A Positivist Evaluation of the New Finance

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> Can political economy do nothing, but only object to everything, and demonstrate that nothing can be done? J. S. Mill, *Principles of Political Economy*

Introduction

The first parts of Professor Merton Miller's 1976 Presidential Address to the American Finance Association, "Debt and Taxes," created considerable discussion in the finance profession. In the last part of his speech, however, he evaluated the current state of the discipline and attempted a synthesis and reconciliation in the following way:

> To say that many, perhaps even most, financial heuristics are neutral is not to suggest, however, that financial decision making is just a pointless charade or treat *(sic)* the resources devoted to financial innovations are wasted. A mutation or a heuristic that is neutral in one environment may suddenly acquire (or lose) survival value if the environment changes. The pool of existing neutral mutations and heuristics thus permits the adaptation to the new conditions to take place more quickly and more surely than if a new and original act of creation were required. Once these types and roles of heuristics in the equilibrating process are understood and appreciated, the differences between the institutionalist and theorist wings of our Association may be seen to be far less fundamental and irreconcilable than the sometimes ferocious polemics of the last 20 years might seem to suggest [23, pp. 272–273].

This viewpoint would appear to be a fair representation of the position of the advocates of the New Finance with respect to events of the last two decades. It reflects an attempt to bridge a chasm that many feel has grown wider in recent years. Our purpose is to ascertain whether the differences between the feuding realms are really as superficial as

Professor Miller believes.

The task may be undertaken in several ways. One would involve an institutional interpretation of the intellectual history of finance over the last 20 years, and this we have provided elsewhere [13]. There we suggested that the discipline got off on the wrong foot through its uncritical acceptance of the methodology of the hard sciences; this, in turn, led to the adoption of attitudes more accurately characterized as scientistic than scientific and approaches which obscure many of the real issues in finance. A careful reading of the literature reveals an increasing tendency in both theoretical and empirical papers to confuse (and thus blur) the distinction between risk and uncertainty, to assume all individuals to be essentially the same, to ignore learning and irreversible time (the *ex-ante–ex-post* substitution), and finally to invoke the one price law of markets (arbitrage processes) to arrive at a model of the world where precious few financial variables really matter. In sum, an institutionalist interpretation of the evolution of the New Finance raises fundamental questions as to whether postulates of rationality and the scientific method are appropriate for any social "science."

For present purposes, however, we shall pursue a more conventional line and accept the methodology of science. Thus, we shall examine the issues in a fashion comfortable to those who may be unprepared to discard many of the critical assumptions and procedures that underlie all neoclassical economic theory. The history of financial thought over the past two decades must necessarily play an important role in our examination. Much of our analysis will focus on those results of the New Finance which may be unambiguously viewed as scientific as opposed to embodiments of assumptions. By tracing the chronology, we arrive at the current state of affairs in the theory of finance: a normative model based upon Walrasian general equilibrium theory buttressed by empirical work rooted in the methodology of positivism.

In the next section we shall examine the former, with particular emphasis upon its ability to explain everything or nothing. In the section following that, we find the New Finance to be replete with implicit value judgments. We suggest that finance should once again become a meaningful contributor to the solution of real-world problems. Despite our generally negative conclusions about the contribution of the New Finance to our understanding, we do not argue that work over the past twenty years has been entirely in vain. Whether the assumptions, methods, and conclusions of the New Finance can coexist with or be grafted onto the body of institutional knowledge that once was finance, however, remains to be seen.

Neoclassical General Equilibrium in Finance

> In fact, equilibrium theory has reached the state where the pure theorist has successfully (though perhaps inadvertently) demonstrated that the main implications of this theory cannot possibly hold in reality, but has not yet managed to pass his message down the line to the textbook writer and to the classroom [20, p. 1240].

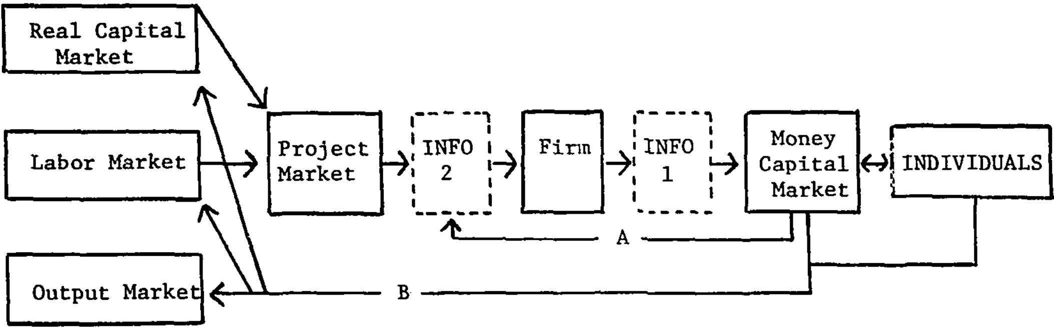
A Brief History

Our interpretation of the development of equilibrium thought in finance may be depicted in terms of the exhibit. Until perhaps the 1950s, the basic left-to-right model prevailed. Income or cash flow estimates for various potential projects were made by the firm based upon the respective parameters of the factor and output markets. A capital budget was then somehow selected, and news of this reached the stock market by some process. This, in turn, might or might not have caused share prices to change as the market evaluated the news. In this most basic model, neither the process by which capital budgets were selected nor the process by which stock prices were determined is specified. Projects with positive windfalls and undervalued shares were certainly allowed to exist.

Normative theory appeared on several fronts. The field of corporate finance, beginning perhaps with Joel Dean and George Terborgh, began to offer rules for selecting capital budgets based upon payback, accounting return and, later, discounted cash flow techniques. The field of investments, taking off from the works of Graham-Dodd and J. B. Williams from the 1930s, began to offer explicit share pricing models. For some period of time, however, corporate finance and investments were viewed as quite separate fields.

The first widely-recognized equilibrium arguments were probably those of Modigliani and Miller (MM) [25]. It must be stressed, however, that theirs was a partial equilibrium argument with respect to the money capital market alone; the firm investment decision (and the supply of shares) was most explicitly held constant in the equilibrium process. Most of the theoretical development of the 1960s was also conducted under partial equilibrium. The original development of the Capital Asset Pricing Model (CAPM) was applied to shares only, and even the Fama–Laffer information article [8] held firm investment constant.

Exhibit. The Multimarket System



Parallel to this development, Solomon, Weston, Gordon, and others were promoting the use of an opportunity cost of capital in the capital budgeting decision. This is depicted by feedback loop A in the exhibit. These authors also seemed to assume that the money capital markets were in equilibrium (or else the measured opportunity cost would have been incorrect), but they did not spend much time worrying about the equilibrium process (certainly not as much as the MM-CAPM writers). There was some consideration of risk-class-altering investments, but there was little discussion of the impact of the investment decisions under consideration upon equilibrium market parameters.

Meanwhile, an economic approach to finance, which brought together all markets (product, factor, and financial), surfaced with the work of Vickers [30, 31, and 32] and others [2, 28, and 37]. This avenue of research attempted to marry the constructs of microeconomics to the partial equilibrium methodology of previously-established financial theory. Simultaneous solutions to the production, investment, and financial problems were provided, and an elaborate framework yielding an intraequilibrium position for the firm was constructed.[1]

A recent statement by Vickers suggests his belief that general equilibrium never exists for the economy as a whole, and that the firm's decisions *always* take place in a state of disequilibrium:

> It is possible and necessary to build explanatory and normative models of individual firms' decision processes, behavior, and optimum operating structures. But it would be a betrayal of economic analysis to imagine that the equilibrium constructions in the analysis were describing precise states of affairs. . . . In the matter of investment, for example, or in relation to financing decisions . . . the firm considers undertaking additional expenditures not because it is in some kind of equilibrium situation, but because it explicitly recognizes a disequilibrium condition; disequilibrium in the sense that additional profit and income opportunities are seen to exist and investment is contemplated to take advantage of them [31, p. 375].

Thus, advocates of the economic approach seem to have investigated general equilibrium structures, found them to be unappealing, and retreated to the haven of sequential analysis in a world of continuous disequilibrium [see also 2].

In the early 1970s, the CAPM concept was broadened to encompass a joint equilibrium of the project and money capital markets. Although early work had been done by Lintner and Mossin, the grand synthesis was probably most elegantly achieved by Fama and Miller [10] in a Walrasian equilibrium across all markets (feedback loop B in the exhibit). It should be noted that they stressed the CAPM as only one possible valuation model in terms of which equilibrium could be reached and also that they only sought to describe the process, not derive micro-decision rules. Others, such as Rubinstein [27], Weston [35], and Haley and Schall [18], did derive decision rules from the generalized CAPM, and capital budgeting with beta is now standard textbook fare.

[1]One of the authors of this paper participated in that effort [37]. Although he appreciates Professor Vickers' critique that his work was a "valuable . . . elegant example . . . of model building" [31, p. 26], he must admit that much of the effort was "art for art's sake."

Walrasian Finance

We might now consider the price the New Finance has paid by adopting Walrasian structures. Not only have we added feedback loop B, but we have also assumed that we keep going around the circle with no deals final until all the markets clear simultaneously (*i.e.*, a *tântonnement* process with costless recontracting and no trading out of equilibrium). In most versions, such as Fama–Miller, we also assume that 1) perfect competition prevails in all markets, 2) any changes in the production decisions made by the firm involve only the scale of operations and not a change in factor proportions (and scale decisions are made with all firms possessing linearly-homogeneous production functions), and 3) the risk exposure of the firm is invariable. If we put these assumptions together, we witness a world where there are no positive net present value projects, no optimal scale for a project or a firm, and no justification for the existence of the firm at the margin, anyway (see [14] and [36] for an extensive discussion of these points). In sum, there are no guides to action, no decision rules, to be found in the pure Fama–Miller type model (nor do they claim any). One might be pardoned for questioning the wisdom of basing *any* practical judgments on such a model that does not allow room for choice:

> It can be reasonably asked, therefore, whether the assumption content of such an analysis does not in fact eliminate the real problems to be solved in real world firms, in particular the determination of the actual factor mix, real capital intensity, and thereby the implied risk class of expected income streams. And if, as can also be shown, these pressing, real-world production and structural problems, particularly as they come to focus in conceivable disequilibrium situations, are by-passed, serious doubt is thrown also on the empirical as well as analytical implications of the other branch of theory we referred to, namely the theory of equilibrium in the capital asset or security markets. To what extent, it then has to be asked, can the equilibrium yields on risky securities be imported directly from the asset market equilibrium theory to the firm's cost of capital and capital investment decision areas? The answer would seem to be that such a direct importation does not have clear and unarguable validity at all [31, p. 385].

As we get to the more pragmatic versions (*e.g.*, Haley–Schall, or Weston), decision rules (at least regarding project selection) do appear (these approaches continue to maintain that the money capital market is in equilibrium and, thus, financial policy does not matter). These arguments have problems, however. In equilibrium, it is necessary to assume an imperfection in one or more of the factor or output markets (and, hence, the project market) to contemplate the existence of positive net present value projects; yet, if the money capital market is efficient, the expected rents would already be capitalized into the stock price [14]. In other words, an efficient market will capitalize the expected net present value of a firm's investment budget over all future time.[2]

The attempt to formulate decision rules poses other problems: was the project currently under evaluation considered by the market in arriving at the equilibrium solution? If it was, then we are conducting intraequilibrium analysis [5]. Although such a procedure can be justified as logically valid (and is what is done in Fama–Miller), it is basically devoid of guides to action (except to say that one should select those projects in the — supposedly observable — equilibrium solution). In other words, an attempt to derive such guides would yield statements on the order of: exercise a call option at maturity if the stock price exceeds the exercise price.

Most of the models that provide decision rules, however, are at least implicitly employing interequilibrium analysis (*i.e.*, comparative statics) in which the firm is confronting the $n + 1^{th}$ project with the parameters of an n project equilibrium. Such a procedure can be an approximation at best, and the question as to how well the n project equilibrium approximates the correct $n + 1$ project equilibrium is an empirical question that has never been addressed to our knowledge. As we have discussed elsewhere [14, 36], some of the necessary conditions for a reasonable approximation would be that the project be small and that it be found soon after the prior equilibrium were established. The former condition is aggravated by the fact that, in frictionless markets, every project of every firm is at the margin. The impact of the latter is increased by all information entering the market since the equilibrium was established. Finally, we must also assume that no other trading occurs out of equilibrium (or else that it has no effect).

All this further assumes that equilibrium exists (at least at some point) and that this is desirable and to be encouraged. As an attribute of an economic system, equilibrium is at best neutral in any moral or aesthetic sense. Furthermore, disequilibrium is preferable to entropy in social systems [21, p. 309].

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[2]We might also note that, other than in a legislated monopoly, there are not many imperfections we can think of which would persist in a market environment where everybody knows about them and where unlimited funds can be raised to enter the market in a timeless, spaceless world.

Some Conclusions on Continuous Equilibrium

> In a state of equilibrium, production and consumption ... are necessarily equal in each market, and in the rarefied world of Walrasian perfection where markets are *continually* in equilibrium, the question of how the market responds to "disequilibria" is ruled out — all equilibrating adjustments are assumed to be instantaneous, either because changes are timeless or because all changes have been perfectly foreseen [20, p. 1247].

Once we have confronted the notion that a state of equilibrium is not necessarily good or bad (in particular, not necessarily good), we can then consider the concept of continuous general equilibrium. Such a system would resemble the efficient stock market notion generalized across all markets. Prices would reflect available information and would change unbiasedly with new information.

One operational implication of all of this is that it may be impossible empirically to distinguish a world of continuous equilibrium from one of continuous disequilibrium. In both cases the price is what it is, and this is neither good nor bad; there is little if anything anyone can do to influence it; and there is no way to predict where it is going next from where it is or has been. The traditional use of equilibrium analysis (*i.e.*, to predict the direction of movement from disequilibrium to the new equilibrium point) is lost in both cases, as the expected future path is random. Hence, neither system provides much guidance for action.

One clear advantage of analysis based upon Walrasian structures has been the demonstration that some earlier contentions from partial equilibrium analysis resulted from inappropriate *ceteris paribus* assumptions rather than true findings. Indeed, it becomes increasingly unclear how one or more markets can be in equilibrium unless all are. If one is going to employ equilibrium analysis at all, general equilibrium appears to be the best.

This point is sufficiently important to deserve amplification. It would appear to us that the only way a partial equilibrium solution could differ significantly from the equivalent general equilibrium solution would be if the *ceteris paribus* assumptions of the former were masking important interactions and feedbacks from other markets. The latter solution would then clearly seem preferable. In like manner, the only way discrete general equilibrium could differ from continuous would involve the periodic persistence of disequilibrium within a market setting where arbitrage was possible. This also appears difficult to rationalize.

Unfortunately, the converse of our points also holds. If general equilibrium is the best that equilibrium analysis has to offer, rejection of the former implies rejection of all the latter. If one market is found not to be in equilibrium, can any market be in equilibrium? In sum, we are either in continuous general equilibrium or an uncertain disequilibrium. There appears to be no reasonable approximation or middle ground in this debate. If we opt for the former, there is little left for us to do. If we opt for the latter, it is not clear where we are or which of the findings of the last two decades may be salvaged.

> The peak reached by the equilibrium theory is extremely impressive, and, perhaps, its present-day adherents are capable of building a look-out tower on this peak. Still, we think that we should descend from the peak to the plains and begin again from a much lower level to climb another, steeper and higher peak [21, p. 376].

Positivism and Value-Free Science

> Economic science is presupposed to be positive, ethically neutral, and subject to unbroken cumulative progress rather than episodic turnarounds. The positive-normative dualism is not helpful. Economics is positive in the sense that the validity of its theories is tested by an appeal to facts, but the appeal is indirect through action that necessarily involves normative values. Ethical neutrality has not been characteristic of innovative economic theories. What needs to be done about ethical judgments is to make them explicit rather than to pretend they do not or should not exist in a manner that bears upon the content of economic theory. Economics is a moral science [6, p. 723].

Positive Finance

From the first MM paper, with its "equivalent risk classes" and "homemade leverage," all efforts to question the theories of the New Finance on the basis of the realism of their assumption content have been deflected by appeals to positivism. As stated by Milton Friedman, the arguments go as follows:

> Positive economics is in principle independent of any particular ethical position or normative judgments. As Keynes says, it deals with "what is" not with "what ought to be." Its task is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances. Its performance is to be judged by the precision, scope, and conformity with experience of the predictions it yields [16, p. 4].

Thus, positive theories are ethically neutral and, so long as the conclusions follow from the assumptions, are to be judged on the basis of how well they work.

Note that any contention that finance should employ the methodology of positivism involves a purely normative judgment. Many philosophical methodologists reject this position for the reasons outlined by Dillard in the quotation that begins this section. They question the benefits from adopting unalloyed positivism as a research and explanatory strategy.

Our second point is a bit ticklish. Under one framework, it can be argued that imperfection can take many forms, while perfection (or efficiency) is unique. Under another, it can be noted that the old literature was not cast in scientific terms and that it was neither well understood nor regarded by the advocates of the New Finance. In any event, the net result from the earliest empirical tests (which were, of course, conducted under the New Finance) has been that the null hypothesis tested has invariably reflected the prior beliefs of those in the New Finance. Indeed, this procedure has become so entrenched that the testing of anything else (see, for example, the discussion of risk pricing below) is viewed as out of the mainstream of current thought.

The juxtaposition of the null with the alternative hypothesis creates several methodological problems. Since the null reflects the prior beliefs of the followers of the New Finance, then clearly the most intuitively appealing results to them (*i.e.*, the best research in their view) will be results that fail to reject the null. The basic effect of this is that what are viewed as the leading journals in our field are increasingly filled with papers reporting negative results.

Matters become more perverse when we contemplate significance levels. In the general case, a movement to a higher level of significance implies a more stringent test of one's position. The exact opposite is true in our case. Indeed, no matter what the data are attempting to say, one can always (unintentionally or otherwise) select a significance level high enough to sustain the null. Nor does it end here. Suppose we attempt to make our research more realistic by recognizing problems with data and methodology, taxes, transactions costs, information costs, and the like. At a given level of significance, all we have done is increased the likelihood of being unable to reject the null. Thus, even if one approaches this area of research with perfectly diffuse prior beliefs, it is far from obvious what significance levels mean or where they should be set.

Because the above problems follow from the choice of the null, we have been asked what null we would choose to overcome these problems. We have no answer, as the "imperfection can take many forms" arguments above are quite compelling. We would rather note that the problems raised add further weight to our prior argument that the scientific paradigm may be premature, if not inappropriate, for finance.

Priors — An Example of Risk Pricing

The impact of implicit and explicit prior beliefs upon the design of tests and interpretation of results can take many forms. A good case study may be found in the literature of the market pricing of risk, a topic we earlier reviewed in *Financial Management* [11]. The observation that individuals hold poorly-diversified portfolios and that many institutions operate under the Prudent Man Rule (which may still be interpreted in terms of individual security risk) has led some researchers to suspect that risk other than beta may be priced in the market. Early work by Lintner [22], Douglas [7], and Arditti [1] suggested that total risk (such as standard deviation or standard error) might be priced in addition to (or even instead of) systematic risk, beta. We concluded that, because of numerous estimation and statistical problems, it was impossible to tell whether the market was pricing beta, the standard deviation, or both [11].

The initial response from the New Finance is found in papers by Miller–Scholes [24] and Black–Jensen–Scholes [3]. Miller–Scholes demonstrated that it was possible that if enough things had gone wrong (*e.g.*, attenuation bias, skewness) the results could have been obtained even if the CAPM were true, while Black–Jensen–Scholes employed the zero beta portfolio [26] to demonstrate that the CAPM could not be rejected by the data. Note that all any of these studies did was to indicate that, if one arrived with a strong belief in the CAPM, one could retain it in the face of contrary results.

Another oft-cited study was performed by Fama and MacBeth [9]. They performed the following regression:

$$\tilde{R}_{i,t} = \tilde{\gamma}_{0t} + \tilde{\gamma}_{1t}\beta_i + \tilde{\gamma}_{2t}\,\beta_i^2 + \tilde{\gamma}_{3t}\,s_i + \tilde{\eta}_{it}.$$

The t-statistics over the entire period 1935 to 1968 were as follows for the full equation:

$$t\,(\hat{\bar{\gamma}}_0) = .55;\ t\,(\hat{\bar{\gamma}}_1) = 1.85;\ t\,(\hat{\bar{\gamma}}_2) = -.86;\ t\,(\hat{\bar{\gamma}}_3) = 1.11.$$

In a theory where β is supposed to explain everything, the above results seem hardly conclusive. Furthermore, if the market were indeed pricing variance in addition to or instead of beta, its effect would be split between γ_1 and γ_3 above, possibly causing the former to be larger than would be the case if variance were included. Finally, although various combinations of the independent variables were run, no results were reported for runs with beta omitted. Fama–MacBeth interpreted the above t's as follows:

> But at least with the sample of the overall period t ($\hat{\gamma}_1$) achieves values supportive of the conclusion that on average there is a statistically observable positive relationship between return and risk. This is not the case with respect to t ($\hat{\gamma}_2$) and t ($\hat{\gamma}_3$). Even, or indeed especially, for the overall period, these t-statistics are close to zero [9, p. 624].

We have been unable to find a definition of what constitutes an "on average statistically observable" versus a "close to zero" relationship, nor arguments as to why a t of 1.85 constitutes the former and 1.11 the latter. Fama–MacBeth concluded, however, that:

> What we found . . . is that there are variables in addition to β_p that systematically affect period-by-period returns. Some of these omitted variables are apparently related to β_p^2 and $s_p(\epsilon_i)$. *But the latter are almost surely proxies, since there is no economic rationale for their presence in our stochastic risk-return model* (emphasis added) [9, p. 629].

In sum, do we interpret Fama–MacBeth as an empirical effort 1) conducted with a CAPM prior which showed that the prior could not be rejected (*i.e.*, similar to Miller–Scholes and Black–Jensen–Scholes above), or 2) conducted with a diffuse prior which found, after an investigation of several plausible alternatives, that the CAPM was (markedly?) superior? This is a key distinction which the literature of the New Finance tends to blur (along with the prove–not disprove distinction). If it is 2), then it should have powers to persuade (or discredit) those of differing beliefs. Yet, if it is 1), it may only be used to fend off those who would attempt to alter the beliefs of CAPM advocates. Although Fama–MacBeth are somewhat vague as to which they think their paper represents, their cited statements, plus our arguments, would cause us to put it much closer to 1).

In this same area, Foster [15] obtained the following results over the 1931–1974 period.

$$R_p = .0044 + .0055\,\hat{\beta}_p$$
$$(2.154)\quad(1.622)$$

and

$$R_p = .0059 + .0031\,\hat{\beta}_p + .0081\,\hat{\sigma}(\epsilon_p).$$
$$(3.246)\quad(.981)\qquad(1.481)$$

Foster noted: "While the coefficient on residual risk (Y_3) is more significant than that on relative risk (Y_2), neither is statistically significant from zero at the .05 level" [15, p. 50]. After noting the multicollinearity between β and $\sigma(\epsilon)$, he fell back to univariate testing. The β results are given above. The $\sigma(\epsilon)$ results were obtained in two forms as follows:

$$R_p - \beta_p\,(R_m - R_f) = Y_1 + Y_3\,\sigma(\epsilon_p)$$
$$\bar{Y}_3 = -.0668$$
$$(-1.197)$$
$$R_p - \beta_p\,(R_m - R_z) = Y_1 + Y_3\,\sigma(\epsilon_p).$$
$$\bar{Y}_3 = .0377$$
$$(1.037)$$

The portfolio grouping technique was then reversed, and the above was re-run. The t on beta was 1.668, while it was -1.004 on the risk-free and 1.271 on the zero beta specification of $\sigma(\epsilon)$. Foster concluded that:

> First, the results are consistent with the capital asset pricing model's prediction that relative risk explains differences in the expected returns of securities. Second, after controlling for differences in the relative risk of securities, there was no statistically significant evidence that residual risk explains differences in the expected returns of securities [15, p. 52].

This paper would not appear to purport to fall into category 2) above, and given t values of 1.48 vs. 1.62 it is not obvious that it provides much comfort in category 1).

All these papers allow β to explain all that it possibly can before $\sigma(\epsilon)$ is allowed into the picture. In particular, the above specification appears to allow $\sigma(\epsilon)$ a disproportionate share of any measurement error. Apparently, nobody tests (or reports) results of controlling for total risk and then seeing if beta has any incremental explanatory power. Nobody seems concerned that a variable which quite literally is not supposed to be there keeps appearing with a t of 1 to 1.5. Finally, not a word is spoken about what sort of model one is testing in the first place which causes a variable to change sign (see Y_3 above on R_f vs. R_z) depending upon its specification.

After a decade of testing, we still do not know what risk the market is pricing. Indeed, we do not seem to know at a statistically significant level from *ex-post* data that it is pricing risk at all (at least in any rational manner). Given the empirical problems with the risk-free version of the CAPM and Roll's [26] criticisms of the zero beta version, it is not obvious that we have a robust model to test the latter question. With the multicollinearity and statistical problems [11 and 15], we appear to be far away from testing the former.

These difficulties prevent us from doing a risk-pricing test with perfectly-diffuse priors. We can, however, use the famous dividend policy paper by Black and Scholes [4] as a brief illustration. They apparently approached the issue in this way and, after extensive investigation, concluded that they could not determine at any significant level whether dividend policy mattered and, if it did, in which direction it mattered. This must be the closest thing to a perfectly-diffuse posterior to ever appear in print. It conveyed either no information (perfectly-diffuse prior) or information that we do not know things we thought we did (false prior).

The point we are attempting to make about these (and many other) studies is that the prior belief seems to dominate everything else in determining the posterior. If one has a strong prior, he can usually "not disprove" it; if he starts with no prior, he usually gets "insignificant" results (which are generally unpublishable). In the spirit of Professor Fama, this leads us to one of those very strong nulls which one would not expect to hold exactly but which is surprisingly hard to refute:

> All the positive and significant results of empirical research in the New Finance represent either large-scale numerical examples of the prior beliefs of the researchers or else a sampling or methodological error which will not hold up on replication or with an alternative specification.

The astonishing thing about this seemingly outrageous statement is how close it comes to being a useful rule, from which exceptions can be duly noted; we suspect that it cannot be rejected at the 1% level. If we pursue it one step further, another point emerges:

> There is no essential difference between theoretical and empirical research in the New Finance. Both represent what, in another day and time, would have been called normative theory.

By this we mean that both demonstrate the logical conclusions to be derived from a given set of assumptions. The theoretical branch pushes through the equations with calculus; the empirical illustrates the process with data. The key point is that the empirical work becomes virtually as tautological as the theoretical.

Values — An Example of Goal Structures

> The most commonplace features of neoclassical and neo-Keynesian economics are the assumptions by which power, and therewith political content, is removed from the subject. The business firm is subordinate to the instruction of the market and, thereby, to the individual or household. The state is subordinate to the instruction of the citizen. There are exceptions, but these are to the general and controlling rule, and it is firmly on the rule that neoclassical theory is positioned. If the business firm is subordinate to the market — if that is its master — then it does not have power to deploy in the economy save as this is in the service of the market and the consumer. And the winning of action to influence or rig the behavior of markets apart, it cannot bring power to bear on the state for there the citizen is in charge [17, p. 2].
>
> Neoclassical economics is not without an instinct for survival. It rightly sees the unmanaged sovereignty of the consumer, the ultimate sovereignty of the citizen and the maximization of profits and resulting subordination of the firm to the market as the three legs of a tripod on which it stands. These are what exclude the role of power in the system [17, p. 5].

In perfectly competitive markets, it can be shown that maximizing behavior (*i.e.*, shareholder wealth maximization — SWM — or decisions made by the market value rule — MVR) is both enforced upon participants (for survival in that case) and Pareto optimal (by the "unseen hand" arguments). It does not appear to be generally understood, however, that the joint existence of enforcement and optimality (or even either one of them) is *only* guaranteed under perfect competition.

It would appear that a sufficient condition for the existence of SWM under imperfect competition would be that managers were perfectly and costlessly monitored agents of owners. The case of the owner-manager or the manager holding large options and receiving bonuses would fall under this classification. A more general circumstance would be that in which owners were aware of the alternatives confronting managers, knew which were accepted, and could immediately and costlessly remove any managers who did not make SWM choices. Obviously, few corporations would be covered by this description.

Generally, positivism appears at this point with the claim that firms may behave as though they were SWM even if they do not meet the above conditions or, for that matter, even if they are not trying to behave that way. The usual discussion may be found in the first chapter of any standard text (*e.g.*, [29]). On this issue, we continue to adopt our earlier position [12] that, in those cases where a decision involves a conflict between owner and manager interests, the managers tend to promote their own.

Our aim here is not to dredge up an old controversy, but rather to discuss the second point these texts invariably make: that whether or not firms are SWM, they should be. Aside from the complications of introducing a purely normative judgment into a positive context, our major difficulty with this position is that its normative basis is unclear and quite possibly wrong. We have at least two clues that these authors are not dealing with a world of perfect competition: 1) that they feel they must "sell" SWM in the first place, and 2) that they spend a good deal of space describing positive NPV projects which they acknowledge to arise from market imperfections.

What then is the normative basis for SWM when markets are imperfectly competitive? The textbook writers contend that positive NPV projects should be selected, that information should be disseminated in such a way that the maximum wealth increment is generated (*i.e.*, adopting the investment budget which maximizes NPV), and that all of this should accrue to the existing shareholders. Yet the NPV is simply the capitalized value of the rents from the oligopoly-oligopsony position of the enterprise. We have never seen any arguments as to the social optimality of even allowing such positions to be created, much less of allocating all the "ill-gotten gains" to shareholders. In any event, when it is put in this light, we are not sure this is a normative argument SWM advocates really want to pursue.

The problem above arises from an effort to impose first-best criteria on second-best situations. It is quite possible that the multiple and conflicting goals that managers trade off, satisficing behavior, and all the rest are really optimal in a second-best sense. At least, to invoke Professor Miller's criterion, they have shown tremendous survival value in the face of SWM and the New Finance.

If SWM quite possibly does not and should not exist in the real world, why is it thriving in the literature? Because, as far as we can tell, when combined with perfect-market assumptions, SWM allows the application of separation theorems to produce unique "answers." Of course, it is not obvious what the correspondence is to any question being raised in the real world.

It would be nice to conclude that SWM was a "harmless mutation," in that nobody really paid any attention to it except writers in arcane journals, and that it was just as well that they did not. Unfortunately, its persistence in the text and journal literature not only undermines whatever modest claim finance might have to be value-free, but it also imposes values of the most retrograde sort.[3] Combined with the perfect market Walrasian framework, any intrusion of government becomes automatically undesirable.[4] Since labor markets are presumed to clear at wage rates equal to the worker's marginal product, unions are similarly unnecessary. Finally, since managers and the firm are presumed subject to the ultimate control of the shareholders and the market, any effort to increase responsibility or information is opposed as, at best, costly and redundant and possibly harmful. Our "value-free science" becomes nothing more than a theoretical justification for the privileged remaining privileged.

> When the modern corporation acquires power over markets, power in the community, power over the state, power over belief, it is a political instrument, different in form and degree but not in kind from the state itself. To hold otherwise — to deny the political character of the modern corporation — is not merely to avoid the reality. It is to disguise the reality. The victims of that disguise are those we instruct in error. The beneficiaries are the institutions whose power we so disguise [17, p. 6].

Conclusions

The purpose of this paper has been to assess the current state of the finance discipline in light of the argument of Professor Miller that the differences between the institutionalists and the theorists of the New Finance in the profession are really not terribly fundamental or irreconcilable. Along the way, we have attempted to evaluate the contribution of the New Finance to our understanding of the world in which we live.

From our analysis, it seems that there exists a stalemate between the methods and conclusions of the theorists of the New Finance and the institutionalists. The latter, however, do not really have a model or a methodology at present which can enable them to develop a satisfying, consistent view of the world. Few want to go back to that earlier age of institutionalism which was characterized by specialized knowledge and lacked any theoretical structure. On the other hand, many share the misgivings about the New Finance that have been outlined in this paper.

Many observers appreciate the rigor of the New Finance but not the nihilism. Among these are the textbook and practitioner writers who back away from the pure models a few paces and attempt to

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[3] "When the empirical and historical justification of *laissez faire* weakened with the rise of mass production, giant corporations, and the growth of business monopoly arising from destructive competition, Smith's theory changed from one of liberal reform to one easily used to justify the status quo" [6, p. 717].

[4] "It is difficult, today, for serious macrotheorists to argue against income policies on the grounds that they distort the price signals that enable a (Walrasian) multimarket system to function efficiently. This is not to say that income policies are 'good things,' but rather that they cannot be assessed from a Walrasian perspective . . . " [33, p. 18].

derive decision rules as reasonable approximations. We also come to question these efforts, however (see [13, 14, 36]). We know by the very construction of their arguments that they cannot be exactly right. The extent to which their rules are reasonable approximations is an empirical question that nobody has addressed (because, among other reasons, to do so would require finding the "exact" answer, which cannot be done or may not be defined). In addition, as discussed above with respect to shareholder wealth maximization, the second-best solution in many cases may be quite far away from some "practical" approximation of the first-best solution.

This observation extends beyond the apparent capacity of such writers to advocate capital budgeting with beta in one chapter and the timing of financial policy in another. Consider instead the mundane task of computing the NPV of a capital budgeting proposal [36]. If all markets are in competitive equilibrium, the expected value of this exercise would not justify any costs of information production; it is a waste of time. If markets are simply in equilibrium, the exercise has a maximum expected incremental value of zero (because all expected rents have already been capitalized). If markets are not in equilibrium, the discount rate, and thus the entire process, is undefined.

We are in fact contending that the only logically consistent corporate finance text published in this decade is Fama-Miller [10]. All the rest attempt the logical impossibility of deriving policy conclusions from (implied) Walrasian structures in which the policy variables are endogenous and already optimized. One can accept the New Finance or one can reject it; one cannot make many modifications without logical error.

Initially, we should observe that our thought processes are surely more organized and our procedures better refined than those employed in the literature two decades ago. We can thank the theorists of the New Finance for this even if we are skeptical of the extremes to which they carry their research methodologies and their conclusions. Also the roles of *all* markets (product, factor, and financial) are now more fully appreciated in finance (even though we may not know exactly how they articulate in practice). This is another very positive contribution of the New Finance. The discipline will never again simply be a lengthy review of all the items on the right-hand side of the balance sheet; and this, too, is a good thing. Nevertheless, finance is neither a science nor a religion, and many of us have come to believe that the researchers of the New Finance curiously regard it as both.

To move ahead, we must all adopt a more flexible attitude with less emphasis on argument and more on open-minded analysis. Those of us who regard ourselves as institutionalists must be prepared to bring analytical procedures to bear on a terribly complicated phenomenon. The institutionalist framework has not been refurbished for a long time. When last seen, it was largely descriptive. New dimensions must be added.

> What has been valid about revolutionary theories during the past two centuries will not necessarily hold in the future. The relation between theory and practice could change. The most acclaimed theories of the future may turn out to be those of greatest analytical and mathematical elegance, without reference to pressing problems of economic life. The circumstances most congenial to this latter condition would be a world in which economic problems cease to be matters of large public concern. Such a state of economic bliss does not seem likely in the foreseeable future [6, p. 723].

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