Risk managers rely on ineffective and illogical techniques. That’s one of the main claims Nassim Nicholas Taleb makes in his controversial new book, *The Black Swan*. Specifically, Taleb argues that quants focus on statistical projection from common, ordinary events when unexpected, low-probability, high-impact events are what determine most profit and loss. He claims that when quants do consider the tails of the distribution, they overestimate the chance of past shocks repeating and ignore the possibility of unprecedented events.

Do his criticisms make sense or are they off base? The following multifaceted article comprises a pair of book reviews (one authored by Aaron Brown and another written by Philippe Jorion), an in-depth interview with Mr. Taleb and a note from the author that further explains his views on risk management.

**The Black Swan: The Impact of the Highly Improbable**

*Author:* Nassim Nicholas Taleb  
*Publisher:* Random House  

**Critique #1**  
*By Aaron Brown*

Most people don’t think about risk. If it’s not raining, they don’t carry umbrellas. They get in their cars without walking around to check the tires and for hidden obstructions. When things run smoothly, they focus attention elsewhere. “If it ain’t broke, don’t fix it,” is their motto. When things do go wrong, it’s “who could have imagined...?”

Other people always carry umbrellas, don’t drive for fear of accidents and worry no matter how well things are going. They’re not thinking about risk either. They’re merely obsessing about bad things. In the movie *For Love of the Game*, actor J. K. Simmons’s character chides actor Kevin Costner’s character for being late. Kevin’s character responds, “In 19 years, have I ever not showed?” J. K.’s character has the unarguable answer of worrywarts, “Well, that’s true of everyone until the first time they don’t show.”

The first kind of person sometimes slips into the risk management profession and manages the appearance of risk. The second kind sometimes slips in and minimizes risk. But both are opposed to the goals of the profession: transparent reporting of risk and aggressive support for calculated risk taking.

**Reasoning from Big and Small**

We have two methods for investigating risk. First is analysis of historical disasters: market crashes, hedge fund blow-ups, financial institution scandals, etc. Second is analysis of common events — for example, using the distribution of daily price movements to predict future movements.

Nassim Taleb’s new book, *The Black Swan*, attacks both of these methods. Taleb argues that historical analysis of rare events is poisoned by the narrative fallacy and that reasoning from everyday experience is an inverse problem.
Taleb defines the narrative fallacy as follows: “We like stories; we like to summarize; and we like to simplify, i.e., reduce the dimension of matters … It is actually a fraud, but to be more polite, I will call it a fallacy. The fallacy is associated with our vulnerability to overinterpretation and our predilection for compact stories over raw truths. It severely distorts our view of the world; it is particularly acute when it comes to the rare event.”

I agree with Taleb’s belief that out of a complex world, we abstract patterns small enough to squeeze into our brains. We know the outcome, and remember events that appear to play a causal role in it. We forget the rest to have a simple, plausible story. We remember a predictable, rational sequence of events when the reality was unpredictable, messy and happened in too many dimensions to define a meaningful sequence. Taleb discusses this tendency with insights from psychology, neuropsychology, literature (serious and popular), history, philosophy and logic.

Risk managers who commit the narrative fallacy confidently recommend safeguards that only make sense in a low-dimensional, storybook world. When Korean steel-maker Hanbo defaulted in January 1997, no one would have linked its fate to Kia, then to East Asian currencies, then to Russia, Brazil, Long-Term Capital Management, the price of oil, worldwide real estate, US interest rates and stock prices — or to any of the other consequences of what, in retrospect, is called the 1997 Asian financial crisis. Many overlapping events affected each other with positive and negative reinforcement. It’s plausible that a minor difference in one event would have changed outcomes completely or that the consequences were predetermined and only the paths to get there were affected by randomness.

What’s not plausible is that events are well described by any simple chain of cause-and-effect — yet the myriad explanations published by contending researchers all fall into this category. These types of explanations are fine for economists and policy analysts, but risk managers must focus on the uncertainty, not the illusion of simple certainty.

Taleb claims that this is impossible. Maybe so. But he also implies that we don’t realize the problem, that we don’t try. That’s not true. The good risk managers I know savor the texture of the past in as much detail as they can get. They distrust pat explanations and confident people. They try to absorb many views — not to distill some essence of truth, but in the hope some faint clue from some account will help them steer better in the future.

I’m a pretty quantitative guy, but I base many decisions on a voice tone, the look in someone’s eye, the smell of a deal; something that reminded me in some unexplainable way of a problem in the past. Sometimes it’s my real past, other times it’s a vicarious past from my reading or discussions with others. I may commit the narrative fallacy in explaining my objection, but I try to avoid reasoning that way.

Risk Problem Two: Mal Posée

We often look at data to guess the generating probability distribution, then make predictions from that distribution. For example, I might look at the last three years of daily log returns of the S&P 500 and compute the standard deviation as 0.67%. If I’m careless, the distribution looks pretty bell-shaped, so I might estimate a one-day 95% VaR as 1.64 times that, or 1.10%. Thirty-eight of the 753 daily log returns were lower than -1.10%; that’s almost exactly 5%.

I could do better. I could look for patterns in the time series, maybe using GARCH (a statistical estimation and prediction technique that assumes volatility changes over time), rather than assuming the distribution is constant. I could account for parameter estimation error. I could incorporate information from longer time series and intra-day observations to refine the estimate. But whatever I do, Taleb claims, I am crippled by the possibility that some high-impact, low-frequency events are not in my data set.
Finding a distribution is an ill-posed problem, he argues, because many different distributions can plausibly generate the historical observations, and they have significantly different implications for risk management decision making. As a result: “Our predictors may be good at predicting the ordinary, but not the irregular, and that is where they fail ultimately. All you need to do is miss one interest rates move from 6% to 1% in a longer term projection (what happened between 2000 and 2001) to have all your subsequent forecasts rendered completely ineffective in correcting your cumulative track record. What matters is not how often you are right, but how large your cumulative errors are. And these cumulative errors depend largely on the big surprises, the big opportunities. Not only do … predictors miss them, but they are quite ashamed to say anything outlandish to their clients — and yet events, as it turns out, are almost always outlandish,” writes Taleb.

The problem is more acute for risk managers. Our profession revolves around the big surprises and the big opportunities. We must convince people that events could be outlandish and keep convincing them, even though (despite Taleb’s glib assertion) 99% of the time they are not (and even though the other 1% are usually explained away with the help of the narrative fallacy).

Taleb is correct that predictions based on analysis of the past without solid theory, however careful, run up against fundamental limits. Tomorrow will certainly contain some unprecedented events, and some of those might cause us severe harm or represent tremendous missed opportunities. But plans based on careful analysis of past events can succeed; we know that because they were designed to succeed in a wide variety of circumstances that happened in the past. The future will not match any of those circumstances exactly, but the plan still might work. If you pay insufficient attention to the past, you might end up with a plan that cannot succeed in any circumstances.

Tomorrow is like a race to be run over unknown terrain. We don’t know whether a car or a horse or a boat will have the advantage, or something else. If tomorrow’s race is over water, the kind of car you have does not matter. Good cars and bad cars both sink. However, if the race is run over a road, a car in good repair has a better chance than a badly maintained car.

Risk management is like automobile maintenance. It anticipates and prevents some problems. It identifies and facilitates some opportunities. While is true that a badly maintained car can still win a road race, that does not mean that risk management and automobile maintenance are not valuable.

This review has only scratched the surface of a fascinating book that covers topics in most branches of the humanities and sciences. It’s not all right, but it’s always fascinating. It is a useful challenge to the risk management profession to rise above the risk minimizers and risk-appeal managers; to state clearly what we can and cannot do; and to make sure our studies of the past, whether qualitative analysis of big events or quantitative analysis of small ones, maintain the highest standards. We’ll never please Taleb, or most of the world (for opposite reasons), so we have to please ourselves.

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Critique #2 By Philippe Jorion

For centuries, “all swans are white” was an expression used in Europe for something obviously true — until black swans were discovered in Australia. “Black Swans” is the term used to describe events with three characteristics: they are rare, they have extreme impact and they are rationalized retrospectively.

The central thesis of Nassim Taleb’s The Black Swan is that we humans are blind to these events (prospectively). Worse, risk managers are described as “charlatans,” because they create measures that exclude “the possibility of the Black Swan” — hence, with no better predictive value than “astrology.” Pretty dramatic stuff.

Actually, I would argue that risk managers do precisely the opposite. Unlike ordinary humans, who are indeed prone to “blindness with respect to randomness,” risk managers are trained to consider randomness. They build forward-looking distributions of profits and losses based on current positions. By construction, such distributions include extreme events.

Obviously, the normal distribution is often inappropriate, because it implies probabilities of extreme events that underestimate what we observe in typical financial data. The risk management profession, however, has gone beyond the normal or Gaussian model. More realistic distributions can be fitted; risk managers now widely apply semi-parametric methods such as extreme value theory. They also routinely use (or should use) stress tests to generate extreme scenarios that may not appear in recent historical data.

A valid criticism, however, is that risk managers tend to present their results as if these were exact numbers. In practice, risk numbers are the result of a long process with many simplifications and approximations and are sample-specific. This creates errors in risk numbers that are too often ignored. More generally, risk managers need to be aware of the limitations of their risk models.
Risk managers will also acknowledge the argument that extreme events can add up to large effects. In *The Black Swan*, an example is given of a hypothetical investor who could have avoided the largest 10 daily moves (mostly negative) in the S&P 500 over the last 50 years. The final wealth of this clairvoyant investor would be twice as much. This is simply a restatement of the fact that equity markets display high volatility. Using monthly data instead, we have the same result by taking out the worst four months of the same sample. The average of these four observations is -15%. Risk managers will recognize that this average is a conditional VaR at the 99.3% confidence level (one minus the proportion of four months in 50 years). Of course, this is a large loss. When compounded four times, this gives a loss of about 50%. When compounded eight times, over 100 years, we should lose about 75% of the alternative final wealth. And so on.

What should we make of this? Nothing. Should we avoid these four fateful months? By all means — if one has the gift of prescience. Should we avoid investing in stocks? Not necessarily. Despite these painful losses, an investment of $1 in the S&P would still have grown to $180 over these 50 years. This is not as good as the final wealth of $340 without these four months, but is still much better than most other investment choices. This alternative final wealth is purely hypothetical, however, and not a relevant alternative.

Taleb’s example demonstrates that risk management should be integrated within the portfolio management process, which considers both risk and expected returns. If the only objective was to minimize risk, perhaps one should keep cash under the proverbial mattress (even this solution has risks, including theft and inflation risk).

The Black Swan argument should also sound familiar to risk managers with the appropriate training in statistics. This example nicely illustrates how extensive confirmation cannot confirm the veracity of a statement. Risk managers, however, should recognize that the same interpretation applies to hypothesis tests, using Fisher’s frequentist treatment. We cannot prove that the null hypothesis is true. We can only reject it or not reject it (at some confidence level). Indeed, discovering one black swan is sufficient to reject the hypothesis that there are only white swans.

Alternatively, Bayesian statisticians would express their judgment on the basis of likelihood ratios. They would give odds that only white swans exist. In contrast, I doubt that risk managers would be willing to wager infinite amounts that extraterrestrials do not exist.

One final point is that it’s not even necessary to identify the precise nature of extreme scenarios for risk management purposes. This is convenient, because it is impossible to imagine all the events that could provoke a stock market crash, for example. All that is needed is to consider the effect of a drop, of, say, 20% in the S&P index. Management could then take corrective action to ensure that the institution survives this crash.

A good example of the indirect benefit of risk management was the preparation for the “Year 2000” (Y2K) computer programming problem. Many financial institutions in New York had established backup centers in New Jersey to restore capabilities in case of a Y2K disaster. In the end, the turn of the century was a non-event. The backup facilities that were built in anticipation of Y2K problems, however, proved critical in the recovery after the September 11 attacks that devastated Wall Street. So, contingency planning often creates protection against the effects of scenarios that are totally unexpected.

Thus, the possibility of Black Swans cannot justify a wholesale condemnation of the risk management profession. This nihilism makes for entertaining reading but does not present credible alternatives for dealing with uncertainty.

All of this explains the inexorable rise in the risk management profession over the last decade. Far from being a group of “charlatans,” risk managers have helped create a much safer financial system.

Interview: Nassim Nicholas Taleb

FIGURATIVE BLACK SWANS

Aaron Brown (AB): You seem to want to have it both ways with respect to Black Swans. You write, “I would recommend to someone to pick a profession that is not scalable,” but also, “Remember that positive Black Swans have a necessary first step: you need to be exposed to them. … Seize any opportunity, or anything that looks like an opportunity.” You also say to avoid situations with negative Black Swan potential.

That seems as if you want one pocket stuffed with lottery tickets and another stuffed with insurance policies, and you expect someone to pay you to stuff your pockets. Or, in Greek, you want that magical position that is long gamma and long theta. Are you telling people to risk blow-up when there are moderate-sized moves? Or are you just as irrational about risk as the rest of us?

Nassim Nicholas Taleb (NNT): I am telling someone to seize opportunities because the asymmetry is rarely appreciated. Also, I am recommending the avoidance to big downside exposures, because these are not priceable. Many people are avoiding these risks not because they are courageous, but rather because they are both chicken and self-deluded.

Indeed I want to have it both ways: guaranteed floor and maximal upside — the barbell strategies I propose; the right mixture of greed and paranoia. I want a safety of a minimal income with the out-of-the-money dream. This is why the book advocates the stochasticity of trial-and-error as a device for research. In other words, you are not paying for that gamma — just look at the world today compared to what it was during the Pleistocene.

When it comes to professional choices, scalable activities are not utility efficient because it is hit or miss, misery or glory, and we might not enjoy glory as much as we hate misery. Even outside of that, it is not great economically because actors, entrepreneurs and writers make collectively less than publishers and venture capitalists — who besides being more successful are also more diversified. But people like to buy a dream: the pursuit of glory is a strong driver, as illustrated in my discussion of Giovanni Drogo’s fate. It makes you avoid the complicated, hedonic satisficing of daily life.

So there is something existential about living in hope that I do not discount. It makes some people happy to be overconfident; there is something inherently human about shooting for glory. And there is something else: Black Swan hunting has an ethical connotation to me. It is honorable to avoid some classes of risks in spite of peer pressure.

LITERAL BLACK SWANS

AB: For centuries “all swans are white” was an expression in Europe for something obviously true. In 1697, black swans were discovered in Australia. European philosophers switched to “all ravens are black,” for which no clear-cut counterexamples have been discovered. Following the philosopher of science Karl Popper, you use this as an illustration of how extensive confirmation cannot prove a statement true.

But is this fair? Perhaps a typical European 400 years ago would say “all swans are white,” but I’m pretty confident he wouldn’t give you infinite odds if you offered to bet on it. I doubt you could have gotten even odds against the proposition that you could produce a black swan. Casual observation and word-of-mouth would convince people correctly that the swans they were likely to see would be overwhelmingly white, and perhaps exclusively so. But anyone with any money left understands the difference between this kind of reasoning and logical certainty.

There were some people for whom a black swan would be a Black Swan (an unexpected, life-changing event). It could make the reputation of a naturalist or the fortune of an exotic animal dealer. These people understood the limits of the evidence; they flocked to voyages of discovery and before that to remote places in Europe. They weren’t looking specifically for black swans; the chances of finding one was too small. But they knew that if you looked in new places, you would find something new.

Granted, there were philosophers who claimed proofs that all swans were white. They did not immediately lose their reputations on the finding of a counterexample. As you point out in the book, experts can be consistently wrong without
much negative consequence. But in a larger view, medieval
Aristotelian thought was overthrown by empirically based
science, in part due to unexpected discoveries like black
swans. Aren’t people a bit smarter than you imply about
black swans and Black Swans?

NNT: Primo, Karl Popper dealt with a logical problem, the
error of stating no black swans. My Black Swan has one addi-
tional component: impact. Being wrong can be extremely
costly, so my Black Swan starts where conventional philoso-
phers stop. My idea of domains where this matters changes
the whole story; in fact, the logical component for me is mini-
mal. You make a mistake about a bird: so what! But here I am
discussing some domains in which you can be a sucker.

Secundo, more technically, in logic it is a problem of
probability. For me it is one of probability times the payoff
(i.e., the moments of the distribution). This makes it easier,
much easier, than the black swan problem. Make sure you
look for these probability-impact pairs where you can be in
trouble.

TEN DAYS IN 50 YEARS

AB: You make the point that the largest 10 daily moves in the
S&P 500 over the last 50 years account for half the total
return over the period. But that’s taking a total, not an aver-
age. The longer the period it takes for the 10 most extreme
log returns to add to an absolute value above ln(2), the less
volatile the market. Looking at things another way, if the
S&P 500 doesn’t change for the next 50 years, you’ll be able
to say “10 days in a century” instead of “10 days in 50 years”
— even though there was no market volatility at all in the sec-
ond half of the period.

Mediocristan statisticians say enough data washes out the
effect of individual observations when computing averages,
but not when computing totals. Those ten days will continue
to represent half the total return of the stock market forever
into the future, because you’re compounding rather than
computing average returns. In average terms, missing those
ten days would raise your average log return from 7.0% to
8.4%, a significant change, but not half the return (and even
smaller-looking if we add dividend return to both).

Isn’t Extremistan the place where people compute totals
instead of averages, rather than a place where underlying phe-
nomena have fat-tails and non-linear effects?

NNT: Here I agree with you, but let’s not nitpick: I wrote
on the graph that it was pedagogical, not as compelling sci-
entifically as other violations of normality. It is the weakest
point in the book from a mathematical standpoint, as the
20 sigmas and the plethora of 10 sigmas in single stocks
and interest rates (where I’ve seen 50 sigmas!) are far more
convincing — but these examples are not noticed. In reality,
it does illustrate skewness far more than fat tails. It is
also the effect of a multiplicative process, which, as you
noticed, compounds variations.

But I disagree with the idea that the effect is benign for
multiplicative returns: in fact, the multiplication com-
pounds the errors, and we must worry a lot more about
misspecification with such a process than an arithmetic
one. Many people such as the economist Robert Gibrat
argued that log returns can cause inequalities such as the
ones we see today — the 5-, 10-, and 20-sigma occurrences
that are in log returns. Another few huge days and your
average would be totally shellacked.

The way I view it, for the broad market, the power law
scale exponent is often calculated as $a=3$ for downside
deviations and close to 5 for upside ones. These are not sit-
uations of hugely fat tails, but they are sufficient to make
portfolio theory completely nonsense mathematically —
along with dynamic option replication. Where things turn
scary is with the nonlinear payoffs like those of derivatives
as the “$a$” of these payoffs becomes very, very low and
their effects turn explosive.

Finally, a little more technical point: my true problem is
that the $a$ can be as low as 1.8 for very, very large deviations,
from something I call the Weron effect, which was rediscover-
ed by my colleagues Mark Spitznagel and Pallop Angsupun
at Universa (formerly of Empirica). If you generate by Monte
Carlo an infinite variance process, a statistician is more likely
to mistake it for a process with much thinner tails.

CONFIRMATION BIAS

AB: You discuss Peter Wason’s famous 2-4-6 task. Subjects
are given 2-4-6 as a triple of numbers that obeys some rule
and asked to deduce that rule by suggesting other triples, to
which the experimenter will say either “yes” or “no” accord-
ing to whether they conform to the rule. Subjects seemed to
guess initially the rule was consecutive even numbers and
many neglected to test odd or nonsequential numbers. This
1960 experiment stimulated a large body of work.

You interpret this result as a confirmation bias; people give
sequences that confirm their initial guess rather than
sequences that test it. But why is this irrational? Suppose my
first guess is 8/10/12, and the experimenter says, “yes.” Now
I can try to narrow the rule or broaden it. I could guess, for
example, “-6/-4/-2,” in case the rule is consecutive even posi-
tive numbers. If I get a “no,” I have narrowed the rule. Or I
could guess “1/3/5,” in case the rule is ascending sequence
with differences of 2. In that case, a “yes” broadens my rule.

Why is one preferable to the other? Suppose I find that
chewing on the bark of a certain tree reduces fever. As a medical investigator, I could try to broaden that rule by having people with fevers chew bark from other trees or people with other symptoms chew bark from the first tree. I learn from a positive result. Or I could try to narrow it by taking the bark from different places on the tree or at different times. Then, I learn from a negative result. Both investigations are valuable, yet you seem to label the first one “empirical medicine” and the second one “confirmation error.”

Why is performing experiments in which you expect a positive result inherently inferior to performing experiments in which you expect a negative result?

NNT: I agree with you that there are situations in which it is efficient to confirm and generalize. Indeed, as always, in Mediocristan it is both efficient and a good approximation. My point again takes us to Mediocristan. In a fat-tailed environment, negative confirmation works much better than in thin-tailed ones. With high-impact events, also. You can confirm guilt more easily than innocence. In thin-tailed environments, you may have situations of equality.

Let me mention something else here: statistics is inherently a confirmatory exercise, and what I want to propose is a veto mechanism to lift confirmation in some domains. Again, the try-not-to-be-a-sucker theory.

**SCALABILITY**

**AB:** When giving examples of scalable phenomena, you seem to mix two different types. Things like wealth, earthquakes and stock market returns are clearly non-Gaussian, even in small samples. If you give me the wealth of 100 randomly selected individuals, I’m likely to notice that the data appear to have an exponential distribution, which will lead me to predict a significant probability of Bill-Gates wealth, even if no one in the sample comes close to that level. Even intraday stock market returns exhibit significant heteroskedasticity, which should lead me to adopt a GARCH or similar model that predicts significant probability of extreme movements.

Of course, in both cases, small data sets will have a lot of estimation error when extrapolating to extreme events, but the small sample data should alert me to the possibility of events many (small sample) standard deviations away from the (small sample) mean, which implies the possibility that the true population mean may be quite different from the small sample mean. People do get fooled by this all the time, but good statistical practice can avoid it.

Far more problematic are things that appear perfectly Gaussian in small samples, but have rare extreme jumps. For example, if you study estimated worldwide influenza mortality rates over the last 300 years leaving out 1732, 1781, 1802, 1830, 1847, 1857, 1918, 1937 and 1968, you could conclude you were looking at a Gaussian distribution. You could study 30 years of consecutive data without a clue that global influenza pandemics are possible and therefore understating the mean and wildly understating the probability of extreme events. No doubt there are a huge number of other things which are more than 10 times as rare, or for which we have less than a 10th as much data, that are entirely unsuspected and will appear as Black Swans.

Most of the examples in your book appear to be things of the first type, which only require improvements in statistical practice to address. But most of the discussion concerns things of the second type, for which statistics can offer no help. Are you mixing apples and oranges?

NNT: I disagree. Many of these are in fact scalable, but we don’t know it because their sample is too small.

GARCH does not work out of sample. It is a good story, but I was unable to use it in predicting squared deviations or mean deviations. Heteroskedasticity — that is, stochastic volatility — is an epiphenomenon, not a phenomenon. It may come from the measurement of a process without tractable higher moments.

**NARRATIVE FALLACY**

AB: You accuse newspapers in particular of the narrative fallacy, assigning importance to events after the fact to make a coherent story, which is then “confirmed” by those same events. Conflicting evidence is deemed unimportant since, by definition, it did not contribute to the story.

Aren’t you guilty of the same thing when you claim Black Swans dominate history and current events? Newspaper headlines are often surprising. As the saying goes, “Man Bites Dog” is a headline, “Dog Bites Man” isn’t. Those surprising events were often unpredictable, sometimes even indefinable, before they happened. But the rest of the newspaper, the part that influences far more people far more of the time, seems to change slowly and predictably, either in cycles, in trends or not at all. The weather, fashion pages, comics, food section, want ads, graduation announcements, local celebrations and other matters contain few surprises and are not hard to predict. Headlines are caused by Black Swans, because headlines need Black Swans, and newspapers will create them if they don’t occur naturally.

Similarly, if history is the story of kings and assassinations and revolutions and market crashes and inventions, then it is a flock of Black Swans. They fill the history books because they’re exciting. But the day-to-day life of most people changes slowly and predictably. Fads come and go, and it
may be hard to predict the next one, but it’s easy to predict there will be a next one. Hemlines go up and down, but people still wear clothes, and with remarkably conservative design, materials and functions. Technology changes, but people integrate it into their lives as convenient; only over long periods of time does it force change on individuals. Are Black Swans just the tip of the narrative fallacy?

NNT: Yes and no. My entire idea is that we overestimate some Black Swans, but not the idea of Black Swans. People make the mistake all the time of thinking that I am saying that all Black Swans are underestimated. I keep writing that we are shallowly suckers for the salient and people change my story to make me a predictor of Black Swans.

Myron Scholes, whom I enjoy putting in a state of rage, changed my story twice in order, attacking me. The first time he accused me of saying that fat tails come from psychology. The last time he used the counterargument that some people look at some extreme events, I guess to defend himself. That is not my point. My point, simply, is the abstract notion of the incomputability of extreme events. The larger the event, the less we can compute it.

As to the timescale, I wrote in Chapter 4 that some slowly-building-up historical events are Black Swans for a longer term predictor. It is a matter of timescales.

Also, I used a narrative to displace a narrative. *The Black Swan* is a good narrative — but it is an honest, non-distorting one.

**BANKS**

**AB:** Most people believe we need banks and that market processes alone are not enough to ensure they hold adequate capital. If you set the capital requirement too high, there aren’t enough banks and they don’t take enough risk, so the economy suffers. If you set the capital requirement too low, there are too many bank failures with the attendant disruption and expense. If you were forced to set the capital requirements for the world’s banks, what would you say? The possibility of Black Swans makes holding capital irrelevant? Hold infinite amounts of capital? No capital? Hold 8% of notional amount of assets because more complicated analysis is worthless? Or simply, I don’t know?

NNT: We do not need banks to gamble using public money and pay bonuses to employees when they are right. Hedge funds can screw their investors, but they are both mature individuals.

We should have let the banks go bankrupt. I am against worrying about the “health of the financial system” by putting Novacaine on it and increasing its long-term risks.

Banks need to go bust once in a while so that we learn to rely on disintermediation with diversified ecology.

**ICE CUBES AND PUDDLES**

**AB:** You use the example of a melting ice cube to illustrate the problem with statistical models. If you see an ice cube, it’s easy to predict the shape of the puddle that will result when it melts. But if you see a puddle, it’s impossible to determine the shape of the ice that created it. Similarly, a statistical model will give precise predictions of possible outcomes; so if you have a model, you can put confidence intervals around events.

However, observing events does not help you build a model. There are an infinite number of models that could plausibly underlie historical events. Unless you have strong theoretical reasons for knowing the type of model, and there are a reasonably small number of parameters relative to the amount of data, modelling is as hopeless as trying to guess the shape of an ice cube from observing a puddle.

But isn’t this the reason the field of non-parametric statistics was developed? It’s possible to make some inferences and predictions from data with only weak, general model assumptions. Also, we have visualization methods, data analysis and robust estimators to give conclusions that are not sensitive to the underlying model. The heat equation that governs ice cube melting is the most famous example in physics of a problem that is easy to solve forward, but ill-posed backward. Most of the time, you can gain some knowledge about the past by examining the present. Isn’t an ice cube an extreme example that overstates your case?

NNT: My point is far deeper, at the core of the knowledge in the tails, and cannot be remedied by non-parametric statistics. We do not observe probabilities; we estimate them from observations and samples.

The principal problem of probabilistic knowledge and discovery is as follows: without a strong *a priori* specification of the underlying reality of a process, epistemic confidence is inversely proportional to the consequences of the knowledge at hand. Large deviations, the one that can have the most meaningful effect on the total properties, are far more difficult to evaluate empirically than regular ones.

Non-parametric statistical methods will not do anything in the tails. Where it helps is in replacing Variance and Gaussian terms with robust ones like mean absolute deviation (MAD). I like working with higher moments like volatility of volatility using nested mean deviations. MAD works better than standard deviation, MAD of MAD works better than volatility of volatility.
And we can do a lot with the idea of risk I am proposing, mainly by ranking investments based on their exposure to tail events. Why? Because finance, particularly derivatives, is dominated by the tails.

The way I view risk, it comes principally from lack of understanding, lack of knowledge and lack of predictability — “epistemic opacity.” So this interpretation of risk has a magic word in it: error. Note that there is another class of “computable” risk, sort of the one that is supposed to prevail in (some) casinos. But it does not seem to exist or matter much in economic life. Because of the “ludic fallacy,” it is found only in laboratories, economics and finance textbooks and games. More about the discipline called finance in a bit.

Against what people tend to think, we can do a lot with my risk concept: it leads us to reasoned and structured skepticism. I am a skeptic and only believe what I see; tail probabilities I don’t see. But consider: payoffs from tail events I can see. I have no clue about the odds of a 10% move in a given security, but I know what happens to my portfolio in such case. One is soft (probability); one is hard (payoff).

So my central idea (in risk and everything) revolves around epistemological robustness — robustness to large variations and model error, which are often the same thing. Where is our knowledge soft? What is it that I don’t see (and know)? How much would the unseen cost me?

To me, risk management must primarily focus on making a distinction between portfolios based on sensitivity to errors in judgment and modelling — i.e., focus on the off-model risks. Nothing short of that is ethically acceptable to me.

Only when we learn that some portfolios are more robust than others — because, risk-wise, we know a lot about their properties — can we become more aggressive. By learning more about the robustness of portfolios to risk assumptions, we will be able to make sound allocations and understand our real downside.

For instance, someone whose risks explode if he or she changes his or her model or its parameters ever so slightly (but does not seem risky otherwise) should be penalized (in the risk measure) compared to someone else who seems
volatile but has no tail risks (mean-variance would propose the opposite in many cases). A portfolio long a deep-in-the-money call is far less risky than a portfolio short puts. A portfolio short tails is riskier than one with a floor on its losses. But this may not show on many models — you need to check for model sensitivity.

Everything comes from the sad fact that financial risks are dominated by the high impact event, of which we know so little, Poisson or no Poisson, and for which our estimation errors have been monstrous.

The Fakers
This brings me to what I call the “expert problem.” Faux experts are people who know about their subject a lot less than they hold, in spite of their empirical record, but, for institutional reasons, manage to claim authority. I established, in The Black Swan, a catalog of suspected faux experts (comparing them to real experts). Professional, non-academic risk managers were not among the suspected faux experts. Risk managers do extremely well in some domains (e.g., engineering, aerospace, medical fields and parts of insurance that are Gaussian). They have no reasons not to do very well in finance, but success will come from listening to bottom-up methods, working on robustness and freeing ourselves from ... irresponsible ideas masquerading as ‘science.’"

“Risk managers ... have no reasons not to do very well in finance, but success will come from listening to bottom-up methods, working on robustness and freeing ourselves from ... irresponsible ideas masquerading as ‘science.’"

“Risk management should not be separated from the activity of risk taking: you need to structure your positions according to the risks you understand. Risks you do not understand should be avoided, period."