Book Review

Toward Finance With Meaning, The Methodology of Finance: What it is and What it Can Be

Frankfurter and McGoun, in their delightful monograph, undertake the impossible, and in the process accomplish a great deal. The ultimate question regarding the nature of things remains unanswered, and an effective remedy for every ill that besets the current theory of finance is neither promised nor delivered. But this fascinating monograph does provide a carefully documented review of the current theory of finance, its philosophical origins, and a critique.

A central theme of the monograph is that financial economists have been indifferent to internal inconsistencies of theory, ignoring obvious flaws in order to preserve “cherished beliefs”. The authors argue that “meaning” in the sense of what the results of “analysis might mean for the finance community” is subordinated to “method” in the research process; that the theory of finance “has become so detached from its cultural meaning that it has little useful to say about society”; and therefore, that the last four decades of work by financial economists “have not greatly enhanced the arsenal of the financial decision maker”.

These points are all well taken and the authors’ support of them is insightful, but I would have preferred a slightly different title, say, On The Methodology of Finance: What it Really Is and How it Got to Be That Way. The monograph is beautifully organized around this topic, and is a most informative treatment of it. The discussion that follows is motivated by this impression of the work, and a sense of appreciation for the many new insights it provides. The monograph is organized in five sections and I will follow this organization in this review.

1. On meaning, methodology, language and translation

Consistent with their declared objective: to “advance methodology and meaning as important concerns of finance”, it seems quite appropriate that the authors begin by clarifying a number of oft-used (and misused) terms. I for one have not given much thought to the meaning of the word “method”, which refers to a technique, step-by-step procedure, or way of resolving a problem (correct or otherwise), and “methodology”, which has to do with the philosophical or logical justification for a method.
Theory uses words and symbols to convey “meaning” in the framework of the artificial reality or abstraction it creates. Implicit in any attempt to draw conclusions about a real economy, a real security, a real investor, a real market, or simply “reality” based on a theory, therefore, is the assumption that the words and symbols minutely defined by the theory to structure a model, have the same meanings and definitions in the reality that theory is supposed to describe. Along with methodology, meaning has become subordinate to method as the research process has become divorced from its subject matter.

... while data acquisition and analysis are central concerns of finance, what the results of that analysis might mean for the finance community, how this meaning is determined by the original data and the process by which it is analyzed, and how research itself unknowingly alter meaning are rarely, if ever, considered.

This theme is woven through the monograph, and illustrated under various headings.

2. Finance as a Social Science

Research in the natural sciences is predicated on the notion that:

... there are immutable laws governing the universe ... and that these laws can be discovered by ... forming hypotheses and testing their implications against observations.

... In the social sciences, however, ... meaning is not a matter of laws, but of ... unique personal, social, and historical conditions.

The authors credit Merton (1995), for example, for providing a “concise and accurate normative definition of the theory of finance” in his recent essay, because his definition focuses on the behavior of agents. They point out, however, that there has been little or no work in finance dealing with the behavior of agents. Instead, financial researchers have attempted to draw inferences from data about behavior (which, in many instances, is related to the behavior of actual agents only by heroic assumption), rather than observing that behavior. “Ostensibly concerned with behavior”, therefore, “but unconcerned with meaning, finance has become a social science masquerading as a natural science”.

3. Risk measurement

The terms “uncertainty” and “risk”, are not universally understood to refer to statistical dispersion of any sort, or even to have the same meaning. Chapter VIII of the monograph is devoted to the history of risk measurement, starting with an early attempt at quantification by Edgeworth (1888), and concluding with a discussion of the work of Markowitz (1952) and others, of the same probabilistic ilk, who are responsible for the institutionalization of (probabilistic) risk measurement represented in terms of statistical dispersion. In the interim, there were periods of apprehension about, and even rejection of, probabilistic risk measurement.

Knight (1921), for example, distinguishing between qualitative uncertainty and risk (measurable uncertainty), argued that:

Business decisions ... deal with situations that are far too unique ... for any sort of statistical tabulation to have any value for guidance. The conception of objectively measurable probability or chance is simply inapplicable. (p. 227)
Knight’s meaning was later slightly modified by probabilists, reacting to Bayesian views that were then in vogue, which defined risk in terms of the outcome of a random process whose “rule of randomness” or probability distribution is known (such as in the case of a flip of a fair coin), and associated uncertainty with the same event when the rule of randomness is not known (as in the case of a toss of a recently bent, and not yet tested, coin).

The authors point out that:

It is not a simple matter . . . to make the successive translations from the common language in which things and events are usually described into the formal language in which they are modeled and then into the observation language in which they are recorded and measured. Unfortunately, the difficulties are often masked by use of the same terms in all three languages,
The authors also remark that “Finance . . . pays insufficient attention to the possibility of translation error”, but seem to miss the point that such translation errors are not always unintended. The “formal language” used by Sharpe (1964) in his seminal paper was so carefully crafted to fit his objectives, for example, that many (if not most) financial researchers, to this day, confuse the meanings of many of the words and symbols he introduces under a cloud of, easily misunderstood and frequently ignored, Bayesian terminology.

The monograph develops from a rather interesting discussion about science and philosophy, to the philosophical aspects of Friedman’s positivism, and then to a critique. To “understand what finance is (and does)”, we are advised, one “must understand its philosophy”.
Friedman, I now learn, “is an instrumentalist (an offshoot of positivism) because for him “a theory is an instrument for action, the validity of which is measured by success”. A primary requisite of theory, according to this positivist perspective, is to “produce acceptable forecasts:

Viewed as a body of substantive hypothesis, theory is to be judged by its predictive power for the class of phenomena which it is intended to “explain.” . . . The only relevant test of the validity of a hypothesis is comparisons of its predictions with experience. . . . Factual evidence can never “prove” a hypothesis; it can only fail to disprove it. (1953, p. 9)

Unfortunately, these statements are easily misconstrued by supporters and detractors alike.

One can debate some of the authors’ interpretations, but for those of us who have not read Friedman’s essays with the care that is clearly justified by the impact the work had in creating the field of financial economics, and continues to have in justifying its theories, this part of the discussion, dealing with Friedman’s positivism and a critique, is especially useful. Some of Friedman’s early assertions are often cited, and others not. We all know, for example, that:

the relevant question to ask about the “assumptions” of theory is not whether they are descriptively “realistic”, for they never are, but whether they are sufficiently good approximations for the purpose at hand (Friedman, 1953, p. 15);

but I for one was never warned that:

efforts to separate valid from invalid assumptions do no more nor less than confuse the issue, promote misunderstanding of empirical evidence . . . , misdirect intellectual efforts that should have been devoted otherwise to the development of theories consistent with the tenets of positive economics God forbid, impede consensus, and result in general “mischief”.


Early in the monograph, the authors cite the CAPM as an example of a model without methodology or meaning which nevertheless continues to maintain “stranglehold” on the profession—perhaps, as they later point out, because it is a source of enormous rhetorical benefit to so many. I have no problem with most of their observations and conclusions, but I do disagree with their interpretations on at least one issue.

It is not clear to me that any of the CAPM’s “underlying assumptions” carry the implication that “risk can be measured statistically”. The model’s equilibrium implications do suggest uniquely different measures of risk for diversified portfolios and undiversified assets; but these are the equilibrium implications of a model that has no empirical support as distinct from its positive assumptions that require no empirical support. Myers (1972) demonstrated rather conclusively, moreover, that “there is no theoretical basis of equilibrium-restricted regressions”. To add to this modest handicap, Roll (1977, p. 131) later explained why “no two investigators who disagree on the market’s composition can be made to agree on the theory’s test results”.

It is difficult to understand, therefore, why two investigators who cannot “be made to agree on the theory’s test results”, should nevertheless agree on the meaning and relevance of measures of variation based on sums-of-squares and cross-product sums obtained by “equilibrium-restricted regression” on proxy variables that would engender similar agreement in tests of the CAPM. This is not to say, of course, that there are no statistical measures of variation that one can glean from the CAPM’s implications, nor that such measurements are not widely used to measure investment risk, but rather that neither the CAPM’s equilibrium assumptions nor tests provide any justification for their calculation or use.

Since mathematical reasoning was so successfully applied in the physical sciences, starting with work by Galileo and Newton, it might be supposed that the mathematization of economics and finance could yield similar rewards and insights. At a fairly mundane level, mathematical reasoning played a role in the development of modern theories of finance, but Bell’s (1933) observation that many such applications are “more numerology than science” seems as true today as then. A major characteristic of this numerology is

that large data bases are relentlessly processed with no regard for the particulars and peculiarities of individual entities. It is casually assumed that the very things that make the data real will be submerged by the laws of large numbers and some sort of meta-reality will remain afloat.

There is nothing wrong with mathematics in finance (or anywhere else), but, as the authors fail to note, the opportunities for abuse increase with each step up the ladder of mathematical sophistication not because reviewers are easily dazzled by mathematical brilliance, but are easily baffled by the lack of it. This is clearly a problem, but for some reason it escaped the authors’ notice. They do not miss the point, however, that an empirical test of a presumptuous model mathematically elegant or not can be more abusive than the model itself.

4. The profession, practice, and implications

The authors begin this part of the monograph with a contrast of academic and practitioner finance, which they argue “coexist as two cultures”, and end it by concluding that:

40 years of research have not greatly enhanced the arsenal of the financial decision maker. . . . Whether one looks at the field from the perspective of useful modeling or from the
vantage point of serious philosophical scrutiny, one must reach the conclusion that finance as a discipline has not created much and that finance as a methodology would have a difficult time demonstrating much independence.

The central issue is not necessarily whether practitioners use the theory, but how and for what purpose. The concept of risk-return tradeoff, for example, “provides an ideal excuse for substandard performance”, and clearly the CAPM has been “a godsend to practitioners in this way”.

Financial economics, the authors cleverly argue, “is a rhetorical science in that plausible stories compete for attention”. When a financial economist also provides a plausible reason for behaving in a certain way, therefore, or simply comes along at the right time, then people may begin behaving in the way that theory assumes they do or suggests they should:

The Black–Scholes/Merton option pricing model, for example, may well explain option pricing behavior only because the model itself is the source of pricing behavior. . . . The model was published shortly after the opening for the first specialized options exchange, and within months, traders were consulting Black–Scholes’ tables of calculations as to what options “should be” worth. . . . The model did not say what pricing behavior was nor did it say what pricing behavior should be. Rather, it convinced traders, and they made it what pricing behavior would be.

5. Toward finance with meaning

Had the monograph been titled: On The Methodology of Finance: What is Really is and How it Got to Be That Way, it might have ended with the presentation of three very sensible implications that the authors garner from the previous discussion: First, we should abandon the pretense of positivism in finance, because “there is simply nothing out there to be positive about”. Second, the methodology in financial economics requires radical revision. . . . Because no theory in finance can exist without a rationale or common-sense interpretation, it is appropriate to go directly to the rationale rather than attempt to concoct a specious theory.

Finally, the way that research is done should be changed. Where, for example, the object is to explain behavior, then the “appropriate methodology is to determine what that behavior was and is and how it came to be that way”.

I think this is sage advice, and would have been happy to end on this positive note. But Frankfurter and McGoun obviously did not begin this undertaking in order to follow a “path of least intellectual risk”, and do not end it in that way. The concluding section, Finance With Meaning, is really very short but is nevertheless the source of a second disagreement that I have with these authors.

If, in fact, human society is not governed by “lawlike associations between causes and effects as the physical universe appears to be”, then perhaps “there are methodologies and methods, in addition to those of the physical sciences which can contribute to understanding in the social sciences, including finance”. The authors argue that finance lacks meaning for at least three reasons: It presupposes a meaning of words independent of language, a meaning of interactions independent of actors, and a meaning of knowledge independent of how it is used.
I have no difficulty with this, but submit that finance lacks meaning for yet a different, and I think more compelling, reason: Much of what we do, much of what we say, and much of what we teach even in the most applied programs was conceived for the benefit of an academic culture that regards fact as a perturbation of reality, as defined and envisaged by theory. In this culture, facets of which cut across every academic discipline, “meaning” is less important than membership in determining pecuniary reward, and that is what makes the wheels go round. I do not believe, therefore, that we can reasonably hope to achieve finance with meaning merely by replacing a dogma “rooted in facts and objects”, with another rooted “in the models or images constructed out of those facts and the uses of the objects”. I do believe, on the other hand, that the proliferation of dogmatic and irrelevant theory will be more rather than less if we willy-nilly hand the next generation of “sophisticated financial researchers” subjective license.

As a case in point, the authors’ characterization of principles that are supposed to govern quantitative research seems motivated by a need to create a dichotomy between subjective and objective approaches, to fit neatly into “a scheme developed by Burrell and Morgan (1979)”. These principles, regardless of where they originate, are indeed subjective.

I agree fully with the authors’ main conclusion that research in finance over the last four decades has had few if any successes, that justification (if any) is based on rhetorical elegance rather than results, and that the ever-increasing emphasis on quantitative sophistication and mathematical elegance has led, increasingly, to subordination of meaning as either a concern or objective in finance. I agree also that finance will not “adopt new methods until it changes its methodology (the logic or philosophy of financial inquiry) and its sociology (what counts and what doesn’t, ...)” but I think the priorities should be reversed. We can keep ourselves fully employed by altering the methodology from time to time, by introducing new methods, and by inventing new stories, but unless we can change the sociology of finance we cannot hope to advance methodology and meaning as important concerns of finance.

References

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