

Book reviews

John H. Cochrane, *Asset Pricing, Revised Edition*, Princeton University Press, Princeton and Oxford (2005). xi + 533 pp., US\$ 85

Anyone who is not shocked by quantum theory does not understand it.
Neils Bohr, 1927.

Nobody understands quantum theory.
Richard Feynman, 1967.

During an informal luncheon discussion with an editor of one of the top finance journals, the chap told me that I was too concerned about philosophy. “We in finance,” he remarked, “do not care much about philosophy. We care about data and what data can tell us.”

If the reader infers from this statement that my mind-set is critical and perhaps even non-conformist, I would point out that I approached this book with anticipation, even a bit of awe. This, after all, is a book that, in its original version, won a prestigious award, and has now been revised (i.e., improved, enhanced, expanded, refined). Sheer volume (533 pp.+) suggests that the book covers every iota of what the ontology and epistemology of “modern finance” dictate when it comes to pricing financial assets.

And indeed it does.

Asset Pricing is a carefully and painstakingly written anthology of all that has been published and deemed worthy of mention by modern finance regarding the pricing of financial assets, i.e., all that is within the domain that it considers worth knowing. The book is the work of a true professional who, without a doubt, is in complete control of the subject. The text is written in a form that is easily readable by a graduate student or by an investment professional with a good quantitative background. I cannot agree more with one of the blurbs on the jacket’s back:

An excellent survey of asset pricing theory from the *modern* [my italics] viewpoint of stochastic discount factors and the associated geometry. This book was already a classic among finance scholars and on Ph.D. syllabi when it circulated in form of class notes.

The book is organized into five parts: (1) asset pricing theory, (2) estimation and evaluation of the models, (3) bonds and options, (4) empirical surveys, and (5) appendixes. The first four parts are compartmentalized into 21 chapters, and part 5 consists of three technical appendixes. Part 4, which comprises mostly the works of the author, cites “surveys,” but the models included here are not surveys in the true sense of the term as it is usually used in the social sciences, but are, in fact, the same old, same old.

Nearly every chapter contains a section of problems, a device that makes this an especially useful textbook for M.B.A. and Ph.D. courses. The sales volume that can be generated by all those graduate and doctoral students was, I suspect, more on the mind of the author than the desirability of producing a monograph or a treatise. Throughout the book, Cochrane is in full command of what he considers asset pricing to be all about. What is left out of this book, or glossed over, is not due, therefore, to lack of knowledge on the part of the author, but to intent.

What Cochrane does in *Asset Pricing*, very intelligently, convincingly, with great detail and superb skill—is purposefully perpetuate the ruling paradigm and its methodology. Entrenchment, however, is not the way to foster growth in scientific knowledge. This kind of iconoclasm, regrettably, is why financial economics is not likely to turn postmodern in our time.

Now that I have come to the crux of my criticism of this book, let us talk a bit about modernity and postmodernity, before I return to what I consider the shortcomings of *Asset Pricing*.

At the *fin de siècle* of the 19th century and the beginning of the 20th, most scientists believed that everything that there was to be discovered had been discovered, and that science (and the scientific method) had the answer to every question, malaise, and unexpected natural occurrence to affect mankind. At the conclusion of WWI, it was even believed that there would be peace “in our time.” This was the era of modernity, and its underlying mechanism was the scientific method.

Finance became modern with the seminal works of Harry Markowitz (1952, 1959) (whose *normative* model of portfolio selection successfully “scientified” a process that previously was totally intuitive and subjective. Yet it took the methodology of positive economics, and efficient markets hypothesis (EMH), along with the capital asset pricing model (CAPM), to turn the knowledge of finance into modern financial theory (MFT). Then, the research program turned *positive* and market efficiency became a political agenda. I find it not surprising that Cochrane has this, *and just this*, to say about Harry’s invention:

The early asset pricing theorists, [note the plural—my comment] in particular Markowitz (1952), posed the question just right: they wanted to treat assets in apples and oranges, indifference curve and budget set framework of microeconomics. The problem was, what labels to put on the axes?

Their brilliant insight was to put the mean and variance of the portfolio return on the axes: to treat these as ‘hedonics’ by which the investor valued their portfolio (ibid., p. 128).¹

Modern finance needed, however, a person whose behavior would be consistent with whatever the model’s creators wanted it to be consistent with. This fictitious person is the *homo economicus* (created on the axiomatic approach of Von Neuman and Morgenstern, 1953) whose decision-making process is referred to as the expected utility maxim (EUM). This non-person person is a perfectly calculating, economically astute and statistically perceptive creation, completely detached from the social environment within which it operates.²

I italicized the words *normative* and *positive* to highlight the distinction. A normative model is good as long as its user believes it is good. This is nothing else but ex-post performance. In the

¹ I will return a bit later to this quotation, especially the brilliant part (and also the hedonics part), because both I and the 1990 Nobel Prize in Economics Committee thought a bit differently about the genius of Markowitz.

² In the economics literature, the EUM has been widely criticized, to the point of being discredited. From my personal experience, when I see two guys with shoulder-length, unwashed hair, wearing dirty baseball caps, driving in an old, beat-up Ford 150 pickup truck with a rear-window sticker of **W04**, I wonder whether this *homo economicus* is not just psychologically flat, but also a sociopath.

positive case, without exception everyone must behave according to the model's dictum. Modern finance to this day rules the land with utmost orthodoxy, denying the existence of anything else with mockery, scorn or complete disregard.

But the times, as Bob Dylan opined, were "a-changing." The Second World War, and the Third (as some now call the Cold War), and perhaps the Fourth, which is globalization and the fight for the oil reserves of the earth, have moved us to postmodernity.

Postmodernity evolved, of course, from the realization, however belated, that modernity did not have the answer for most, if any, of the complex problems facing humanity. In a *Harper's* essay, [Fish \(2002\)](#) succinctly and rather elegantly demarks the boundaries of postmodernism, quoting Edward Rothstein's op ed piece from the September 22, 2001, issue of the *New York Times*:

A full account, or even definition of postmodernism would be out of place here, but it may be enough to look at one offered by Rothstein, who begins by saying "Postmodernists challenge assertions that truth and ethical judgment have any objective validity." Well, it depends on what do you mean by "objective." If you mean a standard of validity and value that is independent of any historically emergent and therefore revisable system, of thought and practice, then it is true that many postmodernist would deny that any such standard is or could ever be available. But if by "objective" one means a standard of validity and value that is backed up by the tried-and-true procedures and protocols of a well-developed practice or discipline—history, physics, economics, psychology, etc.—then such standards are all around us, and we make use of them all the time without any metaphysical anxiety.

So the question is what do we mean by standard? And, how valid and lasting are these standards? In physics, the general theory has been augmented by quantum mechanics, producing new concepts such as quantum foam, molecular computing, subatomic particles research, and neutrinos.

Even economics, one of the most strictly controlled orthodox branches of science (perhaps meta-science?) recognizes behavioral economics, meaning that neoclassicism does not have all the answers. And if one considers the movement started in 2000 that calls itself post autistic economics (PAE), it would be fair to assume that economics finally set out to be post-modern.

Modern finance and the school mostly associated with its methodology and methods fight post-modernity with all the academic might and influence they can muster. That is why, despite the fact that the 2002 Nobel Prize in economics was awarded to Daniel Kahneman and Vernon Smith, precisely because of their contribution to financial theory, *Asset Pricing* fails to mention behavioral finance (which now has a journal to its name), Prospect Theory, over- and under-reaction, contrarian investment strategies, or the works of Asquith and Mullins, Statman and Shefrin, and Dreman. Thaler and DeBondt, whose over-reaction hypothesis is 20 years old, ([DeBondt and Thaler, 1985](#)) are mentioned only in passing, and out of context. As many *champions of orthodoxy* have learned, it is better to banish heretics to obscurity than to burn them at the stake.

Returning now to my earlier quotation regarding Markowitz's contribution, I would point out that, contrary to what Professor Cochrane thinks, Harry's true brilliance was to pull together an axiomatic approach encompassing statistics and mathematical programming, and apply all these to a finance problem, something no one had done before. When one considers that Markowitz developed this approach as a student while taking a graduate course that dealt with applications of programming to problems in economics, the achievement is nothing less than that of a genius. Small wonder then that the many congratulatory articles published after Harry received the Nobel Prize were unanimous in proclaiming him the creator of modern finance.

Even so, the Markowitz model is normative. It was later simplified by his student, William Sharpe (1963) at Markowitz's behest. From this simplification, Sharpe (1964) concocted the CAPM, which is a positive model with normative implications. This feat was accomplished by piling more and more unrealistic assumptions atop those of the normative model. Fama (1965, 1970) went even further with the EMH, which he tied to the CAPM, creating – presto! – the research program of modern finance. So, without Markowitz, Cochrane could not have written *Asset Pricing*. Nowhere in the book, however, does Cochrane mention this, as though the evolution of theories is of no consequence.

I was reminded again of the editor's comment during our informal luncheon when I read this passage in *Asset Pricing*:

These paragraphs do not contain original ideas, and they mirror changes in the philosophy of science more broadly. Fifty years ago, the reigning philosophy of science focused on the idea that scientists provide rejectable hypotheses. This idea runs through philosophical writings exemplified by Popper (1959), classical statistical decision theory, and mirrored in economics by Friedman (1953). However, this methodology contains an important inconsistency. Though researchers are supposed to let the data decide, writers on methodology do not look at how actual theories evolved. It was, as in Friedman's title, a "Methodology of positive economics," not a "positive methodology of economics." Why should methodology be normative, a result of philosophical speculation, and not an empirical discipline like everything else? In a very famous book, Kuhn (1970) looked at the history of scientific revolutions, and found that the actual process had very little to do with the formal methodology. McCloskey (1983, 1998) has gone even further, examining the "rhetoric" of economics: the kinds of arguments that persuaded people to change their minds about economic theories. Needless to say, the largest *t*-statistic did not win!

Kuhn's and especially McCloskey's ideas are not popular in the finance and economics professions. Precisely, they are not popular in how people talk about their work, though they describe well how people actually do their work. Most people in the fields cling to the normative, rejectable-hypothesis view of methodology. But we need not suppose that they would be popular. The ideas of economics and finance are not popular among the agents in the models. How many stock market investors even know what a random walk or the CAPM is, let alone believe those models have even a grain of truth? Why should the agents in the models of how scientific ideas evolve have an intuitive understanding of models? "As if" rationality can apply to us as well.

Philosophical debates aside, a researcher who wants his ideas to be convincing, as well as right, would do well to study how ideas have in the past convinced people, rather than just study a statistical decision theorist's ideas about how ideas *should* convince people. Kuhn, and, in economics McCloskey have done that, and their histories are worth reading. In the end, statistical properties may be a poor way to choose statistical methods (*ibid.*, p. 306).

If I wanted to tax the patience of the reader and test the tolerance of the book review editor of this *Journal*, I would have written a dozen or so pages about the errors, misconceptions, and confusions in this passage concerning method, methodology, what is positive and what is normative, what is positivism and what is realism, and the interpretation of Friedman and/or Popper. And that the mention of Kuhn and McCloskey is nothing short of name-dropping, to show that the author is up-to-date in what is new-age in economics. These are all serious philosophical or methodological issues, but I wish neither to tax, nor test.

It is precisely the studies that are covered so richly in the book in which more often than not statistical properties determine the statistical methods, applied to the ever passive and obedient data that was sold to everyone who could pay the price of Chicago's CRSP tapes: in Fama's (1991) vernacular the creation of a "a cottage industry." Still, I must point out the serious historical inaccuracy regarding the proliferation of academic models, especially the CAPM, in the investment community.³

"How many stock market investors even know what a random walk, or the CAPM is. . ." (ibid.) asks Cochrane leaving us with the understanding that the number must be very small. Not quite so. In fact, the CAPM which was introduced to the professional investment community as a new financial product, packaged as the "Index Fund" had its golden years from the mid 1970s until the next stock market boom. The most ardent supporters, the Templar Knights of beta, were cloistered in Chicago and appeared in their weekly seminars as the "murderers row," ready to tear to shreds the unfortunate practitioner speaker who dared to argue that the market is either not too efficient, or there is a better way to invest than in the market portfolio.

Then the 1990s arrived and with them the CAPM encomia. Modern finance was declared dead, and some argued that the failure of America to compete in world markets was to blame on capital budgeting based on the CAPM. This was so, because M.B.A. programs "tattooed" the model on the brains of their graduates. Both the arguments, as well as the idea of using the CAPM as the only capital budgeting tool were based on falsehood. But the latter was sold by academe, and used by those who according to Cochrane never heard of it.

Perhaps the biggest void of *Asset Pricing* is not what is missing, but rather what is included in its 533 pages. With all that said, I consider Cochrane's book an exemplary of why financial economics is not entering the age of postmodernity any time soon.

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J.E. King (Ed.), *The Elgar Companion of Post Keynesian Economics*, Edward Elgar, Cheltenham, UK/Northampton, MA, USA, 2005 (405 + xvii pp., \$60.00, indexes both for names and subject, ISBN 1845422317).

This book consists of about 80 essays that provide up-to-date, concise and generally very readable guides to key theoretical elements in Post Keynesian thinking, or Post Keynesian perspective on macroeconomics data. It includes a piece on the *Journal of Post Keynesian Economics* but unlike some other volumes in the *Elgar Companion* series, it eschews biographical entries, the editor rightly pointing out that there is already a good source of these in [Arestis and Sawyer \(2000\)](#). It was first published in hard cover form in 2003, and its republication in soft cover form is to be welcomed as it is an excellent reference source for Post Keynesian academics and their advanced-level students and for adherents to any other economics research program interested in seeing what Post Keynesianism has to offer. I suspect that Post Keynesian academics are going to have a dual need to keep a copy readily at hand: not merely will many of the essays in this book be just right for inclusion in course readers and for using when writing lectures, but also, and regrettably, they may need to check whether it has proved all too appealing to students with plagiaristic tendencies. The choice of contributors is generally excellent, as one would expect given that the editor is John King, whose unparalleled knowledge of the [Post Keynesian literature is evident from his \(1995\) annotated bibliography of it up to 1995 and his \(2002\) history of Post Keynesian economics](#). The only entry that I found rather frustrating to read was Laurence Seidman's piece on tax-based incomes policy, which offered a lucid account of the origins of the policy idea and interest it attracted but never really explained how a system of tax incentive might be used to stop a wages–prices spiral.

Anyone who sets out to explore Post Keynesian economics from scratch will be well armed with just five works: this *Elgar Companion*, King's earlier two books, [Victoria Chick's \(1983\) brilliant reconsideration of Keynes's General Theory](#) and [Marc Lavoie's \(1992\) well-conceived attempt to show how the elements of Post Keynesian economics fit together](#). For myself, as one who originally learnt Post Keynesian economics first-hand via the lectures and books of Joan Robinson and Nicholas Kaldor (both of whose distinctive contributions are treated to their own entries) but who had been away from the literature for a decade, the review copy functioned very well as a convenient updating tool whilst also revealing, somewhat depressingly, that Post Keynesian economics lately has made rather little headway. The lack of progress has nothing to do with the Post Keynesian perspective being irrelevant in a world where neo-liberalism and the 'Third Way' (covered in a thought provoking entry by Egon Matzner) dominate the making of economic policy. On the contrary, it is immediately evident that Post Keynesians have something important to say of great contemporary relevance when one sees that in viewing the globalization phenomenon they argue the case for viewing trade patterns in terms of absolute advantage, not comparative advantage, due, amongst other things, to the presence of China as an enormous labour