

If you are comfortable with the concepts of Martingale and Brownian motion, have an interest in finance and are familiar with the Cameron-Martin-Girsanov theorem on change of measure, you possess the tools to read *Mathematics of Derivative Securities*. If this is not the case, but you are nevertheless interested in quantitative finance, a long but worthwhile apprenticeship through more basic textbooks is strongly advised.

From January to June 1995, the Isaac Newton Institute for Mathematical Sciences hosted a research programme on financial mathematics in Cambridge, attended by more than 300 scholars and financial practitioners. Most of the individual papers in this volume of 26 were tabled at one of the 150 seminars that took place as part of the overall programme.

The editors, Michael Dempster and Stanley Pliska, are very well-known names in the field of quantitative finance. I had the privilege to sit through a general presentation by Dempster at Imperial College in 1996 on the state of academic research in finance, and I remember how pleased I was with its clarity — I could go back to my notes several months after the event and still recall in detail what he had said.

The more philosophical aspects of this precious and weighty volume are covered by none other than Robert Merton, who reminds us that the "origins of much of the mathematics in modern finance can be traced to Louis Bachelier's 1900 dissertation on the theory of speculation". He goes on to say that "this work marks the twin births of both the continuous-time mathematics of stochastic processes and the continuous-time economics of derivative-security pricing", rediscovered in modern times by Kiyoshi Ito and Paul Samuelson.

I would like to pursue two lines of thought. First, what is the value of this book? Second, what is the general outlook for academic research in quantitative finance?

No one could deny the value of this volume. It is not an all-inclusive record of the six-month research programme, though it incorporates the best of what was said and done during this time. Its not-so-immediate publication makes it less relevant for the practitioners, who have much more of a keen eye for proprietary research and for a very quick time-to-market, without which the hope to gain a temporary competitive advantage and make a hefty profit on applied research rapidly fades. Academics, however, are used to delayed publication, partly because of their own scale of values and priorities, where perfection features much more prominently than speed, and partly because of the slow but necessary process of peer-group review before publication.

But who does this volume aim to serve on the whole? Academics or practitioners? The value of more refined renderings of issues such as "Stochastic calculus and Markov methods", "The risk premium in trading equilibria", and "(On the) numeraire portfolio" is ever decreasing. This is not a book of vulgarisation or of broad mass education for scores of operators in the finance industry who are not financial experts.

"Hedging long maturity commodity commitments" is, of course, of interest to academics, but only to a minority of financial practitioners — and that minority would probably prefer a single-theme book dedicated to their own specific issues rather than a chapter in a more general volume. "Option pricing in incomplete markets" is such a fundamental issue to academics and practitioners alike that it is bound to generate interest among target readers, alongside more detailed issues such as "Swap derivatives in a Gaussian HJM framework", "Numerical valuation of cross-currency swaps and swaptions" or, earlier in the text, "Some combinations of Asian, Parisian and barrier options".

These days, however, most practitioners are interested in getting a competitive edge

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Luddites beware

in trading and risk-managing credit derivatives or other topical issues and appreciate academics' expertise; they are not interested so much in the n th refinement of their valuation model or in getting to the last 1/100th of a basis point in already largely commoditised products.

Mathematics of Derivative Securities

Edited By Michael Dempster and Stanley Pliska

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I am sure that this book will find its rightful place in many libraries because of its academic authoritativeness and its usefulness as a reference tool. I do not, however, think it has either a novelty value nor a great textbook value. That, of course, does not detract from the fact that I personally enjoyed the fatigue of pushing my modest brain through it. It offers superb insight into many classical and less classical issues of modern quantitative finance.

Now to the second line of thought — what is the general outlook for academic research in quantitative finance?

August 1998: the events in Russia and in emerging markets in general, and the rescue of Long Term Capital Management, have again turned public opinion against derivative securities. And yet, derivative securities are no more responsible for their inappropriate utilisation than the existence of good wine is responsible for those who drive in a drunken stupor. Excessive financial leverage and poor risk management can be achieved in many other ways, not only through the use of derivatives.

More to the point, I strongly doubt that the rate of economic growth in developed countries that we have experienced in the

past two decades could have been achieved without developments in quantitative finance, particularly in derivatives research. My generally optimistic view of human intelligence makes me believe that research in quantitative finance will attract investment.

Today we have very few industrial Luddites among the general public, but tend to concentrate the surviving minority in the national parliaments. I believe financial Luddites will be defeated in the early part of the next century when, hopefully, better-educated journalists will stop pandering to such tendencies and learn the difference between an economic subject and object, the same way as they seem able to do in grammatical terms.

I firmly believe that the challenge to academic research in quantitative finance will not be an external one, but an internal one. If research tries to attack new problems of direct relevance to practitioners, even if the mathematical tools for solving them are not clearly identified, great progress will be achieved and, possibly, even new mathematical techniques and domains will be opened.

When the next programme of the Isaac Newton Institute takes place, I hope it will go single-mindedly for brand-new areas of exploration and make its findings promptly available on the web, even if still under peer review. This will be welcomed by most interested readers and will certainly not prevent us from buying a copy of the edited work. On the contrary, it will just whet our appetites.

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