. Those who think of governments as engines of Pareto optimality will interpret the evidence summarized here as in fact justifying such

<sup>29</sup> Black (1986), in tacit recognition of the frequency with which major discrepancies between prices and values occur, defined a market as efficient if price is within a factor of two of value, and estimated that U.S. capital markets are efficient on the order of 90 percent of the time.

an enlarged role for government. The rest of us, however, will continue to reject major changes along these lines, while acknowledging that the case against such changes is not as clear-cut as it once seemed.

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