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LAWRENCE H. SUMMERS\*

THE FIELDS OF ECONOMICS and finance are allied. Indeed finance is sometimes defined as the subfield of economics concerned with intertemporal and portfolio decisions. And yet we are increasingly witnessing the development of two cultures. Researchers in economics examine questions involving financial markets, in ways which seem to researchers in finance to be hopelessly misguided. Much research in finance is regarded by many economists as doctrinaire or trivial. The paper presented by Marsh [1] at these meetings on Euler equation tests of asset pricing models is not atypically partisan. All references to papers written by researchers located in economics departments are critical. All reference to research by scholars located in finance departments are favorable.

Even critical comment by one group of researchers on the work of the other is not terribly common. In many areas, parallel literatures have developed in economics and finance regarding the same questions with virtually no points of contact. There is a public finance and a "regular finance" literature on the role of dividend taxes. There are enormous largely unconnected literatures in both economics and finance about the effects of inflation on interest rates. Parallel literatures on agency theory and the structure of contracts have also emerged in recent years. Industrial organization and financial economists treat the phenomenon of mergers and takeovers in very different ways. The list could be multiplied. Casual observation suggests that most researchers operating in one tradition are almost entirely ignorant of basic concepts in the rival tradition.

Researchers in finance who doubt this claim should ask themselves how frequently they have encountered concepts such as the " $q < 1$ " theory of the effects of dividend taxes," "the Mundell-Tobin effect," or self-selection constraints. Economists who doubt this claim might note that several papers are published each year discovering in some particular context the standard finance result that increased variance raises option values. Or they might note how few events studied appear in mainline economics journals.

The differences I am discussing may be clarified by considering a field of economics which could but does not exist: ketchup economics. There are two groups of researchers concerned with ketchup economics. Some general economists study the market for ketchup as part of the broader economic system. The other group is comprised of ketchup economists located in Department of Ketchup where they receive much higher salaries than do general economists. Each group has a research program.

General economists are concerned with the fundamental determinants of prices and quantities in the ketchup market. They attempt to examine various factors

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affecting the supply and demand for ketchup such as the cost of tomatoes, wages, the prices of ketchup substitutes and consumers incomes. They examine a number of different types of data in an effort to explain fluctuations in ketchup prices. The models that are estimated have some successes in explaining price fluctuations but there remain puzzles.

Ketchup economists reject out of hand much of this research on the ketchup market. They believe that the data used is based on almost meaningless accounting information and are quick to point out that concepts such as costs of production vary across firms and are not accurately measurable in any event. They believe that ketchup transactions prices are the only hard data worth studying. Nonetheless ketchup economists have an impressive research program, focusing on the scope for excess opportunities in the ketchup market. They have shown that two quart bottles of ketchup invariably sell for twice as much as one quart bottles of ketchup except for deviations traceable to transactions costs, and that one cannot get a bargain on ketchup by buying and combining ingredients once one takes account of transactions costs. Nor are there gains to be had from storing ketchup, or mixing together different quality ketchups and selling the resulting product. Indeed, most ketchup economists regard the efficiency of the ketchup market as the best established fact in empirical economics.

The parallels should be clear. Financial economists like ketchupal economists work only with hard data and are concerned with the interrelationships between the prices of different financial assets. They ignore what seems to many to be the more important question of what determines the overall level of asset prices. It would surely come as a surprise to a layman to learn that virtually no mainstream research in the field of finance in the last decade has attempted to account for the stock market boom of the 1960s or the spectacular decline in real stock prices during the mid-1970s.

General economists studying the ketchup market may be asking the right questions but they will frequently lack the right data to answer them. Indeed, it may not be possible to construct tractable models that account for ketchup price fluctuations. But this does not establish the perversity of the ketchup market, only the inadequacy of current data, theory and empirical methods.

Neither the finance approach to financial markets nor the approach taken by general economists has a unique claim on virtue. Rather they are complementary. Both have the potential to increase our understanding of how the economy operates. It is unfortunate, therefore, that researchers in economics pay so little attention to finance research, and perhaps more unfortunate that financial-economists remain so reluctant to accept any research relating asset prices and fundamental values.

Tremendous controversy has raged over the past several years as to whether or not the stock market is excessively volatile. Essentially the question posed is how much of the fluctuation in stock prices can be accounted for by variations in the expected future dividend streams. Shiller [2] concluded that fluctuations in expected future dividends could account for only a part of the variance in stock prices. This conclusion has been savagely attacked both orally and in print by numerous financial economists. The volatility test controversy is like a

scientific debate about whether one can reject the hypothesis that the earth is flat with a microscope. Shiller's conclusion that the joint hypothesis of rationality and a constant required return on the market can be rejected is exactly the conclusion reached by most modern work in financial economics. Simple regressions of real ex post stock returns on lagged dividend yields find that the null hypothesis that the real ex ante rate is constant can be rejected at almost any level of confidence. Shiller's conclusion is exactly consistent with this result.

Why then has work on volatility testing generated such hostility from mainstream finance researchers. In part, because users of volatility tests have interpreted their rejections of the null hypothesis in controversial ways. In part, because volatility tests raise some interesting and complex methodological issues. But a large part of the answer must be a deep distrust of research purporting to explore fundamentals valuations. Yet surely work on volatility tests raises, even if it does not resolve, major issues for our understanding of financial markets. To what extent do fluctuations in stock prices reflect changes in risk premia, safe rates of return, expected future cash flows, or other factors? Clearly standard approaches with their emphasis on the determinants of ex ante returns are ill-suited to these important questions.

The increasing disjunction of the fields of economics and finance are obviously inefficient. Equally apparent and important is the diversion of intellectual effort into reading about methodology. More important, but less apparent is the absence of sound research on major questions which fall into the interstices between standard finance and economics approaches. We can do better.

#### REFERENCES

1. Terry Marsh. "On Euler-Equation Restrictions on the Temporal Behavior of Asset Returns," Sloan Working Paper 1619-84, 1984.
2. Robert J. Shiller. "Do Stock Prices Move Too Much to be Justified by Subsequent Changes in Dividends." *American Economic Review* 71 (June 1979), 421-36.