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The Education of a Quant

Locked into a single risk model? Better no model at all!

Risk managers love risk. Not in the sense everyone claims to love it, when it is “bold,” and “innovative,” and “outside the box,” as opposed to “reckless.” That is usually judged after the fact. Not as a necessary accompaniment to expected return, a cost accepted to get a benefit. Modern financial risk management as invented in the late 1980s teaches specific quantitative reasons to love risk. These are not hard to understand – in fact, it takes considerable education to confuse people on the issue. There are still people in economics and finance who reflexively model risk as a cost to be minimized subject to an expected return constraint.

I could trace the error back 350 years, but I’ll start in the early 1950s. An Economics graduate student named Harry Markowitz sat in the University of Chicago library trying to come up with a dissertation topic. He was reading an analysis of stock prices, and mused on the question, “Why don’t investors put all their money in whichever stock seems best?” He hit on an answer, “Because that would be too risky.” He worked out a system in which investors try to maximize expected return and minimize risk. This led to the development of Modern Portfolio Theory, which has had extraordinary influence on the financial system (although Harry’s dissertation chairman, Milton Friedman, is said to have observed that it was “nice work, but not Economics”).

Modern Portfolio Theory was a major advance, but when teaching it today, the hard part is explaining why it was not obvious, at least to students familiar with quantitative economics. Markowitz’s formulation has become so deeply ingrained in economic thinking that many students have trouble understanding the many rational alternatives.

Around the same time, John Kelly was thinking about the same question. But he wasn’t in a library. He was probably doing daredevil stunts in an airplane (he had been a fighter pilot in WWII) or winning a pistol competition (he was a genuine Texas gunslinger). He wasn’t at the University of Chicago with Milton Friedman as a mentor, he was a Physics PhD at Bell Labs looking up to Claude Shannon. Fatefully, Kelly asked the question about horse race bettors rather than stock market investors, which led many people to dismiss his results as frivolous.

Had Kelly phrased his argument in terms of stocks it would have been something like, “Because buying one stock virtually guarantees doing worse than buying a larger portfolio. It’s not riskier, it’s just worse.” It’s easier to illustrate this point with commodity futures than with stocks, because stocks do messy things like merge and liquidate. However, the mathematical underpinning is identical.

Table 1 shows the average returns and standard deviations of returns for seven commodities from 1970 to 2010. These are zero-investment returns, so you can think of them as returns in addition to the rate you would earn with low-risk investments like treasury bills, or more or less the same thing, returns after inflation. Cocoa, for example, went up 4.9 percent more than low-risk bonds per year on average, but with a large standard deviation of 30.1 percent. Over the last forty years, it would have turned a $1000 investment into $1106 in constant dollars.

That might seem surprising. 4.9 percent per year times 40 years is 196 percent, why did the investment return only 10.6 percent over the period? Volatility drag. If cocoa goes up 20 percent this year and down 20 percent next year, you’re down 4 percent, not even. A better approximation to the long-term return is 40 x (mean – standard deviation^2). For cotton, hogs, silver, and

Table 1. Average returns and standard deviations of returns for seven commodities from 1970 to 2010

Commodity	Average annual return	Standard deviation	Total return 1970–2010
Cocoa	4.9%	30.1%	10.6%
Corn	−3.5%	22.5%	−91.7%
Cotton	2.4%	22.7%	−6.4%
Hogs	1.6%	24.2%	−42.0%
Silver	2.8%	29.2%	−45.9%
Sugar	5.1%	38.4%	−62.0%
Wheat	−0.5%	25.5%	−78.3%
Portfolio	1.8%	14.3%	34.4%

Table 2. Total returns and optimal investment amounts for seven commodities from 1970 to 2010

Commodity	100% Investment total return 1970–2010	Optimal investment amount	Optimal investment total return 1970–2010
Cocoa	10.6%	52.7%	67.0%
Corn	−91.7%	−70.2%	66.4%
Cotton	−6.4%	46.8%	25.9%
Hogs	−42.0%	27.2%	9.3%
Silver	−45.9%	32.5%	20.1%
Sugar	−62.0%	33.9%	41.1%
Wheat	−78.3%	−7.7%	0.8%
Portfolio	34.4%	85.7%	35.5%

sugar, volatility turned positive average returns into negative returns for the period.

In Markowitz's formulation, investors wouldn't pick one commodity, even if they thought it had the best prospect, because they didn't like high standard deviations. But look at the last line in the table above, which shows the result of splitting your money equally among the seven commodities. It returns 34.4 percent over the period, much better than even the best individual choice. Five of the commodities have higher average return, but the portfolio still dominates because it has less volatility drag. To Kelly, it's not the fear of picking the wrong commodity that leads investors to diversify, it's knowing that even the right commodity will probably do worse than a portfolio.

At first glance, we just seem to have provided a reason for Markowitz's assertion that investors dislike risk. But the distinction is much deeper. Markowitz assumed people disliked risk on the grounds of utility theory, or because it was psychologically distressing, or because it made planning more difficult. Note that this treats risk as a cost, no different from taxes, or fees, or anything else. You minimize risk, subject to an expected return constraint. Kelly said there was an optimal amount of risk. You didn't take it in order to get a better expected return, you took it because it guaranteed doing better.

The real problem with the individual commodity investments above was not that they were undiversified, it was that putting 100 percent of your investment in each was overinvesting. Table 2 shows how you could have done with perfect foresight, if you had invested the correct amount in each commodity. For example, if you had put 52.7 percent of your money in cocoa and the rest in low-risk bonds, you would have made 67 percent over the period from 1970 to 2010, instead of the 10.6 percent you made putting 100 percent of your money in cocoa. In sugar, you turn a −62 percent return into a +41.1 percent return, merely by keeping two-thirds of your money in the bank. But in all cases, you end up worse if you take either less risk or more risk than the optimal amount (we're cheating a bit with corn and wheat, since perfect foresight would have led to short investments).

The reason the portfolio did so much better than the individual assets when we invested 100 percent of our money is not that diversification lowers risk and risk is bad, it's that it just happened to produce a portfolio with near the optimal amount of risk (we see above that we can improve even the portfolio a little by keeping 14.3 percent of our money in low-risk bonds).

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Economics and risk

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Markowitz's ideas go back to the early development of probability theory, which studied gambling games. This is an unusual kind of risk. You know the distribution of outcomes with perfect certainty (by assumption) and know nothing at all about the realization (that is, you can't gain additional information or wait for events to develop, that's "cheating"). It's like physics without air resistance, a useful simplification for some problems (like watching a chandelier swing in a cathedral) but horribly misleading for others (like designing an airplane). Traditional risk models can work pretty well for coin flips if nobody cheats and nothing unexpected happens, but aren't useful for most risks that come up in practice.

The simple form of utility theory Markowitz used is not based on logic or psychology – or indeed any form of analysis or observation. It's a clumsy fix to explain why people sometimes turn down gambles with positive expected value (and completely fails to explain why people very often accept gambles with negative expected value). It is usually explained with an example like water. If you have a liter of water a day, you drink it and value it above almost anything since it's necessary for survival. A second liter is welcome, but worth less. The more water you get, the less valuable uses you put it to.

This is another highly unrealistic state. A person without enough water moves to where there

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is more water, or figures out another way to get it, or dies. People often value familiar things in plentiful supply above exotic rare things (codfish cheeks are rare at a baseball game, hot dogs and beer are easily available, but people will pay a lot more for the hot dogs and beer). As the world has become far richer, people seem to value wealth increases more rather than less. Many things, such as knowledge, seem more valuable the more you have. I don't say there's no insight in the idea of declining marginal utility, but it's not the only way to think of the world.

Kelly had his own simplifications: an infinite series of independent bets, with wealth as both the goal and the constraint. These are no more realistic than Markowitz's, but there are problems for which Kelly gives a reasonable answer and Markowitz does not (and questions for which the reverse is true).

Subsequent researchers have relaxed the assumptions from both models and extended the results. But the simplest forms of one model or the other are still carried around in a lot of people's heads as the only rational view of risk. They are embedded in popular tools that obscure the assumptions.

What's strange about this is that the three centuries since dice-model/risk-is-always-bad was invented have seen a variety of well-known scientific breakthroughs that required entirely different ideas about risk. Practicing risk managers have to be thoroughly familiar with all of them.

Evolution

Random mutation plus natural selection created life and builds far more complex and amazing structures than humans have managed. The brilliant statistician and geneticist R. A. Fisher worked out the basic mathematics almost a century ago, computing the right level of randomness for species survival and improvement. But evolutionary randomness is not at all like a dice throw. It occurs on many levels simultaneously: selfish genes, individuals, populations, ecosystems, and perhaps even Gaia, the web of all living things. Evolution shapes the distribution of throws, for example by inventing sex. If you treat evolutionary randomness as coming from a fixed, known distribution you will misunderstand the whole thing.

Suppose we apply the evolutionary model to capital budgeting. In the standard formulation, projects are penalized for risk by applying a higher rate of return (which isn't even logically consistent, it implies we should prefer risky future liabilities to certain ones with the same expected values). Risk managers understand that having a variety of high-risk projects gives an organization a higher probability of survival and keeps it improving. It is essential to apply rigorous selection, to make sure people "fail fast" rather than throwing money into pet black holes.

In security valuation, standard theory securities require higher expected return if they add more risk to a diversified portfolio. That leads people to expect that of two stocks with similar expected future cash flows, the one that has more historical price volatility is worth less. But that is not how we value most things. We assume that a person or entity that has survived more challenges is a better future bet than an untested one. That's why interviewers always ask about obstacles the candidate has overcome.

Nassim Taleb correctly points out that there is a fallacious version of this belief, that adversity makes things stronger. Evolution teaches that the key is selection. If the person has replaced deficient habits and attitudes with fitter ones, or the entity has changed itself in response to crises, the battle-hardened version might be better than one never forced to make changes.

Of course, this is not always true. The company with more historical volatility may be in an inherently more risky business, or be badly run. But a risk manager must be open to both possibilities.

In thinking about financial markets, evolution leads naturally to consider market organization as a product of natural selection. There are many ways to transact – open outcry, periodic auctions, over the counter – with many others, all in many variants. You can design membership, margin, and clearing rules, and a hundred other details. All of these affect who trades at what prices. Historically, some forms have thrived, attracting providers and users of capital, and others are extinct.

A traditional economist looks at the volatility in the financial markets and argues that it is excessive given uncertainty about future cash flows. The "irrational exuberance" and "animal spirits" seem to come from excitable traders yelling and screaming, deluded speculators, and evil manipulators. An evolutionary thinker assumes there is some reason the excess risk developed, and that tinkering with it is as dangerous as doing surgery without any knowledge of anatomy. There are many examples in nature of behavior that creates risk not imposed by the environment: fights among creatures of the same species and sexual reproduction for examples.

In this case, the traditional model cannot explain persistent features of markets, nor why some markets thrive, nor the tremendous effect of markets on the real economy (which clearly goes far beyond the direct effect of transactions and the value of price discovery). That doesn't mean the evolutionary perspective is correct, but it's certainly worth considering.

How far to take it

Some people read a popular account of scientific advances (and sometimes not even that) and immediately find application everywhere of what they imagine the principles to be. This is usually no better than mysticism.

At the other extreme, serious professional researchers will sometimes apply tools outside their native field. Finance has benefited from this with natural language processing and computational physics. I think the jury is still out on "Econophysics," but it is at least a serious attempt to broaden the study of finance.

I'm discussing something in the middle, using insights from other fields to get fresh perspectives. Doing it properly requires a real understanding of both the contributor and beneficiary fields. I think this is best achieved by working on a team studying interesting and original problems. A common question on Wilmott message boards is, "What courses should I take to become a financial quant?" I think the answer is to get out of the classroom and sign up as a statistician or scientific programmer or mathematical modeler on research projects in physics, chemistry, biology, medicine, and any other field you can find. Learn from geniuses, regardless of field, and steal their tools. Get your hands dirty. A few years of this and you will be far better qualified to contribute to quantitative finance than someone who spent the same time memorizing the solutions to stochastic calculus problems and slogging through finance papers. I don't deny the value of a thorough education in finance and financial mathematics, but without broad practical hands-on skills you're an overpaid calculating machine. Oh, and try to keep up with the fields, at least in some specialized areas. New ideas have a way of generating more new ideas.

Quantum physics

In the Newtonian universe, everything is determined. Randomness cannot exist, a coin flip is no more random than a planetary orbit, we just haven't done the calculation to know which side will come up. But when the 20th century studied small things, the truth turned out to be the reverse. Randomness was the basic state of nature, determinism is a statistical illusion.

This randomness is much different from a coin flip. All possible outcomes interact with each other unless an observation is taken, at which time reality collapses to one outcome. Could this be the case in economics? Suppose there were only two equilibrium paths for oil prices: a low-price path in which producer cartels were weak, consumption was high, economic progress was rapid, and technological advances found new oil and replacements faster than supplies were exhausted; and a high-price path in which producer cartels were strong, the economy and technological advances were slow. Everyone knew these were the only two possible paths.

If this was a coin flip, one path would be chosen; everyone would immediately know it and react accordingly. In neither path would there be any need for strategic storage, only convenience storage. There would be no waste, refineries built and then abandoned, exploration of fields that are never developed, buildings requiring expensive retrofitting to improve energy efficiency. These features are key to understanding energy economics and they exist because doubt exists about which path will be taken. The actual price of oil will not follow either path, it will jump around in a seemingly random manner, inconsistent with any equilibrium. It will not be any sort of average of both states, it will be an entirely new kind of path created by superposition.

Planned economies attempt to eliminate waste by forcing a path. This never works. The usual reason is that the wrong prices are chosen. That may be true, but I think even the right prices would fail. I think any consistent path would lead to economic breakdown, that a vibrant economy requires superposition of many possible future states. The Socialist desire to neaten everything up and avoid waste conflicts with the randomness inherent to progress.

For business decision-making, this argues against good–average–bad case scenario analysis. The right decision is often one that makes sense in no scenario, and a decision that works well in each scenario (like having no strategic storage) may work terribly in practice.

In markets, it suggests that transparent public trading can alter the price behavior of a security, just as observation alters the behavior of a particle. This certainly seems to be the case. If you force all trading to be public and all securities to be liquid, the only equilibrium is one in which all markets clear. But if you allow some prices to be unobserved, people will have to account for all possible values, and may act totally differently than they would at any disclosed price. That makes the decision of what instruments to trade in liquid public form a crucial one, and one that traditional economics cannot even address.

We know that too little trading and transparency is bad. Markets seem to be a prerequisite of economic growth and when pricing doubts get too large in a developed economy, we have a destructive liquidity crisis. But too much trading and transparency seems to be a problem as well, forcing short-term balancing that is difficult to reconcile with efficient business decisions. We see companies gain value when taken private, and we know there are futures contracts that seem to have economic relevance but never attract active trading (while many of the actively traded ones seem of marginal relevance).

In this view, the right amount of risk, and the right amount of waste, and the right amount of measurement is important. None of these things are good or bad in themselves, they are things to optimize, not minimize or maximize.

Game theory

One of the surprising discoveries of game theory is that risk is often good. The best strategy can include introducing artificial randomness into a situation.

An example of the value of thinking this way is the 1992 battle between the UK Treasury and hedge funds over the value of the pound versus the deutsche mark.. The Treasury was reportedly confident because it had ten times the capital that was thought to be available to the hedge funds. As long as it kept buying the pounds that the hedge funds were selling, the price would remain at or above the floor level. As long as everyone was confident the Treasury could do this, there would be no run on the bank.

The hedge funds viewed the situation differently. There was some chance the pound would break through the floor, in which case it would be dumped by many investors and fall even more. There was some chance the pound would maintain its value. But there was no chance the pound would appreciate significantly. The fundamental economic value was not there, even if the government took the unlikely step of raising already high interest rates in a recession. With some upside and no downside, the hedge funds were willing to lever their holdings to the level required to break the pound.

Had the UK Treasury been staffed by game theorists (or poker players), they might have viewed the situation from the standpoint of the enemy. Success required not defending the pound, but giving it too much volatility to leverage. A risky pound could conceivably be safe; a safe pound could only crash in value.

I'm not claiming that I could have saved the pound in 1992; there may have been no way to do that. But thinking about things in game theory terms might have suggested valuable new ideas.

Kelly, evolution, quantum physics, and game theory are only four examples of risk models in which risk can be good. All of them are at least useful thought exercises for financial quants, and some of them might provide deep insights into the markets. If you are locked into a single risk model, especially one based on gambling games and pre-Von Neumann utility theory, you don't know risk.

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