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Discussion: What the Anomalies Mean

Author(s): Marc Reinganum

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open. This means that opening price data are important for understanding day of the week effects. Second, the sample data do not allow us to reject the MCJP as a descriptive stochastic process model of common stock returns. Lastly, January/firm size/turn-of-the-year effects appear to be interrelated with day of the week returns.

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DISCUSSION: WHAT THE ANOMALIES MEAN

MARC R. REINGANUM*: Discovery of exceptions rather than confirmation of rules has characterized empirical studies of equity markets for the past several years. The empirical exceptions, the so-called anomalies, seriously have challenged the simple structures constructed by asset pricing models. The real promise of these models is that they offer a parsimonious way to describe the price behavior of risky assets. Indeed, the persistence of chapters on the Capital Asset Pricing Model in textbooks probably reflects, at least in part, the desire to communicate in a simple yet formal way basic principles which may extend beyond the CAPM. However, if the existing simple theoretical models fail to describe the actual pricing of risky assets within reasonable limits, they must be supplanted by new, more accurate, and hopefully simpler explanations of the data.

Well-entrenched paradigms are not easily killed. Initial work by Banz (1) and Reinganum (14) suggested a serious misspecification in the CAPM: returns on smaller capitalized companies were much greater than those on larger companies, even after adjusting for difference in estimated beta risk. Perhaps most disturbing to theorists, market capitalization was a much better predictor of average return than beta. Yet, this evidence was not taken at face value at first. Researchers naturally attempted to discredit the conclusions of the studies by looking for faults in the data or methodologies; if serious flaws in the original "size effect" research could have been found, then the integrity of the CAPM could have been maintained. A host of conjectures were offered as potential problems. These included suggested misestimation of beta coefficients due to non-synchronous trading (e.g., Roll [17]), possible distortions due to ignored but excessive trans-

* University of Southern California.

action costs (e.g., Stoll and Whaley [19]) and the possibility that the returns distribution is non-stationary (e.g., Brown, Kleidon and Marsh [2]). None of these explanations adequately explained the magnitude of the misspecification initially reported by Banz and Reinganum (for example, see Christie and Hertz [5], Reinganum [13], James and Edmister [10], and Schultz [18]). Keim's (11) work even increased the mystery surrounding the size effect by demonstrating that at least one-half of the average differential in returns between the small and large firms had occurred during the month of January, mostly during the first few trading days of the year. Although the CAPM is certainly a simple model, it no longer offers a reasonable approximation to the pricing of stock market securities.

Not able to explain the size effect with the CAPM, researchers attempted to explain the anomaly within the framework of another, relatively simple theoretical model, the arbitrage pricing theory (APT). This theory permits the pricing of multiple sources of risk without specifying the number of risk factors or identifying what they really are. Using factor analytic methods, Reinganum (15) reported that even a five-factor model could not explain the size anomaly. Based on a different set of firms and a different statistical procedure, Chen (4) offered more optimistic evidence in support of the APT.

One of the most interesting attempts to squeeze the size effect into the APT was a paper by Chan, Chen and Hsieh (3). The paper's methodology closely followed that of Fama and MacBeth (6). Five macroeconomic variables (a stock market index, changes in expected inflation, unanticipated inflation, a measure of the term structure and a measure of changing risk) were regressed on portfolio returns. The estimated slope coefficients from the time-series estimation became the independent variables in the cross-sectional regressions and were regressed on twenty portfolio returns stratified by size. While the authors did not sufficiently test the no-arbitrage, law-of-one-price condition of the APT, the cross-sectional tests nonetheless did provide some interesting evidence on the size effect. The only variable which seemed to be statistically and economically significant in the cross-section tests was the risk premium variable. This variable was actually measured as the difference between the return on a low-rated (under Baa) corporate bond portfolio and the return on a long-term government bond portfolio. The evidence also suggested that the other factors may not have been priced.

The fact that low-rated corporate bonds were involved in an explanation of the size effect raises the suspicion that the authors may have discovered another facet of the size effect rather than the cause of the size effect. The returns on low-rated corporate bonds and the smaller capitalized companies may very well have been driven by similar forces. The suspicion that the returns on low-rated bonds and the small capitalized stocks were driven by similar forces was reinforced by new evidence on seasonality: the difference in the returns between the low-rated bond portfolio and the government bond portfolio exhibited a strong January seasonal, as did the size effect. Furthermore, this seasonal reduced the likelihood that this variable meaningfully measured changing risk as the authors had hoped it would. Does it make any sense that the risk premium had been changing in a predictable fashion in January? What type of reasonable economic

model would generate such a result? Perhaps if the authors had written this paper six years ago, the profession today might be discussing the "low-rated bond effect" instead of the "size effect." The labels are different, but it is not obvious that the phenomena are.

The day-of-the-week effect is another hotbed of empirical research. French (7) and Gibbons and Hess (8) documented that the daily patterns of stock returns was not uniform across trading days of the week. In particular, the closing prices on Monday tended to be lower than the closing prices on Friday. In contrast to Tuesday through Friday, Monday returns on average were negative. Furthermore, as Keim and Stambaugh (12) pointed out, the negative Monday returns persisted even when the New York Stock Exchange was open during Saturdays. Rogalski (16) reported that the day-of-the-week effect was present in only eleven months (February through December) and contends that the negative Monday returns measured using closing prices actually occurred between the Friday closing price and the Monday opening price. Harris (9) disputed Rogalski's finding and reported that only half of the negative return occurred from the Friday closing price to the Monday closing price; he claimed the rest happened during the first several trades on Mondays, with declines in prices during the first forty-five minutes.

As databases expand and computers become more powerful, empirical research is discovering patterns and relationships among data heretofore unknown. Furthermore, this empirical research can described these patterns in a parsimonious way while theory cannot. After all, what kind of theory could predict that Monday opening prices would be lower than the Friday closing prices? What theory could predict that prices tend to rise during the first forty-five minutes of trading on every day except Monday? Good stories to accompany these facts will develop but no one can deny that recent empirical research on the "anomalies" has significantly increased our knowledge about the price behavior of assets in capital markets.

At professional meetings and other gatherings of economists, good-humored debates between theorists and empiricists about the contributions of each group can often be overheard. Although such exchanges are partly in jest, there is an element of real tension. The tension arises because empiricists and theorists use different approaches to characterize and to study the world. Both groups attempt to produce the same product, knowledge, but use a different set of tools. Ultimately, the consumers of the research, whether they be colleagues, students, or the business public, will decide which approach is superior for a given problem. Theorists and empiricists are competing to determine simple and accurate characterizations of the world.

What then do the anomalies mean? They mean that the theories of capital asset pricing (at least as they pertain to equity markets) have been toppled. They mean that the most interesting insights into the pricing behavior of stocks are being discovered by tedious and painstakingly thorough examination of data. They mean that, in the constant ebb and flow between theory and empirics, empirics currently holds the upper hand. Given the rapid technological advances in empirical research (computers, databases, statistical software, etc.), perhaps the recent surge in knowledge generated by empirical methods is not surprising.

While it would be foolish to proclaim that "theory is dead," the anomalies signal that, at least in studies of equity markets, empiricism is currently the king.

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