The basic stylized facts are as follows:

1. In general, measures of risk—volatility, beta, covariances—are uncorrelated with average returns. There has yet to be identified a risk factor that works much of the time, let alone most of the time.
2. For extremely volatile investments, returns tend to be below average.
3. For extremely safe assets, returns tend to be below average.
4. Investors are overconfident: They trade too much, and diversify too little.

The current theory is that as-yet unidentified risk factors, plus some acknowledgment of behavioral biases account for the last item (number 4). Yet the stake in the heart of this approach is that if a risk factor explains the low return on safe assets (number 3), that it is generally absent when explaining most assets (number 1), and seems negatively correlated with volatility at extreme levels (number 2), will require a great deal of implausible rationalization. The fact that risk factors generally cancel out, or are inversely related to intuitive definitions of risk, makes a solution within this paradigm impossible.

My explanation involves three independent assertions:

1. People benchmark against the consensus, that is, each other; so all risk is like idiosyncratic risk in the conventional model: unnecessary, and so is not priced
2. A safe asset is a necessary part of every portfolio
3. Actual risk-taking is based on hope, and follows a rational rule, not a rational act, for maximizing one’s success

These assumptions can explain the data we see. Thus the question should be, are they true? This chapter is primarily about pillar 3. The first pillar,
that people benchmark against others, was Chapter 9. Let us dispense with
the second assertion.

THE SEARCH FOR SAFETY

People crave certainty in some measure when it exists as one alternative.
Consider the fact that in financial crises, correlations tend to all go to 1,
or $-1$, whatever hurts your portfolio most. It is nice to think there is some
asset—gold, cash, T-bills—that will not go to zero in such a situation, so
you can pay your mortgage, and buy food and necessities. Safe assets are
like a lifeboat for tough times, and we all eventually need a lifeboat. A
modest allocation to some sure thing is very comforting, because while we
can all manage adversity, most investors consider bankruptcy something
that should be safeguarded against if possible, and pay a premium in terms
of lower return. Having money stuffed under one’s mattress is like a life
insurance policy, an insurance against an improbable event that will allow
one to get back on one’s feet.

The number of assets considered safe is relatively small, and there
are huge barriers to entry, so that supply is limited. A government, like
Switzerland, has developed a reputation for a stable currency, whereas Brazil
would have to show several decades of unseen behavior to reach that status.
But in every situation, there is some asset that acts as the best, and most reli­
able, and generally, people’s perceptions of these assets is log-linear. That is,
when I was at Moody’s, I noted that the agency ratings started out as a mere
ordinal ranking, from very safe AAA, to safe AA, and so on. Then, in the
late 1980s, Moody’s and S&P started putting out data on the actual default
rates of these securities, and they turned from an intuitive classification to
a more quantitative one. Interestingly, initially even Moody’s analysts were
not sure what the default rates were for each grade. I saw an internal report
circa 1989, and it noted that B-rated bonds were very risky, with estimated
default rates greater than 15 percent (it’s actually more like 6 percent an­
ually). But interestingly, the scale was almost linear in logarithmic space
(which means exponential in raw default rates). Please see Figure 10.1.

So, the default rate actually rises exponentially, which if you have
worked in credit risk management at a bank, you see all the time. Buck­
eting of risks generates exponential losses as one moves down the intuitive
buckets one ranks risks into. When people rank order things, they tend to
be in log space. Many things are in log space, such as decibels and the
Richter scale. Benford’s law, the finding that most numbers of real things
has a power law distribution, is based on the fact that so many things
have logarithmic distributions. Thus, nature, and our intuition, is built on a
logarithmic sensibility.
Fechner’s Law, articulated in 1860, states that humans sense stimuli differences at a logarithmic scale, so that the proportionality of a difference is the limit of human sensation, as opposed to a computer, which might use an absolute difference, or Euclidean difference.\textsuperscript{1} Our number intuition is logarithmic, not linear. More recently researchers have found that babies first learn that numbers are different at a ratio level: 4 or 16 is different from 8 by a factor of 2, and it is this ratio, not an additive difference, that initially develops from intuition.\textsuperscript{2}

The point is that if we have a log sense of risk, and safe means extremely little risk, the safest assets are insanely unique. It must be 100 times safer than something merely pretty safe, and when you have to give someone credibility that something is that safe, it basically implies there must be decades of proven safety. This necessarily leaves very few assets, so supply cannot take advantage of this cheap funding. Supply is necessarily constrained by this requirement, and keeps the price of safety assets high, and their returns low.

The desire for a little haven of indestructible wealth amid a portfolio of benchmarks is one of the little takeaways from the famous Kelly Criterion, by which one is trying to maximize geometric wealth by the end of a given time. One of the basic principles of this criterion is to make sure the probability that total wealth goes to zero, is zero, because after wealth goes to zero you have nothing to make money with in the future.
MOST FINANCIAL RISK TAKERS ARE FOOLISH

Investors, in aggregate, appear to be quite foolish. That investment is in aggregate uncompensated was a theme of Keynes’s *General Theory*, which noted, “If the animal spirits are dimmed and the spontaneous optimism falters, leaving us to depend on nothing but a mathematical expectation, enterprise will fade and die.” As someone familiar with financial markets, Keynes was no doubt aware that it was not so much the investors who get rich off finance, but the brokers. Adding up their expenses, their undiversified gambles, generally produced a weak return for the average investor, as argued in Chapter 6. A major puzzle in economics is gambling, where the expected return is actually negative, and the payoff random, seemingly a two-fer for badness.

It all starts with the behavior of retail investors, who trade too much (which is costly), do not pick winners better than random picks, and are massively undiversified relative to alternatives such as index funds. And, it only gets worse from there, as research shows that financial overconfidence increases with age, experience, and success, ensuring that hubris offsets any extra skill acquired. The sad thing is that this implies a Peter Principle in Alpha: People will stretch their alpha until it is gone, making all individual alpha ephemeral, and thus any positive alpha of questionable relevance prospectively. Thus, mutual fund managers deviate from the passive benchmarks, adding risk, and underperforming almost exactly equal to their transaction expenses.

Contemporary psychologists agree that “on nearly any dimension that is both subjective and socially desirable, most people see themselves as better than average.” Thus, we don’t mind being inadequate in something parochial, like math or ping pong, but in something generally esteemed like wit or courage, people generally think they are above average. Overconfidence invites people to deviate from the norm—to take risk—because they think they know some fact or theory most other people do not sufficiently appreciate. Yet this appears massively irrational, objectively.

TWO TYPES OF PRICED RISK

If someone would assume all your financial risks in health, home, auto, and other such areas, most people would pay some price for this transfer of risk. The ability of a large group to assume such risks and charge a price slightly above the statistical expected value of these risks is generally presented as the prototype of risk and return: the power of diversification, and the fact that people prefer to pay to avoid it. But it is rarely recognized there are
many other types of random events about which we would do the opposite: pay to retain. To have these risks eliminated would eliminate our hopes and dreams. The distinction is hope, in that the one thing a good risk has is that it plays into our dreams, whereas the bad risk is an annoyance we minimize or transfer.

Hope applies only to something we do not have, a favored anticipation of something uncertain. There must be a chance of it not happening to call in hope, otherwise, it is merely reality, which one may like or dislike. Thus hope requires uncertainty, volatility, or risk. Hope is related to our dreams because it refers to an uncertain outcome we highly desire, something that mere contemplation makes us feel energized about, alive.

There have been previous assertions of two types of risk in the past. The popularity of these approaches highlights the intuitive appeal standard approaches of turning risk into a probability problem, like solving for the expected value of an option price, is insufficient. Thus with behavioral economics, we have two types of risk: those occurring in the risk-loving portion of one’s utility curve, and risks occurring outside of that portion. With post-Keynesians we also have two types of risk: those with Knightian or Keynesian uncertainty, and those without it. But the problem of the risk-loving theory is that it is all post hoc, or descriptive. The problem with the Knightian uncertainty is that as uncertainty is positively correlated with risk, one should still see a positive volatility or covariance relation with returns, which we do not see. For the most volatile assets, which almost always have the most uncertainty, their returns are below average, which makes the distinction unhelpful.

**UNCERTAINTY IN INNOVATION**

Alpha creators look at the world as if everything needs fixing. Inventors are constantly criticizing, extending, and simplifying, which leads to the design of new things. The successful inventor needs a lot of highly specific knowledge of the state of the art, so his ideas are not duplicative, and the state of the market, so he can know a user’s unmet needs. But looking at the current set of risk reports, portfolios, or capital structures that one might improve upon presents problems because these things currently work—however well—in a particular configuration, at a particular scale, in a particular context. Trying to find the essence of something that works is hardly straightforward; just think about the varied contradictory reasons given for why the United States is a relatively prosperous nation.

Risk taking predicated on the hope that one has an edge, is about a competitive advantage, alpha. The idea that you have a special skill in
investing, or some special insight in measuring risk, or hedging, or selling
home-made jewelry, is something that takes knowledge, but also a little
chutzpah. You can go into an organization and follow your boss’s lead, and
there is a need for people with sheer competence. Yet many people think of
starting their own thing, creating their own project, if not company, based
on their ideas. This necessarily involves uncertainty because any good idea is
novel, a step into the unknown. This is the essence of Keynes’s observation
that most real-world decisions are not unlike the uncertainty of a roulette
wheel.

An attempt to innovate is like navigating a labyrinth in the dark. We
have some idea of what to expect, but we also know that we will have to
adjust our path as we occasionally smack into things. To try to estimate
the risks in such ventures is like trying to foresee which trends continue (for
example, Moore’s law), and which do not (for example, housing prices).
Success takes actually doing something, because one does not learn the
essential details until one actually tries something.

WHY RISK TAKING HURTS

Jean-Claude Killy noted that “to win, you have to risk loss,” but for most
of us, it is not the physical courage of a downhill skier or a warrior, but
rather the intellectual courage needed to stand up to colleagues who dismiss
our new ideas. Though much more subtle than physical courage, intellec-
tual courage is considerably more difficult, as researchers have found that
emotional pain is more stressful, more lasting, than any physical pain. To
seriously try to dance with flair or wear a really eye-catching new outfit
invites the scorn, the ridicule, of failing so bad, the joke is not the failure,
but your mind-numbingly-clueless thought that you are John Travolta or
Jennifer Lopez. If you have objectively low alpha in some area, your will-
ingness to attempt to ply it will not be seen positively by your status group.
If you fail, there is a chance that this will be more than a localized failure
because of the implied obtuseness in such an objectively doomed risk-taking.
Courage is therefore not viewed in isolation, because if it is rash or exces-
sive, it is considered merely foolhardy, not admirable, as when Graham and
Dodd deride gamblers.

The easiest way for a putatively innovative person to avoid humiliation is
to extend an existing line of reasoning by trying something logically implied
yet untried because it is insanely difficult. This is what most academics
do, as they try to patch an existing theory through inside-the-paradigm
puzzle solving, what Thomas Kuhn called mopping up. To apply top-level
mathematics to extend a particular result is considered world-class economic
theorizing, regardless of whether the result was already well known (see Debreu’s *Theory of Value*). Academics generate a lot of hair splitting, but this is because their extensions tend to be merely extending the current ideas in obvious but tedious directions, because academics are good at difficult but straightforward problems, and are as risk-averse as anyone else.

In standard models of risk, there is a price of risk, and if sufficiently diversified, one gets what one pays for. There should be little regret for an unfortunate risk, any more than one bemoans the fact that their lottery ticket did not win. You took the risk, you knew the odds, and there you are. Say you offered me a bet where if it does not rain in Phoenix, Arizona, on June 21 of next year, you pay me $100, but if it rains, I pay you $100. I would take that bet because the probability of it raining are well below 50 percent, and if I end up losing the bet, I will dislike losing, but not regret the bet because the bet was better than fair. Yet, in practice, risk taking generates a lot of anxiety. With the benefit of hindsight, many risks reveal themselves to have been doomed from the start, because knowing how things worked out, it is all so obvious, and we were fools for thinking otherwise. The absence of anxiety in traditional risk models is a prominent data point against it, because this misses a prominent part of actual risk.

The key point about searching for alpha is that when testing whether one is good at something, invariably this can only occur after some expertise in the area is acquired. Learning to finger an instrument or solve math problems is difficult and time consuming, but straightforward. Eventually, however, if you wish to apply alpha and demonstrate true excellence, you need to apply that expertise in a novel way. Thus, a sufficiently self-aware person realizes the quantum gulf between, say, being able to play a song correctly and play it well, or more importantly to write a good new song. Failure is thus not inconsequential, because you cannot be in a position to make a serious effort here without developing colleagues and mentors in the field, and it is their collective judgment that will evaluate your efforts. Such feedback is stressful, because life is finite, and if this effort does not work out one then has to start anew at something else. Further, failure suggests a clueless optimism about one’s actual abilities, which can be pathetic, and has reputational effects. Starting over and accepting failure with dignity without losing one’s enthusiasm for life are extremely important and take intellectual courage, mainly because doing so is so emotionally stressful. This is why the only way initiatives are shut down in large organizations is when the control is taken away from the founders. It is rare one can kill their creation without using force.

Intellectual courage is only admired *ex post* for those who were successful doing something unorthodox, deviating from the consensus and being right. For example, we consider Galileo a courageous innovator, yet if we
were to assess current scientists with similarly unpopular views today, most of us would think they are not courageous, but rather foolish. A scientist who believes in intelligent design or cold fusion is an outcast for good reason. Intellectual courage in real time—when the decision is made but before the outcome—means the average respectable person will lessen his estimation of you. It is easy to forget that Galileo’s famous observation that all objects accelerate at the same speed was not so obvious. For example, if you push something faster, it accelerates—a heavy weight pushes harder on your hand, ergo, it should push downward faster. Add to that the observation that leaves fall to the ground more slowly than rocks, and I could see why people would assume weight is positively correlated with acceleration. Around the same time, Tycho Brahe, the man whose measurements allowed Kepler to formulate his laws of motion, did not accept the heliocentric model of the solar system, in spite of his very good data and good-natured persuasion from Kepler—is it not obvious that we are at rest? In contrast, there were many theories less outside the consensus that have shown themselves to be embarrassingly wrong: lobotomies, communism, Microsoft Bob, the Oedipus complex. If you failed, you were not merely unfortunate, but wrong, with hindsight.

CONFUSION OF RISK AND GAMBLING

Gambling has a long history of being criticized by value gurus Graham and Dodd, and their disciple Warren Buffett, as pure folly, the number one mistake of investors. We know people scoring lower on cognitive tests choose the higher expected value lottery less frequently, and that lower socioeconomic status is correlated with greater lottery play, and lower IQ. Evaluating probabilistic payoffs is something that less intelligent, less educated people do less well, and hope leads them astray.

When Bernard Baruch suggested an investment to J. P. Morgan, and noted “It’s a good gamble,” J. P. cut him short and said, “I don’t gamble.” Baruch lamented using the phrase, knowing it was a deal breaker, but clearly even famous investors do not appreciate the difference. In a sense, gambling is just a word for “taking on volatility without a positive expected return,” but this presumes everyone agrees on both the returns, the odds, and the nature of randomness—many people believe in luck, that somehow good karma will give them a special deal in drawing from the urn of chance.

Thus, many people think they are risk takers, but are actually gamblers, like the little ladies with a bucket of nickels playing the slots in a casino. It’s all electronic. The odds are against them. The longer they play, the more they will lose, statistically. It would be easy to say, simply, don’t invest on the basis
of hope, and gambling appears objectively foolish, based on miscalibrated hope. Yet there is a continuum between gambling and investing based on hopes and dreams that make this difficult in practice.

FROM THE MOVIE DUMB AND DUMBER:

_Lloyd_: What are the chances of a girl like you and a guy like me ... ending up together?

_Mary_: Well, that’s pretty difficult to say.

_Lloyd_: Hit me with it! I’ve come a long way to see you, Mary. The least you can do is level with me. What are my chances?

_Mary_: Not good.

_Lloyd_: You mean, not good like one out of a hundred?

_Mary_: I’d say more like one out of a million.

[pause]

_Lloyd_: So you’re telling me there’s a chance!

It is common to overemphasize the possible merely because we want it so very much. Some people’s hopes are based on objective gambling, especially when the winning payout is especially large, as in lottery tickets. While these are objectively poorly returning investments, they are still based on hope, a belief in numerology or fate. If I could vet people’s investment strategies, I suspect that I would be like Simon Cowell on the TV show _American Idol_, telling 95 percent of these people they are deluded if they think they will succeed with their current proposal, and to start over; too much hope, not enough alpha. The market will offer alternatives, and after looking in the Investments section of your local bookstore, you will find books instructing you on how to make money in a variety of venues where the odds for a retail investor are so low it defies credulity: how to succeed in penny stocks, options, or day trading. I would estimate that half of the shelf space in these sections encourages gambling based only on hope disguised as investing. Invariably, they have the disarming story about how, before they made their fortune, many esteemed doubters told them they would never succeed.

And those dismissing the experts have a point, because the experts are often horribly wrong. Lord Kelvin, a man prominent in nineteenth-century physics, stated that “heavier-than-air flying machines are impossible.” A Yale management professor gave Fred Smith a grade of “C” for his business proposal that became FedEx. Several companies took a pass on what was
to become the revolutionary Apple computer, and the founders of Google offered themselves to Yahoo! for a mere $1 million. A music executive at Decca Recording Company rejected the Beatles in 1962. The list of such mistakes is endless, and gives everyone who faces skepticism hope. It is not restricted to business, as I have often seen posters with Einstein’s quote “Great spirits have always found violent opposition from mediocrities” on the wall of scientists, a motivation for times of lack of support.

In addition to experts missing good ideas, there is the issue of over-confidence for probabilities. Researchers have asked people questions like, “Which magazine had the largest circulation in 1970, Playboy or Time?” A respondent would then say, “Time,” with a probability of 99.9 percent, or odds of 1,000 to 1, for example. But when people’s odds were incredibly extreme, as when they reported million-to-one odds of being right, these odds were actually more like 20 to 1 of being correct. So, poor Lloyd in Dumb and Dumber perhaps was not so dumb (indeed, the actors started dating after the movie).

Thus, every bad idea actually has a chance that it too, is like those previous successes that were dismissed by the Establishment, and those previous million-to-one certainties, those expert skeptics, were wrong. Everything has got a chance, in that sense. The problem is that gambling is a lot like alpha-based risk taking, and a poor analogy is at the bottom of all bad ideas. The more objective alpha in the risky decision, the greater chance of success. But any case will be highly parochial, the assessment of alpha highly subjective, and evaluation is rarely disinterested, especially when you evaluate yourself.

There is no list of great ex ante investments, because most of us consider a person’s reasons why a risk went wrong as special pleading, spurious argumentation where one tendentiously highlights some reasons and excludes others. Profits, however, speak for themselves. As Norman Mailer put it: “In the middle classes, the remark, ‘He made a lot of money’ ends the conversation. If you persist, if you try to point out that that money was made by digging through his grandmother’s grave to look for oil, you are met with a middle-class shrug.”

Just as unethical decisions tend to acquire a gloss, stupid gambles that pay off are often viewed uncritically. How many millionaires who sold out at the height of the Internet bubble, selling a business for millions that soon would be bankrupt, are still considered savvy businessmen, even though objectively it would be more accurate to call them lucky? That their existence depended upon a bizarre period of irrational exuberance does not really diminish their status. I have never met a wealthy portfolio manager who considered his success primarily due to luck, and these are the people who form anecdotes and analogies for others making business decisions. This conflation encourages gambling, because many successful gamblers of the past are so often presented as savvy businessmen or investors.
Standard Alpha Contradiction

Most presentations on maximizing alpha assume one has a bunch of data that allows one to apply mean variance optimization. That is, assume a set of potential investments, they could be assets, or asset classes, or portfolio managers with alpha. This gives one a set of expected returns, volatilities, and correlations. The implied set of portfolio weights is now strictly a math problem that is straightforward and soluble.

The assumption that alpha exists in this form is a contradiction. Remember, alpha, as originally introduced by Jensen in 1968, is from an equation

$$r_i = \alpha + r_f + \beta(r_m - r_f)$$

This particular equation assumes the CAPM, but it generalizes to other factors by merely adding more betas ($\beta$), applied to more priced risk factors ($r_m - r_f$). The key is, alpha is a free lunch and if it were incontrovertible, it would not be there. Most excess returns are hotly contested debates about whether it implies a market inefficiency or a potentially misspecified market model, whereas I think the argument should center on the returns, whether they accurately reflect expected returns.

A good example of this is in mutual funds, where for decades mutual fund managers asserted that they had alpha, and they seemed to make their clients money. Many people assumed the returns on their accounts came from alpha (they did not call it that, as Jensen introduced the term in 1968). But in fact, the mutual fund managers have historically underperformed the passive indexes representing the set of stocks they chose from, the returns were merely from beta. They appeared to deliver, but only because investors did not have the right model: They did not include the market factor in the evaluation; they had a misspecified model.

This might seem quaint, but how often have you heard that investing in a strategy that is uncorrelated with the relevant benchmark by some manager with an impressive set of credentials, has alpha. Yet, his strategy is vague ("We seek value while maintaining a disciplined focus on capital preservation using a combined seventy-eight years of capital markets experience"), his track record is not obvious—it is often allied with a group he was part of (for example, "He used to work at Goldman Sachs"), and we cannot be sure it is merely survivorship bias. Nonetheless, with this information, we too assume some positive alpha. Why should we believe him? This usually comes down to the idea that a smart, wealthy guy with experience in the field should outperform a passive index. This is the mistake equity mutual fund investors made for decades, and is no less incorrect because of its long
popularity (after all, bloodletting was a common cure for ailments for centuries, even though this killed far more patients than it saved). Expected returns, volatilities, and correlations really beg the question, because it presumes the hardest part of investing—figuring out expected return—has been reduced to a problem already solved (portfolio optimization). Any asset or manager’s particular alpha necessarily implies most people do not agree with you about the expected return, properly amortized over the business cycle.

As much stock investing is highly undiversified, many people believe they understand the fundamentals of some company’s business model (for example, the Peter Lynch principle of investing in what you know, such as firms where relatives work, or places you shop), and so believe it is undervalued. They believe the stock is worth $20, even though it’s trading at $10. Indeed, using this kind of thinking, Warren Buffett considers risk and return to be inversely correlated: A stock he considers worth $30 trading at $10, has both higher expected return, and lower risk, than a stock he considers worth $20 trading at $10. But for everyone but Buffett, why should they believe they know better than the market?

Economists do not really know what to do with situations where people agree to disagree. It seems irrational. To the extent you value an asset for $20, and I value it at $10, your assessment of my valuation, and my symmetric rationality should cause you to cut your price, and similarly, I should lower my price. Game theorists have examined this extensively, and deduced that two rational people, even with different information, should eventually come to agree on some number between $10 and $20, because of complex reasoning involving the hypothetical “If I were you and thought it was worth $20, then I must know X” whereas your counterpart does the same, until you agree, at which point you no longer wish to trade because the price equals your valuation. This is the Groucho Marx Theorem of Trade: You should not join a club that would have you as a member. An example of how fundamentally incorrect economists have this situation, most models of how information gets into prices assumes that, to the extent someone gathers information that is useful, it must have a cost. If information was costless and useful, it would provide arbitrage profits for everyone and thus you do not have an equilibrium. Thus, models by Grossman and Stiglitz (1980), or Kyle (1985), always have an informed agent making money, after paying for information, whereas the uninformed accept a lower gross return, but the same net return, as the informed investor. The problem is that no one has identified a set of informed investors. Certainly not stock analysts or mutual fund managers, who have no alpha, but clearly are spending a lot of their time and effort acquiring information. Thus, standard models have a rotten core as to why people trade, which is empirically false at the micro level, as well as for its implications.
A neat recent solution to this paradox about the irrationality of agreeing to disagree was a paper that noted that if we consider the number of facts people know, the number of combinations of those facts rises exponentially. Thus the state space blows up, and it is impossible to reverse engineer what combination of facts someone else is using to form their opinion in finite time, even if you know their facts, because there are so many combinations of facts that may be the driving distinction. If you disagree, you simply cannot reverse engineer your different assumptions.

In a similar way, even after trying to supplement your formal analysis of why a strategy is a good investment with words that articulate your reasons, the connections between your data and your final alpha estimation are simply too convoluted for others to fully grasp, and so we often agree to disagree. No test is definitive, because judgment calls are inevitable. Did the analysis control for non-normality? Are the data on interest rates from before leaving the gold standard relevant for the behavior of interest rates after we left the gold standard? Are data from mortgage defaults relevant to credit card charge-offs? Many of these issues are considered either irrelevant, or of essential importance, depending on the individual, for reasons too complex to ever fully delineate. The alpha you propose is necessarily risky, a statement about your judgment, and while it is good to apply mean variance to your final estimates of means and covariances with your portfolio, the latter tactic is trivial compared to the alpha judgment call.

WHY WE TAKE RISK ANYWAY

People do not apply a sharp distinction to financial risks versus the risks they take every day, attempting a joke, arguing for a plan based on an analogy. Most decisions we make about our education, including informal education such as learning a skill useful for planning parties, or building our own deck, involve an investment of time and money, and have an uncertain outcome. Investing in stocks is just part of that continuum.

The rewards of risk taking must be seen as based on an optimal rule, not an optimal act. An example of this rule approach is the ultimatum game where two players interact to decide how to divide $100 that is given to them. The first player proposes how to divide the sum between themselves, and the second player can either accept or reject this proposal. If the second player rejects, neither player receives anything. If the second player accepts, the money is split according to the proposal. The game is played only once, and anonymously, so that reciprocation is not an issue. Rationally, the second player should accept anything above zero, because there are no reputational
effects by construction, and $1 is worth more than nothing. Offers of less than $20 were generally rejected.  

Nobel laureate Robert Aumann argues that this is an example of rule rationality over act rationality. One is following the rule not to be a chump, because to do so has negative reputational effects. That people follow this rule in this game, in his opinion is irrational—the second player should take the $10—but this is completely understandable because people have evolved some intuitive strategic rules ("Don't be a pushover"). The application of the rule may be irrational in any one case, but when trying to describe actual behavior, the rule rationality dominates act rationality. It seems reasonable that such a rule would arise over time, because such a rule can dominate the alternative, having to calculate the rationality of every specific act separately. So people adopt rules that work on average because the lost opportunities are small relative to the cost of having to evaluate every act without a rule.

The rule of taking risk, in spite of naysayers, skepticism, a lack of objective evidence, is a good one. As Ray Bradbury says, "You've got to jump off cliffs all the time and build your wings on the way down." You have to take risk to build those wings, because only real-life risk generates the incentives and feedback needed to become an expert at something related to the initial objective, and the payoffs are often serendipitous. For example, many baseball coaches were former minor league players who never achieved their original goal, but only through this experience could they learn how to be good managers. Consider that Jacques Morali and Leonard Bernstein wanted to be famous classical composers, and fretted about their inadequacy in this quest their entire lives, yet they gave us the song "YMCA" and the score to West Side Story, not quite their dreams, but spectacular successes nonetheless. The lure is often impractical—our dreams usually are—but these dreams, in combination with hard work and feedback can generate objective success. Having dreams, and more important, actually acting on them, motivates us to succeed, often in ways we never could have expected.

When I was putting together a risk management project for a regional bank in the 1990s, I had to purchase some software to calculate various value-at-risk statistics, and it was better to buy it than build it from scratch. I bought one product because it had the greatest scope of products and could accommodate a variety of risk metrics: parametric value at risk, a Monte Carlo value at risk, stress tests, arbitrary scenarios and so forth. Interestingly, the company was run by a man who centered his initial pitch based on a "patented optimization algorithm" within the software. His primary motivation for building this tool was that this optimization algorithm would allow someone to allocate capital, and hedge risks, more efficiently, as he had built his capital stake on the basis of creating a way for a firm to replicate an index using a subset of that index. He thought, similarly, his algorithm had a more general application to almost any large, complex
trading operation, not just one trying to replicate an index return most efficiently. Yet for a trading desk, general optimization algorithms are not very useful, in that most traders hedged their trades adequately, and they were not about to transfer any of their responsibilities to staffers in risk management. Furthermore, proprietary hill-climbing techniques in optimization are a dime a dozen. But the software was good at the problem we wanted to solve. He developed a useful solution for a more prosaic problem incidentally, while failing at his primary objective targeting a much sexier objective, and his company is now tremendously successful. His incorrect estimation of his alpha and prioritization of the high-profile solution was necessary to finding his more prosaic niche.

A life filled with hope connects you to the future with optimism, and optimistic people are happier, and more fun to be around. People who hold positive illusions about themselves, their abilities, and their future prospects are mentally healthier, happier, and better liked than people who lack such illusions. Taking risk is essential to fulfilling one's potential, finding your best fit in a complex society that has lots of parochial niches that little children do not esteem or understand. Taking no risk, taking the first sure thing job, date, or investment that came along, implies we probably could have found a better fit if we actually took some risks, even if we failed at first.

There are many who say one must experience risk taking to fully appreciate it, as in the famous line about how explaining investing to someone who has not invested is like explaining sex to a virgin. Yet, taking risk is not nearly as foreign as sex, because the anxieties and insecurities in attempting to ply alpha is something everyone can relate to, especially during the insecure adolescent years, when we were very sensitive to how our peers viewed our individuality. Financial risk taking is about appearing foolish, not bold. If everyone thinks what you are doing is merely bold, not foolish, you are not taking risk, in contrast to what standard theory implies.

The hope that underlies risk taking is good or bad only to the degree it helps us become better people, or more successful. Dreams often serve as goals to arrange our priorities, and affect short-term goals, such as finishing a project, or long-term goals, such as choosing a major in college. Each dream requires small steps and these practical steps build a strong foundation for success. People like their dreams, and these dreams often make them better people. Like Darwinian evolution, this path of discovery is not random experimentation, but experimentation guided by selection, and so it is not so much having a dream, but acting on them wisely: choosing intelligent tactics, responding to feedback, and sometimes giving up and starting over. The best way to have a good idea is to have a lot of them, and actually implement them.

You have to see alpha-based risk taking as generating two paths. In one, you learn you have no alpha and are merely gambling, and upon this
recognition, alter your strategy or change your objective. In the other, you find you have alpha, and play again and again. This is the hope present in alpha, because if you have some alpha, you can leverage this in many ways, over time, through repetition, borrowing. The result is decidedly nonlinear, so the initial risk is really a sampler. Consider if you play blackjack. If you have no alpha, but merely play the optimal strategy, you lose 0.5 percent per hand. The more you sit, the more certain you lose, as the law of large numbers catches up to you. After reading Ed Thorpe's *Beat the Dealer*, if you *can* count cards sufficiently, you can make 1.5 percent per hand and have yourself a new career. But until you take risk, until you actually try it, you don’t know if it’s feasible. Is it worth three hours sitting at a blackjack table to try? For a few, yes; for most people, no.

People take risk based on hope, and hope is a function of one’s dreams. In 1961, Walter Gutman noted that “growth stocks might better be called dream stocks,” and that “dreams are real—we have them every day. It’s a big mistake to think dreams are unreal and what is called real life is real.”

This is a simple, profound, model of the value effect, where stodgy, low beta firms without much upside generate a higher return than stocks that can be classified by some as the next Yahoo! Dreams, in moderation, of course, are paradoxically as real as anything, and assets that embolden dreams have extra value for investors, and they are willing to accept a lower average return because in taking risk, they are already ignoring the skepticism of the consensus.

Just as we tell our kids that as long as they try their best, they should hold their heads up high, so, too, adults should try their best, learn, and then either modify one’s strategy or goals—or both. As a rule, having dreams, taking risk based on hope is part of a healthy attitude, and in general it is good for finding one’s comparative advantage, a vocation, or avocation, that has high returns relative to merely following the flow. Nonetheless, it drives the irrational behavior we see, where investments with the greatest uncertainty, the highest risk have the lowest average returns, the lack of diversification, the overtrading. Like gambling, much investing is based on too much hope, a misapplication of the risk-taking rule to a game only introduced within too short of time for it to affect the hardwired intuition we have for risk taking. They are the result of a risk-taking rule that is great in general, but not appropriate for financial markets. Most people should focus their risk capital in areas less well trodden than merely buying stocks.

**EXPERIMENTS, RISK, AND ALPHA**

Many economists studying risk and the risk premium are not even concerned with finance. *The Journal of Risk and Uncertainty* plainly states they do not
address asset pricing. The idea that risk does not beget a return premium, however, would seem preposterous to them, in that people buy insurance all the time as a way to avoid risk, so people pay a premium, that is, more than the amortized expected cost, to avoid this. Consider the various types of experiments.

- **Introspection.** Beginning with the St. Petersburg Paradox, people have framed questions that lent themselves to obvious answers. Other famous examples include Allais’s Paradox and the Ellsberg Paradox.
- **Laboratory experiments.** In these cases, people take undergraduates and present them with gambles. Gamble A might be a 50–50 chance for $2 versus $1.60 versus Gamble B of a 50–50 chance for $3.85 versus $0.10. Vary the probabilities and payouts, and look at when people choose A over B, and where the cutoff is.18
- **Data from television game show participants.** The benefit of this approach is, unlike experiments funded by grants, these contain very large payoffs. For example, some economists examined the Mexican version of *Deal or No Deal*, where participants choose briefcases opened by attractive ladies that can lead to winning $1 million.19 Most of these game shows are purely random, while some, like choosing how much to bet in Final Jeopardy, involve some skill.20
- **Hypothetical survey questions,** which can involve choices between random outcomes.21 A typical question would be to ask people to consider a fair lottery in which you can double your yearly income with a 50 percent chance while you can lose a percentage, say $x percent of your income in the other 50 percent cases. What is the highest loss $x that you would be willing to incur to accept playing this lottery? The average answer is $x = 23$ percent.
- **Data drawn from market decisions,** such as whether or not to pay the high or low deductible on an insurance policy, where a high deductible generates a lower cost, but exposes one to more small losses. Or they might look at a decision to buy the extended warranty when purchasing expensive electronics.

Research in this area gets pretty involved. For example, consider this from the 2007 *Journal of Risk and Uncertainty*:

*We use survey data on income and experimental data on bet choice in a risk game to calculate rural Paraguayans' coefficients of relative risk aversion.... We surveyed 223 rural households in Paraguay in 2002. All households who participated in the survey were invited to send one household member to participate in economic*
experiments and 188 chose to do so. The rules of the risk game were as follows: The player was given 8,000 guaranies (two-thirds of a day's wages) and could choose to bet nothing, 2,000, 4,000, 6,000, or all 8,000 guaranies. The experimenter then rolled a die to determine the player's payoffs.22

Now, the thought of earnest economists traveling to rural Paraguay to observe locals playing dice is kind of amusing, as if the exoticness of the situation makes it a more authentic measurement of risk preferences. But such is experimental work, for which Vernon Smith won the Nobel Prize in 2002.

These experiments lack the essence of real financial markets for all the reasons I argue are essential. First, a game hardly presents a benchmark relevant to a peer group one is interested in. Making money relative to a group of fellow experiment participants is not the same as losing 10 percent when the market goes up 10 percent. Experiments are inevitably idiosyncratic risk. Furthermore, there is no need for certainty, because there is no chance these experiments will bankrupt them. Lastly, there is trivial hope in these games, so no lure from dreams, because there is no option value from discovering that one has alpha in these games, as they are one-off little gambles, with payoffs too small to generate dreams.

Choosing my preferences for lotteries, with their explicit randomness, is quite different from choosing my preferences for engaging in actual activity that is partly a function of skill, effort, and luck, at least in regard to market activity like investment or occupational choice, because people self-select into arenas where they have reason to believe their odds are better than average, and take risks there. One's desire, or aversion, is totally different when facing random volatility I cannot control, than in cases where I think I may have alpha. In this way, risk is like sex: In one context, you might pay for it, and in another pay to avoid it (implicitly, of course). It might be the same activity, but because of the context and the resulting effect on my preference toward it, they are alike only to a mind-blind robot.

Peter Bossaerts has done a lot of empirical work in this vein, where he sets up simulated markets and observes the market prices, and he describes the situation as follows:

A number of subjects are endowed with a set of securities whose liquidation values depend on the realization of a state that is randomly drawn with commonly known probabilities (usually equal likelihood). The subjects are allowed to trade securities during certain periods before the state is drawn and liquidation values are observed.23
And indeed Bossaerts’s experiments find that people try to maximize their Sharpe ratio (return divided by volatility) as the equilibrium, consistent with standard theory. But in the real world, most trading is based on risk-taking, in which deviations from the CAPM are predicated on perceived alpha, which is generally an undiversified bet on something. The fact that the CAPM results when you remove alpha and generate idiosyncratic risks to participants, merely highlights the importance of alpha and context to actual risk taking. Yet this is what financial economists presume, and that’s why asset pricing is so sterile. The stock market contains a wide variety of dreams, and is a benchmark for investors. An experiment with known expected returns and covariances is not a benchmark, nor does it plausibly contain any alpha. The results are merely relevant for those types of risk, like those of insuring against a car accident.

Most risk taking is a combination of hope and uncertainty, and so the fact that the objective odds are against them is of little relevance, because taking risk is about bucking the odds. If you thought you were relevant to the unconditional data, you would not be taking the risk, but your hope is based on some reason to think you are special. Thus, in equilibrium, highly volatile assets generate lower-than-average returns, because the averages are like conventional wisdom, which is necessarily ignored in risk taking.

The three assumptions presented are a model of asset pricing, far more parsimonious than the alternative. It generates strong falsifiable implications: that risk metrics will generally (that is, usually), be uncorrelated with return because benchmarking implies risk is symmetric, and therefore not priced; that really safe, and really risky, assets will have lower-than-average returns, the first because it satisfies a craving for certainty, the latter because it plays into our hopes. The standard current explanation is that ultimately, returns are a function of risk and luck, though there are no robust measures of risks, merely a collection of anomalies, some of which are rebranded as risk factors. Furthermore, the obvious inefficiency of most portfolios—too much trading, too little diversification—must be explained by overconfidence in any theory, but my approach notes the consistency of overconfidence in investing with the approach people take toward all risky decisions.

Your average investor has many choices to make, whether to invest in stocks or bonds, or which stocks to buy. One way to look at this decision, the economist’s way, is to assume the decision maker knows only what everyone else knows, and then calculate an optimal portfolio for himself. In contrast, risky decisions are an intrinsic part of life, where one is on a personal journey to find one’s comparative advantage. This causes many people to approach risk taking in securities markets like they view the risks they take every day, defying skepticism of people like myself, and relying on hope and moxie to
make something successful. Thus the demand for highly undiversified bets that have, on average, a poor return. These decisions are irrational when looked at in isolation, but they follow a rational rule. Wisdom benefits any risk taker, and understanding which risks are gambles, and which potentially have alpha, is why wisdom and hard work is correlated with market success, because having good ideas, and getting feedback by actually doing things, makes for better decisions.