Stochastic volatility, power laws and long memory

Benoit B Mandelbrot comments on the paper by Blake LeBaron, on page 621 of this issue, by tracing the merits and pitfalls of power-law scaling models from antiquity to the present.

The model of price variability that Blake LeBaron puts forward in his paper does not actually 'generate financial power laws and long memory'. It only provides a representation that is valid over a finite sample. It shows that several effects that had been previously predicted very simply from power laws and long memory can also be mimicked tolerably well by a sum of three factors. My experience shows that, as the sample length increases, the slowest of those factors must be made slower. Preferably, the number of factors in such models should increase; if not, the quality of fit decreases.

An acknowledged feature of financial prices is that, compared to Gaussianity and independence or Markovian behaviour, their variability is extremely 'anomalous'. I responded with a power law relative to long tailedness (Mandelbrot 1963; the first economist to follow up was E F Fama) and a power law relative to infinite dependence (Mandelbrot 1965; the first economist to follow up was C W J Granger). Three-factor models acknowledge those anomalies, but assume against the evidence (see, e.g., figure 3 of Mandelbrot 2001a) that they constitute a transient behaviour. The power laws acknowledge that the anomaly extends, at least, to the scale of centuries. If so, everything is simplified at no cost by using a model that implies that the anomaly extends forever.

R A Fisher once reduced science to a search for statistically significant quantitative models. I think one comes closer to reality by describing it as a search for insights and simple invariances. Power laws are consequences of scale invariance. They do not claim to provide a microscopically precise multiparameter representation of everything. What they aim at is a 'macroscopic' description of reality. In particular, they claim to have identified macroscopic measures of the financial records' 'roughness'.

Be that as it may, LeBaron's effort adds to a long tradition of three-factor models; some authors prefer four factors to three, and two has been tried but judged to be not enough. Some such models are ancient; many more are being advanced in response to every one of my fractal (power-law scaling) models in many fields. Altogether, the three-factor tradition is substantial and represented in many fields of science. But I propose to argue from long experience that—in addition to the flaw stated in the third sentence of this text—it is ill conceived and short sighted. All this carries a lesson that is best understood and appreciated against a very long historical pattern of dead ends.

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In the 2nd century AD, Claudius Ptolemaeus represented the motions of the Sun and the planets by the following three-factor model. Each heavenly body follows a circle called an epicycle and each epicycle centre follows a circle called a cycle. Circles were injected because familiarity (and nothing else) had made the scientists of antiquity believe that they needed no further justifiction. In addition, last but not least, the cycle's centre had to be excentric, that is, other than the Earth. This three-factor analysis proved very robust, insofar as it was painlessly adjusted after Copernicus exchanged the roles of the Earth and the Sun.

In which way were Kepler's ellipses an improvement? In terms of acceptable curve fitting for Mars, the fit hardly improved. But Kepler reduced the number of parameters. Also, it alone represented the more excentric newly found heavenly bodies. In terms of the future move from *ad hoc* superpositions to general principles that promised (and delivered!) a theory, Kepler was, of course, the right path to follow and Ptolemaeus a dead end.

As to power laws, they have been long known but invariably elicited strong resistance that took about the same form in all cases. Innumerable counter-proposals—many of them involving three (or four) factors!—claim, as their principal asset, the fact that they keep to traditional tools rather than ill-understood and avoidable innovations.

In the first half of the 19th century, close associates of Gauss studied the return to equilibrium in twisted glass wires and electrostatically charged Leyden jars and discovered that the dynamics of return to equilibrium was not exponential, as expected, but followed a power law. Hopkington of Kings College, London, pointed out that the sum of four exponential components suffices to mimic a power law. James Clerk Maxwell and Ludwig Boltzmann entered the fray. They objected scathingly that those exponential components have no individual existence and that the power law represented a transient, but not a phenomenon that must be faced. Indeed, several years' long experiments confirmed the power law.

In the 1950s, Harold Edwin Hurst made puzzling observations about the persistence of yearly discharges of the river Nile and introduced an empirical power law. A Markov model of persistence was far from the mark. The next model, mine, introduced fractional Brownian motion as the simplest way to fit the data. This was the first context where concrete science met FBM and infinite dependence. I promptly extended those power laws to finance, followed by C W J Granger. The FBM model's key parameter, H (a fractal codimension 2-D), soon became well known.

However, statisticians who commented on Hurst found his empirical exponent and my theoretical H to be totally foreign. Therefore, extensive efforts were made to avoid it by sticking to the familiar. One countermodel was a Markov process in which the state is not one datum but M successive data. To provide a good fit, it proved necessary for M to be one third of the sample size. This feature thoroughly discredited Markov models.

Three-factor or ARMA models followed. They were all based on juxtapositions of elementary 'black boxes' familiar from other problems. Enough separately adjustable parameters insure over a short run that imitating the effects of the power law behaviour is not only possible but can easily be done in several ways. But what about the long run? ARMA models assume fundamentally that the anomalous behaviour of river discharges is a short-run transient. In fact, many available fossil records cover millennia and show that the would-be transient continues forever.

In any event, what is gained by sticking to scaling and lost by *ad hoc* alternatives? Hydrology being an ancient topic, J R Wallis and I (see our joint papers reprinted in Mandelbrot 2001d) found it urgent to relate H to qualitative traditions in the field. Comparing different rivers in or near France, we found that the exponent H is not much above 1/2 for rivers with a source in the Alps and well above 1/2 for rivers with a source in the Massif Central. Pierre Masse, a famed economist who had started as a designer of dams, was approached in 1973. He confided that long experience had made the existence and deep practical significance of some such difference familiar to 'his crowd'. Neglecting all detail, they had hoped that this difference could be pinned down by statistics and parametrized. But Massé felt that none of the innumerable parameters necessary to a good fit had been of any help. He was an intellectual as well as a very successful practical man, and hailed as a breakthrough the fact that H was an intrinsic single number of obvious practical use and resulted from an abstract principle of scaling. He agreed with me that scaling and H promise to open a path from a loose impression of qualitative difference towards a quantitatively well-posed theoretical issue.

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To conclude, similar examples occurred in many other contexts in the past and motivated the very skeptical attitude that is expressed in these comments and elaborated upon in Mandelbrot (1982), pages 417–9. Of course, a model's success or failure in one field does not affect success or failure in another. But I think that three-factor models have no positive motivation in any field and that little can be expected in finance/economics from efforts like those of LeBaron. Power-law behaviours exemplify a 'wildly random' phenomenon. They do not go away by only looking at them through hasty and *ad hoc* approximations that exemplify 'mild randomness' and underestimate the difficulty and the conceptual novelty of the field.

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