Professor Vickers evaluates my version of the theory of the firm under uncertainty along three central themes of economic analysis: time, uncertainty, and the notion of economic equilibrium. His views exhibit prescriptive preferences for alternative paradigms with respect to each of the above themes. My response will focus, mostly, on our paradigmatic differences.

First, let me reply to Vickers’ comments aimed directly at my equations (1) and (2). While the Modigliani-Miller theorems are, indeed, implicit in the paper, they do not play any role in my analysis, i.e., my results are independent of the question of the validity of these theorems. Moreover, these theorems are implicit in my paper only in the sense that they have been shown to hold in a variety of frameworks of frictionless capital markets, the CAPM framework included. The second comment seems to question, on some unspecified grounds, whether the costs of equity and debt are properly expressed in relationship to risk. The reply to this comment is best understood when my equation (1) is manipulated, from its present certainty-equivalent form, into the Modigliani-Miller risk-adjusted discount rate of $V_t = E[R_t]/p_t$, where $p_t = r + \lambda \text{Cov}(r_t, y, \text{var}(y))$, $r_t = R_t/V_t$ and $y_t = M/V_t$. This form shows, clearly, that the cost of money capital, in equations (1) and (2), is the capital market equilibrium risk-adjusted rate, $p_t$, where the risk premium is determined by the operating risk of the firm. This cost of capital, $p_t$, can be decomposed into the cost of equity, $k_e$, and the cost of debt, $r$, where $k_e$ is a linear increasing function of financial leverage (see Hamada 1969). Thus the operating risk and the leverage risk are fully reflected in $p_t$ and $k_e$. True, the cost of debt, $r$, is the riskless rate in the Modigliani-Miller model as well as in my equation (1). However, Black (1972) has generalized the CAPM to the case where there is no riskless borrowing. He shows that the CAPM prevails with the sole change that $r$ is replaced by $E[r_t]$, where $E[r_t]$ is the rate of return on the zero beta portfolio having the property $E[r_t] = r$. This replacement does not affect any part of the analysis in this paper.

Much of Professor Vickers’ criticism is aimed at the underlying assumptions of the CAPM theory on which my paper is based. In particular, he finds the following assumptions most objectionable: (1) single period horizon; (2) wealth as the object of choice; (3) mean variance utility function; (4) the size and composition of the investment opportunity set is given and fixed, i.e., the real sector is exogenous; (5) homogeneous estimates; and (6) investors borrow at the risk-free rate. Indeed, the CAPM was originally obtained under the above assumptions, but Vickers is mistaken when he claims that the result is derivable only under the above restrictive assumptions. As a matter of fact, an ongoing body of research shows that the above original assumptions can be relaxed substantially.

First, Sharpe (1964) andLintner (1965) used the observations by Tobin (1964) and Merton (1965), that the multivariate normal distribution of returns is a sufficient condition for investment choices to satisfy the two-fund separation principle, to conjecture that the CAPM can be derived for a variety of preference structures. That is, they have shown
that the quadratic mean-variance utility function is not necessary and the normally distributed returns are sufficient. Fama (1965) was quick to extend the sufficiency conditions to versions of the multivariate stable class. Moreover, Ross' (1976) Arbitrage Pricing Theory (APT) further weakens the distributional assumptions by dropping the need for joint normality or stable class of distributions. Chamberlain (1983), further extends the admissible class of distributions. Second, Black (1972) extends the CAPM to a world without riskless borrowing and obtains the zero beta version of two risky funds separation principles. The resulting CAPM preserve all its risk-return characteristics by simply replacing the risk-free rate with the higher rate of return on the zero beta portfolio. Third, Fama (1972) formulates the opportunity set in terms of uncertain consumption (rather than terminal wealth) to show that, at least in a single period setting, the consumption and wealth objectives yield the same CAPM equilibrium. Fifth, Stiglitz (1972) extends the CAPM in single-period general equilibrium setting. That is, he starts the analysis with initial endowments, production possibilities and preferences over consumption to derive the CAPM as a result of a simultaneous clearing of the real and the financial markets. Thus, an exogenously fixed real sector is not necessary.

Sixth, the question of multiperiod CAPM has been studied by different researchers who have come up with a variety of solutions. Fama (1970) starts with a multiperiod utility function for consumption, in an intertemporal economy where each consumer makes sequential consumption and investment decisions over his lifetime. The key to Fama's success is the use of dynamic programming to go from utility of lifetime consumption to simple derived functions of terminal wealth. Others follow Fama in the construction of discrete-time multiperiod formulations which yield the multiperiod CAPM.

Among the most successful in this line of inquiry are Rubinstein (1976) and Stapleton and Subrahmanyam (1978). The former utilizes logarithmic utility to obtain period-by-period myopic valuation. The latter use the combination of normal distribution and exponential utility to derive a period-by-period CAPM, in spite of the existence of links between the equilibrium prices of the different periods. Metron (1973) offers a continuous-time alternative to the discrete-time multiperiod approaches. He makes the critical assumption that the investor-consumer faces returns generated by an Ito process. Then, he obtains a dynamic CAPM which holds at each instant of time. This model was extended and further generalized by, among others, Ross (1978), Cooner (1977) and Breeden (1979). Finally, the assumption of homogenous expectations is relaxed by Constantinides (1980) who presents the most general CAPM to date. It is a multiperiod general equilibrium formulation in terms of fundamentals, including production possibilities, endowments, beliefs, and preferences. Moreover, consumers are heterogenous in terms of endowments and beliefs. Additionally, the investment opportunity set changes stochastically over time so that the market portfolio is non-stationary. The author overcomes the complexities of heterogeneous consumers through the establishment of equilibrium prices by means of an aggregation property.

The above response to the criticism concerning the axiomatic foundations of the CAPM shows that the model has come a long way. While the CAPM is far from being a perfectly robust model, its foundations have been broadened over the years and we now have a better understanding of the sufficient conditions for this theory to hold. This understanding has major implications for the evaluation of the merits of the main argument against my paper, the argument that the paper is cast in the Walrasian tradition of (inept) static analysis. While I do not deny the fact that my article is based on a single-period static analysis, I do claim that its major conclusions remain valid in a time-oriented multiperiod setting. That is, these conclusions hold at least as long as the CAPM is valid, and the validity of the CAPM in multiperiod settings has been demonstrated, in the discrete-time framework (Stapleton and Subrahmanyam (1978) and Constantinides (1980)) as well as in the dynamic continuous-time approach (Merton (1973)).

Vickers finds our neoclassical probabilistic approach to uncertainty, or risk, as inadequate as our treatment of time. According to him, probability theory is logically relevant only when outcomes are, either generated by a known and stable event-generating mechanism, or they must be drawn from replicable controlled experiments. The argument continues that, since economic variables cannot be described neither by a priori objective distribution nor by an empirical frequency distribution, economic observations are non-distributional, i.e., they occur "only once at a point on the passing continuum of time." The critical argument, carried to its full implications, is tantamount to an outright rejection of empirical testing in economics and the social sciences, because of the necessary distributional assumptions in statistics and econometrics. The critic is definitely aware of the school of subjective probability, but he seems to dismiss it lightly with the following, somewhat strange, question: "What in that case, it has to be asked, is the basis in reason or fact, or what, in either words, is the ontological or existential foundation for justification for the postulation of probabilities. In short, where, then, do the "probabilities" come from and what do they mean?"

This question concerning the meaning of subjective probability has been examined carefully by major exponents of the subjunctive-logical approach, including Logullo (1713), Ramsey (1931), de Finetti (1937), Koopman (1940), Good (1950), and Savage (1954). The following representative answer is taken from Savage (p. 3):

"Personalistic views hold that probability measures the confidence that a particular individual has in the truth of a particular proposition, for example, the proposition that it will rain tomorrow. These views postulate that the individual concerned is in some way "reasonable," but they do not deny the possibility that two reasonable individuals faced with the same evidence may have different degrees of confidence in the truth of the same proposition."

These subjective probabilities are a numerical expression of the faith or confidence that a particular individual has in the truth of a specific proposition. Such probabilities are not arbitrary; rather, they are subject to fundamental requirements of coherence and consistency. Further elaboration, as to the axiomatic foundations, as well as to the ontological meaning of subjective probability, is available in Savage (1954), Fishburn (1964), and Kyburg and Smokler (1964).

I, personally, find the rejection of subjective probabilities strange and harmful on many grounds. First, the fact that we do not know where subjective probabilities come from is quite beside the point. Actually, tastes, preferences, beliefs, including probability beliefs, and expectations are closely related. Does Vickers object to our reliance on preference axioms on the grounds that we don't know where tastes come from? As a matter of fact, the axioms of subjective probability and the axioms of preferences play the same role in the economic theory of choice. They provide the setting for a non-circular consistent individual choice. This role of subjective probability is emphasized in the following quote from Fishburn (1964, p. 170):

"The logic of subjective views is not some as yet incomplete system of inductive logic leaving no room for individual judgements, interpretations, and beliefs, but rather a set of criteria for consistency and reasonableness in our beliefs or judgements (or behavior)."
Preferences and beliefs are, and should be, the primitive concepts of the economic theory of choice. In fact, in many formulations probabilities are derived from the primitive notion of preference axioms. It appears that Vickers' objections are aimed at the rational theory of choice, where decision-makers act in accord with their preferences and beliefs. Do we have a viable alternative to this building block of economic reasoning? Second, subjective probability is valuable exactly because it allows tractable formulation of diverse beliefs, which Vickers calls for, where "... heterogeneity of expectations, and of actions based upon them, that made market outcomes possible and mutually beneficial..." Vickers would have us replace risk in the probabilistic sense with uncertainty in the Knightian sense, justifying the replacement by the claim that economic variables are non-distributional variables. His claims concerning the non-distributional nature of economic variables, as illustrated in his discussion of the annual rate of return on a firm's common stock, is based on the false notion that only an identically and independently distributed (i.i.d.) variable is representable by a probabilistic representation and, therefore, the CAPM is invalid. The infancy of this argument becomes apparent when we examine Constantinides' intertemporal CAPM, cast in a stochastic general equilibrium framework where the interest rate and the market portfolio, as well as firms' policies and rates of returns, reflect different probability distributions at various points of time, resulting from a non-stationary stochastic process. Still, while the question of the relative merit of the Knightian uncertainty versus the probabilistic approach might be of some interest, I cannot see how the former approach allows for "true residual uncertainty" or "ignorance" which, according to Vickers, have been effectively abolished within the probabilistic approach. First, to set the record straight, choices and decisions under true ignorance are meaningless. They are defined only in relationship to some set of information. The main difference is that the Knightian approach describes the opportunity set in terms of possible outcomes while the probabilistic approach associates it with possible outcomes and their corresponding probabilities. Both approaches involve only partial information, since it is unknown which of the possible outcomes will actually materialize. The question of where possible outcomes come from applies equally to both approaches, and the answer that, in the absence of these parameters of the economy, potential outcomes are a matter of subjective perceptions and beliefs is common to both situations. Then, one wonders, why it is that a decision-maker, who is capable of perceiving possible outcomes, cannot assign probabilities to these same occurrences. It would seem that the natural answer is that as far as the information goes, probability estimate is not forthcoming. But this seems to be just the right setting to invoke the Laplacian principle of insufficient reason or of sufficient ignorance, i.e., to assign an equiprobable distribution. In Fishburn's words (1964, P. 141), "if there is no known reason for assigning unequal probabilities to possible contingencies, then relative to our knowledge, they must be assigned equal probabilities." This argument trivializes the distinction between risk and uncertainty in a world of subjective beliefs.

Finally, we now turn to the notion of equilibrium and its relevance. The importance of equilibrium analysis, in professor Hahn's (1981) words, is that "the equilibrium prices impose order on potential chaos" and, Hahn (1973), it defeats the "... belief that nothing in economics is either true or false, or if you like, empirically or logically falsifiable." Moreover, Hahn continues, "... our need for equilibrium concepts is largely connected with ignorance of precisely those features of an actual economy which the view under discussion wishes us to be precise about." Another justification for equilibrium analysis is that it frequently yields robust results, i.e., results which survive substantial alterations in the initially posited environment. For example, the Arrow-Debreu equilibrium and the CAPM have been shown to remain valid under substantial relaxation of initial assumptions. Actually, the Arrow-Debreu state-contingent claim model receives new support from the game-theoretical literature of the Core. An allocation of resources is said to be in the Core if there is no incentive to upset it by any coalition of agents. Since the concept of the Core is independent of any particular market setting or ruling prices, it is justly considered to be a more general set of possible terminal states of the economy. Now, it has been shown that every Arrow-Debreu equilibrium is in the Core and, more important, that the Core and the Arrow-Debreu equilibrium coincide for "large economies" in general, and for "small economies" under some conditions. While this state of affairs does not establish economic equilibrium as the sole terminal state of an economy, it does strengthen the plausibility of equilibrium analysis and, above all, it shows the facility of attempts to discredit a model by objection to some of its assumptions. The CAPM theory of the firm has been shown to yield empirical propositions which, at least in principle, are testable. Those who believe in the explanatory superiority of the Knight-Shackle paradigm of uncertainty, bear the burden of formulating an alternative theory of the firm and deriving a set of different testable implications. In summary, while I sympathize with Professor Vickers' uneasiness about the state of economic analysis, I cannot help feeling that he is shooting his arrows at the wrong villains; namely, time-limited static tradition, probabilistic calculus, and equilibrium theory. The truth is that intertemporal dynamic formulations, within the optimal control framework or otherwise, have too frequently produced results analogous to those of the static framework. Furthermore, game-theoretic probabilistically-free approaches failed to revolutionize economics or to invalidate any fundamental concepts of economic analysis under conditions of certainty. Finally, disequilibrium formulations are mostly ad hoc, lacking the generality and the relative robustness of the equilibrium models. I agree with Vickers that we do not have a satisfactory theory of money capital, and I believe that such a theory is essential for the understanding of the behavior of individual economic agents and for the evaluation of economic policies. Yet, the fact is that time and uncertainty, per se, are not sufficient for the development of a theory of money and, in particular, of money capital. Money capital has no room in the Arrow-Debreu multi-period and heterogeneous expectations model, nor does it play a role in the non-stationary model with heterogeneous expectations by Constantinides. Money continues-time dynamic models under uncertainty, whether of the Mertonian CAPM gender or of the growth theory version, have not contributed to the understanding of the role of money capital. The chief villain in this failure to establish a meaningful economic role for the concept of money capital must be, undoubtedly, the assumption of complete markets. The assumption that all consumers and producers can span the entire space of contingencies, across all states and dates, transforms the future into the present and it provides the mechanism for the translation of diverse endowments, preferences and beliefs, into a unanimously preferred allocation. Yes, money, and money capital in particular, is a time-and-uncertainty phenomenon which takes on a meaningful real economic role only under incomplete markets. Thus, incompleteness of markets, exacerbated by other sources of friction, in the form of costly information and transaction costs, opens the road to the
new real-world-oriented economics. I am strongly convinced, based on the above considerations, that the greatest promise for further progress in economic analysis does not lie in the ad hoc disequilibrium formulations nor in the reliance on vague concepts like 'uncertainty about uncertainty', rather, explicit formulations of incomplete markets, costly information, and transaction costs call for a revision in the concept of equilibrium as suggested by Professor Hahn (1973) in his inaugural lecture. That is, incompleteness necessitates re-opening of markets and trading on every date, resulting in a sequence of economies. Now, equilibrium must come to grips with the distinction between the subjectively perceived environment and the actual environment. Professor Hahn (1973) offers the following extension of the concept of equilibrium:

"The traditional notion of equilibrium which I described at the outset requires the equilibrium actions of agents to be consistent, whereas I have the weaker requirement that they not be systematically and persistently inconsistent. Again, in the sequential formulation of the traditional notion, single valued expectations are exactly met while I very roughly require the convergence of prior probabilities to frequencies."

References


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