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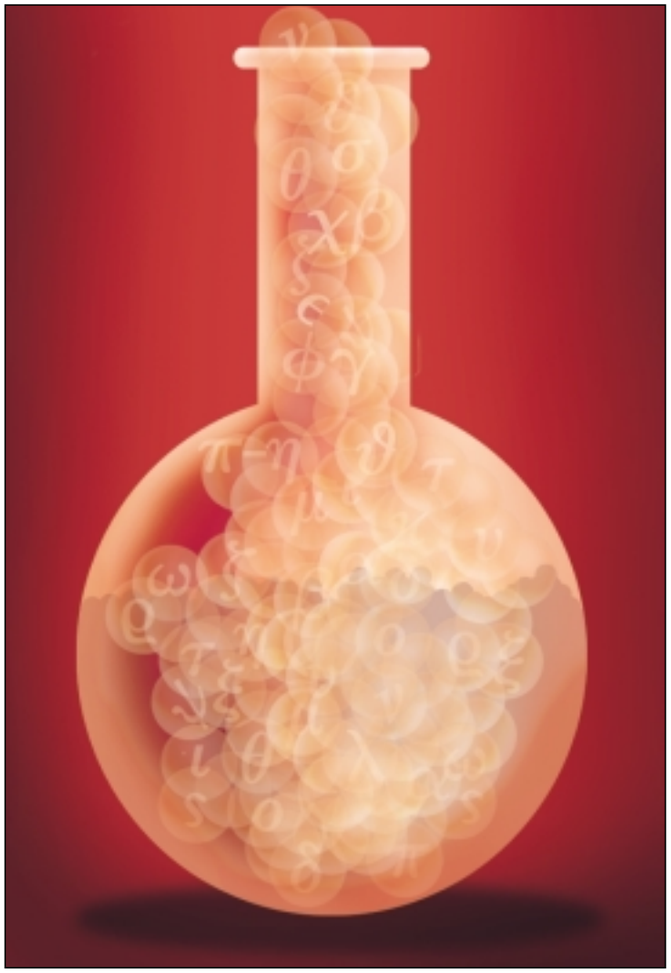
A New Philosophy for a New Science

Introducing a new voice in option language and the central question all quants need to ask



It strikes us as an equal misunderstanding of the critical dimension that we have uncovered in option pricing models (including Black-Scholes), and of the critical role of the option delta hedge, that someone like Nassim Taleb should argue so overtly against financial engineering and option models, and declare that his hedge fund, Kurtosis, will "take away the money from those who take the models of quantitative finance too seriously[1]." As the name of his hedge fund operator 'Empirica Capital LLC' indicates, Taleb believes only in empirical reality and its extraordinary capacity to falsify any inductive generalization that our rationalizing tendencies produce all too often. Argue from the pattern to the law, Taleb says, adopt a quantitative model, and you are almost sure to have found a name (and at the same time, an excuse) for your future downfall. Believe in Black-Scholes, and you will have named the skew as your enemy. Attune your implied volatility curve to the skew, and you will be in trouble trying to guess its dynamics. Generalize your probability distribution, add jump processes to your diffusion process, make the volatility stochastic, and you will end up with an increased number of unobservable parameters (volatility of volatility, mean and variance of the distribution of jump sizes) that you can only try to calibrate from option market data, at the risk of over-fitting and instability of the calibration procedure. Not mentioning that you may be wrong settling for any one probability distribution at any one time, for the "true" generator of the random process may be itself randomly changing and not corresponding to any generator of known type and calculable moments[2]. If such were the case, you would no longer be able to tell whether the rare event that brings you down is the legitimate, although unhappy, child of your probabilistic set-up, or a killer from nowhere, whose own generator was generated at the same time he was born. A situation that Taleb describes as "bad" and utterly "pessimistic," yet a situation he says traders and risk managers do face most of the time in reality. It sums up to this: adopt a theoretical model, and you will have named (empirical) reality as your executioner.

It may sound, on a naïve reading, as if Taleb has found the unbeatable trading strategy. Bet against any theory. Take the set-theoretic complement of all the generalizing and rationalizing attempts above. "We focus principally, he says, on what we do not know." The problem is, how can we place ourselves outside our knowledge? How can Taleb know what he does not know? Surely no one believes that Taleb simply takes the opposite bets to people using quantitative models[3], for he would be using them himself, indirectly. And he would have to spend more money developing them than any believer in them, for he would have to develop them in totality and forever. Every time Taleb would take an opposite bet, an anti-quant person at Empirica should be busy developing an anti-model in order to check whether Taleb's bet is not itself accountable in some larger theory. For if this were the case, Taleb would have to take the opposite-opposite bet. This sounds like the problem of induction in reverse, don't you think?

The point I am driving at is a point I alluded to earlier. There is no reality (empirical or not empirical) outside language. And recall that models and theory do exceed the true-false

dichotomy in our critical philosophy, by being accountable as language and not taken at face value. So before we elaborate any of this, you can feel the question that we are dying to put to Taleb: "What is your language? Granted, what you say you do at Empirica is beyond, or before, theory, but what are your objects? Surely you cannot just be dealing with fleeting and unstructured empirical reality, so what is your structure? What is the structure behind the structure (of theory) that you have so easily dismissed? In a word, what is your model?" Granted, Taleb wishes to situate himself outside the strict logic and the strict prediction of any given quantitative model. This is the move against the "illusion of the science" in option pricing models that we have been advocating ourselves. No option pricing model is true, we all agree to that. As a matter of fact we've been busy lately extracting the riches from the opposite side, and precisely exploring the meaningful dimension of a false Black-Scholes. Now Taleb's specific claim is that no option pricing model can be true. The situation is inherently severe in quantitative finance, or in its epistemology, the methodology of risk, because by the very definition of a model for risk your biggest risk is model risk. "What by definition can hurt you is what you expect the least," says Taleb[4]. Closure of the epistemological circle is impossible in the science of risk and Taleb refers to this predicament as the central problem of risk management.

The central problem of risk management

Because of the problem of induction, it is already the case that no science dealing with empirical reality, that no lawful generalization of an empirical phenomenon, can hope to be in a better position than to wait for the next empirical fact to come and falsify it. This is known as the falsifiability of scientific theories, and has been recommended by Popper as the decisive criterion for science instead of the eternal un-verifiability of an eternal truth. The make-up of our world is such that the realm of logic and hypo-deductive systems stands on the one side and empirical reality on the other. There just is no way that the first can have a grip on the second. Hume, for instance, suggested that our "laws of nature" are mere repetitions of disconnected empirical facts that our mind gets accustomed to and believes are produced by necessity. Reason and empirical reality are in fact so dissimilar that we really should be amazed that our scientific theories should work at all and that there may exist such a thing as science and the progress of science[5].

In quantitative finance, there is an additional twist to the problem of induction and that is that "empirical reality" and theory are not separate after all. While it is an empirical fact that an empirical finding may come and falsify a given physical theory – and again it may not –, it is almost written in advance that such a falsification should obtain in the science of risk. Risk, we may say, is a special kind of "empirical reality." It is empirical reality with a vengeance. While the empirical reality of Popper is at worst deaf to our theoretical summons, risk has ears behind ears and spreads itself into between the lines of the science meant to frame it. Because of the central problem of risk management, risk is structurally recessive and eternally withdrawn as a "reality." You cannot "realize" risk once and for all. When there is theory made up for risk, more significantly there is risk made up for theory. Risk and theoretical representation are definitely not indifferent to each other but are literally moved by each other.

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True, it may not be possible to confirm a physical theory from any number of past successful instances, and tomorrow can bring us the physically falsifying fact, but surely we do not expect that fact to be totally alien to the "style in which we have, and up to now have had, the world" (Husserl[6]). That fact will have to retain something "physically-looking" about it. It is not as though the world tomorrow would suddenly become physically incompatible with our world today (suddenly massive bodies lose their extension) or logically incompatible with it (suddenly all synthetic statements become analytic and the problem of induction disappears). Although there is nothing in theory (or should I say, "in reality"?) to stop the problem of induction from being that severe, we should not overstate it. It is only a philosophical problem after all and its only philosophical relevance is to block the inductive step from successful prediction to validation of a law. The purpose of the problem of induction is to insist that the only valid step, in the empirical sciences, is the deductive step. From theory you can only legitimately deduce an empirical prediction[7] and try to confirm that prediction by experiment. The consequence of this one-way protocol is that two different theories may come to produce the same successful predictions, and be equally accepted as "empirically adequate." This is the only working consequence of the problem of induction: the so-called "under-determination of theory by the observational evidence." The purpose of the problem of induction is to make us feel uneasy about theory and theoretical overconfidence not about empirical reality.

However, the situation is different in quantitative finance. Yes, our financial world and financial markets tomorrow can be physically and logically incompatible with our world today. To the extent that physics is what we thought was physically imaginable, and logic what we thought was logically imaginable, markets can literally produce the unimaginable. And yes, we can start feeling uneasy about that special kind of empirical reality we have called risk. In the traditional empirical sciences, tomorrow's new evidence can indeed precipitate theory change or even paradigm shift, but empirical reality itself will not be lost. We will still have it, and we will still "have the world." By contrast, when you

the trader or risk manager are hit by the rare event which has this rare and delicate trait about it that it precisely comes to you from outside your whole conception of risk, it is not just "risk as you know it" that you lose, but you lose all this money! You lose much more than your empirical reality, you lose that essence itself that the whole idea of risk and quantifiable risk was supposed to safeguard from loss. Empirical scientists can be proven right or wrong, but the nature of the game they play remains the same regardless. They remain the masters of the game and the masters of empirical reality in the sense that they can dismiss it altogether in the limit, by opting for suicide instead. Until the end, there is total separation between the characters and the play. Risk managers, by contrast (and from now on, "risk managers" will also designate the traders as I see no difference between the two), are not just proven wrong when they lose. When you blow up, you are kicked both out of the game and out of the possibility of the game. You are out yet at the same time you are caught. Taleb has expressed a weaker version of the category difference[8] between the loss of the traditional empirical scientist and the loss of the risk manager by noting that no physical loss is greater than death – and we know death – while financial loss is not bounded by any such visible bound. You can lose potentially infinite sums of money. Now I am sure Taleb does not mean that losing money is more serious than death. He doesn't mean to compare a quantity of money with a quantity of death. What he has in mind, I think, is the sudden methodological disruption that occurs at the time of financial loss (all of sudden the "science of risk" pales beside the irreversibility of loss) and doesn't occur elsewhere. Blowing up is not an ordinary case of theoretical disconfirmation. A washed up trader is a different man to a mistaken scientific speculator.

The problem of induction and transcendental philosophy

Now to go back to Hume and to the twist that Kant gave to his empiricism, let us note that the Scottish philosopher was not a sceptic with respect to science. He believed that science existed just as plainly as empirical reality, and surely must have imputed the successes of the empirical sciences to the same empirical regularities which had impressed us with the idea of laws of nature in the first place. Hume was sceptic with respect to metaphysics. His point was that the causal or necessary connexions, that we think are the true drivers of the laws of nature, are nowhere to be observed in the world, and that 'custom or habit' is really what gives us our beliefs about the unobserved. Our beliefs and inductive generalisations, Hume thought, are the fruits of our imagination, not of our reason. Kant came and displaced the necessity unto the realm of reason itself. By asking how science is possible, and more significantly, how such a meta-scientific, or metaphysical, question is itself possible – a question which indeed presupposes that (philosophical) knowledge may transcend the bounds of actual experience – Kant turned the flat and uniform picture of Humean conjunctions and regularities back into the dynamic interplay of possibility and necessity. For to ask how science is possible is not to wonder flatly whether science exists, and how it empirically exists. Science exists, no doubt about that. The question 'How such a thing as science is possible?' is significant only to the extent that the word 'science' is charged and means 'science as universally and necessarily valid.' Otherwise, why bother asking?

Hume may have deduced the subjective necessity of the sciences from the conjunction of our subjective experience and our imagination, but this 'empirical deduction' or 'physiological derivation' misses its point. And it misses its point precisely because it does not, in the end, recognize that which is most important in the sciences *qua* sciences (i.e. independently of the question of whether the sciences really are in the world or really are in our mind), namely their objectivity. The fact of science, mentioned earlier, remains foreign to such empirical deduction. And once we ask the question 'How is science possible?' with the objectivity of science in mind, we can only expect, as satisfactory answer, something in the mood of necessity. The possibility of science and objective knowledge has to be transcendentally deduced. Objective science must be possible. Not because of some hidden necessary connexions, but because concepts which are "a priori conditions of the possibility of experience are for this very reason necessary", and because those a priori concepts "serve as antecedent conditions under which alone anything [...] can be thought as object in general". The conditions of possibility of experience, so the slogan goes, are the conditions of possibility of the objects of experience. Since the former are necessary, the latter will be too. Objective knowledge is necessarily possible. As Kant writes:

"[...] since [Hume] could not explain how it can be possible that the understanding must think concepts, which are not themselves connected in the understanding [i.e. not analytically connected], as being necessarily connected in the object, and since it never occurred to him that the understanding might itself, perhaps, through these concepts, be the author of the experience in which its objects are found, he was constrained to derive them from experience, namely, from a subjective necessity (that is, from custom), which arises from repeated association in experience, and which comes mistakenly to be regarded as objective. [And] from these premises he argued quite consistently. It is impossible, he declared, with these concepts and the principles to which they give rise, to pass beyond the limits of experience. Now this empirical derivation [...] cannot be reconciled with the scientific a priori knowledge which we do actually possess, namely, pure mathematics and general science of nature; and this fact therefore suffices to disprove such derivation."

What necessity and causality there are in the sciences are therefore grounded in our understanding not in some reality beyond experience. And metaphysics, in the sense of an investigation of the "unobserved" and the meta-empirical (laws of nature, causation, universal and necessary validity of science, in a word, anything that transcends the bounds of experience), has to proceed as a critique of pure reason, not as a metaphysical speculation. In Richardson's words[9]: "The business of philosophy changes from an attempt to give a priori principles of a realm

beyond experience to an examination of the forms of the faculties of knowledge that synthesize the material of sensation to yield objective knowledge. This is Kant's transcendental turn."

Is quantitative finance a science?

What this digression into the dialectic of empirical and transcendental deduction shows is that the problem of induction is not the interesting (philosophical) consequence of the disconnection between empirical reality and our rational apparatus. (No harm meant to Popper and his followers). Rather, the principal gain that we get from this divorce, and from the fact that the validity of science has to be treated apodictically nevertheless, is that the objectivity of science and the objectivity of knowledge have to be adjudicated independently both of experience and of any metaphysical realm supposedly lying beyond experience. Kant's transcendental philosophy is a rebuttal both of metaphysical scepticism à la Hume and of metaphysical realism, or rationalism, à la Leibniz. To repeat, objectivity is a different notion to truth. A scientific theory can be deemed objective without necessarily being true or false, and by that we mean that the question of its truth shall not even be raised. Yet objectivity cannot be empirically reduced, or empirically deduced. It is a philosophical notion with an over-empirical pretension, not unlike truth. So in a sense, Kant is as much of an empirical philosopher as Hume, only he takes science more seriously. More importantly, since the transcendental turn presupposes the fact of science, he takes philosophy more seriously than Hume by placing it in the wake of science.

It seems, then, that the most significant response to the sceptical question about the truth of science is to change the question and ask about the conditions of possibility of science instead. Science being here presupposed. But what if it wasn't? What if the very possibility of science was itself the subject of scepticism, and not just its metaphysics? What if the question was one of agreeing first on what to call science – provided such a thing must exist – before asking how such a science can be possible? Just as we argued that Lipton and Hagan were the last representatives of a tradition which did not appreciate the significance (or the critical dimension) of Black-Scholes and believed that the right solution to the smile problem lay in the true smile model, we wish to argue now that Taleb is the last representative of the philosophical tradition which believes that quantitative finance can be questioned and grounded and addressed in the same fashion as the ordinary "empirical" sciences. We say Lipton and Hagan are last because they brought the smile problem to the stage where the right questions were finally asked (How do I make sure my smile model prices the exotics right? How do I make sure it produces the right delta?), the stage that we have called "the beginning of the smile problem," yet they did not cross over unto the domain of significance of the option pricing models where 'right' is no longer equal to 'true,' and reverted to the old metaphysics of truth and empirical confirmation. Likewise, we say that Taleb is last in the traditional philosophical questioning of quantitative finance because he brought the question right to the stage where it is about to turn critical, and philosophy of quantitative finance about to take the transcendental turn, yet he reverted to the reiteration of the problem of induction, and to criticizing "those who take the models of quantitative finance too seriously."

If we were to follow Taleb, we would find that the problem of induction is not really applicable to the kind of scepticism he is promoting.

Indeed, due to what Taleb has called 'the central problem of risk management,' and more generally, due to the unordinary entanglement between the "science of risk" and the "empirical reality" it is dealing with, we claim that Taleb is in fact voicing an unconditional doubt against the very possibility of quantitative finance as science, not a second-order doubt about the metaphysics of such a science, conditional on its existence and empirical successes. So Taleb is sceptical about the very (Kantian) move supposed to deflect the metaphysical scepticism of Hume. He is a critical sceptic. When Taleb argues that "what by definition can hurt you is what you expect the least" and that you may be wrong, in the first place, assuming that the "generator is of a certain general type," he is not just denying you the metaphysical guarantee that your theory of risk may be true given its past confirmations, so you could answer him back, like Kant answered Hume, that the really interesting question is: "How is my science of risk (nevertheless) possible?" For that would be the ordinary problem of induction that Taleb thinks it is, and that would be the ordinary critical answer to that problem. Rather, Taleb is posing the most severe question directly himself. He may not be fully appreciative of the scope of his scepticism but he is in fact asking: "Is the science of risk possible at all?" Quantitative finance is not being presupposed as science by Taleb's radical scepticism, as the ordinary problem of induction implies it should. Recall that Taleb is basically setting the stage for his blanket rejection of all quantitative models. The empirical reality he wants to embrace is a totalizing empirical reality, freed of any remnant of science and structured knowledge ("We focus principally on what we do not know"). So in effect, if we were to follow Taleb, we would find that the problem of induction is not really applicable to the kind of scepticism he is promoting. He is not posing a problem for the science of quantitative finance or a problem internal to its philosophy, because we are not even sure yet that such a science exists. As a matter of fact, under the "ordinary" problem of induction, empirical reality is just the dialectical counterpart of established scientific theory. It is the part supposed to counterbalance science's metaphysical preten-

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sion to truth. Scientific theory exists, empirical reality exists, and the problem of induction is only supposed to banish the extra-scientific, extra-empirical, metaphysical connexions.

Dynamic Hedging and the first Taleb

So how can we answer Taleb's radical scepticism? We cannot argue, like Kant, that "that the science of quantitative finance must be possible is proved by the fact that it exists." Does it exist? If derivative pricing theory, and quantitative models, and risk management, etc., are not possible, then what is possible? Couldn't we argue from the existence of people who (think they) are scientifically involved in those fields to the existence of the fields? We may agree with Taleb that true risk managers do not in fact exist, but don't traders truly exist? And if they did, wouldn't their knowledge exist, and wouldn't it have to be structured? What are the ideal traders in Taleb's epistemology? Mere empirical actors and "reactors" who only focus on what they do not know, or are they the seekers of some form of truth? And if truth is out of the question, can't we at least say that the traders are the holders (or the beholders) of some form of objectivity? Or perhaps they are just philosophers, lovers of wisdom without the preamble of science?

Is Taleb really dismissing the whole of science? And if the belief in the science must be suspended, are we to suspend the belief in the language of the science? Is Taleb not himself a user of that language, and a very competent one at that? Is Dynamic Hedging a scientific book? And if we conceded that it is not, in the sense of not proposing a theory or a model or an explanation of option prices, shouldn't we recognize that it is the book par excellence which explores and exploits all the variations of the option language? By showing us how rich and complex the dynamic trading and hedging of options can be, how options relate to each other under various conditions, and how we should expect their sensitivities and relative values to evolve over time and different market scenarios, is Taleb not patiently inculcating in our trading (and risk-assessing) minds all the elements of a structured knowledge that ultimately qualifies as a science? He may not be proposing a system of knowledge, or a systematic option theory which starts with the phrase: "Let us assume a very general jump-diffusion stochastic volatility process of the underlying, and let us consider the following derivative structures...", he may not be referring to any option pricing formula or any option pricing model other than Black-Scholes[10], but is not the science, or the art, that Taleb is communicating essentially aimed at arming the option trader to face realities beyond the Black-Scholes world? If volatility were not suspected to be stochastic, or implied volatility smiles were not supposed to exist, and if the trader were not supposed to keep the balance of his option book in a world of difference and dissimilarity and slippage rather than a world of replication and redundancy and monotony, if, in other words, Black-Scholes were right in letting us trust the underlying in all our replicating strategies, and in making us believe that convexity can be hedged away with another convexity, no matter how far apart the strikes of the options or their maturity dates, would Taleb be spending so much time animating all sides of the Black-Scholes model and all kinds of idiosyncratic option trades into such a colourful and lively picture?

If option pricing was as finished, complete, and peaceful as the Black-Scholes closed-form formula suggests it is, would Espen Haug be referring to option trading as 'war'? And what would it matter to "Know your weapon," as Haug likes to call the Black-Scholes formula, and to get acquainted with all sorts of partial derivatives and cross-derivatives that the formula admits of (higher order greeks such as DvegaDvol or DgammaDspot, or cross-sensitivities such as DdeltaDvol or DdeltaDtime), if those derivatives were just a matter of deriving the formula mathematically, and if the dynamics of trading and hedging was really the seamless and automatic process presupposed by the Black-Scholes model? What difference would it make to call those greeks by such suggestive names as 'Delta Bleed' or 'Speed' or 'Colour,' if each single one of them were not supposed to come alive in reality and capture a specific measure of risk that would otherwise remain imperceptible in the monochrome Black-Scholes picture? To put it in Haug's words: "An option trader knowing the ins and outs of the Black-Scholes-Merton formula can beat a trader using a state-of-the-art stochastic volatility model." Implicit in this statement is of course the assumption that the real battlefield is a world of stochastic volatility, jumps, and not so perfect hedging.

The enlarged option science

That "option manuals" such as Taleb's *Dynamic Hedging* or "instruction manuals" such as Haug's Know Your Weapon are possible, despite the apparent sterility of the Black-Scholes model and against Taleb's best suspension of judgement about the existence of the science, is the best proof and illustration of the so-called robustness of the Black-Scholes model. How could Black-Scholes not be robust, when Taleb and Haug are able to extract from it such a quantity of knowledge and develop it into such a body of knowledge, or in other words, shape it into such language? How could the validity of Black-Scholes, to use one of our favourite terms, be discussed in isolation any longer, or questioned independently of the extension that Taleb and Haug have given it? Are we not saying that the best and most appropriate – and perhaps the most scientific –

generalization of the Black-Scholes model are Taleb's and Haug's? And do we not mean that they are in fact proposing a new kind of model, which befits the new kind of science, a "critical" and all-encompassing model which admits, as proper constitutive parts, a) the given analytical model – in their case, the Black-Scholes-Merton formula – b) the stepping outside the model – its "ins and outs" and the various displays, that Taleb and Haug produce, of higher order greeks whose proper contexts of expression are market behaviours definitely opposite Black-Scholes – c) and the very alive option trader without the participation and the active knowledge of whom the "model" could not function as an operative whole and would remain a sundry collection of mathematical formulae and partial derivatives? Given our critical line of argument, which finds science in Black-Scholes precisely outside the closed-form model and towards such dynamic uses and "abuses" of the Black-Scholes formula as displayed by Taleb and Haug, are we not in fact saying that the possibility of those manuals is the best proof of the existence of the science? Only we mean an enlarged science, which is neither the science that Taleb's risk managers think they are doing nor the science that Taleb thinks he is denying them.

What science there is in Black-Scholes, and more generally in any smile model capable of improving on the scientific and linguistic objects first created by Black-Scholes, this "option science" we are still looking for, has to be delimited and demarcated with other philosophical means than the usual empirical sciences. Again, the best introduction to that new science and to its domain is to ask the question: "Does Taleb's and Haug's discourse qualify as true science when clearly the base model – Black-Scholes-Merton in that instance – does not?" Given the essentially interpretative nature of Taleb's and Haug's work – for both authors are clearly overstepping the Black-Scholes framework and intend to play their instrument by stretching its chords beyond the Black-Scholes score – this inquiry has to take in broader philosophical categories than are usually associated with the exact sciences. There is a tension inherent in Taleb's *Dynamic Hedging* and Haug's Know Your Weapon due to the fact that the partial derivatives and cross-derivatives they are reviewing are strictly speaking useless in the theoretical framework that gave them birth. And one cannot resolve this tension unless one rises above the purely mathematical or hypo-deductive logic and no longer interprets gamma, vega, vanna, speed, bleed, etc., as the pure partial derivatives that they are. The question "What are Taleb and Haug really doing?" has to be addressed with a view to semantic and hermeneutic issues surpassing the purely analytical routine that their derivations and mathematical formulae seem immersed in.

"What new meaning do those higher-order greeks bestow on options and option trading?" is the right question to ask, not "What truth do they add to Black-Scholes?" Depending on the scope of one's philosophical view, Taleb's and Haug's "unscientific" commentary of higher order greeks can be viewed either as the most disappointing, most uneventful, addendum to Black-Scholes, or as its most exciting generalization, the most promising smile model after Black-Scholes[11]. It is really the same story as with the option delta, a story, remember, that could equally be told as the story of perfect replication and complete elimination of the option, or the story of its scientific establishment and advent as independent and definitive object. Are Taleb's and Haug's manuals not indeed the continuation of the objectification process that we have already found was taking place on the right side of a false Black-Scholes?

The meaning of dynamic hedging

And now you can see where exactly we disagree with Taleb. Had Taleb interpreted his own *Dynamic Hedging* philosophically and critically, and asked, the way we did, what objective science was exactly transpiring from that book instead of stepping back into conventional epistemology and wondering whether quantitative finance was exact science, he would have realized that the central problem of risk management, far from being a reason for dropping the possibility of science altogether, is on the contrary the one reason why a new science has to be established and branded for quantitative finance. Taleb missed the chance of becoming the new philosopher of the new breed of science. He chose instead to be the last philosophy writer[12], writing from a philosophical tradition not really applicable to the specific field. Instead of writing a *Critique of Pure Option Models* and enlightening us about the real meaning of 'dynamic' in *Dynamic Hedging,* he wrote *Fooled By Randomness* where the dynamic hedgers are suddenly advised to retire in their attic, away from the beating heart of the market, and to indulge in Monte Carlo simulations.

And what we think is the real meaning of 'dynamic' in *Dynamic Hedging* is something that could never be captured in a static formula or a quantitative model. (To that extent, we agree with Taleb's anti-quantitative crusade). The meaning of 'dynamic hedging' that we understand from the first Taleb and from Haug, is not the 'dynamic hedging' that first comes to mind when one realizes that the authors are talking about option pricing models and dynamically hedged contingent claims. (Let us call that first impression the "theoretical dynamic hedging"). True, the Black-Scholes formula, whose multiple facets Haug and the first Taleb are exploring, is based precisely on a principle of dynamic hedging under Brownian motion. However, the something profoundly dynamic that we believe is the real subject matter of Taleb's first book and Haug's article is none other than the real, animated, world of trading and hedging that the option trader will find himself dynamically, that is to say actually, engaged in, no matter the particular "weapon" he is carrying or the particular "metal" the weapon is made of. To put it in Haug's words: "In this installment I will not show the nerdy quants how to come up with the BSM formula using some new fancy mathematics – you don't need to know how to melt metal to use a gun. Neither is it a guideline on how to trade. It is meant rather like a short manual of how your weapon works in extreme situations." And to cite Taleb's own opening comments in the preface of his book:

"The core of dynamic hedging includes:

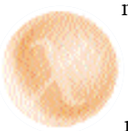
- The need for a methodology for the implementation of the Black-Scholes-Merton replicating process for options or any other nonlinear securi-

We are back again to the big differentiating factor, already noted by Taleb, which puts quantitative finance in a class of its own

ty under the constraints imposed by the marketplace.

- The need to generalize the Black-Scholes-Merton framework to cover other parameters than the underlying security in the replicating process [...].
- The awareness that transaction costs and frequency can cause a departure from the canons of continuous time finance.
- The awareness that distributions are unstable and hard to model[13] (and we emphasize 'awareness')."

The first and easy notion of 'dynamic' (that we have called the "theoretical dynamic hedging") is what afforded us the theoretical model and the corresponding pricing formula in the first place. It had to do with the specific dynamics that was assumed for the underlying. This is the place of the theoretical random generator that Taleb is notably inveighing against. By contrast, the second and deepest notion of 'dynamic' – the notion motivating the whole of Taleb's book and rightly providing its title, and the notion justifying the whole list of Haug's partial derivatives and their special names – cannot but exceed the theoretical model and the given pricing formula, to the point of literally including the trader. What is irretrievably dynamic in the dynamic picture that Haug and Taleb are drawing is the required presence of the trader and his "awareness." To repeat: What could be the purpose of identifying, individuating, and naming all these partial derivatives, if the only assumed dynamics was the theoretical dynamics presupposed by the given quantitative model? And why would Haug ask us to attend so carefully to each one of these partial derivatives, if the process was all written up in advance and the rule of mathematical derivation perfectly able to attend to itself? Could it be because of the money? Could the reason for such endless elaborations and commentaries of the Black-Scholes model be that people make and lose money out of option trading, and that money is such a serious business? It seems we are back again to the big differentiating factor, already noted by Taleb, which puts quantitative finance in a class of its own, opposite the traditional physical sciences.



■ *To be continued in the next issue*

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2 I am drawing on the following paper co-authored by Taleb: Random Processes, Opacity and Knowledge: The Central Problem, N. Taleb and A. Pilpel, May 2002 (http://home.netcom.com/~ntaleb/knowledge.pdf).

3 Although, in an essentially rhetorical moment, Taleb claims he does: "Drs. Merton and Scholes helped put your humble author on the map and caused the birth of your humble crisis-hunter's firm, Empirica – as capital started flowing to people who did the exact opposite of what they did for a living." (*Fooled by Randomness*, p. 189).

4 2001 Hall of Fame, Derivatives Strategy, March 2001 (http://home.netcom.com/~ntaleb/dscover.pdf).

5 From this astonishment, Kant by the way suggested that the right philosophical question ought not to be: "How is science true?" but: "How is science possible?" This is how he turned metaphysics into critical philosophy and the big question of science into the question of the objectivity of science. "If we accept [Hume's] conclusions, Kant says, then all that we call metaphysics is a mere delusion whereby we fancy ourselves to have rational insight into what, in actual fact, is borrowed solely from experience, and under the influence of custom has taken the illusory semblance of necessity. If he had envisaged our problem in all its universality, he would never have been guilty of this statement, so destructive of all pure philosophy. [...] Since these sciences [pure mathematics and the pure science of nature] actually exist, it is quite proper to ask how they are possible; for that they must be possible is proved by the fact that they exist." (From Kant's *Critique of Pure Reason*, Introduction). Compare the move of a Popper who resigned himself to falsifiability as the reaction against the hopeless metaphysics of truth in the empirical sciences.

6 "Universally, says Husserl in a specifically anti-Humean mood, things and their occurrences do not arbitrarily appear and run their course but are bound a priori by [the invariant general style of the intuitive world], by the invariant form of the intuitable world." And, in what may sound as a weakening of the principle of causality abhorred by Hume: "Through a universal causal regulation, all that is together in the world has a universal immediate or mediate way of belonging together." (Edmund Husserl, *The Crisis of European Sciences*).

7 Although some contemporary critics of Popper argue that even this step may not be granted. Deduction of the observational evidence to expect cannot proceed from theory alone, but requires all kinds of implicit ingredients like background knowledge, etc. As Robert Klee puts it in "The Underdetermination of Theory": "We cannot establish the move from a general principle (hypothesis) to a specific observable (experiment) without additional assumptions... What implies an observational prediction is that theory together with a myriad of interdependent beliefs, presumptions, guesses, and other theories."

8 "Category" in the sense of "category mistake" first introduced by Gilbert Ryle. See for instance his *The Concept of Mind*.

9 Op. cit. pp. 95-96.

10 Of Black-Scholes and its "uniqueness" to traders' minds and usage Taleb says: "Despite the criticisms of the formula, traders have refused alternatives because they have learned its limitations. No experienced trader would willingly trade Black-Scholes-Merton for another pricing tool." (*Dynamic Hedging*, p. 109).

11 Witness the division among the readers of *Dynamic Hedging* half of whom think it is the best book ever written on the subject while the other half dismisses it as the worst and most incomplete.

12 And, as a matter of fact, the first. To our knowledge, Taleb is the first philosopher of science writing specifically about quantitative finance. Too bad he had to transcribe and reiterate the traditional epistemological schemas (problem of induction, decision under uncertainty) for the newborn science of derivative pricing, instead of realizing the new philosophical opportunity that this new science represents.

13 *Dynamic Hedging*, p. vi.