A minute with Peter Bossaerts

In each issue, *Algorithmic Finance* features a brief interview with one member of our advisory or editorial boards or another leading academic or practitioner. These brief conversations are intended to provide a glimpse of their current thinking. In this issue, we talk with Peter Bossaerts.

Peter Bossaerts has recently been apppointed David Eccles Professor of Finance at the David Eccles School of Business of the University of Utah. Before, he was at the California Institute of Technology (Caltech), arriving there from Carnegie Mellon University in 2000, promoting from assistant to associate to full to chaired professor. At Caltech, he was Executive Officer for the Social Sciences, Chair (Dean) of the Division of Humanities and Social Sciences, and Director of Caltech's Linde Institute for Economics and Management Sciences. Peter Bossaerts has also had appointments at Tilburg University, Yale University, the Ecole Polytechnique Fédérale Lausanne (EPFL), and the University of Melbourne.

Peter Bossaerts' research and publications have focused on financial risk and financial risk taking. The work covers many areas of theoretical, empirical and experimental finance, and extends to fields such as econometrics, game theory, general equilibrium theory, cognitive psychology and neurobiology. His work has been published in a wide portfolio of journals across finance, economics, neuroscience, psychology and general science. He is Fellow of the Econometric Society, and of the Society for the Advancement of Economic Theory.

What are your research interests right now?
What do you see as academically exciting?
What would you work on if you had lots of time?

For over 15 years, I have been working on developing the tools to make finance an experimental science. With incredibly limited budgets, we have been trying to study, in a controlled setting, human decision making under uncertainty and interaction through, among

others, (online) markets. At the market level, we have been focusing on asset pricing theories. This body of theories claims to predict what happens to prices, volume, allocation of risk, information, etc., in markets that are devoid of the many imperfections (broker fees; taxes; etc.) and complications (e.g., investors are supposed to know the distribution of asset payoffs, but in reality they don't) of real-world financial markets. It is important to appreciate that even if one eliminates these imperfections and complications, the theoretical predictions are not a foregone conclusion, because the predictions are EQUILIBRIUM predictions: they presume that markets somehow find the equilibrium, and that the trading on the way towards equilibrium does not change the equilibrium. Unlike physics (which has a "law of entropy"), economists do not have a sensible theory of "equilibrium price discovery." We know since the 60s that the often-cited walrasian theory, that prices adjust in the direction of excess demand, won't do when there are more than two securities/goods, and fortunately, our own experiments have demonstrated conclusively that the walrasian theory is wrong. Prices in a market can go up, for instance, even if there is excess supply. (We have been working on a theory that should replace the walrasian theory of equilibration, going back and forth between experimental observation and theorising).

One canonical example of asset pricing theory, the CAPM, for instance, works remarkably well in the laboratory . . . once you make sure it is really a one-period setting (subjects trade a number of securities for a while, and after markets close, these securities pay a liquidating dividend), everyone knows what the payoff distributions are, and everyone trades through a centralised, continuous double auction. CAPM (and some other theories) works so well that we have started to teach a "financial markets laboratory" class (joint with my colleague Prof. Elena Asparouhova) where we teach the theory based on a number of purposely designed trading sessions. Students prepare for trading, they participate in the trading session (and their

grade depends on performance), then we analyse the data in class, teach the theory, confront the theory with the data, and finally, we provide perspective by linking the phenomena we see in the lab and in the theory to real-world data and events.

Lately, we have been interested in robots (algorithmic traders). We have adjusted our online markets software (flex-e-markets; see flexemarkets.com) to allow participants to upload simple python scripts, launch the scripts and stop them as they desire, perhaps upload a different script, etc. We are interested, not only in developing algorithmic traders, but more importantly, in human-robot interface. Questions of interest are: what robots do traders choose to deploy? when do they deploy those? when do they switch off their robots? how do others (humans) react when they sense that there are robots in the marketplace? It is amazing how little is known about this — and yet the vast majority of order submission and even trading nowadays is done by robots, controlled by, and competing with, humans! I always give my students the following analogy: when the FAA was asked to come up with regulation for drones (another type of robot, also controlled by and interacting with humans), they decided to do what I think is sensible: organise test sites and start experimenting . . . In contrast, the SEC allows robots without any sound scientific testing. Now the SEC is starting to think about regulation, but on what basis?

This brings me to my view on economics and finance. Too often, these fields are nothing more than natural philosophy: a collection of models (whether based on "rational decision making" or "behavioural finance") inspired by real-world phenomena, aimed at explaining those phenomena, and that at best generate equations that help one "organise" real-world data. The famous Fama-French three-factor model is an example. This model generates an equation that conveniently summarises the data, but whether the fit proves right the asset pricing theory from which it obtained, is another matter. At the end of the day, it's just a convenient equation that you know beforehand would fit perfectly if only you had the right factors . . . There is no verification of whether the core of the theory, namely, equilibration, obtains. There is no true scientific testing, which would be to see whether the predictions of the theory can be falsified in a controlled setting. Or to merely confirm whether observation changes when parameters change.

My finance colleagues often object that one cannot do experimentation because "real-world markets, the object of our study, are too big." They often refer to astronomy as their "sister science:" one cannot experiment with stars either. But anyone familiar with astronomy will tell you that astronomers do have labs, and these labs test whether the physical laws are really true, laws with which astronomers interpret observations emanating from real stars. When I show them our CAPM results, my finance colleagues shrug their shoulders, and tell me that we know since the early nineties that CAPM does not hold in the real world. No physicist would ever do that: they would never deny the prediction that two objects of different weight fall at the same speed, just because it's not true in the "real world." (And besides, why do my finance colleagues still insist on teaching the CAPM if they believe it is "dead?!").

Observing how our subjects make decisions in our lab has gotten me interested in individual decision making under uncertainty. I contrast behaviour with the theory (decision theory; game theory), look at how the psychologists or behavioural finance scholars explain the discrepancies, and naturally get pushed into . . . neuroscience. I got interested in neurobiology because honestly I find behavioural finance too descriptive. There is no attempt at obtaining a more fundamental understanding. We do want to know: Why? Why are humans subject to a disposition effect (the tendency to wait too long to sell after losses, while selling too quickly after gains)? What in evolution has made us like that? It turned out that, when I started playing with neurobiology now about 11 years ago, neurobiologists had become interested in decision theory and game theory as well, because they wanted to understand the computing algorithms the brain was making — including so-called "emotional" parts of the brain — in determining choice, and therefore needed a formal framework with which to design experiment and interpret the data. A new field was born, decision neuroscience (some would call it neuroeconomics, but it is more than just neuroscience & economics, because it involves also computer science, statistics, and psychology, among others). The advances in that field have been spectacular. To the point that we now have a better understanding of, e.g., behavioural side effects of medications such as levadopa.

Which brings me to the my last point. I constantly feel that I do not have enough time. Decision making under uncertainty touches upon the most fundamental processes in the human brain, involving, e.g., many of the key neuromodulators such as dopamine, serotonin,

norepinephrine and acetylcholine, or brain structures that appear to be key to understanding why we are aware, like anterior insula. At the end of the day, one ends up in . . . psychiatry. Neuro-psychiatry that is. If I had more time, I would start a huge project on deciphering compulsive gambling, and with it, hopefully other addictions that do not have a direct chemical cause (unlike, e.g., nicotine addiction). But that will be for another life, I'm afraid. Unless funding for finance experiments remains as dismal as it is, and unless it take another ten years to convince my academic finance colleagues that finance, like astronomy, needs experiments. (I don't have to convince my colleagues in neuroscience about that!).