

# Financial Economics, Risk and Information:

## An Introduction to Methods and Models

Author: Macelo Bianconi • Publisher: World Scientific, Nov. 2003 • ISBN: 9812385029

I can review this book in one sentence. It presents the same material as Jonathan Ingersoll's "Theory of Financial Decision Making," from the standpoint of economics rather than finance. I don't recommend anyone buy this instead of Ingersoll; that book remains an essential work in quantitative finance. If you already understand Ingersoll, Bianconi's book can introduce you to economics. If you already understand economics, it can introduce you to Ingersoll. If you already understand neither or both, you don't need this book.

I am now going to prove I was a professor by making you wade through a half century of history and some theory, to elaborate on that simple thought. Despite the close connection between finance and economics, the two fields were never united. Early economists paid little attention to financial markets, which seems strange given both the influence of markets on the economy and the high-quality data they generate. The field preferred abstractions like interest rates, capital and trade flows to observable things like bonds, capital markets and actual trading. Academic finance was a purely descriptive field. Students who could not master difficult fields were taught what letter of credit was and what documents were needed for a bond underwriting, in preparation for going to work in granddad's investment bank.

During the Applied Mathematics revolution of the 1950s, both finance and economics professors took their first serious looks at financial markets. But their approaches were different. Born as a descriptive field, finance developed models to explain why things are the way they are. Economics, in contrast, was full of hubris as a proscriptive field at the time, and developed models to demonstrate the way things should be.

In finance, the core principle was efficient markets; models had to explain actual price movements in purely rational terms. Over the next 20 years this led to the development of important models like the Modigliani-Miller model of capital structure, the Capital Asset Pricing Model and the Black-Scholes options-pricing model.

Most economists laughed at these constructs. They had

none of the features of good models in economics. The assumptions were clearly false, the mathematics were trivial and, despite enormous effort, there was no convincing empirical support. Meanwhile, economists like Kenneth Arrow, Gerard Debreu, John Nash and John Von Neumann were developing beautiful mathematics starting from common-sense assumptions, and other economists like Paul Samuelson and Alan Greenspan were producing tour de force empirical studies. The serious finance researchers took refuge at places like the University of Chicago, far from the centers of status and power.

In the following 20 years, things reversed. Finance professors won back their home turf from economists, and what came to be called the Chicago School took over mainstream economics. Respect for post-war economic theory was savaged by world events in the 1970s, while the 1980s saw glory (along with lots of money) lavished on finance. Marxism, Keynesianism, Neoclassicism and other economic sects were banished to liberal arts universities and government — while students, journalists and employers flocked to finance departments. Finance might not have been mathematically elegant or supported by data, but it worked.

The triumph of finance has been so complete that many beautiful and important ideas are in danger of being lost. Young people with an interest in money and the mathematical talent to tackle topology, game theory and functional analysis are likely to be steered directly into stochastic partial differential equations — in preparation for

This review was written by Aaron Brown, a member of GRR's editorial board. Mr. Brown is the vice-president of risk technology and systems at Citigroup in New York.

## BOOK REVIEW

becoming quants on trading desks. Even if students take the time to read classics, like Debreu's "Theory of Value" or Arrow's "Aspects of the Theory of Risk-Bearing," these works will seem unrelated to modern theory.

Bianconi has done an excellent job of culling the best of this material and integrating it seamlessly into a standard presentation of mathematical finance. Attribution and reference is meticulous. Viewed as an annotated bibliography of financial economics, the work is without peer. It adds historical and theoretical depth to the core ideas of finance.

However, it has some drawbacks as a text to be read sequentially. Notation is ugly. For example, when a variable or integral sign has both a superscript and a subscript, they are not aligned horizontally; rather, either the superscript or the subscript is floating off on its own. In addition, fractions are written on the same line with a slash separating the numerator and denominator, and other multilevel mathematical expressions are written the same way. There's no excuse for this sort of thing with LaTeX available.

Aside from the problems I just mentioned, the notation is also inconsistent from section to section. That can actually help someone looking up just one part and comparing with referenced papers, but it interferes with use as a textbook.

Further dampening its visual appeal, the book's text is laid out poorly. It is too crowded on the page and symbols are mixed sloppily with text. The figures, however, are carefully drawn in a beautifully minimal style; that's a level of quality that you rarely see these days, even in the most expensive technical books.

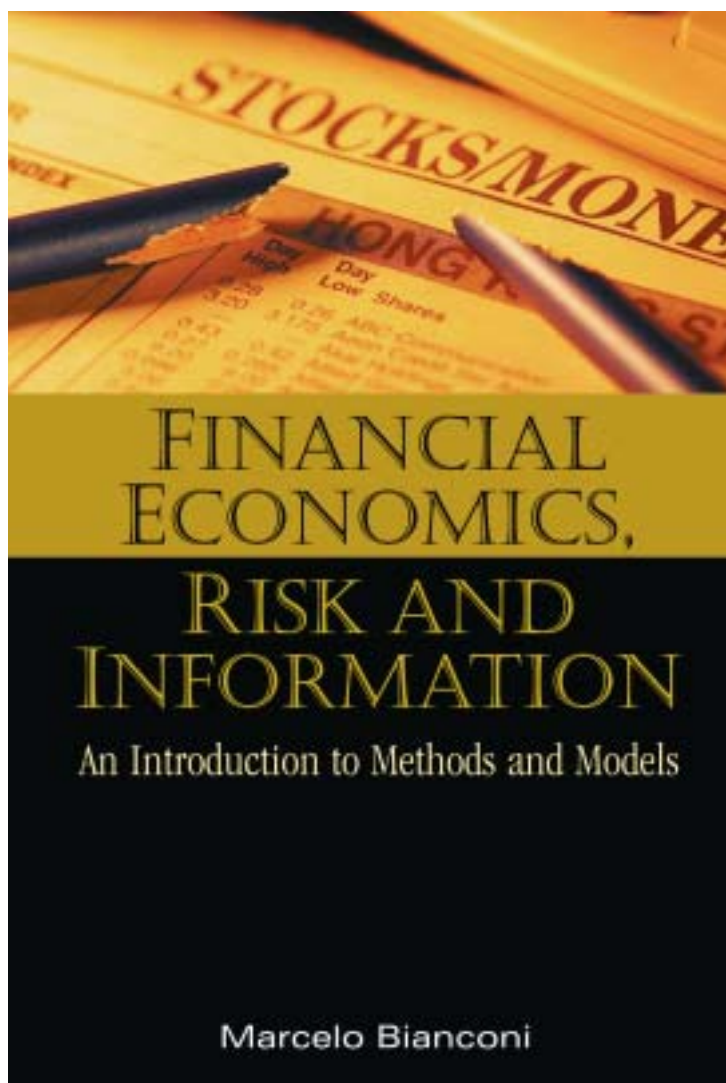
Anyone considering adopting this book as a textbook should be aware that the chapter summaries are barely more than placeholders, and the chapter problems merely ask students to repeat specific material from the book.

On the plus side, the book is self-contained. All necessary mathematics is explained in early chapters, starting from a very basic level; so students can refer back as needed. (Unfortunately, the inconsistent notation makes this more challenging than it should be.)

The book's excellent references and literature notes are invaluable, and it does a very good job of managing the mathematical level. Almost all of it is comprehensible to someone who has passed a college calculus course, but it is possible to read at a greater depth as well. The more difficult ideas are explained in both heuristic and mathe-

matical ways, so readers can learn in their preferred manner. The book would benefit from numerical examples and problems, both of which are absent.

A second edition of the book should be printed, as long as the publisher agrees to appoint a meticulous editor to enhance the design *and* retains a graduate student to improve the chapter summaries and problems. That would make it an excellent textbook for a course in Economics of Finance, or a supplementary text in Introduction to Quantitative Finance. A pleasing design would also improve its appeal to theoretically-minded practitioners.



While I'm giving my wish list, a new edition of Ingersoll is long past due. A lot of things have changed since 1987 (and the book is virtually identical to my notes from his course in 1981). Is there a publisher out there interested in a boxed set with revised editions of both these books? That would be a fitting unification to the sundered fields. ■

# Modern Risk Management: A History

Editor: Peter Field • Publisher: Risk Books, July 2003 • ISBN: 1904339050

**T**his book is a collection of 55 (yes!) discrete essays on various aspects of risk management. Edited by Peter Field, it divides its chapters into ten categories, which I paraphrase and condense as “history,” “philosophy,” “software,” “quantitative aspects,” “practical application,” “regulatory issues,” “derivative disaster case studies” and “biographies.”

The challenge for this reviewer is to give a fair sense of the breadth and value of “Modern Risk Management,” without simply transcribing notes for all or most of the 55 contributions. Further, the question is not just whether “value” exists in this book (it does), but whether buying and reading “Modern Risk Management” (MRM) will likely justify the reader’s investments of time and expense – both of which are substantial.

My first observation of MRM is that it’s dense, both physically and informationally. With just more than six hundred pages, the book is surprisingly heavy for its size. It’s quite a challenge to read MRM with one hand while standing on a moving subway train.

MRM shares all the advantages and disadvantages of books that are really just a collection of essays. There’s no cohesion at all among the chapters, with the exception of literally just a handful of references in one chapter to topics in other chapters. Yet the lack of cohesion means that the reader gets many viewpoints. MRM at some point appeals to all readers, because the authors themselves cover a wide range of topics.

One of my favorite passages is Paul Samuelson’s (Nobel laureate academic) lament that he must “[clean] out Augean stables” by disabusing more common folk of “the notion that the geometric mean maximiser will achieve meaningful maximization of a portfolio’s ‘growth rate.’” This may come as news to the academics, but I can think of many more menial tasks in the implementation of risk management principles. Writing code that understands the difference between period end dates and payment dates in bond and swap calculations is just one example!

At the other end of the book’s spectrum was the “Analysis of the Orange County Disaster,” which assumes that readers don’t understand interest-rate duration. The author then teaches the concept. Many other

articles, primarily in the “practical application” and “derivative disaster case studies” categories, also assume that readers have little prior knowledge.

In some ways I feel MRM fits me particularly well. My educational background is entirely quantitative. I’d never studied economics, finance or accounting prior to beginning a capital markets career 11 years ago. Virtually everything I know in these fields, then, is either self-taught or stems from on-the-job training. I have many knowledge gaps as a result.

Excellent reviews of important topics included chapters entitled “Markowitz Mean-Variance Portfolio Theory,” “Equilibrium Asset Pricing and Discount Factors” and “The Modigliani-Miller Propositions.”

In the Markowitz review, the author writes well and uses mathematics in just the right measure to explain the subject. With remarkable economy, he explains the construction of the efficient frontier for optimal investment portfolio performance. The discussion fully incorporates the author’s assumptions and the limitations of those assumptions. The chapter on Modigliani-Miller is also stimulating. The second proposition (that weighted average cost of capital is independent of leverage) challenges my own implementation of shareholder value in portfolio management modeling. I had treated cost of equity as if this were senior management’s target return for its shareholders, and cost of debt as market-driven. I now need to re-consider this underlying premise, given the Modigliani-Miller observation that the market dictates both components.

The “Equilibrium Asset Pricing” chapter affected me differently. In very clear and logical exposition, the author gives the economists’ view of asset pricing and explains that the paradigm is more general than the capital asset

This review was written by Dr. Joseph Pimbley, a member of GRR’s editorial board. Dr. Pimbley is the managing director of quantitative risk management at American Capital Access.

pricing model (CAPM). My reaction alternated frequently between fascination and (intellectual) scorn. The fascination ensued from the marked difference of the author's view from mine. For example, I consider a "discount factor" to be merely a reflection of interest rates and risk governing a future payment. The economist's view is not nearly so elementary. My scorn is that I consider it folly to pretend to derive the value of a "market variable." The market sets the value of GE stock, for example. So I don't believe any model that purports to derive the value of GE stock from assumptions and imprecise observations of the economy, investor utility functions and similar factors. But maybe I'm wrong! The real point is that the article itself makes its case eloquently and provocatively.

"Extreme Value Theory and Statistics for Heavy Tail Data," on the other hand, is an excellent essay, because the authors show and analyze real data in a manner that makes the theory come alive. In the spirit of much of MRM, this article teaches the subject well to readers with no prior exposure to extreme value theory. Another article that shares this characteristic of teaching a new subject, and demonstrates the diversity of MRM, is "Evolution of the Global Weather Derivatives Market." In just 18 pages, the author provides enough information to enable readers to be highly conversant in the key issues of this market.

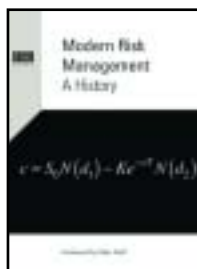
However, the essay that stands out most clearly for me – of all the fifty-five – is "Theory and Practice of Model Risk Management." The level of thought and logic is deep and philosophical. What a pleasure to read! While I likely expected the theme to be something like "model risk is the risk that you're using a bad model," the author asks and answers far more interesting questions. He plants his stake in the ground via the following definition: "Model risk is the risk of occurrence of a significant difference between the mark-to-model value of a complex and/or illiquid instrument, and the price at which the same instrument is revealed to have traded in the market." The author explains clearly why this is the right definition, and shows where the interests of the trader part with those of the risk manager and institution. He argues that validating a model is not equivalent to finding the "best" model and emphasizes that it's important to know what models other players use. It's deeply satisfying to have a teacher lead you to conclusions that were not at all obvious at the outset.

As a broad statement, the nine chapters on "derivative disasters" disappointed me. My expectations were high since this is a topic I relish. It's great to learn from the mistakes of others and to get a good view of how other people understand – and sometimes fumble – the issues that we encounter in our profession. I had hoped to find some new insights but, with one exception, found none.

Let me hasten to add that all these chapters read well and kept me focused. Moments of revelation, however, were too few.

The "disaster stories" in MRM are those of Long-Term Capital Management, Metallgesellschaft, Orange County, Allied Irish Bank, Bankers Trust (Procter & Gamble), Barings, Hammersmith and Fulham and Sumitomo Corporation. I wish MRM had included Kidder Peabody. That's one of my favorite stories and I've yet to hear a proof or disproof of my pet theory: that Kidder's model of the infamous trade was wrong, that the trader believed the model and that he thought he'd discovered a "brilliant" trading strategy, which he guarded zealously.

An interesting case is that of Metallgesellschaft (MG). The author of this chapter paints the picture we remember of the fixed-price contracts that MG held with retail customers for gasoline, heating oil and diesel fuel. MG hedged this risk with appropriate positions in futures contracts for various petroleum products. The most glaring complication is that the futures contracts required daily liquidity, while the retail contracts had no such cash settlements and ran for five to 10 years! Thus, though the fair value of the two sets of trades should have balanced, the liquidity



requirements could not have been more different. This was the story I already knew, but the author added more. MG inexplicably unwound the retail contracts (which should have been deeply "in-the-money" to MG) at no cost to the customers. Though the numbers appear not to be public, the author's estimate is that MG took a loss of \$800 million, rather than enforce its long-dated contracts!

What logic could possibly support such a decision? I love the author's suggestion that the management team responsible for the hedging program was long gone at this point. The allegation is that new management chose to take the loss, blame prior management and "wipe the slate clean," rather than maximize MG's recovery. Politics über alles!

While the "derivative disasters" chapters are a bit disappointing, the biographies are a pleasant surprise. MRM contains accounts (some of them autobiographical) of Fischer Black, John Cox, Emanuel Derman, John Hull, Jonathan Ingersoll, Robert Jarrow, Harry Markowitz, Robert Merton, Merton Miller, Franco Modigliani, Stephen Ross, Mark Rubinstein, Paul Samuelson, Myron Scholes, William Sharpe and Oldrich Vasicek. While it may be going too far to say that one can learn concrete lessons from the lives and careers of these gentlemen, their experiences are nonetheless captivating.

All in all, I benefited significantly from poring through "Modern Risk Management," and therefore recommend the experience to capital markets colleagues who enjoy understanding the deeper meaning of our profession. ■