We can state it in the following way. If we need data to obtain a probability distribution to gauge knowledge about the future behavior of the distribution from its past results, and if, at the same time, we need a probability distribution to gauge data sufficiency and whether or not it is predictive of the future, then we face a severe regress loop. This is a problem of self-reference akin to that of Epimenides the Cretan stating whether or not Cretans are liars. Indeed, it is too uncomfortably close to the Epimenides situation, since a probability distribution is used to assess the degree of truth but cannot reflect on its own degree of truth and validity. And, unlike many problems of self-reference, those related to risk assessment have severe consequences. The problem is more acute with small probabilities.

An Undecidability Theorem

This problem of self-reference, published with Pilpel after *The Black Swan*, went unnoticed as such. So Raphael Douady and I re-expressed the philosophical problem mathematically, and it appears vastly more devastating in its practical implications than the Gödel problem.

Raphael is, among the people I know, perhaps the man with the greatest mathematical erudition—he may have more mathematical culture than anyone in modern times, except perhaps for his late father, Adrien Douady.

At the time of writing, we may have produced a formal proof using mathematics, and a branch of mathematics called "measure theory" that was used by the French to put rigor behind the mathematics of probability. The paper is provisionally called "*Undecidability: On the inconsistency of estimating probabilities from a sample without binding a priori assumptions on the class of acceptable probabilities.*"

It's the Consequences …

Further, we in real life do not care about simple, raw probability (whether an event happens or does not happen); we worry about consequences (the size of the event; how much total destruction of lives or wealth, or other losses, will come from it; how much benefit a beneficial event will bring). Given that the less frequent the event, the more severe the consequences (just consider that the hundred-year flood is more severe, and less frequent, than the ten-year

flood; the bestseller of the decade ships more copies than the bestseller of the year), our estimation of the *contribution* of the rare event is going to be massively faulty (contribution is probability times effect; multiply that by estimation error); and nothing can remedy it.*

So the rarer the event, the less we know about its role—and the more we need to compensate for that deficiency with an extrapolative, generalizing theory. It will lack in rigor in proportion to claims about the rarity of the event. Hence theoretical and model error are more consequential in the tails; and, the good news, *some representations are more fragile than others*.

I showed that this error is more severe in Extremistan, where rare events are more consequential, because of a lack of scale, or a lack of asymptotic ceiling for the random variable. In Mediocristan, by comparison, the collective effect of regular events dominates and the exceptions are rather inconsequential—we know their effect, and it is very mild because one can diversify thanks to the "law of large numbers." Let me provide once again an illustration of Extremistan. Less than 0.25 percent of all the companies listed in the world represent around half the market capitalization, a less than minuscule percentage of novels on the planet accounts for approximately half of fiction sales, less than 0.1 percent of drugs generate a little more than half the pharmaceutical industry's sales—and less than 0.1 percent of risky events will cause at least half the damages and losses.

*From Reality to Representation**

Let me take another angle. The passage from theory to the real world presents two distinct difficulties: inverse problems and preasymptotics.

Inverse Problems. Recall how much more difficult it is to re-create an ice cube from the results of the puddle (reverse engineering) than to forecast the shape of the puddle. In fact, the solution is not unique: the ice cube can be of very many shapes. I have discovered that the Soviet-Harvard method of viewing the world (as opposed to the Fat Tony style) makes us commit the error of confusing the two arrows (from ice cube to puddle; from puddle to ice cube). It is another manifestation of the error of Platonicity, of thinking that the Platonic form you have in your mind is the one you are observing outside the window. We see a lot of evidence of confusion of the two arrows in the history of medicine, the rationalistic medicine based on Aristotelian teleology,

which I discussed earlier. This confusion is based on the following rationale. We assume that we know the logic behind an organ, what it was made to do, and thus that we can use this logic in our treatment of the patient. It has been very hard in medicine to shed our theories of the human body. Likewise, it is easy to construct a theory in your mind, or pick it up from Harvard, then go project it on the world. Then things are very simple.

This problem of confusion of the two arrows is very severe with probability, particularly with small probabilities.*

As we showed with the undecidability theorem and the self-reference argument, in real life we do not observe probability distributions. We just observe events. So I can rephrase the results as follows: we do not know the statistical properties—until, of course, after the fact. Given a set of observations, plenty of statistical distributions can correspond to the exact same realizations—each would extrapolate differently outside the set of events from which it was derived. The inverse problem is more acute when more theories, more distributions can fit a set of data, particularly in the presence of nonlinearities or nonparsimonious distributions.† Under nonlinearities, the families of possible models/parametrization explode in numbers.‡

But the problem gets more interesting in some domains. Recall the Casanova problem in Chapter 8. For environments that tend to produce negative Black Swans, but no positive Black Swans (these environments are called negatively skewed), the problem of small probabilities is worse. Why? Clearly, catastrophic events will be necessarily absent from the data, since the survivorship of the variable itself will depend on such effect. Thus such distributions will let the observer become prone to overestimation of stability and underestimation of potential volatility and risk.

This point—that things have a bias to appear more stable and less risky in the past, leading us to surprises—needs to be taken seriously, particularly in the medical field. The history of epidemics, narrowly studied, does not suggest the risks of the great plague to come that will dominate the planet. Also I am convinced that in doing what we are to the environment, we greatly underestimate the potential instability we will experience somewhere from the cumulative damage we have done to nature.

One illustration of this point is playing out just now. At the time of writing, the stock market has proved much, much riskier than innocent retirees were

led to believe from historical discourses showing a hundred years of data. It is down close to 23 percent for the decade ending in 2010, while the retirees were told by finance charlatans that it was expected to rise by around 75 percent over that time span. This has bankrupted many pension plans (and the largest car company in the world), for they truly bought into that "empirical" story—and of course it has caused many disappointed people to delay their retirement. Consider that we are suckers and will gravitate toward *those variables that are unstable but that appear stable*.

Preasymptotics. Let us return to Platonicity with a discussion of preasymptotics, what happens in the short term. Theories are, of course, a bad thing to start with, but they can be worse in some situations when they were derived in idealized situations, the asymptote, but are used outside the asymptote (its limit, say infinity or the infinitesimal). Mandelbrot and I showed how some asymptotic properties do work well preasymptotically in Mediocristan, which is why casinos do well; matters are different in Extremistan.

Most statistical education is based on these asymptotic, Platonic properties, yet we live in the real world, which rarely resembles the asymptote. Statistical theorists know it, or claim to know it, but not your regular user of statistics who talks about "evidence" while writing papers. Furthermore, this compounds what I called the ludic fallacy: most of what students of mathematical statistics do is assume a structure similar to the closed structures of games, typically with a priori known probability. Yet the problem we have is not so much making computations once you know the probabilities, but finding the true distribution for the horizon concerned. Many of our knowledge problems come from this tension between a priori and a posteriori.

Proof in the Flesh

There is no reliable way to compute small probabilities. I argued philosophically the difficulty of computing the odds of rare events. Using almost all available economic data—and I used economic data because that's where the clean data was—I showed the impossibility of computing *from the data* the measure of how far away from the Gaussian one was. There is a measure called kurtosis that the reader does not need to bother with, but that represents "how fat the tails are," that is, how much rare events play a role.

Well, often, with ten thousand pieces of data, forty years of daily observations, one single observation represents 90 percent of the kurtosis! Sampling error is too large for any statistical inference about how non-Gaussian something is, meaning that if you miss a single number, you miss the whole thing. The instability of the kurtosis implies that a certain class of statistical measures should be totally disallowed. This proves that everything relying on "standard deviation," "variance," "least square deviation," etc., is bogus.

Further, I also showed that it is impossible to use fractals to get acceptably precise probabilities—simply because a very small change in what I called the "tail exponent" in Chapter 16, coming from observation error, would make the probabilities change by a factor of 10, perhaps more.

Implication: the need to avoid exposure to small probabilities in a certain domain. We simply cannot compute them.

FALLACY OF THE SINGLE EVENT PROBABILITY

Recall from Chapter 10, with the example of the behavior of life expectancy, that the conditional expectation of additional life drops as one advances in age (as you get older you are expected to live a smaller number of years; this comes from the fact that there is an asymptotic "soft" ceiling to how old a human can get). Expressing it in units of standard deviations, the conditional expectation of a Mediocristani Gaussian variable, conditional on it being higher than a threshold of 0, is .8 (standard deviations). Conditional on it being higher than a threshold of 1, it will be 1.52. Conditional on it being higher than 2, it will be 2.37. As you see, the two numbers should converge to each other as the deviations become large, so conditional on it being higher than 10 standard deviations, a random variable will be expected to be just 10.

In Extremistan, things work differently. The conditional expectation of an increase in a random variable does not converge to the threshold as the variable gets larger. In the real world, say with stock returns (and all economic variables), conditional on a loss being worse than 5 units, using any unit of measure (it makes little difference), it will be around 8 units. Conditional that a move is more than 50 units, it should be around 80 units, and if we go all the way until the sample is depleted, the average move worse than 100 units is 250 units! This extends to all areas in which I found sufficient samples. This tells us that there is "no" typical failure and "no" typical success. You may be able to predict the occurrence of a war, but you will not be able to gauge its effect! Conditional on a war killing more than 5 million people, it should kill around 10 million (or more). Conditional on it killing more than 500 million, it would kill a billion (or more, we don't know). You may correctly predict that a skilled person will get "rich," but, conditional on his making it, his wealth can reach $1 million, $10 million, $1 billion, $10 billion—there is no typical number. We have data, for instance, for predictions of drug sales, conditional on getting things right. Sales estimates are totally uncorrelated to actual sales—some drugs that were correctly predicted to be successful had their sales underestimated by up to 22 times.

This absence of "typical" events in Extremistan is what makes something called prediction markets (in which people are assumed to make bets on events) ludicrous, as they consider events to be binary. "A war" is meaningless: you need to estimate its damage—and no damage is typical.

Many predicted that the First World War would occur, but nobody really predicted its magnitude. One of the reasons economics does not work is that the literature is almost completely blind to this point.

Accordingly, Ferguson's methodology (mentioned in Chapter 1) in looking at the prediction of events as expressed in the price of war bonds is sounder than simply counting predictions, because a bond, reflecting the costs to the governments involved in a war, is priced to cover the probability of an event *times* its consequences, not just the probability of an event. So we should not focus on whether someone "predicted" an event without his statement having consequences attached to it.

Associated with the previous fallacy is the mistake of thinking that my message is that these Black Swans are necessarily more probable than assumed by conventional methods. They are mostly *less* probable, but have bigger effects. Consider that, in a winner-take-all environment, such as the arts, the odds of success are low, since there are fewer successful people, but the payoff is disproportionately high. So, in a fat-tailed environment, rare events can be less frequent (their probability is lower), but they are so powerful that their contribution to the total pie is more substantial.

The point is mathematically simple, but does not register easily. I've enjoyed giving graduate students in mathematics the following quiz (to be answered intuitively, on the spot). In a Gaussian world, the probability of exceeding one standard deviation is around 16 percent. What are the odds of exceeding it under a distribution of fatter tails (with the same mean and variance)? The right answer: lower, not higher—the number of deviations drops, but the few that take place matter more. It was puzzling to see that most graduate students get it wrong.

Back to stress testing again. At the time of writing, the U.S. government is having financial institutions stress-tested by assuming large deviations and checking the results against the capitalization of these firms. But the problem is, Where did they get the numbers? From the past? This is so flawed, since the past, as we saw, is no indication of future deviations in Extremistan. This comes from the atypicality of extreme deviations. My experience of stress testing is that it reveals little about the risks—but the risks can be used to assess the degree of model error.

Psychology of Perception of Deviations

Fragility of Intuitions About the Typicality of the Move. Dan Goldstein and I ran a series of experiments about the intuitions of agents concerning such conditional expectations. We posed questions of the following sort: What is the average height of humans who are taller than six feet? What the average weight of people heavier than 250 pounds? We tried with a collection of variables from Mediocristan, including the above-mentioned height and weight, to which we added age, and we asked participants to guess variables from Extremistan, such as market capitalization (what is the average size of companies with capitalization in excess of $5 billion?) and stock performance. The results show that, clearly, we have good intuitions when it comes to Mediocristan, but horribly poor ones when it comes to Extremistan—yet economic life is almost all Extremistan. We do not have good intuition for that atypicality of large deviations. This explains both foolish risk taking and how people can underestimate opportunities.

Framing the Risks. Mathematically equivalent statements, I showed earlier with my example of survival rates, are not psychologically so. Worse, even professionals are fooled and base their decisions on their perceptual errors. Our research shows that the way a risk is framed sharply influences people's understanding of it. If you say that, on average, investors will lose all their money every thirty years, they are more likely to invest than if you tell them they have a 3.3 percent chance of losing a certain amount every year.

The same is true of airplane rides. We have asked experimental participants: "You are on vacation in a foreign country and are considering flying a local airline to see a special island. Safety statistics show that, if you fly once a year, there will be on average one crash every thousand years on this airline. If you don't take the trip, it is unlikely you'll visit this part of the world again. Would you take the flight?" All the respondents said they would. But when we changed the second sentence so it read, "Safety statistics show that, on average, one in a thousand flights on this airline have crashed," only 70 percent said they would take the flight. In both cases, the chance of a crash is 1 in 1,000; the latter formulation simply sounds more risky.

What Is Complexity? I will simplify here with a functional definition of complexity—among many more complete ones. A complex domain is characterized by the following: there is a great degree of interdependence between its elements, both temporal (a variable depends on its past changes), horizontal (variables depend on one another), and diagonal (variable A depends on the past history of variable B). As a result of this interdependence, mechanisms are subjected to positive, reinforcing feedback loops, which cause "fat tails." That is, they prevent the working of the Central Limit Theorem that, as we saw in Chapter 15, establishes Mediocristan thin tails under summation and aggregation of elements and causes "convergence to the Gaussian." In lay terms, moves are exacerbated over time instead of being dampened by counterbalancing forces. Finally, we have nonlinearities that accentuate the fat tails.

So, complexity implies Extremistan. (The opposite is not necessarily true.)

As a researcher, I have only focused on the Extemistan element of complexity theory, ignoring the other elements except as a backup for my considerations of unpredictability. But complexity has other consequences for the conventional analyses, and for causation.

Induction

Let us look again, from a certain angle, at the problem of "induction." It becomes one step beyond archaic in a modern environment, making the Black Swan problem even more severe. Simply, in a complex domain, the discussion of induction versus deduction becomes too marginal to the real problems (except for a limited subset of variables, even then); the entire Aristotelian distinction misses an important dimension (similar to the one discussed earlier concerning the atypicality of events in Extremistan). Even other notions such as "cause" take on a different meaning, particularly in the presence of circular causality and interdependence.* The probabilistic equivalent is the move from a conventional random walk model (with a random variable moving in a fixed terrain and not interacting with other variables around it), to percolation models (where the terrain itself is stochastic, with different variables acting on one another).

Driving the School Bus Blindfolded

Alas, at the time of writing, the economics establishment is still ignorant of the presence of complexity, which degrades predictability. I will not get too involved in my outrage—instead of doing a second *deserto,* Mark Spitznagel and I are designing another risk management program to robustify portfolios against model error, error mostly stemming from the government's error in the projection of deficits, leading to excessive borrowing and possible hyperinflation.

I was once at the World Economic Forum in Davos; at one of my sessions, I illustrated interdependence in a complex system and the degradation of forecasting, with the following scheme: unemployment in New York triggered by Wall Street losses, percolating and generating unemployment in, say, China, then percolating back into unemployment in New York, is not analyzable analytically, because the feedback loops produced monstrous estimation errors. I used the notion of "convexity," a disproportionate nonlinear response stemming from a variation in input (as the tools for measuring error rates go out of the window in the presence of convexity). Stanley Fisher, the head of the central bank of Israel, former IMF hotshot, co-author of a classic macroeconomics textbook, came to talk to me after the session to critique my point about such feedback loops causing unpredictability. He explained that we had input-output matrices that were good at calculating such feedbacks, and he cited work honored by the "Nobel" in economics. The economist in question was one Vassili Leontieff, I presume. I looked at him with the look "He is arrogant, but does not know enough to understand that he is not even wrong" (needless to say, Fisher was one of those who did not see the crisis coming). It was hard to get the message across that, even if econometric methods could track the effects of feedback loops in normal times (natural, since errors are small), such models said nothing about large disturbances. And I will repeat, large disturbances are everything in Extremistan.

The problem is that if I am right, Fisher's textbook, and his colleagues' textbooks, should be dispensed with. As should almost every prediction method that uses mathematical equations.

I tried to explain the problems of errors in monetary policy under nonlinearities: you keep adding money with no result … until there is hyperinflation. Or nothing. Governments should not be given toys they do not

understand.

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* The “a priori” I am using here differs from the philosophical “a priori” belief, in the sense that it is a theoretical starting point, not a belief that is nondefeasible by experience.

* Interestingly, the famous paper by Reverend Bayes that led to what we call Bayesian inference did not give us “probability” but expectation (expected average). Statisticians had difficulties with the concept so extracted probability from payoff. Unfortunately, this reduction led to the reification of the concept of probability, its adherents fogetting that probability is not natural in real life.

* The intelligent reader who gets the idea that rare events are not computable can skip the remaining parts of this section, which will be extremely technical. It is meant to prove a point to those who studied too much to be able to see things with clarity.

* This is an extremely technical point (to skip). The problem of the unknown distribution resembles, in a way, Bertrand Russell’s central difficulty in logic with the “this sentence is true” issue—a sentence cannot contain its own truth predicate. We need to apply Tarski’s solution: for every language, a metalanguage will take care of predicates of true and false about that language. With probability, simply, a metaprobability assigns degrees of credence to every probability—or, more generally, a probability distribution needs to be subordinated to a metaprobability distribution giving, say, the probability of a probability distribution being the wrong one. But luckily I have been able to express this with the available mathematical tools. I have played with this metadistribution problem in the past, in my book *Dynamic Hedging* (1997). I started putting an error rate on the Gaussian (by having my true distribution draw from two or more Gaussians, each with different parameters) leading to nested distributions almost invariably producing some class of Extremistan. So, to me, the variance of the distribution is, epistemologically, a measure of lack of knowledge about the average; hence the variance of variance is, epistemologically, a measure of lack of knowledge about the lack of knowledge of the mean—and the variance of variance is analog to the fourth moment of the distribution, and its kurtosis, which makes such uncertainty easy to express mathematically. This shows that: fat tails = lack of knowledge about lack of knowledge.

† A Gaussian distribution is parsimonious (with only two parameters to fit). But the problem of adding layers of possible jumps, each with a different probability, opens up endless possibilities of combinations of parameters.

‡ One of the most common (but useless) comments I hear is that some solutions can come from “robust statistics.” I wonder how using these techniques can create information where there is none.

* One consequence of the absence of “typicality” for an event on causality is as follows: Say an event can cause a “war.” As we saw, such war will still be undefined, since it may kill three people or a billion. So even in situations where we can identify cause and effect, we will know little, since the effect will remain atypical. I had severe problems explaining this to historians (except for Niall Ferguson) and political scientists (except for Jon Elster). Please explain this point (very politely) to your professor of Near and Middle Eastern studies.

VI

THE FOURTH QUADRANT, THE SOLUTION TO THAT MOST USEFUL OF PROBLEMS*

Did Aristotle walk slowly?—Will they follow the principles?—How to manufacture a Ponzi scheme and get credit for it

It is much more sound to take risks you can measure than to measure the risks you are taking.

There is a specific spot on the map, the Fourth Quadrant, in which the problem of induction, the pitfalls of empiricism come alive—the place where, I repeat, absence of evidence does not line up with evidence of absence. This section will allow us to base our decision on sounder epistemological grounds.

David Freedman, RIP

First, I need to pay homage to someone to whom knowledge has a large debt. The late Berkeley statistician David Freedman, who perhaps better than anyone uncovered the defects of statistical knowledge, and the inapplicability of some of the methods, sent me a farewell gift. He was supposed to be present at the meeting of the American Statistical Association that I mentioned earlier, but canceled because of illness. But he prepared me for the meeting, with a message that changed the course of the Black Swan idea: be prepared; they will provide you with a certain set of self-serving arguments and you need to respond to them. The arguments were listed in his book in a section called “The Modelers’ Response.” I list most of them below.

The Modelers’ Response: *We know all that. Nothing is perfect. The assumptions are reasonable. The assumptions don’t matter. The assumptions are conservative. You can’t prove the assumptions are wrong. We’re only doing what everybody else does. The decision-maker has to be better off with us than without us. The models aren’t totally useless. You have to do the best you can with the data. You have to make assumptions in order to make*

progress. You have to give the models the benefit of the doubt. Where's the harm?

This gave me the idea of using the approach "This is where your tools work," instead of the "This is wrong" approach I was using before. The change in style is what earned me the hugs and supply of Diet Coke and helped me get my message across. David's comments also inspired me to focus more on iatrogenics, harm caused by the need to use quantitative models.

David Freedman passed away a few weeks after the meeting.* Thank you, David. You were there when the Black Swan needed you. May you and your memory rest in peace.

Which brings us to the solution. After all this undecidability, the situation is not dire at all. Why? We, simply, can build a map of where these errors are more severe, what to watch out for.

DECISIONS

When you look at the generator of events, you can tell a priori which environment can deliver large events (Extremistan) and which environment cannot deliver them (Mediocristan). This is the only a priori assumption we need to make. The only one.

So that's that.

I. The first type of decision is simple, leading to a "binary" exposure: that is, you just care about whether something is true or false. Very true or very false does not bring you additional benefits or damage. Binary exposures do not depend on high-impact events as their payoff is limited. Someone is either pregnant or not pregnant, so if the person is "extremely pregnant" the payoff would be the same as if she were "slightly pregnant." A statement is "true" or "false" with some confidence interval. (I call these M0 as, more technically, they depend on what is called the zeroth moment, namely on the probability of events, and not on their magnitude—you just care about "raw" probability.) A biological experiment in the laboratory and a bet with a friend about the outcome of a soccer game belong to this category.

Clearly, binary outcomes are not very prevalent in life; they mostly exist in laboratory experiments and in research papers. In life, payoffs are usually openended, or, at least, variable.

II. The second type of decision is more complex and entails more openended exposures. You do not just care about frequency or probability, but about the impact as well, or, even more complex, some function of the impact. So there is another layer of uncertainty of impact. An epidemic or a war can be mild or severe. When you invest you do not care how many times you gain or lose, you care about the cumulative, the expectation: how many times you gain or lose *times* the amount made or lost. There are even more complex decisions (for instance, when one is involved with debt) but I will skip them here.

We also care about which

A. Event generators belong to Mediocristan (i.e., it is close to impossible for very large deviations to take place), an a priori assumption.

B. Event generators belong to Extremistan (i.e., very large deviations are possible, or even likely).

Which provides the four quadrants of the map.

THE FOURTH QUADRANT, A MAP

First Quadrant. Simple binary payoffs, in Mediocristan: forecasting is safe, life is easy, models work, everyone should be happy. These situations are, unfortunately, more common in laboratories and games than in real life. We rarely observe these in payoffs in economic decision making. Examples: some medical decisions (concerning one single patient, not a population), casino bets, prediction markets.

TABLE 1: TABLEAU OF DECISIONS BY PAYOFF	
M0 “True/False”	M1 Expectations
Medical results for one person (health, not epidemics)	Epidemics (number of persons infected)
Psychology experiments (yes/no answers)	Intellectual and artistic success (defined as book sales, citations, etc.)
Life/Death (for a single person, not for n persons)	Climate effects (any quantitative metric)
Symmetric bets in roulette	War damage (number of casualties)
Prediction markets	Security, terrorism, natural catastrophes (number of victims)
	General risk management
	Finance: performance of a nonleveraged investment (say, a retirement account)
	Insurance (measures of expected losses)
	Economics (policy)
	Casinos

Second Quadrant. Complex payoffs in Mediocristan: statistical methods may work satisfactorily, though there are some risks. True, use of Mediocristan models may not be a panacea, owing to preasymptotics, lack of independence, and model error. There clearly are problems here, but these have been addressed extensively in the literature, particularly by David Freedman.

Third Quadrant. Simple payoffs in Extremistan: there is little harm in being wrong, because the possibility of extreme events does not impact the payoffs. Don't worry too much about Black Swans.

Fourth Quadrant, the Black Swan Domain. Complex payoffs in Extremistan: that is where the problem resides; opportunities are present too. We need to avoid prediction of remote payoffs, though not necessarily ordinary ones. Payoffs from remote parts of the distribution are more difficult to predict than those from closer parts.*

Actually, the Fourth Quadrant has two parts: exposures to positive or negative Black Swans. I will focus here on the negative one (exploiting the positive one is too obvious, and has been discussed in the story of Apelles the painter, in Chapter 13).

TABLE 2: THE FOUR QUADRANTS

	I Simple Payoffs	II Complex Payoffs
A Mediocristan	First Quadrant ***Extremely Safe***	Second Quadrant ***(Sort of) Safe***
B Extremistan	Third Quadrant ***Safe***	Fourth Quadrant ***Black Swan Domain***

The recommendation is to move from the Fourth Quadrant into the third one. It is not possible to change the distribution; it is possible to change the exposure, as will be discussed in the next section.

What I can rapidly say about the Fourth Quadrant is that all the skepticism associated with the Black Swan problem should be focused there. A general principle is that, while in the first three quadrants you can use *the best* model or theory you can find, and rely on it, doing so is dangerous in the Fourth Quadrant: no theory or model should be better than just any theory or model.

In other words, the Fourth Quadrant is *where the difference between absence of evidence and evidence of absence becomes acute*.

Next let us see how we can exit the Fourth Quadrant or mitigate its effects.

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* This section should be skipped by those who are not involved in social science, business, or, something even worse, public policy. Section VII will be less mundane.

* David left me with a second surprise gift, the best gift anyone gave me during my *deserto:* he wrote, in a posthumous paper, that "efforts by statisticians to refute Taleb proved unconvincing," a single sentence which turned the tide and canceled out hundreds of pages of mostly ad hominem attacks, as it alerted the reader that there was no refutation, that the criticisms had no substance. All you need is a single sentence like that to put the message back in place.

* This is a true philosophical a priori since when you assume events belong to Extremistan (because of the lack of structure to the randomness), no additional empirical observations can possibly change your mind, since the property of Extremistan is to hide the possibility of Black Swan events—what I called earlier the masquerade problem.

WHAT TO DO WITH THE FOURTH QUADRANT

NOT USING THE WRONG MAP: THE NOTION OF IATROGENICS

So for now I can produce phronetic rules (in the Aristotelian sense of *phronesis*, decision-making wisdom). Perhaps the story of my life lies in the following dilemma. To paraphrase Danny Kahneman, for psychological comfort some people would rather use a map of the Pyrénées while lost in the Alps than use nothing at all. They do not do so explicitly, but they actually do worse than that while dealing with the future and using risk measures. They would prefer a defective forecast to nothing. So providing a sucker with a probabilistic measure does a wonderful job of making him take more risks. I was planning to do a test with Dan Goldstein (as part of our general research programs to understand the intuitions of humans in Extremistan). Danny (he is great to walk with, but he does not do aimless strolling, *"flâner")* insisted that doing our own experiments was not necessary. There is plenty of research on anchoring that proves the toxicity of giving someone a wrong numerical estimate of risk. Numerous experiments provide evidence that professionals are significantly influenced by numbers that they know to be irrelevant to their decision, like writing down the last four digits of one's social security number before making a numerical estimate of potential market moves. German judges, very respectable people, who rolled dice before sentencing issued sentences 50 percent longer when the dice showed a high number, without being conscious of it.

Negative Advice

Simply, don't get yourself into the Fourth Quadrant, the Black Swan Domain. But it is hard to heed this sound advice.

Psychologists distinguish between acts of commission (what we do) and acts of omission. Although these are economically equivalent for the bottom line (a dollar not lost is a dollar earned), they are not treated equally in our minds. However, as I said, recommendations of the style "Do not do" are

more robust empirically. How do you live long? By avoiding death. Yet people do not realize that success consists mainly in avoiding losses, not in trying to derive profits.

Positive advice is usually the province of the charlatan. Bookstores are full of books on how someone became successful; there are almost no books with the title *What I Learned Going Bust*, or *Ten Mistakes to Avoid in Life*.

Linked to this need for positive advice is the preference we have to *do something rather than nothing*, even in cases when doing something is harmful.

I was recently on TV and some empty-suit type kept bugging me for precise advice on how to pull out of the crisis. It was impossible to communicate my "what not to do" advice, or to point out that my field is error avoidance not emergency room surgery, and that it could be a stand-alone discipline, just as worthy. Indeed, I spent twelve years trying to explain that in many instances it was better—and wiser—to have no models than to have the mathematical acrobatics we had.

Unfortunately such lack of rigor pervades the place where we expect it the least: institutional science. Science, particularly its academic version, has never liked negative results, let alone the statement and advertising of its own limits. The reward system is not set up for it. You get respect for doing funambulism or spectator sports—following the right steps to become "the Einstein of Economics" or "the next Darwin" rather than give society something real by debunking myths or by cataloguing where our knowledge stops.

Let me return to Gödel's limit. In some instances we accept the limits of knowledge, trumpeting, say, Gödel's "breakthrough" mathematical limit because it shows elegance in formulation and mathematical prowess—though the importance of this limit is dwarfed by our practical limits in forecasting climate changes, crises, social turmoil, or the fate of the endowment funds that will finance research into such future "elegant" limits. This is why I claim that my Fourth Quadrant solution is the most applied of such limits.

Iatrogenics and the Nihilism Label

Let's consider medicine (that sister of philosophy), which only started saving lives less than a century ago (I am generous), and to a lesser extent than

initially advertised in the popular literature, as the drops in mortality seem to arise much more from awareness of sanitation and the (random) discovery of antibiotics rather than from therapeutic contributions. Doctors, driven by the beastly illusion of control, spent a long time killing patients, not considering that "doing nothing" could be a valid option (it was "nihilistic")—and research compiled by Spyros Makridakis shows that they still do to some extent, particularly in the overdiagnoses of some diseases.

The nihilism label has always been used to harm. Practitioners who were conservative and considered the possibility of letting nature do its job, or who stated the limits of our medical understanding, were until the 1960s accused of "therapeutic nihilism." It was deemed "unscientific" to avoid embarking on a course of action based on an incomplete understanding of the human body—to say, "This is the limit; this is where my body of knowledge stops." It has been used against this author by intellectual fraudsters trying to sell products.

The very term *iatrogenics*, i.e., the study of the harm caused by the healer, is not widespread—I have never seen it used outside medicine. In spite of my lifelong obsession with what is called type 1 error, or the false positive, I was only introduced to the concept of iatrogenic harm very recently, thanks to a conversation with the essayist Bryan Appleyard. How can such a major idea remain hidden from our consciousness? Even in medicine, that is, modern medicine, the ancient concept "Do no harm" sneaked in very late. The philosopher of science Georges Canguilhem wondered why it was not until the 1950s that the idea came to us. This, to me, is a mystery: how professionals can cause harm for such a long time in the name of knowledge and get away with it.

Sadly, further investigation shows that these iatrogenics were mere rediscoveries after science grew too arrogant by the Enlightenment. Alas, once again, the elders knew better—Greeks, Romans, Byzantines, and Arabs had a built-in respect for the limits of knowledge. There is a treatise by the medieval Arab philosopher and doctor Al-Ruhawi which betrays the familiarity of these Mediterranean cultures with iatrogenics. I have also in the past speculated that religion saved lives by taking the patient away from the doctor. You could satisfy your illusion of control by going to the Temple of Apollo rather than seeing the doctor. What is interesting is that the ancient Mediterraneans may have understood the trade-off very well and may have accepted religion partly as a tool to tame the illusion of control.

You cannot do anything with knowledge unless you know where it stops, and the costs of using it. Post-Enlightenment science, and its daughter superstar science, were lucky to have done well in (linear) physics, chemistry, and engineering. But at some point we need to give up on elegance to focus on something that was given short shrift for a very long time: the maps showing what current knowledge and current methods do not do for us; and a rigorous study of generalized scientific iatrogenics, what harm can be caused by science (or, better, an exposition of what harm has been done by science). I find it the most respectable of pursuits.

Iatrogenics of Regulators. Alas, the call for more (unconditional) regulation of economic activity appears to be a normal response. My worst nightmares have been the results of regulators. It was they who promoted the reliance on ratings by credit agencies and the "risk measurement" that fragilized the system as bankers used them to build positions that turned sour. Yet every time there is a problem, we do the Soviet-Harvard thing of more regulation, which makes investment bankers, lawyers, and former-regulators-turned-Wall-Street-advisers rich. They also serve the interest of other groups.

PHRONETIC RULES: WHAT IS WISE TO DO (OR NOT DO) IN REAL LIFE TO MITIGATE THE FOURTH QUADRANT IF YOU CAN'T BARBELL?

The most obvious way to exit the Fourth Quadrant is by "truncating," cutting certain exposures by purchasing insurance, when available, putting oneself in the "barbell" situation described in Chapter 13. But if you are not able to barbell, and cannot avoid the exposure, as with, say, climate notions, exposure to epidemics, and similar items from the previous table, then we can subscribe to the following rules of "wisdom" to increase robustness.

1. Have respect for time and nondemonstrative knowledge.

Recall my respect for Mother Earth—simply because of its age. It takes much, much longer for a series of data in the Fourth Quadrant to reveal its properties. I had been railing that compensation for bank executives, who are squarely in the Fourth Quadrant, is done on a short-term window, say yearly, for things that blow up every five, ten, or fifteen years, causing a mismatch between observation window and window of a sufficient length to reveal the properties. Bankers get rich in spite of long-term negative returns.

Things that have worked for a long time are preferable—they are more likely to have reached their ergodic states. At the worst, we don't know how long they'll last.*

Remember that the burden of proof lies on someone disturbing a complex system, not on the person protecting the status quo.

2. Avoid optimization; learn to love redundancy.

I've discussed redundancy and optimization in Section I. A few more things to say.

Redundancy (in terms of having savings and cash under the mattress) is the opposite of debt. Psychologists tell us that getting rich does not bring happiness—if you spend your savings. But if you hide it under the mattress, you are less vulnerable to a Black Swan.

Also, for example, one can buy insurance, or construct it, to robustify a portfolio.

Overspecialization also is not a great idea. Consider what can happen to you if your job disappears completely. Someone who is a Wall Street analyst (of the forecasting kind) moonlighting as a belly dancer will do a lot better in a financial crisis than someone who is just an analyst.

3. Avoid prediction of small-probability payoffs—though not necessarily of ordinary ones.

Obviously, payoffs from remote events are more difficult to predict.

4. Beware the "atypicality" of remote events.

There are suckers' methods called "scenario analysis" and "stress testing"—usually based on the past (or on some "make sense" theory). Yet (I showed earlier how) past shortfalls do not predict subsequent shortfalls, so we do not know what exactly to stress-test for. Likewise, "prediction markets" do not function here, since bets do not protect an open-ended exposure. They might work for a binary election, but not in the Fourth Quadrant.

5. Beware moral hazard with bonus payments.

It's optimal to make a series of bonuses by betting on hidden risks in the Fourth Quadrant, then blow up and write a thank-you letter. This is called the moral hazard argument. Bankers are always rich because of this bonus mismatch. In fact, society ends up paying for it. The same applies to company executives.

6. Avoid some risk metrics.

Conventional metrics, based on Mediocristan, adjusted for large deviations, don't work. This is where suckers fall in the trap—one far more extensive than just assuming something other than the Gaussian bell curve. Words like "standard deviation" are not stable and do not measure anything in the Fourth Quadrant. Neither do "linear regression" (the errors are in the Fourth Quadrant), "Sharpe ratio," Markowitz optimal portfolio, ANOVA shmanova, Least square, and literally anything mechanistically pulled out of a statistics textbook. My problem has been that people can accept the role of rare events,

agree with me, *and* still use these metrics, which leads me to wonder if this is a psychological disorder.

7. Positive or negative Black Swan?

Clearly the Fourth Quadrant can present positive or negative exposures to the Black Swan; if the exposure is negative, the true mean is more likely to be underestimated by measurement of past realizations, and the total potential is likewise poorly gauged.

Life expectancy of humans is not as long as we suspect (under globalization) because the data are missing something central: the big epidemic (which far outweighs the gains from cures). The same, as we saw, with the return on risky investments.

On the other hand, research ventures show a less rosy past history. A biotech company (usually) faces positive uncertainty, while a bank faces almost exclusively negative shocks.

Model errors benefit those exposed to positive Black Swans. In my new research, I call that being "concave" or "convex" to model error.

8. Do not confuse absence of volatility with absence of risk.

Conventional metrics using volatility as an indicator of stability fool us, because the evolution into Extremistan is marked by a lowering of volatility—and a greater risk of big jumps. This has fooled a chairman of the Federal Reserve called Ben Bernanke—as well as the entire banking system. It will fool again.

9. Beware presentations of risk numbers.

I presented earlier the results showing how risk perception is subjected to framing issues that are acute in the Fourth Quadrant. They are much more benign elsewhere.

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* Most of the smear campaign I mentioned earlier revolves around misrepresentation of the insurance-style properties and performance of the hedging strategies for the barbell and "portfolio robustification" associated with Black Swan ideas, a misrepresentation perhaps made credible by the fact that when one

observes returns on a short-term basis, one sees nothing relevant except shallow frequent variations (mainly losses). People just forget to cumulate properly and remember frequency rather than total. The real returns, according to the press, were around 60 percent in 2000 and more than 100 percent in 2008, with relatively shallow losses and profits otherwise, so it would be child's play to infer that returns would be in the triple digits over the past decade (all you need is one good jump). The Standard and Poor's 500 was down 23 percent over the same ten-year period.

VIII

THE TEN PRINCIPLES FOR A BLACK-SWAN-ROBUST SOCIETY*

I wrote the following "ten principles" mostly for economic life to cope with the Fourth Quadrant, in the aftermath of the crisis.

1. What is fragile should break early, while it's still small.

Nothing should ever become too big to fail. Evolution in economic life helps those with the maximum amount of hidden risks become the biggest.

2. No socialization of losses and privatization of gains.

Whatever may need to be bailed out should be nationalized; whatever does not need a bailout should be free, small, and risk-bearing. We got ourselves into the worst of capitalism and socialism. In France, in the 1980s, the socialists took over the banks. In the United States in the 2000s, the banks took over the government. This is surreal.

3. People who were driving a school bus blindfolded (and crashed it) should never be given a new bus.

The economics establishment (universities, regulators, central bankers, government officials, various organizations staffed with economists) lost its legitimacy with the failure of the system in 2008. It is irresponsible and foolish to put our trust in their ability to get us out of this mess. It is also irresponsible to listen to advice from the "risk experts" and business school academia still promoting their measurements, which failed us (such as Value-at-Risk). Find the smart people whose hands are clean.

4. Don't let someone making an "incentive" bonus manage a nuclear plant —or your financial risks.

Odds are he would cut every corner on safety to show “profits” from these savings while claiming to be “conservative.” Bonuses don’t accommodate the hidden risks of blowups. It is the asymmetry of the bonus system that got us here. No incentives without disincentives: capitalism is about rewards and punishments, not just rewards.

5. Compensate complexity with simplicity.

Complexity from globalization and highly networked economic life needs to be countered by simplicity in financial products. The complex economy is already a form of leverage. It’s the leverage of efficiency. Adding debt to that system produces wild and dangerous gyrations and provides no room for error. Complex systems survive thanks to slack and redundancy, not debt and optimization. Capitalism cannot avoid fads and bubbles. Equity bubbles (as in 2000) have proved to be mild; debt bubbles are vicious.

6. Do not give children dynamite sticks, even if they come with a warning label.

Complex financial products need to be banned because nobody understands them, and few are rational enough to know it. We need to protect citizens from themselves, from bankers selling them “hedging” products, and from gullible regulators who listen to economic theorists.

7. Only Ponzi schemes should depend on confidence. Governments should never need to “restore confidence.”

In a Ponzi scheme (the most famous being the one perpetrated by Bernard Madoff), a person borrows or takes funds from a new investor to repay an existing investor trying to exit the investment.

Cascading rumors are a product of complex systems. Governments cannot stop the rumors. Simply, we need to be in a position to shrug off rumors, be robust to them.

8. Do not give an addict more drugs if he has withdrawal pains.

Using leverage to cure the problems of too much leverage is not homeopathy,

it's denial. The debt crisis is not a temporary problem, it's a structural one. We need rehab.

9. Citizens should not depend on financial assets as a repository of value and should not rely on fallible "expert" advice for their retirement.

Economic life should be definancialized. We should learn not to use markets as warehouses of value: they do not harbor the certainties that normal citizens can require, in spite of "expert" opinions. Investments should be for entertainment. Citizens should experience anxiety from their own businesses (which they control), not from their investments (which they do not control).

10. Make an omelet with the broken eggs.

Finally, the crisis of 2008 was not a problem to fix with makeshift repairs, any more than a boat with a rotten hull can be fixed with ad hoc patches. We need to rebuild the new hull with new (stronger) material; we will have to remake the system before it does so itself. Let us move voluntarily into a robust economy by helping what needs to be broken break on its own, converting debt into equity, marginalizing the economics and business school establishments, shutting down the "Nobel" in economics, banning leveraged buyouts, putting bankers where they belong, clawing back the bonuses of those who got us here (by claiming restitution of the funds paid to, say, Robert Rubin or banksters whose wealth has been subsidized by taxpaying schoolteachers), and teaching people to navigate a world with fewer certainties.

Then we will see an economic life closer to our biological environment: smaller firms, a richer ecology, no speculative leverage—a world in which entrepreneurs, not bankers, take the risks, and in which companies are born and die every day without making the news.

After this foray into business economics, let us now move to something less vulgar.

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* This passage was published as an editorial in 2009 in the *Financial Times*. Some editor—who no doubt had not read *The Black Swan*—changed my "Black-Swan-robust" into "Black-Swan-proof." There is no

such thing as Black Swan proof, but robust is good enough.

IX

AMOR FATI: HOW TO BECOME INDESTRUCTIBLE

And now, reader, time to part again.

I am in Amioun, the village of my ancestors. Sixteen out of sixteen great-great-grandparents, eight out of eight great-grandparents, and four out of four grandparents are buried in the area, almost all within a four-mile radius. Not counting the great-uncles, cousins, and other relatives. They are all resting in cemeteries in the middle of groves of olive trees in the Koura valley at the base of Mount Lebanon, which rises so dramatically that you can see the snow above you only twenty miles away.

Today, at dusk, I went to St. Sergius, locally called Mar Sarkis, from the Aramaic, the cemetery of my side of the family, to say hello to my father and my uncle Dédé, who so much disliked my sloppy dressing during my rioting days. I am sure Dédé is still offended with me; the last time he saw me in Paris he calmly dropped that I was dressed like an Australian: so the real reason for my visit to the cemetery was more self-serving. I wanted to prepare myself for where I will go next.

This is my plan B. I kept looking at the position of my own grave. A Black Swan cannot so easily destroy a man who has an idea of his final destination.

I felt robust.

*

I am carrying Seneca on all my travels, in the original, as I relearned Latin—reading him in English, that language desecrated by economists and the bureaucrats of the Federal Reserve Bank of the United States, did not feel right. Not on this occasion. It would be equivalent to reading Yeats in Swahili.

Seneca was the great teacher and practitioner of Stoicism, who transformed Greek-Phoenician Stoicism from metaphysical and theoretical discourse into a practical and moral program of living, a way to reach the *summum bonum*, an untranslatable expression depicting a life of supreme moral qualities, as

perceived by the Romans. But, even apart from this unreachable aim, he has practical advice, perhaps the only advice I can see transfer from words to practice. Seneca is the one who (with some help from Cicero) taught Montaigne that *to philosophize is to learn how to die*. Seneca is the one who taught Nietzsche the *amor fati*, "love fate," which prompted Nietzsche to just shrug and ignore adversity, mistreatment by his critics, and his disease, to the point of being bored by them.

For Seneca, Stoicism is about dealing with loss, and finding ways to overcome our loss aversion—how to become less dependent on what you have. Recall the "prospect theory" of Danny Kahneman and his colleagues: if I gave you a nice house and a Lamborghini, put a million dollars in your bank account, and provided you with a social network, then, a few months later, took everything away, you would be much worse off than if nothing had happened in the first place.

Seneca's credibility as a moral philosopher (to me) came from the fact that, unlike other philosophers, he did not denigrate the value of wealth, ownership, and property because he was poor. Seneca was said to be one of the wealthiest men of his day. He just made himself ready to lose everything every day. Every day. Although his detractors claim that in real life he was not the Stoic sage he claimed to be, mainly on account of his habit of seducing married women (with non-Stoic husbands), he came quite close to it. A powerful man, he just had many detractors—and, if he fell short of his Stoic ideal, he came much closer to it than his contemporaries. And, just as it is harder to have good qualities when one is rich than when one is poor, it is harder to be a Stoic when one is wealthy, powerful, and respected than when one is destitute, miserable, and lonely.

Nihil Perditi

In Seneca's Epistle IX, Stilbo's country was captured by Demetrius, called the Sacker of Cities. Stilbo's children and his wife were killed. Stilbo was asked what his losses were. *Nihil perditi*, I have lost nothing, he answered. *Omnia mea mecum sunt!* My goods are all with me. The man had reached the Stoic self-sufficiency, the robustness to adverse events, called *apatheia* in Stoic jargon. In other words, *nothing that might be taken from him did he consider to be a good*.

Which includes one's own life. Seneca's readiness to lose everything extended to his own life. Suspected of partaking in a conspiracy, he was asked by the emperor Nero to commit suicide. The record says that he executed his own suicide in an exemplary way, unperturbed, as if he had prepared for it every day.

Seneca ended his essays (written in the epistolary form) with *vale*, often mistranslated as "farewell." It has the same root as "value" and "valor" and means both "be strong (i.e., robust)" and "be worthy." *Vale*.

GLOSSARY

Academic libertarian: someone (like myself) who considers that knowledge is subjected to strict rules but not institutional authority, as the interest of organized knowledge is self-perpetuation, not necessarily truth (as with governments). Academia can suffer from an acute **expert problem** (q.v.), producing cosmetic but fake knowledge, particularly in **narrative disciplines** (q.v.), and can be a main source of Black Swans.

Apelles-style strategy: A strategy of seeking gains by collecting positive accidents from maximizing exposure to "good Black Swans."

Barbell strategy: a method that consists of taking both a defensive attitude and an excessively aggressive one at the same time, by protecting assets from all sources of uncertainty while allocating a small portion for high-risk strategies.

Bildungsphilister: a philistine with cosmetic, nongenuine culture. Nietzsche used this term to refer to the dogma-prone newspaper reader and opera lover with cosmetic exposure to culture and shallow depth. I extend it to the buzzword-using researcher in nonexperimental fields who lacks in imagination, curiosity, erudition, and culture and is closely centered on his ideas, on his "discipline." This prevents him from seeing the conflicts between his ideas and the texture of the world.

Black Swan blindness: the underestimation of the role of the Black Swan, and occasional overestimation of a specific one.

Black Swan ethical problem: Owing to the nonrepeatable aspect of the Black Swan, there is an asymmetry between the rewards of those who prevent and those who cure.

Confirmation error (or Platonic confirmation): You look for instances that confirm your beliefs, your construction (or model)—and find them.

Empty-suit problem (or "expert problem"): Some professionals have no differential abilities from the rest of the population, but for some reason,

and against their empirical records, are believed to be experts: clinical psychologists, academic economists, risk "experts," statisticians, political analysts, financial "experts," military analysts, CEOs, et cetera. They dress up their expertise in beautiful language, jargon, mathematics, and often wear expensive suits.

Epilogism: A theory-free method of looking at history by accumulating facts with minimal generalization and being conscious of the side effects of making causal claims.

Epistemic arrogance: Measure the difference between what someone actually knows and how much he thinks he knows. An excess will imply arrogance, a deficit humility. An epistemocrat is someone of epistemic humility, who holds his own knowledge in greatest suspicion.

Epistemic opacity: Randomness is the result of incomplete information at some layer. It is functionally indistinguishable from "true" or "physical" randomness.

Extremistan: the province where the total can be conceivably impacted by a single observation.

Fallacy of silent evidence: Looking at history, we do not see the full story, only the rosier parts of the process.

Fooled by randomness: the general confusion between luck and determinism, which leads to a variety of superstitions with practical consequences, such as the belief that higher earnings in some professions are generated by skills when there is a significant component of luck in them.

Future blindness: our natural inability to take into account the properties of the future—like autism, which prevents one from taking into account the existence of the minds of others.

Locke's madman: someone who makes impeccable and rigorous reasoning from faulty premises—such as Paul Samuelson, Robert Merton the minor, and Gerard Debreu—thus producing phony models of uncertainty that make us vulnerable to Black Swans.

Lottery-ticket fallacy: the naïve analogy equating an investment in collecting positive Black Swans to the accumulation of lottery tickets. Lottery tickets

are not scalable.

Ludic fallacy (or uncertainty of the nerd): the manifestation of the Platonic fallacy in the study of uncertainty; basing studies of chance on the narrow world of games and dice. A-Platonic randomness has an additional layer of uncertainty concerning the rules of the game in real life. The bell curve (Gaussian), or GIF (Great Intellectual Fraud), is the application of the ludic fallacy to randomness.

Mandelbrotian Gray Swan: Black Swans that we can somewhat take into account—earthquakes, blockbuster books, stock market crashes—but for which it is not possible to completely figure out the properties and produce precise calculations.

Mediocristan: the province dominated by the mediocre, with few extreme successes or failures. No single observation can meaningfully affect the aggregate. The bell curve is grounded in Mediocristan. There is a qualitative difference between Gaussians and scalable laws, much like gas and water.

Narrative discipline: the discipline that consists in fitting a convincing and well-sounding story to the past. Opposed to experimental discipline.

Narrative fallacy: our need to fit a story or pattern to a series of connected or disconnected facts. The statistical application is data mining.

Nerd knowledge: the belief that what cannot be Platonized and studied does not exist at all, or is not worth considering. There even exists a form of skepticism practiced by the nerd.

Platonic fold: the place where our Platonic representation enters into contact with reality and you can see the side effects of models.

Platonicity: the focus on those pure, well-defined, and easily discernible objects like triangles, or more social notions like friendship or love, at the cost of ignoring those objects of seemingly messier and less tractable structures.

Probability distribution: the model used to calculate the odds of different events, how they are "distributed." When we say that an event is distributed according to the bell curve, we mean that the Gaussian bell curve can help provide probabilities of various occurrences.

Problem of induction: the logical-philosophical extension of the Black Swan problem.

Randomness as incomplete information: simply, what I cannot guess is random because my knowledge about the causes is incomplete, not necessarily because the process has truly unpredictable properties.

Retrospective distortion: examining past events without adjusting for the forward passage of time. It leads to the illusion of posterior predictability.

Reverse-engineering problem: It is easier to predict how an ice cube would melt into a puddle than, looking at a puddle, to guess the shape of the ice cube that may have caused it. This "inverse problem" makes narrative disciplines and accounts (such as histories) suspicious.

Round-trip fallacy: the confusion of absence of evidence of Black Swans (or something else) for evidence of absence of Black Swans (or something else). It affects statisticians and other people who have lost part of their reasoning by solving too many equations.

Scandal of prediction: the poor prediction record in some forecasting entities (particularly narrative disciplines) mixed with verbose commentary and a lack of awareness of their own dire past record.

Scorn of the abstract: favoring contextualized thinking over more abstract, though more relevant, matters. "The death of one child is a tragedy; the death of a million is a statistic."

Statistical regress argument (or the problem of the circularity of statistics): We need data to discover a probability distribution. How do we know if we have enough? From the probability distribution. If it is a Gaussian, then a few points of data will suffice. How do we know it is a Gaussian? From the data. So we need the data to tell us what probability distribution to assume, and we need a probability distribution to tell us how much data we need. This causes a severe regress argument, which is somewhat shamelessly circumvented by resorting to the Gaussian and its kin.

Uncertainty of the deluded: people who tunnel on sources of uncertainty by producing precise sources like the great uncertainty principle, or similar, less consequential matters, to real life; worrying about subatomic particles while forgetting that we can't predict tomorrow's crises.

To Benoît Mandelbrot,
a Roman among Greeks

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Ralph Gomory and Jesse Ausubel of the Sloan Foundation run a research funding program called the Known, the Unknown, and the Unknowable. They offered their moral and financial help for the promotion of my ideas—I took the invaluable moral option. I also thank my business partners, coauthors, and intellectual associates: Espen Haug, Mark Spitznagel, Benoît Mandelbrot, Tom Witz, Paul Wilmott, Avital Pilpel, and Emanuel Derman. I also thank John Brockman and Katinka Matson for making this book possible, and Max Brockman for his comments on the draft. I thank Cindy, Sarah, and Alexander for their tolerance. In addition, Alexander helped with the graphs and Sarah worked on the bibliography. Mark Fandetti, Mark Horowitz, Bruce Waxman, Spiros Makridakis, Jack Schwagger, and Elie Ayache helped with the more technical typos. The readers Jonathan Skinner, Harry Thayer, and David Evans helped correct technical and factual mistakes. I thank Linda Eckstein and Justin Fox for suggesting to Mandelbrot and me the graph of the SP500.

I tried to give my editor, Will Murphy, the impression of being an unbearably stubborn author, only to discover that I was fortunate that he was an equally stubborn editor (but good at hiding it). He protected me from the intrusions of the standardizing editors. They have an uncanny ability to inflict maximal damage by breaking the internal rhythm of one’s prose with the minimum of changes. Will M. is also the right kind of party animal. I was also flattered that Daniel Menaker took the time to edit my text. I also thank Janet Wygal and Steven Meyers. The staff at Random House was accommodating —but they never got used to my phone pranks (like my trying to pass for Bernard-Henri Lévy). One of the highlights of my writing career was a long lunch with William Goodlad, my editor at Penguin, and Stefan McGrath, the managing director of the group. I suddenly realized that I could not separate the storyteller in me from the scientific thinker; as a matter of fact, the story came first to my mind, rather than as an after-the-fact illustration of the concept.

Part Three of this book inspired my class lectures at the University of Massachusetts at Amherst. I thank Dean Tom O'Brien for his support and encouragement. He loved to see me shake up indoctrinated Ph.D. students. I also thank my second home, the Courant Institute of Mathematical Sciences of New York University, for allowing me to lecture for three quarters of a decade.

It is unfortunate that one learns most from people one disagrees with—something Montaigne encouraged half a millennium ago but is rarely practiced. I discovered that it puts your arguments through robust seasoning since you know that these people will identify the slightest crack—and you get information about the limits of their theories as well as the weaknesses of your own. I tried to be more graceful with my detractors than with my friends—particularly those who were (and stayed) civilized. So, over my career, I learned a few tricks from a series of public debates, correspondence, and discussions with Robert C. Merton, Steve Ross, Myron Scholes, Philippe Jorion, and dozens of others (though, aside from Elie Ayache's critique, the last time I heard something remotely new against my ideas was in 1994). These debates were valuable since I was looking for the extent of the counterarguments to my Black Swan idea and trying to figure out how my detractors think—or what they did not think about. Over the years I have ended up reading more material from those I disagree with than from those whose opinion I share—I read more Samuelson than Hayek, more Merton (the younger) than Merton (the elder), more Hegel than Montaigne, and more Descartes than Sextus. It is the duty of every author to represent the ideas of his adversaries as faithfully as possible.

My greatest accomplishment in life is to have managed to befriend people, such as Elie Ayache and Jim Gatheral, in spite of some intellectual disagreements.

Most of this book was written during a peripatetic period when I freed myself of (almost) all business obligations, routines, and pressures, and went on meditative urban walks in a variety of cities where I gave a series of lectures on the Black Swan idea.* I wrote it largely in cafés—my preference has always been for dilapidated (but elegant) cafés in regular neighborhoods, as unpolluted with persons of commerce as possible. I also spent much time in Heathrow Terminal 4, absorbed in my writing to the point that I forgot about my allergy to the presence of strained businessmen around me.

* I lost his business card, but would like to warmly thank a scientist traveling to Vienna aboard British Airways flight 700 on December 11, 2003, for suggesting the billiard ball illustration in Chapter 11. All I know about him is that he was fifty-two, gray-haired, English-born, wrote poetry on yellow notepads, and was traveling with seven suitcases since he was moving in with his thirty-five-year-old Viennese girlfriend.

* It is impossible to go very deep into an idea when you run a business, no matter the number of hours the occupation entails—simply put, unless you are insensitive, the worries and feelings of responsibility occupy precious cognitive space. You may be able to study, meditate, and write if you are an employee, but not when you own a business—unless you are of an irresponsible nature. I thank my partner, Mark Spitznagel, for allowing me—thanks to the clarity of his mind and his highly systematic, highly disciplined, and well engineered approach—to gain exposure to high-impact rare events without my having to get directly involved in business activities.

NOTES

BEHIND THE CURTAIN: ADDITIONAL NOTES, TECHNICAL COMMENTS, REFERENCES, AND READING RECOMMENDATIONS

I separate topics thematically; so general references will mostly be found in the chapter in which they first occur. I prefer to use a logical sequence here rather than stick to chapter division.

PROLOGUE and CHAPTER 1

Bell curve: When I write *bell curve* I mean the Gaussian bell curve, a.k.a. normal distribution. All curves look like bells, so this is a nickname. Also, when I write *the Gaussian basin* I mean all distributions that are similar and for which the improbable is inconsequential and of low impact (more technically, nonscalable—all moments are finite). Note that the visual presentation of the bell curve in histogram form masks the contribution of the remote event, as such an event will be a point to the far right or far left of the center.

Diamonds: See Eco (2002).

Platonicity: I'm simply referring to incurring the risk of using a wrong form —not that forms don't exist. I am not against essentialisms; I am often skeptical of our reverse engineering and identification of the right form. It is an inverse problem!

Empiricist: If I call myself an empiricist, or an empirical philosopher, it is because I am just suspicious of confirmatory generalizations and hasty theorizing. Do not confuse this with the British empiricist tradition. Also, many statisticians, as we will see with the Makridakis competition, call themselves "empirical" researchers, but are in fact just the opposite—they fit theories to the past.

Mention of Christ: See Flavius Josephus's *The Jewish War*.

Great War and prediction: Ferguson (2006b).

Hindsight bias (retrospective distortion): See Fischhoff (1982b).

Historical fractures: Braudel (1985), p. 169, quotes a little known passage from Gautier. He writes, "'This long history,' wrote Emile-Félix Gautier, 'lasted a dozen centuries, longer than the entire history of France. Encountering the first Arab sword, the Greek language and thought, all that heritage went up in smoke, as if it never happened.'" For discussions of discontinuity, see also Gurvitch (1957), Braudel (1953), Harris (2004).

Religions spread as bestsellers: Veyne (1971). See also Veyne (2005).

Clustering in political opinions: Pinker (2002).

Categories: Rosch (1973, 1978). See also Umberto Eco's *Kant and the Platypus*.

Historiography and philosophy of history: Bloch (1953), Carr (1961), Gaddis (2002), Braudel (1969, 1990), Bourdé and Martin (1989), Certeau (1975), *Muqaddamat* Ibn Khaldoun illustrate the search for causation, which we see already present in Herodotus. For philosophy of history, Aron (1961), Fukuyama (1992). For postmodern views, see Jenkins (1991). I show in Part Two how historiographers are unaware of the epistemological difference between forward and backward processes (i.e., between projection and reverse engineering).

Information and markets: See Shiller (1981, 1989), DeLong *et al.* (1991), and Cutler *et al.* (1989). The bulk of market moves does not have a "reason," just a contrived explanation.

Of descriptive value for crashes: See Galbraith (1997), Shiller (2000), and Kindleberger (2001).

CHAPTER 3

Movies: See De Vany (2002). See also Salganik *et al.* (2006) for the contagion in music buying.

Religion and domains of contagion: See Boyer (2001).

Wisdom (madness) of crowds: Collectively, we can both get wiser or far more foolish. We may collectively have intuitions for Mediocristan-related matters, such as the weight of an ox (see Surowiecki, 2004), but my conjecture is that we fail in more complicated predictions (economic variables for which crowds incur pathologies—two heads are worse than one). For decision errors and groups, see Sniezek and Buckley (1993). Classic: Charles Mackay's *Extraordinary Popular Delusions and the Madness of Crowds*.

Increase in the severity of events: Zajdenweber (2000).

Modern life: The nineteenth-century novelist Émile Zola welcomed the arrival of the market for culture in the late 1800s, of which he seemed to be one of the first beneficiaries. He predicted that the writers' and artists' ability to exploit the commercial system freed them from a dependence on patrons' whims. Alas, this was accompanied with more severe concentration—very few people benefited from the system. Lahire (2006) shows how most writers, throughout history, have starved. Remarkably, we have ample data from France about the literary tradition.

CHAPTER 4

Titanic: The quote is from Dave Ingram's presentation at the Enterprise Risk Management Symposium in Chicago on May 2, 2005. For more on LTCM, see Lowenstein (2000), Dunbar (1999).

Hume's exposition: Hume (1748, 2000).

Sextus Empriricus: "It is easy, I think, to reject the method of induction (επαγωγη). For since by way of it they want to make universals convincing on the basis of particulars, they will do this surveying all the particulars or some of them. But if some, the induction will be infirm, it being that some of the particulars omitted in the induction should be contrary to the universal; and if all, they will labor at an impossible task, since the particulars and infinite are indeterminate. Thus in either case it results, I think, that induction totters." *Outline of Pyrrhonism*, Book II, p. 204.

Bayle: The *Dictionnaire historique et critique* is long (twelve volumes, close to 6,000 pages) and heavy (40 pounds), yet it was an intellectual bestseller in its day, before being supplanted by the *philosophes*. It can be downloaded from the French Bibliothèque Nationale at www.bn.fr.

Hume's inspiration from Bayle: See Popkin (1951, 1955). Any reading of Bishop Huet (further down) would reveal the similarities with Hume.

Pre-Bayle thinkers: *Dissertation sur la recherche de la vérité*, Simon Foucher, from around 1673. It is a delight to read. It makes the heuristics and biases tradition look like the continuation of the pre-Enlightenment prescientific revolution atmosphere.

Bishop Huet and the problem of induction: "Things cannot be known with perfect certainty because their causes are infinite," wrote Pierre-Daniel Huet in his *Philosophical Treatise on the Weaknesses of the Human Mind*. Huet, former bishop of Avranches, wrote this under the name Théocrite de Pluvignac, Seigneur de la Roche, Gentilhomme de Périgord. The chapter has another exact presentation of what became later known as "Hume's problem." That was in 1690, when the future David Home (later Hume) was minus twenty-two, so of no possible influence on Monseigneur Huet.

Brochard's work: I first encountered the mention of Brochard's work (1888)

in Nietzsche's *Ecce Homo*, in a comment where he also describes the skeptics as straight talkers. "An excellent study by Victor Brochard, *Les sceptiques grecs*, in which my Laertiana are also employed. The skeptics! the only *honourable* type among the two and five fold ambiguous philosopher crowd!" *More* trivia: Brochard taught Proust (see Kristeva, 1998).

Brochard seems to have understood Popper's problem (a few decades before Popper's birth). He presents the views of the negative empiricism of Menodotus of Nicomedia in similar terms to what we would call today "Popperian" empiricism. I wonder if Popper knew anything about Menodotus. He does not seem to quote him anywhere. Brochard published his doctoral thesis, *De l'erreur*, in 1878 at the University of Paris, on the subject of error—wonderfully modern.

Epilogism: We know very little about Menodotus except for attacks on his beliefs by his detractor Galen in the extant Latin version of the *Outline of Empiricism (Subfiguratio empírica)*, hard to translate:

> *Memoriam et sensum et vocans* epilogismum *hoc tertium, multotiens autem et preter memoriam nihil aliud ponens quam* epilogismum. (In addition to perception and recollection, the third method is *epilogism sensum*, as the practitioner has, besides memory, nothing other than *epilogism* senses; Perilli's correction.

But there is hope. Perilli (2004) reports that, according to a letter by the translator Is-haq Bin Hunain, there may be a "transcription" of Menodotus's work in Arabic somewhere for a scholar to find.

Pascal: Pascal too had an idea of the confirmation problem and the asymmetry of inference. In his preface to the *Traité du vide*, Pascal writes (and I translate):

> In the judgment they made that nature did not tolerate a vacuum, they only meant nature in the state in which they knew it, since, so claim so in general, it would not be sufficient to witness it in a hundred different encounters, nor in a thousand, not in any other number no matter how large, since it would be a single case that would deny the general definition, and if one was contrary, a single one …

Hume's biographer: Mossner (1970). For a history of skepticism, Victor Cousin's lectures *Leçons d'histoire de la philosophie à la Sorbonne* (1828) and Hippolyte Taine's *Les philosophes classiques*, 9th edition (1868, 1905). Popkin (2003) is a modern account. Also see Heckman (2003) and Bevan (1913). I have seen nothing in the modern philosophy of probability linking

it to skeptical inquiry.

Sextus: See Popkin (2003), Sextus, House (1980), Bayle, Huet, Annas and Barnes (1985), and Julia Anna and Barnes's introduction in Sextus Empiricus (2000). Favier (1906) is hard to find; the only copy I located, thanks to Gur Huberman's efforts, was rotten—it seems that it has not been consulted in the past hundred years.

Menodotus of Nicomedia and the marriage between empiricism and skepticism: According to Brochard (1887), Menodotus is responsible for the mixing of empiricism and Pyrrhonism. See also Favier (1906). See skepticism about this idea in Dye (2004), and Perilli (2004).

Function not structure; empirical tripod: There are three sources, and three only, for experience to rely upon: observation, history (i.e., recorded observation), and judgment by analogy.

Algazel: See his *Tahafut al falasifah*, which is rebutted by Averroës, a.k.a. Ibn-Rushd, in *Tahafut Attahafut*.

Religious skeptics: There is also a medieval Jewish tradition, with the Arabic-speaking poet Yehuda Halevi. See Floridi (2002).

Algazel and the ultimate/proximate causation: "… their determining, from the sole observation, of the nature of the necessary relationship between the cause and the effect, as if one could not witness the effect without the attributed cause of the cause without the same effect." *(Tahafut*)

At the core of Algazel's idea is the notion that if you drink because you are thirsty, thirst should not be seen as a *direct* cause. There may be a greater scheme being played out; in fact, there *is*, but it can only be understood by those familiar with evolutionary thinking. See Tinbergen (1963, 1968) for a modern account of the proximate. In a way, Algazel builds on Aristotle to attack him. In his *Physics*, Aristotle had already seen the distinction between the different layers of cause (formal, efficient, final, and material).

Modern discussions on causality: See Reichenbach (1938), Granger (1999), and Pearl (2000).

Children and natural induction: See Gelman and Coley (1990), Gelman and Hirschfeld (1999), and Sloman (1993).

Natural induction: See Hespos (2006), Clark and Boyer (2006), Inagaki and Hatano (2006), Reboul (2006). See summary of earlier works in Plotkin (1998).

CHAPTERS 5–7

"Economists": What I mean by "economists" are most members of the mainstream, neoclassical economics and finance establishment in universities—not fringe groups such as the Austrian or the Post-Keynesian schools.

Small numbers: Tversky and Kahneman (1971), Rabin (2000).

Domain specificity: Williams and Connolly (2006). We can see it in the usually overinterpreted Wason Selection Test: Wason (1960, 1968). See also Shaklee and Fischhoff (1982), Barron Beaty, and Hearshly (1988). Kahneman's "They knew better" in Gilovich *et al.* (2002).

Updike: The blurb is from Jaynes (1976).

Brain hemispheric specialization: Gazzaniga and LeDoux (1978), Gazzaniga *et al.* (2005). Furthermore, Wolford, Miller, and Gazzaniga (2000) show probability matching by the left brain. When you supply the right brain with, say, a lever that produces desirable goods 60% of the time, and another lever 40%, the right brain will correctly push the first lever as the optimal policy. If, on the other hand, you supply the left brain with the same options, it will push the first lever 60 percent of the time and the other one 40—it will refuse to accept randomness. Goldberg (2005) argues that the specialty is along different lines: left-brain damage does not bear severe effects in children, unlike right-brain lesions, while this is the reverse for the elderly. I thank Elkhonon Goldberg for referring me to Snyder's work; Snyder (2001). The experiment is from Snyder *et al.* (2003).

Sock selection and retrofit explanation: The experiment of the socks is presented in Carter (1999); the original paper appears to be Nisbett and Wilson (1977). See also Montier (2007).

Astebro: Astebro (2003). See "Searching for the Invisible Man," *The Economist*, March 9, 2006. To see how the overconfidence of entrepreneurs can explain the high failure rate, see Camerer (1995).

Dopamine: Brugger and Graves (1997), among many other papers. See also Mohr *et al.* (2003) on dopamine asymmetry.

Entropy and information: I am purposely avoiding the notion of entropy

because the way it is conventionally phrased makes it ill-adapted to the type of randomness we experience in real life. Tsallis entropy works better with fat tails.

Notes on George Perec: Eco (1994).

Narrativity and illusion of understanding: Wilson, Gilbert, and Centerbar (2003): "Helplessness theory has demonstrated that if people feel that they cannot control or predict their environments, they are at risk for severe motivational and cognitive deficits, such as depression." For the writing down of a diary, see Wilson (2002) or Wegner (2002).

E. M. Forster's example: reference in Margalit (2002).

National character: Terracciano *et al.* (2005) and Robins (2005) for the extent of individual variations. The illusion of nationality trait, which I usually call the "nationality heuristic," does connect to the halo effect: see Rosenzweig (2006) and Cialdini (2001). See Anderson (1983) for the ontology of nationality.

Consistency bias: What psychologists call the consistency bias is the effect of revising memories in such a way to make sense with respect to subsequent information. See Schacter (2001).

Memory not like storage on a computer: Rose (2003), Nader and LeDoux (1999).

The myth of repressed memory: Loftus and Ketcham (2004).

Chess players and disconfirmation: Cowley and Byrne (2004).

Quine's problem: Davidson (1983) argues in favor of local, but against total, skepticism.

Narrativity: Note that my discussion is not existential here, but merely practical, so my idea is to look at narrativity as an informational compression, nothing more involved philosophically (like whether a self is sequential or not). There is a literature on the "narrative self"—Bruner (2002) or whether it is necessary—see Strawson (1994) and his attack in Strawson (2004). The debate: Schechtman (1997), Taylor (1999), Phelan (2005). Synthesis in Turner (1996).

"Postmodernists" and the desirability of narratives: See McCloskey

(1990) and Frankfurter and McGoun (1996).

Narrativity of sayings and proverbs: Psychologists have long examined the gullibility of people in social settings when faced with well-sounding proverbs. For instance, experiments have been made since the 1960s where people are asked whether they believe that a proverb is right, while another cohort is presented with the opposite meaning. For a presentation of the hilarious results, see Myers (2002).

Science as a narrative: Indeed scientific papers can succeed by the same narrativity bias that "makes a story." You need to get attention. Bushman and Wells (2001).

Discovering probabilities: Barron and Erev (2003) show how probabilities are underestimated when they are not explicitly presented. Also personal communication with Barron.

Risk and probability: See Slovic, Fischhoff, and Lichtenstein (1976), Slovic *et al.* (1977), and Slovic (1987). For risk as analysis and risk as feeling theory, see Slovic *et al.* (2002, 2003), and Taleb (2004c). See Bar-Hillel and Wagenaar (1991)

Link between narrative fallacy and clinical knowledge: Dawes (1999) has a message for economists: see here his work on interviews and the concoction of a narrative. See also Dawes (2001) on the retrospective effect.

Two systems of reasoning: See Sloman (1996, 2002), and the summary in Kahneman and Frederick (2002). Kahneman's Nobel lecture sums it all up; it can be found at www.nobel.se. See also Stanovich and West (2000).

Risk and emotions: Given the growing recent interest in the emotional role in behavior, there has been a growing literature on the role of emotions in both risk bearing and risk avoidance: the "risk as feeling" theory. See Loewenstein *et al.* (2001) and Slovic *et al.* (2003a). For a survey see Slovic *et al.* (2003b) and see also Slovic (1987). For a discussion of the "affect heuristic," see Finucane *et al.* (2000). For modularity, see Bates (1994).

Emotions and cognition: For the effect of emotions on cognition, see LeDoux (2002). For risk, see Bechara *et al.* (1994).

Availability heuristic (how easily things come to mind): See Tversky and

Kahneman (1973).

Real incidence of catastrophes: For an insightful discussion, see Albouy (2002), Zajdenweber (2000), or Sunstein (2002).

Terrorism exploitation of the sensational: See the essay in Taleb (2004c).

General books on psychology of decision making (heuristics and biases): Baron (2000) is simply the most comprehensive on the subject. Kunda (1999) is a summary from the standpoint of social psychology (sadly, the author died prematurely); shorter: Plous (1993). Also Dawes (1988) and Dawes (2001). Note that a chunk of the original papers are happily compiled in Kahneman *et al.* (1982), Kahneman and Tversky (2000), Gilovich *et al.* (2002), and Slovic (2001a and 2001b). See also Myers (2002) for an account on intuition and Gigerenzer *et al.* (2000) for an ecological presentation of the subject. The most complete account in economics and finance is Montier (2007), where his beautiful summary pieces that fed me for the last four years are compiled—not being an academic, he gets straight to the point. See also Camerer, Loewenstein, and Rabin (2004) for a selection of technical papers. A recommended review article on clinical "expert" knowledge is Dawes (2001).

More general psychology of decision presentations: Klein (1998) proposes an alternative model of intuition. See Cialdini (2001) for social manipulation. A more specialized work, Camerer (2003), focuses on game theory.

General review essays and comprehensive books in cognitive science: Newell and Simon (1972), Varela (1988), Fodor (1983), Marr (1982), Eysenck and Keane (2000), Lakoff and Johnson (1980). The *MIT Encyclopedia of Cognitive Science* has review articles by main thinkers.

Evolutionary theory and domains of adaptation: See the original Wilson (2000), Kreps and Davies (1993), and Burnham (1997, 2003). Very readable: Burnham and Phelan (2000). The compilation of Robert Trivers's work is in Trivers (2002). See also Wrangham (1999) on wars.

Politics: "The Political Brain: A Recent Brain-imaging Study Shows That Our Political Predilections Are a Product of Unconscious Confirmation Bias," by Michael Shermer, *Scientific American*, September 26, 2006.

Neurobiology of decision making: For a general understanding of our

knowledge about the brain's architecture: Gazzaniga *et al.* (2002). Gazzaniga (2005) provides literary summaries of some of the topics. More popular: Carter (1999). Also recommended: Ratey (2001), Ramachandran (2003), Ramachandran and Blakeslee (1998), Carter (1999, 2002), Conlan (1999), the very readable Lewis, Amini, and Lannon (2000), and Goleman (1995). See Glimcher (2002) for probability and the brain. For the emotional brain, the three books by Damasio (1994, 2000, 2003), in addition to LeDoux (1998) and the more detailed LeDoux (2002), are the classics. See also the shorter Evans (2002). For the role of vision in aesthetics, but also in interpretation, Zeki (1999).

General works on memory: In psychology, Schacter (2001) is a review work of the memory biases with links to the hindsight effects. In neurobiology, see Rose (2003) and Squire and Kandel (2000). A general textbook on memory (in empirical psychology) is Baddeley (1997).

Intellectual colonies and social life: See the account in Collins (1998) of the "lineages" of philosophers (although I don't think he was aware enough of the Casanova problem to take into account the bias making the works of solo philosophers less likely to survive). For an illustration of the aggressiveness of groups, see Uglow (2003).

Hyman Minsky's work: Minsky (1982).

Asymmetry: Prospect theory (Kahneman and Tversky [1979] and Tversky and Kahneman [1992]) accounts for the asymmetry between bad and good random events, but it also shows that the negative domain is convex while the positive domain is concave, meaning that a loss of 100 is less painful than 100 losses of 1 but that a gain of 100 is also far less pleasurable than 100 times a gain of 1.

Neural correlates of the asymmetry: See Davidson's work in Goleman (2003), Lane *et al.* (1997), and Gehring and Willoughby (2002). Csikszentmihalyi (1993, 1998) further explains the attractiveness of steady payoffs with his theory of "flow."

Deferred rewards and its neural correlates: McLure *et al.* (2004) show the brain activation in the cortex upon making a decision to defer, providing insight on the limbic impulse behind immediacy and the cortical activity in delaying. See also Loewenstein *et al.* (1992), Elster (1998), Berridge (2005). For the neurology of preferences in Capuchin monkeys, Chen *et al.*

(2005).

Bleed or blowup: Gladwell (2002) and Taleb (2004c). Why bleed is painful can be explained by dull stress; Sapolsky *et al.* (2003) and Sapolsky (1998). For how companies like steady returns, Degeorge and Zeckhauser (1999). Poetics of hope: Mihailescu (2006).

Discontinuities and jumps: Classified by René Thom as constituting seven classes; Thom (1980).

Evolution and small probabilities: Consider also the naïve evolutionary thinking positing the "optimality" of selection. The founder of sociobiology, the great E. O. Wilson, does not agree with such optimality when it comes to rare events. In Wilson (2002), he writes:

> The human brain evidently evolved to commit itself emotionally only to a small piece of geography, a limited band of kinsmen, and two or three generations into the future. To look neither far ahead nor far afield is elemental in a Darwinian sense. *We are innately inclined to ignore any distant possibility not yet requiring examination. It is, people say, just good common sense*. Why do they think in this shortsighted way?
>
> The reason is simple: it is a hardwired part of our Paleolithic heritage. For hundreds of millennia, those who worked for short-term gain within a small circle of relatives and friends lived longer and left more offspring—even when their collective striving caused their chiefdoms and empires to crumble around them. The long view that might have saved their distant descendants required a vision and extended altruism instinctively difficult to marshal.

See also Miller (2000): *"Evolution has no foresight. It lacks the long-term vision of drug company management. A species can't raise venture capital to pay its bills while its research team … This makes it hard to explain innovations."*

Note that neither author considered my age argument.

CHAPTER 8

Silent evidence bears the name *wrong reference class* in the nasty field of philosophy of probability, *anthropic bias* in physics, and *survivorship bias* in statistics (economists present the interesting attribute of having rediscovered it a few times while being severely fooled by it).

Confirmation: Bacon says in *On Truth*, "No pleasure is comparable to the standing upon the vantage ground of truth (a hill not to be commanded and where the air is always clear and serene), and to see the errors, and wanderings, and mists, and tempests, in the vale below." This easily shows how great intentions can lead to the confirmation fallacy.

Bacon did not understand the empiricists: He was looking for the golden mean. Again, from *On Truth*:

> There are three sources of error and three species of false philosophy; the sophistic, the empiric and the superstitious. … Aristotle affords the most eminent instance of the first; for he corrupted natural philosophy by logic—thus he formed the world of categories. … Nor is much stress to be laid on his frequent recourse to experiment in his books on animals, his problems and other treatises, for he had already decided, without having properly consulted experience as the basis of his decisions and axioms. … The empiric school produces dogmas of a more deformed and monstrous nature than the sophistic or theoretic school; not being founded in the light of common notions (which however poor and superstitious, is yet in a manner universal and of general tendency), but in the confined obscurity of a few experiments.

Bacon's misconception may be the reason it took us a while to understand that they treated history (and experiments) as mere and vague "guidance," i.e., epilogy.

Publishing: Allen (2005), Klebanoff (2002), Epstein (2001), de Bellaigue (2004), and Blake (1999). For a funny list of rejections, see Bernard (2002) and White (1982). Michael Korda's memoir, Korda (2000), adds some color to the business. These books are anecdotal, but we will see later that books follow steep scale-invariant structures with the implication of a severe role for randomness.

Anthropic bias: See the wonderful and comprehensive discussion in Bostrom (2002). In physics, see Barrow and Tipler (1986) and Rees (2004). Sornette (2004) has Gott's derivation of survival as a power law. In finance, Sullivan *et al.* (1999) discuss survivorship bias. See also Taleb (2004a). Studies that ignore the bias and state inappropriate conclusions: Stanley and Danko

(1996) and the more foolish Stanley (2000).

Manuscripts and the Phoenicians: For survival and science, see Cisne (2005). Note that the article takes into account physical survival (like fossil), not cultural, which implies a selection bias. Courtesy Peter Bevelin.

Stigler's law of eponymy: Stigler (2002).

French book statistics: *Lire*, April 2005.

Why dispersion matters: More technically, the distribution of the extremum (i.e., the maximum or minimum) of a random variable depends more on the variance of the process than on its mean. Someone whose weight tends to fluctuate a lot is more likely to show you a picture of himself very thin than someone else whose weight is on average lower but remains constant. The mean (read skills) sometimes plays a very, very small role.

Fossil record: I thank the reader Frederick Colbourne for his comments on this subject. The literature calls it the "pull of the recent," but has difficulty estimating the effects, owing to disagreements. See Jablonski *et al.* (2003).

Undiscovered public knowledge: Here is another manifestation of silent evidence: you can actually do lab work sitting in an armchair, just by linking bits and pieces of research by people who labor apart from one another and miss on connections. Using bibliographic analysis, it is possible to find links between published information that had not been known previously by researchers. I "discovered" the vindication of the armchair in Fuller (2005). For other interesting discoveries, see Spasser (1997) and Swanson (1986a, 1986b, 1987).

Crime: The definition of economic "crime" is something that comes in hindsight. Regulations, once enacted, do not run retrospectively, so many activities causing excess are never sanctioned (e.g., bribery).

Bastiat: See Bastiat (1862–1864).

Casanova: I thank the reader Milo Jones for pointing out to me the exact number of volumes. See Masters (1969).

Reference point problem: Taking into account background information requires a form of thinking in *conditional* terms that, oddly, many scientists (especially the better ones) are incapable of handling. The difference between the two odds is called, simply, conditional probability. We are

computing the probability of surviving *conditional* on our being in the sample itself. Simply put, you cannot compute probabilities if your survival is part of the condition of the realization of the process.

Plagues: See McNeill (1976).

CHAPTER 9

Intelligence and Nobel: Simonton (1999). If IQ scores correlate, they do so very weakly with subsequent success.

"Uncertainty": Knight (1923). My definition of such risk (Taleb, 2007c) is that it is a normative situation, where we can be certain about probabilities, i.e., no metaprobabilities. Whereas, if randomness and risk result from epistemic opacity, the difficulty in seeing causes, then necessarily the distinction is bunk. Any reader of Cicero would recognize it as his probability; see epistemic opacity in his *De Divinatione*, Liber primus, LVI, 127:

> Qui enim teneat causas rerum futurarum, idem necesse est omnia teneat quae futura sint. Quod cum nemo facere nisi deus possit, relinquendum est homini, ut signis quibusdam consequentia declarantibus futura praesentiat.

"He who knows the causes will understand the future, except that, given that nobody outside God possesses such faculty …"

Philosophy and epistemology of probability: Laplace. *Treatise*, Keynes (1920), de Finetti (1931), Kyburg (1983), Levi (1970), Ayer, Hacking (1990, 2001), Gillies (2000), von Mises (1928), von Plato (1994), Carnap (1950), Cohen (1989), Popper (1971), Eatwell *et al.* (1987), and Gigerenzer *et al.* (1989).

History of statistical knowledge and methods: I found no intelligent work in the history of statistics, i.e., one that does not fall prey to the ludic fallacy or Gaussianism. For a conventional account, see Bernstein (1996) and David (1962).

General books on probability and information theory: Cover and Thomas (1991); less technical but excellent, Bayer (2003). For a probabilistic view of information theory: the posthumous Jaynes (2003) is the only mathematical book other than de Finetti's work that I can recommend to the general reader, owing to his Bayesian approach and his allergy for the formalism of the idiot savant.

Poker: It escapes the ludic fallacy; see Taleb (2006a).

Plato's normative approach to left and right hands: See McManus (2002).

Nietzsche's *bildungsphilister*: See van Tongeren (2002) and Hicks and Rosenberg (2003). Note that because of the confirmation bias academics will tell you that intellectuals "lack rigor," and will bring examples of those who do, not those who don't.

Economics books that deal with uncertainty: Carter *et al.* (1962), Shackle (1961, 1973), Hayek (1994). Hirshleifer and Riley (1992) fits uncertainty into neoclassical economics.

Incomputability: For earthquakes, see Freedman and Stark (2003) (courtesy of Gur Huberman).

Academia and philistinism: There is a round-trip fallacy; if academia means rigor (which I doubt, since what I saw called "peer reviewing" is too often a masquerade), nonacademic does not imply nonrigorous. Why do I doubt the "rigor"? By the confirmation bias they show you their contributions yet in spite of the high number of laboring academics, a relatively minute fraction of our results come from them. A disproportionately high number of contributions come from freelance researchers and those dissingly called amateurs: Darwin, Freud, Marx, Mandelbrot, even the early Einstein. Influence on the part of an academic is usually accidental. This even held in the Middle Ages and the Renaissance, see Le Goff (1985). Also, the Enlightenment figures (Voltaire, Rousseau, d'Holbach, Diderot, Montesquieu) were all nonacademics at a time when academia was large.

CHAPTER 10

Overconfidence: Albert and Raiffa (1982) (though apparently the paper languished for a decade before formal publication). Lichtenstein and Fischhoff (1977) showed that overconfidence can be influenced by item difficulty; it typically diminishes and turns into underconfidence in easy items (compare with Armelius [1979]). Plenty of papers since have tried to pin down the conditions of calibration failures or robustness (be they task training, ecological aspects of the domain, level of education, or nationality): Dawes (1980), Koriat, Lichtenstein, and Fischhoff (1980), Mayseless and Kruglanski (1987), Dunning *et al.* (1990), Ayton and McClelland (1997), Gervais and Odean (1999), Griffin and Varey (1996), Juslin (1991, 1993, 1994), Juslin and Olsson (1997), Kadane and Lichtenstein (1982), May (1986), McClelland and Bolger (1994), Pfeifer (1994), Russo and Schoernaker (1992), Klayman *et al.* (1999). Note the decrease (unexpectedly) in overconfidence under group decisions: see Sniezek and Henry (1989)—and solutions in Plous (1995). I am suspicious here of the Mediocristan/Extremistan distinction and the unevenness of the variables. Alas, I found no paper making this distinction. There are also solutions in Stoll (1996), Arkes *et al.* (1987). For overconfidence in finance, see Thorley (1999) and Barber and Odean (1999). For cross-boundaries effects, Yates *et al.* (1996, 1998), Angele *et al.* (1982). For simultaneous overconfidence and underconfidence, see Erev, Wallsten, and Budescu (1994).

Frequency vs. probability—the ecological problem: Hoffrage and Gigerenzer (1998) think that overconfidence is less significant when the problem is expressed in frequencies as opposed to probabilities. In fact, there has been a debate about the difference between "ecology" and laboratory; see Gigerenzer *et al.* (2000), Gigerenzer and Richter (1990), and Gigerenzer (1991). We are "fast and frugal" (Gigerenzer and Goldstein [1996]). As far as the Black Swan is concerned, these problems of ecology do not arise: we do not live in an environment in which we are supplied with frequencies or, more generally, for which we are fit. Also in ecology, Spariosu (2004) for the ludic aspect, Cosmides and Tooby (1990). Leary (1987) for Brunswikian ideas, as well as Brunswik (1952).

Lack of awareness of ignorance: "In short, the same knowledge that underlies the ability to produce correct judgment is also the knowledge that underlies the ability to recognize correct judgment. To lack the former is to be deficient in the latter." From Kruger and Dunning (1999).

Expert problem in isolation: I see the expert problem as indistinguishable from Matthew effects and Extremism fat tails (more later), yet I found no such link in the literatures of sociology and psychology.

Clinical knowledge and its problems: See Meehl (1954) and Dawes, Faust, and Meehl (1989). Most entertaining is the essay "Why I Do Not Attend Case Conferences" in Meehl (1973). See also Wagenaar and Keren (1985, 1986).

Financial analysts, herding, and forecasting: See Guedj and Bouchaud (2006), Abarbanell and Bernard (1992), Chen *et al.* (2002), De Bondt and Thaler (1990), Easterwood and Nutt (1999), Friesen and Weller (2002), Foster (1977), Hong and Kubik (2003), Jacob *et al.* (1999), Lim (2001), Liu (1998), Maines and Hand (1996), Mendenhall (1991), Mikhail *et al.* (1997, 1999), Zitzewitz (2001), and El-Galfy and Forbes (2005). For a comparison with weather forecasters (unfavorable): Tyszka and Zielonka (2002).

Economists and forecasting: Tetlock (2005), Makridakis and Hibon (2000), Makridakis *et al.* (1982), Makridakis *et al.* (1993), Gripaios (1994), Armstrong (1978, 1981); and rebuttals by McNees (1978), Tashman (2000), Blake *et al.* (1986), Onkal *et al.* (2003), Gillespie (1979), Baron (2004), Batchelor (1990, 2001), Dominitz and Grether (1999). Lamont (2002) looks for reputational factors: established forecasters get worse as they produce more radical forecasts to get attention—consistent with Tetlock's hedgehog effect. Ahiya and Doi (2001) look for herd behavior in Japan. See McNees (1995), Remus *et al.* (1997), O'Neill and Desai (2005), Bewley and Fiebig (2002), Angner (2006), Bénassy-Quéré (2002); Brender and Pisani (2001) look at the Bloomberg consensus; De Bondt and Kappler (2004) claim evidence of weak persistence in fifty-two years of data, but I saw the slides in a presentation, never the paper, which after two years might never materialize. Overconfidence, Braun and Yaniv (1992). See Hahn (1993) for a general intellectual discussion. More general, Clemen (1986, 1989). For Game theory, Green (2005).

Many operators, such as James Montier, and many newspapers and

magazines (such as *The Economist)*, run casual tests of prediction. Cumulatively, they must be taken seriously since they cover more variables.

Popular culture: In 1931, Edward Angly exposed forecasts made by President Hoover in a book titled *Oh Yeah?* Another hilarious book is Cerf and Navasky (1998), where, incidentally, I got the pre-1973 oil-estimation story.

Effects of information: The major paper is Bruner and Potter (1964). I thank Danny Kahneman for discussions and pointing out this paper to me. See also Montier (2007), Oskamp (1965), and Benartzi (2001). These biases become ambiguous information (Griffin and Tversky [1992]). For how they fail to disappear with expertise and training, see Kahneman and Tversky (1982) and Tversky and Kahneman (1982). See Kunda (1990) for how preference-consistent information is taken at face value, while preference-inconsistent information is processed critically.

Planning fallacy: Kahneman and Tversky (1979) and Buehler, Griffin, and Ross (2002). The planning fallacy shows a consistent bias in people's planning ability, even with matters of a repeatable nature—though it is more exaggerated with nonrepeatable events.

Wars: Trivers (2002).

Are there incentives to delay?: Flyvbjerg *et al.* (2002).

Oskamp: Oskamp (1965) and Montier (2007).

Task characteristics and effect on decision making: Shanteau (1992).

Epistēmē* vs. *Technē: This distinction harks back to Aristotle, but it recurs then dies? down—it most recently recurs in accounts such as tacit knowledge in "know how." See Ryle (1949), Polanyi (1958/1974), and Mokyr (2002).

Catherine the Great: The number of lovers comes from Rounding (2006).

Life expectancy: www.annuityadvantage.com/lifeexpectancy.htm. For projects, I have used a probability of exceeding with a power-law exponent of 3/2: f= $Kx^{3/2}$. Thus the conditional expectation of x, knowing that x exceeds a

$$\mathrm{E}[x|x>a]=\frac{\int_a^{\infty} x f(x)dx}{\int_a^{\infty} f(x)dx}.$$

CHAPTERS 11–13

Serendipity: See Koestler (1959) and Rees (2004). Rees also has powerful ideas on fore-castability. See also Popper's comments in Popper (2002), and Waller (2002a), Cannon (1940), Mach (1896) (cited in Simonton [1999]), and Merton and Barber (2004). See Simonton (2004) for a synthesis. For serendipity in medicine and anesthesiology, see Vale *et al.* (2005).

"Renaissance man": See www.bell-labs.com/project/feature/archives/cosmology/.

Laser: As usual, there are controversies as to who "invented" the technology. After a successful discovery, precursors are rapidly found, owing to the retrospective distortion. Charles Townsend won the Nobel prize, but was sued by his student Gordon Gould, who held that he did the actual work (see *The Economist*, June 9, 2005).

Darwin/Wallace: Quammen (2006).

Popper's attack on historicism: See Popper (2002). Note that I am reinterpreting Popper's idea in a modern manner here, using my own experiences and knowledge, not commenting on comments about Popper's work—with the consequent lack of fidelity to his message. In other words, these are not directly Popper's arguments, but largely mine phrased in a Popperian framework. The conditional expectation of an unconditional expectation is an unconditional expectation.

Forecast for the future a hundred years earlier: Bellamy (1891) illustrates our mental projections of the future. However, some stories might be exaggerated: "A Patently False Patent Myth still! Did a patent official really once resign because he thought nothing was left to invent? Once such myths start they take on a life of their own." *Skeptical Inquirer*, May–June, 2003.

Observation by Peirce: Olsson (2006), Peirce (1955).

Predicting and explaining: See Thom (1993).

Poincaré: The three body problem can be found in Barrow-Green (1996), Rollet (2005), and Galison (2003). On Einstein, Pais (1982). More recent

revelations in Hladik (2004).

Billiard balls: Berry (1978) and Pisarenko and Sornette (2004).

Very general discussion on "complexity": Benkirane (2002), Scheps (1996), and Ruelle (1991). For limits, Barrow (1998).

Hayek: See www.nobel.se. See Hayek (1945, 1994). Is it that mechanisms do not correct themselves from railing by influential people, but either by mortality of the operators, or something even more severe, by being put out of business? Alas, because of contagion, there seems to be little logic to how matters improve; luck plays a part in how soft sciences evolve. See Ormerod (2006) for network effects in "intellectuals and socialism" and the power-law distribution in influence owing to the scale-free aspect of the connections—and the consequential arbitrariness. Hayek seems to have been a prisoner of Weber's old differentiation between *Natur-Wissenschaften* and *Geistes Wissenschaften*—but thankfully not Popper.

Insularity of economists: Pieters and Baumgartner (2002). One good aspect of the insularity of economists is that they can insult me all they want without any consequence: it appears that only economists read other economists (so they can write papers for other economists to read). For a more general case, see Wallerstein (1999). Note that Braudel fought "economic history." It was history.

Economics as religion: Nelson (2001) and Keen (2001). For methodology, see Blaug (1992). For high priests and lowly philosophers, see Boettke, Coyne, and Leeson (2006). Note that the works of Gary Becker and the Platonists of the Chicago School are all marred by the confirmation bias: Becker is quick to show you situations in which people are moved by economic incentives, but does not show you cases (vastly more numerous) in which people don't care about such materialistic incentives.

The smartest book I've seen in economics is Gave *et al.* (2005) since it transcends the constructed categories in academic economic discourse (one of the authors is the journalist Anatole Kaletsky).

General theory: This fact has not deterred "general theorists." One hotshot of the Platonifying variety explained to me during a long plane ride from Geneva to New York that the ideas of Kahneman and his colleagues must be rejected because they do not allow us to develop a general equilibrium

theory, producing “time-inconsistent preferences.” For a minute I thought he was joking: he blamed the psychologists’ ideas and human incoherence for interfering with his ability to build his Platonic model.

Samuelson: For his optimization, see Samuelson (1983). Also Stiglitz (1994).

Plato’s dogma on body symmetry: “Athenian Stranger to Cleinias: In that the right and left hand are supposed to be by nature differently suited for our various uses of them; whereas no difference is found in the use of the feet and the lower limbs; but in the use of the hands we are, as it were, maimed by the folly of nurses and mothers; for although our several limbs are by nature balanced, we create a difference in them by bad habit,” in Plato’s *Laws*. See McManus (2002).

Drug companies: Other such firms, I was told, are run by commercial persons who tell researchers where they find a “market need” and ask them to “invent” drugs and cures accordingly—which accords with the methods of the dangerously misleading Wall Street security analysts. They formulate projections as if they know what they are going to find.

Models of the returns on innovations: Sornette and Zajdenweber (1999) and Silverberg and Verspagen (2005).

Evolution on a short leash: Dennet (2003) and Stanovich and West (2000).

Montaigne: We don’t get much from the biographies of a personal essayist; some information in Frame (1965) and Zweig (1960).

Projectibility and the grue paradox: See Goodman (1955). See also an application (or perhaps misapplication) in King and Zheng (2005).

Constructionism: See Berger and Luckmann (1966) and Hacking (1999).

Certification vs, true skills or knowledge: See Donhardt (2004). There is also a franchise protection. Mathematics may not be so necessary a tool for economics, except to protect the franchise of those economists who know math. In my father’s days, the selection process for the mandarins was made using their abilities in Latin (or Greek). So the class of students groomed for the top was grounded in the classics and knew some interesting subjects. They were also trained in Cicero’s highly probabilistic view of things—and selected on erudition, which carries small side effects. If anything it allows you to handle fuzzy matters. My generation was

selected according to mathematical skills. You made it based on an engineering mentality; this produced mandarins with mathematical, highly structured, logical minds, and, accordingly, they will select their peers based on such criteria. So the papers in economics and social science gravitated toward the highly mathematical and protected their franchise by putting high mathematical barriers to entry. You could also smoke the general public who is unable to put a check on you. Another effect of this franchise protection is that it might have encouraged putting "at the top" those idiot-savant-like researchers who lacked in erudition, hence were insular, parochial, and closed to other disciplines.

Freedom and determinism: a speculative idea in Penrose (1989) where only the quantum effects (with the perceived indeterminacy there) can justify consciousness.

Projectibility: uniqueness assuming least squares or MAD.

Chaos theory and the backward/forward confusion: Laurent Firode's *Happenstance*, a.k.a. *Le battement d'ailes du papillon / The Beating of a Butterfly's Wings* (2000).

Autism and perception of randomness: See Williams *et al.* (2002).

Forecasting and misforecasting errors in hedonic states: Wilson, Meyers, and Gilbert (2001), Wilson, Gilbert, and Centerbar (2003), and Wilson *et al.* (2005). They call it "emotional evanescence."

Forecasting and consciousness: See the idea of "aboutness" in Dennett (1995, 2003) and Humphrey (1992). However, Gilbert (2006) believes that we are not the only animal that forecasts—which is wrong as it turned out. Suddendorf (2006) and Dally, Emery, and Clayton (2006) show that animals too forecast!

Russell's comment on Pascal's wager: Ayer (1988) reports this as a private communication.

History: Carr (1961), Hexter (1979), and Gaddis (2002). But I have trouble with historians throughout, because they often mistake the forward and the backward processes. Mark Buchanan's *Ubiquity* and the quite confused discussion by Niall Ferguson in *Nature*. Neither of them seem to realize the problem of calibration with power laws. See also Ferguson, *Why Did the Great War?*, to gauge the extent of the forward-backward problems.

For the traditional nomological tendency, i.e., the attempt to go beyond cause into a general theory, see *Muqaddamah* by Ibn Khaldoun. See also Hegel's *Philosophy of History*.

Emotion and cognition: Zajonc (1980, 1984).

Catastrophe insurance: Froot (2001) claims that insurance for remote events is overpriced. How he determined this remains unclear (perhaps by backfitting or bootstraps), but reinsurance companies have not been making a penny selling "overpriced" insurance.

Postmodernists: Postmodernists do not seem to be aware of the differences between narrative and prediction.

Luck and serendipity in medicine: Vale *et al.* (2005). In history, see Cooper (2004). See also Ruffié (1977). More general, see Roberts (1989).

Affective forecasting: See Gilbert (1991), Gilbert *et al.* (1993), and Montier (2007).

CHAPTERS 14–17

This section will also serve another purpose. Whenever I talk about the Black Swan, people tend to supply me with anecdotes. But these anecdotes are just corroborative: you need to show that *in the aggregate* the world is dominated by Black Swan events. To me, the rejection of nonscalable randomness is sufficient to establish the role and significance of Black Swans.

Matthew effects: See Merton (1968, 1973a, 1988). Martial, in his *Epigrams: "Semper pauper eris, si pauper es, Aemiliane./Dantur opes nullis (nunc) nisi divitibus."* (Epigr. V 81). See also Zuckerman (1997, 1998).

Cumulative advantage and its consequences on social fairness: review in DiPrete *et al.* (2006). See also Brookes-Gun and Duncan (1994), Broughton and Mills (1980), Dannefer (2003), Donhardt (2004), Hannon (2003), and Huber (1998). For how it may explain precocity, see Elman and O'Rand (2004).

Concentration and fairness in intellectual careers: Cole and Cole (1973), Cole (1970), Conley (1999), Faia (1975), Seglen (1992), Redner (1998), Lotka (1926), Fox and Kochanowski (2004), and Huber (2002).

Winner take all: Rosen (1981), Frank (1994), Frank and Cook (1995), and Attewell (2001).

Arts: Bourdieu (1996), Taleb (2004e).

Wars: War is concentrated in an Extremistan manner: Lewis Fry Richardson noted last century the uneveness in the distribution of casualties (Richardson [1960]).

Modern wars: Arkush and Allen (2006). In the study of the Maori, the pattern of fighting with clubs was sustainable for many centuries—modern tools cause 20,000 to 50,000 deaths a year. We are simply not made for technical warfare. For an anecdotal and causative account of the history of a war, see Ferguson (2006).

S&P 500: See Rosenzweig (2006).

The long tail: Anderson (2006).

Cognitive diversity: See Page (2007). For the effect of the Internet on

schools, see Han *et al.* (2006).

Cascades: See Schelling (1971, 1978) and Watts (2002). For information cascades in economics, see Bikhchandani, Hirshleifer, and Welch (1992) and Shiller (1995). See also Surowiecki (2004).

Fairness: Some researchers, like Frank (1999), see arbitrary and random success by others as no different from pollution, which necessitates the enactment of a tax. De Vany, Taleb, and Spitznagel (2004) propose a market-based solution to the problem of allocation through the process of voluntary self-insurance and derivative products. Shiller (2003) proposes cross-country insurance.

The mathematics of preferential attachment: This argument pitted Mandelbrot against the cognitive scientist Herbert Simon, who formalized Zipf's ideas in a 1955 paper (Simon [1955]), which then became known as the Zipf-Simon model. Hey, you need to allow for people to fall from favor!

Concentration: Price (1970). Simon's "Zipf derivation," Simon (1955). More general bibliometrics, see Price (1976) and Glänzel (2003).

Creative destruction revisited: See Schumpeter (1942).

Networks: Barabási and Albert (1999), Albert and Barabási (2000), Strogatz (2001, 2003), Callaway *et al.* (2000), Newman *et al.* (2000), Newman, Watts, and Strogatz (2000), Newman (2001), Watts and Strogatz (1998), Watts (2002, 2003), and Amaral *et al.* (2000). It supposedly started with Milgram (1967). See also Barbour and Reinert (2000), Barthélémy and Amaral (1999). See Boots and Sasaki (1999) for infections. For extensions, see Bhalla and Iyengar (1999). Resilence, Cohen *et al.* (2000), Barabási and Bonabeau (2003), Barabási (2002), and Banavar *et al.* (2000). Power laws and the Web, Adamic and Huberman (1999) and Adamic (1999). Statistics of the Internet: Huberman (2001), Willinger *et al.* (2004), and Faloutsos, Faloutsos, and Faloutsos (1999). For DNA, see Vogelstein *et al.* (2000).

Self-organized criticality: Bak (1996).

Pioneers of fat tails: For wealth, Pareto (1896), Yule (1925, 1944). Less of a pioneer Zipf (1932, 1949). For linguistics, see Mandelbrot (1952).

Pareto: See Bouvier (1999).

Endogenous vs. exogenous: Sornette *et al.* (2004).

Sperber's work: Sperber (1996a, 1996b, 1997).

Regression: If you hear the phrase *least square regression*, you should be suspicious about the claims being made. As it assumes that your errors wash out rather rapidly, it underestimates the total possible error, and thus overestimates what knowledge one can derive from the data.

The notion of central limit: very misunderstood: it takes a long time to reach the central limit—so as we do not live in the asymptote, we've got problems. All various random variables (as we started in the example of Chapter 16 with a +1 or −1, which is called a Bernouilli draw) under summation (we did sum up the wins of the 40 tosses) become Gaussian. Summation is key here, since we are considering the results of adding up the 40 steps, which is where the Gaussian, under the first and second central assumptions becomes what is called a "distribution." (A distribution tells you how you are likely to have your outcomes spread out, or distributed.) However, they may get there at different speeds. This is called the central limit theorem: if you add random variables coming from these individual tame jumps, it will lead to the Gaussian.

Where does the central limit not work? If you do not have these central assumptions, but have jumps of random size instead, then we would not get the Gaussian. Furthermore, we sometimes converge very slowly to the Gaussian. For preasymptotics and scalability, Mandelbrot and Taleb (2007a), Bouchaud and Potters (2003). For the problem of working outside asymptotes, Taleb (2007).

Aureas mediocritas: historical perspective, in Naya and Pouey-Mounou (2005) aptly called *Éloge de la médiocrité*.

Reification (hypostatization): Lukacz, in Bewes (2002).

Catastrophes: Posner (2004).

Concentration and modern economic life: Zajdenweber (2000).

Choices of society structure and compressed outcomes: The classical paper is Rawls (1971), though Frohlich, Oppenheimer, and Eavy (1987a, 1987b), as well as Lissowski, Tyszka, and Okrasa (1991), contradict the notion of the desirability of Rawl's veil (though by experiment). People prefer

maximum average income subjected to a floor constraint on some form of equality for the poor, inequality for the rich type of environment.

Gaussian contagion: Quételet in Stigler (1986). Francis Galton (as quoted in Ian Hacking's *The Taming of Chance)*: "I know of scarcely anything so apt to impress the imagination as the wonderful form of cosmic order expressed by 'the law of error.'"

"Finite variance" nonsense: Associated with CLT is an assumption called "finite variance" that is rather technical: none of these building-block steps can take an infinite value if you square them or multiply them by themselves. They need to be bounded at some number. We simplified here by making them all one single step, or finite standard deviation. But the problem is that some fractal payoffs may have finite variance, but still not take us there rapidly. See Bouchaud and Potters (2003).

Lognormal: There is an intermediate variety that is called the lognormal, emphasized by one Gibrat (see Sutton [1997]) early in the twentieth century as an attempt to explain the distribution of wealth. In this framework, it is not quite that the wealthy get wealthier, in a pure preferential attachment situation, but that if your wealth is at 100 you will vary by 1, but when your wealth is at 1,000, you will vary by 10. The relative changes in your wealth are Gaussian. So the lognormal superficially resembles the fractal, in the sense that it may tolerate some large deviations, but it is dangerous because these rapidly taper off at the end. The introduction of the lognormal was a very bad compromise, but a way to conceal the flaws of the Gaussian.

Extinctions: Sterelny (2001). For extinctions from abrupt fractures, see Courtillot (1995) and Courtillot and Gaudemer (1996). Jumps: Eldredge and Gould.

FRACTALS, POWER LAWS, and SCALE-FREE DISTRIBUTIONS

Definition: Technically, $P>x= K\,x^{-\alpha}$ where α is supposed to be the power-law exponent. It is said to be scale free, in the sense that it does not have a characteristic scale: relative deviation of $\frac{P>x}{P>nx}$ does not depend on x, but on n—for *x* "large enough." Now, in the other class of distribution, the one that I can intuitively describe as nonscalable, with the typical shape p(x) = Exp[-a x], the scale will be a.

Problem of "how large": Now the problem that is usually misunderstood.

This scalability might stop somewhere, but I do not know where, so I might consider it infinite. The statements *very large and I don't know how large* and *infinitely large* are epistemologically substitutable. There might be a point at which the distributions flip. This will show once we look at them more graphically.

Log $P>x = -\alpha$ Log $X + C^t$ for a scalable. When we do a log-log plot (i.e., plot $P>x$ and x on a logarithmic scale), as in Figures 15 and 16, we should see a straight line.

Fractals and power laws: Mandelbrot (1975, 1982). Schroeder (1991) is imperative. John Chipman's unpublished manuscript *The Paretian Heritage* (Chipman [2006]) is the best review piece I've seen. See also Mitzenmacher (2003).

"To come very near true theory and to grasp its precise application are two very different things as the history of science teaches us. Everything of importance has been said before by somebody who did not discover it." Whitehead (1925).

Fractals in poetry: For the quote on Dickinson, see Fulton (1998).

Lacunarity: Brockman (2005). In the arts, Mandelbrot (1982).

Fractals in medicine: "New Tool to Diagnose and Treat Breast Cancer," *Newswise*, July 18, 2006.

General reference books in statistical physics: The most complete (in relation to fat tails) is Sornette (2004). See also Voit (2001) or the far deeper Bouchaud and Potters (2002) for financial prices and econophysics. For "complexity" theory, technical books: Bocarra (2004), Strogatz (1994), the popular Ruelle (1991), and also Prigogine (1996).

Fitting processes: For the philosophy of the problem, Taleb and Pilpel (2004). See also Pisarenko and Sornette (2004), Sornette *et al.* (2004), and Sornette and Ide (2001).

Poisson jump: Sometimes people propose a Gaussian distribution with a small probability of a "Poisson" jump. This may be fine, but how do you know how large the jump is going to be? Past data might not tell you how large the jump is.

Small sample effect: Weron (2001). Officer (1972) is quite ignorant of the

point.

Recursivity of statistics: Taleb and Pilpel (2004), Blyth *et al.* (2005).

Biology: Modern molecular biology pioneers Salvador Luria and Max Delbrück witnessed a clustering phenomenon with the occasional occurrence of extremely large mutants in a bacterial colony, larger than all other bacteria.

Thermodynamics: Entropy maximization without the constraints of a second moment The two exhaustive domains of attraction: vertical or straight line with slopes either negative infinity or constant negative α. Note that since probabilities need to add up to 1 (even in France) there cannot be other alternatives to the two basins, which is why I narrow it down to these two exclusively.

FIGURE 15: TYPICAL DISTRIBUTION WITH POWER-LAW TAILS (HERE A STUDENT T)

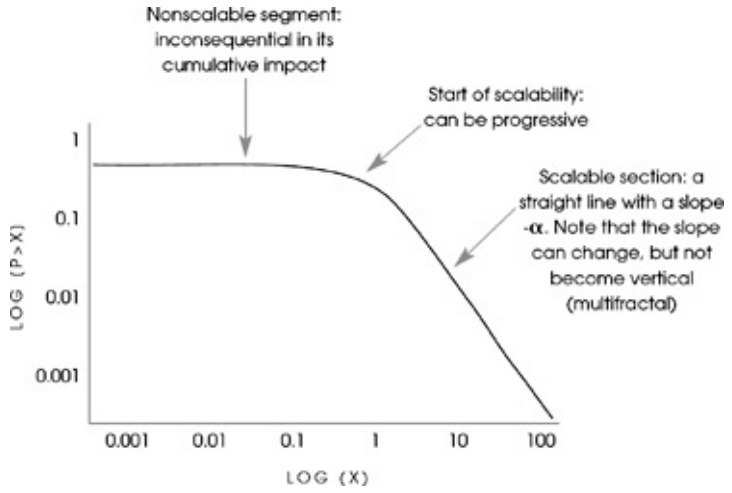
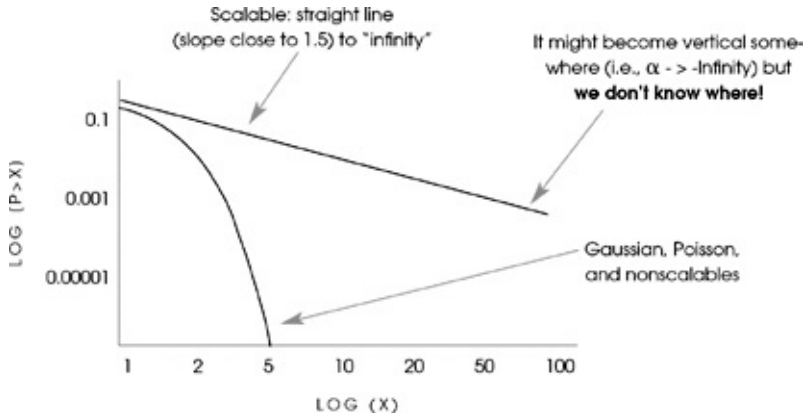


FIGURE 16



My ideas are made very simple with this clean cut polarization—added to the problem of not knowing which basin we are in owing to the scarcity of data on the far right. leads to a Levy-stable distribution—Mandelbrot's thesis of 1952 (see Mandelbrot [1997a]). Tsallis's more sophisticated view of entropy leads to a Student T.

Imitation chains and pathologies: An informational cascade is a process where a purely rational agent elects a particular choice ignoring his own private information (or judgment) to follow that of others. You run, I follow you, because you may be aware of a danger I may be missing. It is efficient to do what others do instead of having to reinvent the wheel every time. But this copying the behavior of others can lead to imitation chains. Soon everyone is running in the same direction, and it can be for spurious reasons. This behavior causes stock market bubbles and the formation of massive cultural fads. Bikhchandani *et al.* (1992). In psychology, see Hansen and Donoghue (1977). In biology/selection, Dugatkin (2001), Kirpatrick and Dugatkin (1994).

Self-organized criticality: Bak and Chen (1991), Bak (1996).

Economic variables: Bundt and Murphy (2006). Most economic variables seem to follow a "stable" distribution. They include foreign exchange, the GDP, the money supply, interest rates (long and short term), and industrial production.

Statisticians not accepting scalability: Flawed reasoning mistaking for sampling error in the tails for a boundedness: Perline (2005), for instance, does not understand the difference between absence of evidence and evidence of absence.

Time series and memory: You can have “fractal memory,” i.e., the effect of past events on the present has an impact that has a “tail.” It decays as power-law, not exponentially.

Marmott’s work: Marmott (2004).

CHAPTER 18

Economists: Weintraub (2002), Szenberg (1992).

Portfolio theory and modern finance: Markowitz (1952, 1959), Huang and Litzenberger (1988) and Sharpe (1994, 1996). What is called the Sharpe ratio is meaningless outside of Mediocristan. The contents of Steve Ross's book (Ross [2004]) on "neoclassical finance" are completely canceled if you consider Extremistan in spite of the "elegant" mathematics and the beautiful top-down theories. "Anecdote" of Merton minor in Merton (1992).

Obsession with measurement: Crosby (1997) is often shown to me as convincing evidence that measuring was a great accomplishment not knowing that it applied to Mediocristan and Mediocristan only. Bernstein (1996) makes the same error.

Power laws in finance: Mandelbrot (1963), Gabaix *et al.* (2003), and Stanley *et al.* (2000). Kaizoji and Kaizoji (2004), Véhel and Walter (2002). Land prices: Kaizoji (2003). Magisterial: Bouchaud and Potters (2003).

Equity premium puzzle: If you accept fat tails, there is no equity premium puzzle. Benartzi and Thaler (1995) offer a psychological explanation, not realizing that variance is not the measure. So do many others.

Covered writes: a sucker's game as you cut your upside—conditional on the upside being breached, the stock should rally a lot more than intuitively accepted. For a representative mistake, see Board *et al.* (2000).

Nobel family: "Nobel Descendant Slams Economics Prize," *The Local*, September 28, 2005, Stockholm.

Double bubble: The problem of derivatives is that if the underlying security has mild fat tails and follows a mild power law (i.e., a tail exponent of three or higher), the derivative will produce far fatter tails (if the payoff is in squares, then the tail exponent of the derivatives portfolio will be half that of the primitive). This makes the Black-Scholes-Merton equation twice as unfit!

Poisson busting: The best way to figure out the problems of the Poisson as a substitute for a scalable is to calibrate a Poisson and compute the errors out

of sample. The same applies to methods such as GARCH—they fare well in sample, but horribly, horribly outside (even a trailing three-month past historical volatility or mean deviation will outperform a GARCH of higher orders).

Why the Nobel: Derman and Taleb (2005), Haug (2007).

Claude Bernard and experimental medicine: *"Empiricism pour le présent, avec direction a aspiration scientifique pour l'avenir."* From Claude Bernard, *Principe de la médecine expérimentale*. See also Fagot-Largeault (2002) and Ruffié (1977). Modern evidence-based medicine: Ierodiakonou and Vandenbroucke (1993) and Vandenbroucke (1996) discuss a stochastic approach to medicine.

CHAPTER 19

Popper quote: From *Conjectures and Refutations*, pages 95–97.

The lottery paradox: This is one example of scholars not understanding the high-impact rare event. There is a well-known philosophical conundrum called the "lottery paradox," originally posed by the logician Henry Kyburg (see Rescher [2001] and Clark [2002]), which goes as follows: "I do not believe that any ticket will win the lottery, but I do believe that all tickets will win the lottery." To me (and a regular person) this statement does not seem to have anything strange in it. Yet for an academic philosopher trained in classical logic, this is a paradox. But it is only so if one tries to squeeze probability statements into commonly used logic that dates from Aristotle and is *all or nothing*. An *all or nothing* acceptance and rejection ("I believe" or "I do not believe") is inadequate with the highly improbable. We need shades of belief, degrees of faith you have in a statement other than 100% or 0%.

One final philosophical consideration. For my friend the options trader and Talmudic scholar Rabbi Tony Glickman: life is convex and to be seen as a series of derivatives. Simply put, when you cut the negative exposure, you limit your vulnerability to unknowledge, Taleb (2005).

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