Research habits in financial modelling: the case of non-normality of market returns in the 1970s and the 1980s

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To the extent that the Global Financial Crisis is seen as a moral crisis, most commentators consider it to be a crisis caused by an elite of highly educated finance professionals relentlessly and excessively pursuing their own interests. They see the crisis as a crisis of greed. And when politicians, policymakers and private citizens call for the restoration of trust in finance, the primary target is the bonus culture, in which bankers are lavishly compensated if upside risks manifest, but where the tax payer covers the downside risks. If trust is what we want to restore, then it is important to see that intelligently placed trust [56] depends on trustworthiness. Trustworthiness, not trust, has to come first.

Following a simple theory, trustworthiness depends on two components: the trusted person's motivation, and his or her competence. You trust your doctor to the extent that she is willing and capable to do the work she has to do. Using this theory, one might say that so far commentators and policymakers have primarily focussed on the motivation component of the financial services industry: culture programmes – which are being implemented in almost any bank today – are programmes aimed at getting employee motivation right. But if that is true, then we run the risk of doing too little to boost competence in the industry.

This brings us to a second observation about the way most commentators see finance. Bankers may be depicted as greedy, but they are hardly ever seen as incompetent. Quite to the contrary, many of them are portrayed as highly skilled quantitative geniuses that work on products that one can only understand if one has a PhD in physics. The *quants*, as they are sometimes called, may have all sorts of immoral motivations to do the work they do, but at least they are clever. Or so most people believe.

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That belief is wrong. And this has serious consequences. Economists [38, 77], sociologists [59] and philosophers [14] have convincingly demonstrated widespread lack of competence, a failure to practise open-mindedness and a genuine aversion against evidence-based methodologies in many parts of the financial sector, the regulatory and supervisory authorities included [21, 47]. The credit rating of structured securities is a case in point [5], and it is quite likely that the subprime mortgage crisis would have hit with much less force if mortgage-backed securities had been rated in more rigorous and methodologically sophisticated ways [43, 58].

This chapter is motivated by a concern for competence in finance. But unlike the above-reference publications, it considers finance at its very foundations, namely, at the place where assumptions are being made about the ways to measure the two key ingredients of finance: risk and return. Despite such exceptions as Bachelier's [3] early work on asset returns, financial practice was up to the 1960s informed by what can be called *folk theories* telling practitioners how to value securities as described in Bernstein's memories [6]. Technical analysis or *chartism* worked hand in hand with the analysis of fundamentals and a motley of not-too-complicated mathematical techniques [23, 60, 63]. Inspired by probability theory and with the emergence of increased availability of empirical data [45], Harry Markowitz [48], William Sharpe [69] and Eugene Fama [28, 29] started developing a rigorous and novel approach to financial economics qualified by Mehrling [49] as a 'revolutionary idea of finance'. Large databases were created, stimulated by the increased relevance of computer technology, with the Chicago Center for Research in Security Prices (1960) as the most important academic player. And gradually, this new approach to finance gained hold of business school curricula as well.

Ironically, however, it did not take too long for discrepancies between theory and empirical reality to appear. Returns for a large class of assets display a number of *stylized facts* [17] that cannot be squared with the traditional views of 1960s financial economics. This traditional view, arisen in the 1930s, is to the effect that return follow Gaussian random walks, where a random walk is a stochastic process in discrete time in which increments are independent and identically distributed (IID). If the distribution is Gaussian, it is a Gaussian random walk; if the distribution is, say, a Gamma distribution, it is a Gamma random walk, and so on.

Already in the 1950s, empirical studies revealed some of the problems of this view. It appears that the empirical distributions of returns are *leptokurtic* (from the Greek words *leptos*, peaked, and *kurtosis*, curvature); that is, they are more peaked that the Gaussian bell, exhibiting fat tails and values clustered around the mean, with the result that extreme events are more likely than under a normal distribution. They are also asymmetric or negatively skewed in the sense that it is particularly negative extreme events that are more likely. And the paths of returns also display volatility clustering, which means that large changes tend to be followed by large, and small by small changes. These stylized facts, which are clearly a violation of the IID hypothesis, have long been known in the academic community. In a 1953 landmark paper Kendall noted that the results from price data between 1883 and 1934 appear 'rather leptokurtic' [40, 13], and this view was reinforced in later publications during the 1950s and early 1960s by many authors (see below).

Despite the empirical counterevidence, normality and continuity assumptions are part and parcel of financial theory and practice, even today. They form the common core of mean-variance analysis, Black-Scholes-Merton option price theory, and fundamental asset pricing methods that emerged from the work of Harrison, Kreps, and Stanley between 1979 and 1981. They are widely used in Value-at-Risk (VaR) analysis in banks and insurance companies. For instance, the so-called *square-root-of-time-rule* for calculating minimum capital underlying such regulatory requirements as Basel III and Solvency II is a very narrow subset of time scaling rule of risk, and comes directly from a Gaussian framework committed to IID. Even though financial modelling progresses in non-Gaussian and non-Brownian directions [8, 9, 18, 24, 25, 36, 39, 41, 66, 62], the bulk of business school finance is still traditional, supporting the myth that risk can be completely tamed.

In this chapter we tentatively exploit an interesting analogy between financial modelling and another respected branch of mathematical economics: game theory. Game theory studies strategic interaction between economic agents, applying and extending results from probability theory and topology. But just as finance, it has suffered a rather serious clash with empirical reality: the behaviour of real players is often very different from what the Nash equilibrium concept or its refinements predict. In a series of publications, De Bruin [10, 11, 12, 13] has interpreted this episode in the history of economic thought as resulting from the adherence to a number of fruitless research habits that stem from an uncritical adoption of the view that the social sciences need only be true in the abstract. These research habits include overmathematization, the use of intuitions as data, introversion, model tinkering and instrumentalism. To be sure, some of these habits were much less prominent in financial modelling. Unlike game theory, financial economists have from the very start been concerned with data – not seldom was their main research aims the rather practical preoccupation to help the financial industry. Moreover, the history of financial modelling seems more complex and multifaceted than that of game theory. In a series of publications related to the history of financial thought, Walter [72, 73, 74, 76] has produced an history of financial modelling and presented the intertwinings between financial modelling and financial practices, exhibiting the role of the random walk model as a backbone of the financial modelling in the twentieth century. The notion of 'leptokurtic phenomenon' [75] has been introduced at the heart of this history to isolate a crucial moment for financial modelling issues: that of the birth of hard scientific controversies concerning the nature of the dynamics of markets. As a result our rather more modest aim in the present chapter is to build on these works for extracting from the 'leptokurtic crisis' some clues revealing the use of one research strategy in particular – model tinkering.

1 Model tinkering and research habits: a case study

The methodology we employ here was developed in a series of papers in which De Bruin compared two research programmes in game theory, a branch of mathematical economics the stated aim of which it is to explain the strategic behaviour of interacting rational agents [10, 11, 12, 13]. Summarizing some of these materials, we here introduce the methodology and some of the results to the extent that our argument about financial modelling depends on it.

While a number of nineteenth century economists count among the precursors of game theory, the field started with the publication of *Theory of Games and Economic Behavior* in 1944, written by John von Neumann, a mathematician, and Oscar Morgenstern, an economist that would also write a number of important papers in mathematical finance [55]. Their book developed novel mathematical tools to examine strategic interaction between economic agents, stimulated several generations of researchers to work in a field that would soon be known as *game theory*. An important role in this field would be played by John Nash. A student of von Neumann's – the film *A Beautiful Mind* recounts his life – Nash developed an equilibrium concept describing rational interaction in one-shot situations. Here is how he begins:

> We proceed by investigating the question: what would be a 'rational' prediction of the behavior to be expected of rational [*sic*] playing the game in question? By using the principles that a rational prediction should be unique, that the players should be able to deduce and make use of it, and that such knowledge on the part of each player of what to expect the others to do should not lead him to act out of conformity with the prediction, one is led to the concept of a solution defined before. [54, 23]

From these assumptions, Nash developed his eponymous equilibrium solution concept. Yet the Nash equilibrium was soon found to be 'intuitively unreasonable' [68] or to deliver results 'inconsistent with our intuitive notions about what should be the outcome of game' [52] – criticisms that started the quest for 'refinements' of the Nash equilibrium, that is, for alternative solution concepts that would keep 'right' and eliminate 'irrational' outcomes. Some examples include the subgame-perfect equilibrium [67], the perfect equilibrium [68] and the proper equilibrium [52]. The Nash Equilibrium Refinement Programme (NERP) gained great influence within the field. All respected textbooks in the field contain elaborate treatments of the various refinements, Nobel Prizes were awarded to Nash and two refiners (John Harsanyi and Reinhard Selten), and as one commentator said, 'the equilibrium concept of Nash… together with its refinements, is without doubt the single game theoretic tool that is most often applied in economics' [2, 1].

It was, however, clear from the very start of NERP that the Nash equilibrium suffered not just from the fact that it was incompatible with certain intuitive notions of rational strategic interaction; rather plain empirical counterevidence was available too. NERP solutions were criticized for failure adequately to describe experimental and empirical results [15], as well as for their being unable to ground their attempted explanations in the processes that were thought to be ultimately explanatory: the players' rationality, that is, their maximizing their utility given their probabilistic beliefs. These two factors – let us call them *empirical inadequacy* and *inadequate grounding* – were among the reasons that led to the development of a different strand of game theoretic research: the Epistemic Programme. Firmly committed to explaining strategic interaction on the basis of players' beliefs, utility functions and rationality, researchers in this programme used methods from measure theory and topology, pursuing a research agenda centred around the concept of *interactive rationality*. The Epistemic Programme is not the only available alternative to NERP; behavioural [15], evolutionary [65] and stochastic [31] methods have been proposed as well. As witnessed by the number of recent publications devoted to it in such journals as *Econometrica* and *Games and Economic Behavior* as well as by the importance it has gained in recent textbooks, however, the Epistemic Programme seems to have taken centre stage [34, 71].

What might explain the relative success of the Epistemic Programme? De Bruin [10, 11, 12, 13] has shown that a conceptually and historically fruitful way to distinguish NERP and the Epistemic Programme focusses on a number of research habits that characterize NERP, and we draw from these publications to give a brief introduction here[1]. An early source of inspiration here is John Stuart Mill's 1836 article 'On the definition of political economy; and on the method of philosophical investigation in that science' [51]. Mill there introduces the distinction between *a posteriori* and *a priori* methods in science, that is, the distinction between inductive methods that start with experience, on the one hand, and deductive methods that start with abstract views about the world, on the other; and he observes that what sets these methods apart is the possibility of using an *experimentum crucis*, a decisive experiment to bring the theory to the test. Mill maintains that while this is possible in the natural sciences, the social sciences do not so easily allow for decisive experiments because of the sheer number of causal factors and because of the difficulties attached to replication of experiments. Unlike the inductive natural scientist, the deductive social scientist, according to Mill, therefore resides to a process in which observations are simplified and abstracted so as to deliver statement that 'are true, as the common phrase is, *in the abstract*'. Some causal factors will, in the process of abstraction and simplification, find no representation in the model. When applied to a concrete case at hand, these 'disturbing causes' that 'have not fallen under the cognizance of science' can, and must, be included. This makes sense, Mill thinks, because 'that which is true in the abstract, is always true in the concrete with proper *allowances*'. What in the end makes social sciences different from natural sciences is the presence of disturbing causes.

This *true-in-the-abstract* view of social science finds an echo in the attitudes several game theorists – and economists, more broadly – adopt towards their field. Robert Aumann, for instance, holds onto the view that game theory is not descriptive 'in the sense that physics or astronomy are' [2]. Rather he believes that game theory describes *homo rationalis*, which, according to him, is a 'mythical species like the unicorn and the mermaid'. In fact, he finds it 'somewhat surprising that our disciplines have any relation at all to real behavior', and that we can gain '*some* insight into the behavior of *Homo sapiens* by studying *Homo rationalis*' [2, 36]. This is a version of Mill's true-in-the-abstract view, albeit a rather extreme one. The relevance in the context of the present discussion is that a social scientist holding it is, we believe, susceptible to a number of research habits. To begin with, the true-in-the-abstract supports *mathematization* of social science narratives, with publications

[1] See, in particular, [13, 128–134].

in economics and other social science journals hardly differing from papers in mathematics with their conformance to Bourbakian templates of definitions, theorems, proofs. Mathematization as such is not, of course, put into question. What is at stake is, however, the relative amount of attention paid to mathematics at the expense of scientific input and inspiration from such fields as psychology, cognitive science, sociology or anthropology, which is reflected in the fact that only a small proportion of references in economics are to publications outside economics, as opposed to such fields as sociology.

A second research habit that a true-in-the-abstract view leads to is the appeal to the researcher's private *intuitions* to support particular modelling assumptions. Without too much exaggeration one might say that this habit lies at the very bottom of the entire neo-classical approach to economics, dependent as it is on the concept of preference and utility as von Neumann and Morgenstern developed it. Myerson asks the question of '[w]hy should I expect that any simple quantitative model can give a reasonable description of people's behavior?' And he answers it by noting that '[t]he fundamental results of decision theory directly address this question, by showing that any decision-maker who satisfies certain *intuitive axioms* should always behave so as to maximize the mathematical expected value of some utility function, with respect to some subjective probability distribution' [53, 5, emphasis added]. Intuitions keep the research in the realm of the true-in-the-abstract; they are easier to generate than empirical data; they allow researcher to sidestep econometric subtleties that typically plague experimental researchers; they are more difficult to refute than experimental data; they are obtained at no cost – and they are expressed verbally, with all the inherent vagueness and room for rhetorics and intended ambiguity. Just as mathematization, an appeal to intuitions is not as such at odds with the aims of social science. What should be a reason to worry is when intuitions replace experiments where experiments could have carried out.

An appeal to intuitions instead of empirical data is often accompanied by *introversion*, that is, the research habit of excessively focussing on internal and often technical problems instead of on empirically or experimentally motivated issues. This habit comes to the fore very clearly again in the way in which Aumann, the game theorist, explains the attractions of the principle of utility maximization, of which, according to him, the valididy 'does not depend on its being an accurate description of true individual behavior'; instead, it 'derives from its being the underlying postulate that pulls together most of economic theory', or in other words, '[i]n judging utility maximization, we must ask not "Is it plausible?" but "What does it tie together, where does it lead?"' [2]. This, by the way, also functions as a theoretical tool in Aumman's attack on Herbert Simon's [70] more empirically adequate principle of satisfying. Such and similar alternatives to the principle of utility maximization 'have proved next to useless in this respect', Aumann maintains, because '[w]hile attractive as hypotheses, there is little theory built on them; they pull together almost nothing; they have few interesting consequences' [2].

Introversion also shows when researchers tend to overinterpret the models they make. When this happens, what counts for a researcher is not just whether the model adequately describes a particular phenomenon, but rather whether all and every aspect of the model has a potential interpretation; and if they do not, then this is considered paradoxical. Certain games, for instance, that do not seem to model any real-life strategic interaction receive excessive attention. Relatedly, introversion can be witnessed in the way in which researchers set aside or describe certain results as ‘undesired’. Rather than seeing empirical falsification as the ultimate counterargument against a theoretical construct, their main worry seems to be whether the construct is compatible with existing theory. Introversion, then, sanctions a rather conservative attitude in science.

The last research habit or attitude associated with true-in-the-abstract conceptions of economics is *instrumentalism*, which was most famously defended by Milton Friedman [30]. According to Friedman, what matters about a theory is whether it predicts well, not whether it makes realistic assumptions. Instrumentalism, however, risks lowering explanatory ambitions. An oft-quoted example is this:

> Consider the density of leaves around a tree. I suggest the hypothesis that the leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives, given the positions of its neighbors, *as if* it knew the physical laws determining the amount of sunlight that would be received in various positions and could move rapidly or instantaneously from any one position to any other desired and unoccupied position. [30]

The density of leaves may be adequately described by Friedman’s theory, but if that is all we care about, the need to do empirical research on plants becomes less pressing, and the likelihood of discovering related phenomena smaller. How would an instrumentalist, for instance, discover that the plants respond to *blue* but not red light? The instrumentalist adopting a true-in-the-abstract ‘as-if’ view cannot, moreover, explain the difference between human, animal and plant agency. He or she cannot avoid vacuity or multi-realizability of explanations. For instance, a typical human action is compatible with a whole range of combinations of probability distributions and utility functions. An instrumentalist does not care which of them is the ‘right’ one. Where theories are developed for purely instrumental reasons and need to be true in the abstract only the adequacy of the representation of the underlying causal structures and processes matters much less; and where theory and reality clash, the method of model tinkering has, if we adopt such a view, much to recommend, for instead of revising the theory where it hurts, mathematical epicycles can be added to the model resolving the conflict with the data – and it does not matter whether these are empirical, or based on intuition only.

It is true that *ad hoc* models can be found in physics and other scientific disciplines as well; the best-known such model is indeed still Ptolemy’s theory of planetary motion. But physicists would generally try to avoid introducing theoretic terms and statistical parameters that cannot be given a physical meaning. The theories of secondary school physics do not correctly describe the trajectories of balloons and other exceedingly light objects. Including air resistance as a causal determinant makes the description more accurate. The increased descriptive accuracy is not, however, the only reason to embrace the more complex theory; another is that it uncovers a reality that was so far hidden: the impact air has on the movement and acceleration of falling objects.

2 Financial modelling: the non-normal puzzle

Intuitions as data, introversion, instrumentalism – we argue now that these research habits played a significant role in shaping developments in financial modelling in the second half of the twentieth century. We first explain the standard model of market returns, the exponential Brownian motion, which in a sense was the Nash equilibrium of financial modelling. Subsequently, we zoom in on a number of mutually competing programmes (mixed diffusion jumps processes, ARCH modelling, stochastic volatility, infinite activity, tempered stable processes and Mandelbrot's programme). Given the space we have, we cannot hope to cover all details of this complex and extensive territory, and hence we aim at pointing out a number of events in the history of financial modelling making our main claim reasonably plausible. We do presuppose a fair amount of knowledge of financial modelling here, as we see philosophers of science and financial mathematicians as our primary audience.

2.1 Standard model

Let us denote the price of any financial asset (stock, bond or other) at time t by $S(t)$. Practitioners (traders, risk managers, etc.) are generally interested in the cumulative continuous rate of return between times 0 and t, $X(t) = \ln S(t) - \ln S(0)$. The classic Brownian motion representation of return dynamics, due to Louis Bachelier [3] and Maury Osborne [57] is given by

$$X(t) = \mu t + \sigma W(t), \tag{1}$$

where $W(t)$ is a standard Wiener process, μ a trend parameter and σ a volatility parameter meant to capture what is thought as market risk in the classical paradigm of Markowitz-Sharpe (portfolio theory) and Black-Scholes-Merton (option pricing theory). This model became the standard model of market dynamics in 1965 with Paul Samuelson. This equation (1) has three important consequences. First, it supports a Gaussian distribution with mean μt and variance $\sigma^2 t$ for the marginal law of empirical returns $X(t)$. Secondly, it underscores the idea that $(X(t), t \geq 0)$ is a stochastic process in which the increments are independent and identically distributed, the IID hypothesis we encountered earlier. And thirdly, it entails time scaling of distributions – and consequently time scaling of risk – in the sense that a given horizon (e.g., t) of a return distribution is scaled to another (e.g., $t \times a$) as in

$$X(t \times a) \stackrel{d}{=} X(t) \times \sqrt{a} \tag{2}$$

where the symbol $\stackrel{d}{=}$ is used to signify equality in distributions. This is called the *scaling property* of Brownian motion or the *square-root-of-time rule* of scaling.

If this is a quick and dirty overview of Brownian motion, it is important to emphasize the widespread use of these models, particularly in the financial industry and for regulatory purposes – and to emphasize its wide empirical inadequacy at the same time. Let us begin with the scaling property and its wide use in financial industry. The scaling property (2) leads to scaling of volatility in the sense that

$$\sigma(a \times t) = \sigma(t) \times \sqrt{a} \tag{3}$$

which supports the widely used practice to compute the annual volatility from the weekly one. In this example, $t = 1$ week so $a = 52$ weeks, therefore annual volatility $=$ weekly volatility $\times \sqrt{52}$. From this simple relationship is derived the intuition of the so-called variance-ratio tests (annual volatility/weekly volatility $= \sqrt{52}$) initiated by Lo and MacKinlay who conclude that 'The random walk model is strongly rejected for the entire sample period (1962-1985)' [44, 41].

The so-called square-root-of-time rule is widely used in Basel III and Solvency II regulations which support the calculation and the implementation of a probabilistic measure of market risk called 'Value-at-Risk' [37] (herafter VaR). The minimum capital requirement is an estimated quantile of a return distribution (10 days 95% VaR metrics). The 10 days VaR is obtained with a simple application of time scaling of risk using the square-root-of-time rule: we have VaR 10 days $=$ VaR 1 day $\times \sqrt{10}$. But anticipating later criticisms, the square-root-of-time rule leads to a systematic underestimation of risk that worsens with the time horizon. As Danielson and Zigrand [22] argue, 'even if the square-root-of-time rule has widespread applications in the Basel Accords, it fails to address the objective of the Accords'. VaR scaling does not contribute to the realization of the Basel objectives [21]. The $\sqrt{10}$ explains incidentally the multiplicative factor of 3 in Basel Accords which is intended to compensate for errors that can arise in simplifying assumptions [32, 255].

Nor is time scaling the only trouble. As we mentioned before, Brownian motion clashes with a number of empirical stylized facts about return distributions. Returns distributions violate normality in that empirical distributions are leptokurtic compared to the normal distribution, and return processes violate the IID hypothesis in the sense that there is serial correlation in squared returns, that is, volatility dependence. Perhaps most importantly, continuity is violated. These stylized facts had been known for a long time. Merton, for instance, wrote that 'there is a *prima facie* case for the existence of jumps' [50], that is, for discontinuities, and Cox and Ross agreed that 'exploring alternative forms [of motion] is useful to construct them as jump processes' [20]. A decade later when Black-Scholes option pricing models had become widely popular, Ball and Torous pointed out that 'empirical evidence confirms the systematic mispricing of the Black-Scholes call option pricing model', noting that '[t]he Merton model which explicitly admits jumps in the underlying security return process, may potentially eliminate these biases' [4]. And more recently still, Carr and his co-authors wrote that they 'seek to replace this process with one that enjoys all of the fundamental properties of Brownian motion, *except for pathwise continuity and scaling*, but that permits a richer array of variation in higher moment structure, especially at shorter horizons' [16]. Unlike the perhaps more introverted game theorists, the finance academics were well aware of the discrepancies between model and reality. The challenge was how to respond to them.

2.2 Leptokurtic refutations

As early as the 1950s, empirical studies pointed out the problems of the Brownian representation of market dynamics. Below are some examples of this evidence. We quote rather extensively to point to what is historically and epistemologically an intriguing phenomenon: while statisticians found increasing evidence backing the diagnosis of non-normality of market dynamics, around 1965 finance academics made a conscious choice to ignore the evidence, opting for the standard model of reducing return processes to Brownian motion. In a 1953 landmark paper published in the respected *Journal of the Royal Statistical Society*, Maurice Kendall observed of price data between 1883 and 1934 that '[t]he distributions are accordingly *rather leptokurtic*' [40, 13, emphasis added]). Seven years later, Arnold Larson noted that '[e]xamination of the pattern of occurrence of all price changes in excess of three standard deviations from zero... indicated... presence in the data of an *excessive number of extreme values*' [42, 224]. In a very important article published in 1961 in the *American Economic Review*, Houthakker wrote said that

> [t]he distribution of day-to-day changes in the logarithms of prices does not conform to the normal curve. It is... *highly leptokurtic*... The variance of price changes does not seem to be constant over time... Very large deviations, in fact, seem to come in bunches. The non-normality mentioned above may be related to the changing variance. [35, 168, emphasis added]

And in the same year, Sydney Alexander emphasized pithily that '[a] rigorous test... would lead to dismiss the hypothesis of normality... This sort of situation (*leptokurtic*) is frequently encountered in economic statistics'[1, 16].

All this concerns only one stylized fact: non-normality. But as early as 1959, Harry Roberts had argued in the *Journal of Finance* against the assumption of IID, stating that

> modern statistical theory has been largely built up on the assumption of independence. Much of it also assumes... that the underlying distribution is a normal distribution in the technical sense of that term. The assumption of normality usually seems far less crucial to the applicability of statistical methods than does that of independence. [64, 14]

The assumption of IID was attacked from other sides as well. In an influential publication, Paul Cootner observed that real markets do not follow the random walk model. And while '[t]he way in which actual markets operate is one of the more fascinating of current economic questions', he admitted that '[i]f their behavior is more complicated than the random walk models suggest, it will take more sophisticated statistical testing to discover it' [19, 44].

2.3 *Turning point: mild randomness and wild randomness*

We now use a perspicuous representation of the two tracks within financial modelling as they appear in the 1960s, in order to make the research habits visible. We do this by moving to a discrete multiperiod model of securities markets with a finite number of trading dates occurring at time $k \in \mathbf{N}$. This move can be viewed as a way for simplifying the representation. In this framework, equation (1) becomes

$$X_k = X_{k-1} + \mu + \sigma u_k, \tag{4}$$

with $u_k \to \mathcal{N}(0,1)$ capturing Gaussian white noise with unit variance, and σ the volatility of returns, a constant. Let us write $\varepsilon_k = \sigma u_k$. This quantity 'shapes' the randomness, i.e. the risk of the market dynamics

$$\varepsilon_k = \sigma \times u_k, \tag{5}$$

which expresses the idea that risk equals 'scale' of fluctuations times 'shape' of randomness.

This leads to the notion of 'states of randomness' introduced by Benoît Mandelbrot [46] to describe the level of smoothness of the price charts, that is, the irregularity due to jumps. He made a distinction between two types of state named'mild randomness' and 'wild randomness'. He wrote that 'the roughness of a price chart [used to be] measured by its volatility – yet that volatility, analysts find, is itself volatile', and he described his contribution to be '[r]oughness is the very essence of many natural objects – and of economic ones' and to have developed a 'geometry of roughness' .

2.3.1 The pivotal choice

To address the stylized facts listed above in an adequate way, two routes are possible, namely, one exploiting scale of fluctuations (modelling changing volatility), and the other exploiting shape of randomness (adding jumps to the continuous paths). Models either work on the scale of fluctuations (the volatility part of risk) with unchanged shape of randomness (Bronwian paradigm), or on shape of the randomness (the jump part of risk) with unchanged scale (constant volatility). In the first case, one moves outside the IID world because, as Houthakker [35] already observed in the 1960s, variance will change over time – volatility is itself volatile – even though this solution permits one to keep the paradigm of the Gaussian world. If one wants to stay in the IID framework, as in the second case, one has to change the randomness and leave the Brownian paradigm. This is summarized in the scheme below :

$$\text{Turning point} = \text{choice} = \begin{cases} \textsc{Route 1} \quad \text{Wild} \quad \text{IID with roughness} \\ \\ \textsc{Route 2} \quad \text{Mild} \begin{cases} \text{IID with jumps} \\ \text{non IID} \end{cases} \end{cases} \tag{6}$$

2.3.2 IID and rough fluctuations: into the wild

Leaving the mild randomness was in fact Mandelbrot's initial suggestion in 1962. He did not simultaneously attempt to address all stylized facts, but rather tried to find the simplest stochastic process to replace Brownian motion, while keeping time scaling of risk and the IID hypothesis in place. This was accomplished by α-stable motion, due to Paul Lévy. Equation (5) – or better, a version written here with squared errors to display variance, that is, $\varepsilon_k^2 = \sigma^2 \times u_k^2$ – then becomes

$$\varepsilon_k^\alpha = \gamma^\alpha \times \ell_k^\alpha, \tag{7}$$

where $\ell_k \to \mathscr{L}_\alpha$ and $\mathscr{L}_\alpha$ is an α-stable motion with α the characteristic exponent (intensity of jumps) and γ a scale parameter corresponding to the volatility in the α-stable world. In this model, however, the variance (second moment) is infinite (except when $\alpha = 2$, for then we find classical volatility). To obtain non-normality, Mandelbrot introduced infinite variance.

Equation (7) means that market risk equals constant scale parameter times intensity of jumps. The corresponding scaling relationship (2)

$$X(t \times a) \stackrel{d}{=} X(t) \times a^{1/2}$$

becomes

$$X(t \times a) \stackrel{d}{=} X(t) \times a^{1/\alpha} \tag{8}$$

What Mandelbrot did was, in the end, nothing more than changing the exponent, and one might think such model tinkering would be greeted with little criticism. It was, however, the assumption of infinite variance that spurred a vehement controversy between Mandelbrot and the advocates of new portfolio theory and option pricing models. It was their aim to overcome the inadequacies of Brownian motion by tackling the issue of discontinuities without accepting infinite variance. We elaborate below on this.

2.3.3 The mild route:IID with stochastic jumps

Press [61] and Merton [50] put forward this approach; their idea was to add a compound Poisson-normal (CPN) process to the diffusive Brownian component. As Merton observed,

> the total change in the stock price is posited to be the composition of two types of changes: diffusion and jumps. The natural prototype process for the continuous component of the stock price change is a Wiener process, so the prototype for the jump component is a 'Poisson-driven' process [50].

If we consider what effects this modelling approach has on the classic Brownian representation of return dynamics we encountered in equation (1), this transforms the earlier equation into

$$X(t) = \underbrace{\mu t + \sigma W(t)}_{\text{Brownian motion}} + \underbrace{\sum_{i=1}^{N_t} Y(t)}_{\text{Jump part}}. \tag{9}$$

A component $\sum_{i=1}^{N_t} Y(t)$ is added, with a Poisson process N_t that has a normally distributed size $Y(t)$. The underbraced glosses vividly illustrate model tinkering: in order to describe jumps, a CPN process is added to the model, but this epicycle itself is *perfectly consistent with the Gaussian intuition* (mild randomness): jump size has a normal distribution. Non-normality, in other words, is obtained by adding a jump component. To say it differently, we create 'wild' behaviour with 'mild' ingredients.

2.3.4 The mild route: non IID with time-varying volatility

We now proceed to examine the universe of non-IID processes with time-varying volatility of so-called ARCH modelling. The initial relation (5) – written again with squared errors to display variance as $\varepsilon_k^2 = \sigma^2 \times u_k^2$ now becomes

$$\varepsilon_k^2 = h_k \times u_k^2. \tag{10}$$

This is the explicit generating equation of an ARCH process. The constant variance σ^2 is replaced by a time-varying variance h_k of which the value depends on k. The time-varying variance is autoregressive and conditional in the sense that the variance h_k depends on past values of the squared errors ε_k^2, usually described using the term *heteroskedastic* (from the Greek words *heteros*, different/time-varying, and *skedastos*, dispersion/variance). These characteristics of modelling gave the name of this family: the Auto-Regressive Conditional Heteroskedasticity models (ARCH) introduced in 1982 by Nobel Prize winner Robert Engle [26]. The functional form of h_k is crucial because it is meant to capture such phenomena as clustering of large shocks. This is brought out best by considering, rather heuristically, a model in which the conditional time-varying variance is formulated as a linear model on squared perturbations:

$$h_k = \alpha_0 + \alpha_1 \varepsilon_{k-1}^2. \tag{11}$$

The corresponding model, called ARCH(1), is given by the following equation, with glosses as above:

$$\varepsilon_k^2 = \underbrace{\left(\alpha_0 + \alpha_1\, \varepsilon_{k-1}^2\right)}_{\text{Gaussian distribution}} \times \underbrace{u_k^2}_{\text{Gaussian noise}} \tag{12}$$

If one calculates the kurtosis coefficient of the marginal distribution with an ARCH model, that is $K(\varepsilon)$, one finds that a value greater than 3, which is the normal value (Gaussian distribution). The interpretation of this is that the ARCH(1) model has tails heavier than the Gaussian distribution. With the temporal dependence of h_k, the marginal distribution then appears to be leptokurtic even though the conditional distributions are still Gaussian. Like epicycles, one has created non-normality by embedding Gaussian distributions in a Gaussian distribution; or less impressionistically, we see that the ARCH principle rescales an underlying Gaussian noise by multiplying it by the conditional time-varying standard deviation (square root of the variance), which is a function of the past values.

As one may suspect, it may sometimes have been necessary to postulate an excessively large number of lags to capture the fine structure of dependence [27], implying the necessity of estimating a large number of parameters, leading to a high order ARCH process. To be parsimonious in mathematical terms and statistical estimations, the ARCH model was therefore generalized in Generalized ARCH (GARCH) models by Bollerslev [7]. This is a generalization that makes it possible to reduce the number of mathematical lags in the squared errors – and thus to reduce the computational burden. The GARCH model provides a parsimonious parameterization and is consistent with volatility clustering pattern. The heuristic intuition of the GARCH approach is easily glanced from

$$h_k = \underbrace{\alpha_0 + \alpha_1 \varepsilon_{k-1}^2}_{\text{ARCH(1)}} + \underbrace{\beta_1 h_{k-1}}_{\text{G(1)}}, \tag{13}$$

where h_{k-1} as previously. More generally, a GARCH(p,q) process is defined as

$$h_k = \underbrace{\alpha_0 + \alpha_1 \varepsilon_{k-1}^2 + \cdots + \alpha_p \varepsilon_{k-p}^2}_{\text{ARCH(p)}} + \underbrace{\beta_1 h_{k-1} + \cdots + \beta_q h_{k-q}}_{\text{G(q)}}. \tag{14}$$

We again see how the embeddedness of Gaussian building blocks made it possible to create non-normality without abandoning the Gaussian tools. Unconditionally, the GARCH process is homoskedastic with non-normality. Conditionally, the GARCH process is heteroskedastic with normality.

Towards the end of the 1990s, Gouriéroux and Le Fol expressed a view than many seem to have held onto at the time:

> [T]he recent inflation of basic model varieties and terminology GARCH, IGARCH, EGARCH, TARCH, QTARCH, SWARCH, ACD-ARCH reveals that this approach appears to have reached its limits, cannot adequately answer to some questions, or does not make it possible to reproduce some stylized facts.[33, our translation]

Particularly, it was noticed that the kurtosis coefficient implied by the ARCH and GARCH models tended to be far less than the sample kurtosis observed in empirical return series: the ARCH-GARCH ways of obtaining non-normality were, then, incompatiable with returns empirically observed. The ARCH approach was able to generate excess kurtosis, but it did not go far enough, and hence a new move had to be made.

2.4 Ten years after...

This meant a return to the tinkering technique of adding jump processes to Brownian motion. While the earlier separation of a continuous Brownian source of market movements and a Poisson source creating discontinuities was simple and convenient, it significantly limited modelling applications. In particular, it turned out that it was necessary to account for three factors: there is a very large number of very small jumps, there is a large number of larger jumps, and there is a very small number of very large jumps. To have an intuition of the move to this new paradigm, it is useful to write the Press-Merton solution in the Fourier space (the space of characteristic functions). In this space, the Press-Merton solution is:

$$\Psi_{X_t}(u) = \underbrace{t\left(i\mu u - \frac{1}{2}\sigma^2 u^2\right)}_{\text{Brownian motion}} + \underbrace{t\int_{-\infty}^{+\infty}\left(e^{iux}-1\right)\lambda f_Y(x)dx}_{\text{Jump part}}, \tag{15}$$

The generalization of the Press-Merton solution was captured by the Lévy-Khinchin formula:

$$\Psi_{X_t}(u) = \underbrace{t\left(i\mu u - \frac{1}{2}\sigma^2 u^2\right)}_{\text{Brownian motion}} + \underbrace{t\int_{\mathbf{R}^*}\left(e^{iux}-1-iux\mathbf{1}_{|x|<1}(x)\right)\nu(dx)}_{\text{Jump part}}, \tag{16}$$

where $\nu(.)$ is the Lévy measure the decomposition of which is

$$\Psi_{X_t}(u) = \underbrace{t\left(i\mu u - \frac{1}{2}\sigma^2 u^2\right)}_{\text{Brownian motion}} + \tag{17}$$

$$\underbrace{\underbrace{t\int_{|x|<1}\left(e^{iux}-1-iux\right)\nu(dx)}_{\text{small jumps}} + \underbrace{t\int_{|x|\geq 1}\left(e^{iux}-1\right)\nu(dx)}_{\text{large jumps}}}_{\text{Jump part}} \tag{18}$$

The Lévy measure determines the fluctuations of the jumps, as well as the skewness and the kurtosis of the increments of the stochastic process, and it contains all information needed to characterize the trajectory of a Lévy process apart from its tendency and its diffusive fluctuation scale (that is, volatility). It is the quantity that shapes the size of the tails of distribution, and the patterns of jumpy fluctuations. To say it differently, the Lévy measure shapes the roughness of the fluctuations.

But did the probabilistic representation of market fluctuations ultimately entail the use of the Brownian diffusive component? The diffusive part of probabilistic representations is needed for the modelling of the small movements only in the case of finite activity. Only with finite activity, the process required the addition of another component. In the 1990s, understanding was reached that infinite activity was possible. The usefulness of the diffusive component disappeared and a pure jump process seemed to be sufficient to represent the entire stock market phenomenon, that is, its bumpiness at all scales. The argument is well described in by 2002 as follows:

> The rationale usually given for describing asset returns as jump-diffusions is that diffusions capture frequent small moves, while jumps capture rare large moves. Given the ability of infinite activity jump processes to capture both frequent small moves and rare large moves, the question arises as to whether it is necessary to employ a diffusion component when modelling asset returns. [16]

But that is another story.

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