The CAPM is still considered a first-order intellectual achievement, in spite of the current thought leaders also describing it as being “empirically vacuous” (Fama and French, 2006) or that “having a low, middle or high beta does not matter; the expected return is the same” (Ross, 1993).\(^1\) Indeed, I would say the situation is worse, as volatility and beta are generally negatively correlated with returns.

Its extensions have proven equally impoverished. There’s clearly a greater truth at work in this case, the greater truth is that some asset pricing model will work, and it should have the neat CAPM properties of being linear in risk factors, not include residual risk, and include something very like the market as one of the prominent factors. The theory has little equal by way of praise from economists, who see the success of derivatives as proving the risk-reward portion of academic finance as manifestly fruitful. I doubt even Keynesianism had such high approval ratings at its apex. But the flaw of the CAPM isn’t that it is not perfect, but rather does not predict relative returns, even slightly, quite different from the flaws of Newtonian mechanics, which become apparent near the speed of light.

The golden age of the CAPM was from 1972 to 1992, from Fama’s initial confirmation of the theory to his rejection of it. Not that it ever really worked, or that portfolio managers ever mainly relied on beta in constructing portfolios. No, it was that among experts, the belief was that the CAPM was an innovation in our abstract understanding that would stand the test of time like Euclid’s Elements. Refined, extended, to be sure, but never junked. The concurrent development of the APT, and the general equilibrium SDF approach, were merely extensions. To the extent CAPM had difficulties; these more general approaches would fix them, but the basic ideas of the CAPM were paramount: linearity in pricing, and the positive premium of some systematic risk factors. It was further assumed that the
main factor would be the market, and so a multifactor approach would merely add second order terms, in a very consistent way.

Today, beta is generally recognized as a risk factor for many investments, in that any good hedge fund wishes to have a low beta; yet for stocks, or funds, that perforce have positive beta, it is an afterthought. In a Bloomberg screen, it is listed inconspicuously with 10 other stock characteristics like P/E. In Morningstar’s reports, you can’t even find the beta for stocks or funds, because they replace this with a bevy of idiosyncratic ratios and subjective ratings (for example, “four stars out of five”). Academics, meanwhile, would never assume that merely controlling for beta controls for risk, unless they are explaining risk in MBA texts.

But this did not diminish the praise for this line of research, an intellectual achievement that was supposedly practical and profound, intuitive and elegant. The shadow cast by the initial apparent success of the theory created a strange longing for a time of order, even if that order was based on ignorance. As prominent finance professor John Cochrane wrote, the CAPM “proved stunningly successful in a quarter century of empirical work,” meaning it seemed to explain the data pretty well, until we found out this was merely due to beta picking up the size effect, which itself was mainly measurement risk in errors.2

This longing for a prior sense of ignorance, when a popular theory appeared to be working, is a rather bizarre stance for an economist. It’s like saying how fun Christmas was when we believed in Santa Claus. True, but that’s harmless fun, not science. The CAPM was never a benign conspiracy of professors to create joy in MBAs, but a serious hypothesis about the way the world works. In a similar vein, Paul Samuelson stated about Karl Marx in his 1995 edition of Economics, after the fall of the Berlin Wall reduced the allocation of his text on Marxism to a footnote: “Marx was wrong about many things—notably the superiority of socialism as an economic system—but that does not diminish his stature as an important economist.”3

Ah, the “being wrong” part—never mind!—Marx was popular and inspirational. Clearly, economists see theories in context larger than falsifiable predictions. Any idea that generates a large literature, supposedly, is good. The thought that an idea was edifying, even if wrong, is comforting to academics who often investigate unproven ideas. I disagree. There are an infinite number of bad ideas, so eliminating many of them puts hardly a dent in the shelf from which bad ideas are drawn. This is especially true when an error merely postpones an inevitable education with reality, like saying drinking and driving was a great idea until I hit a school bus. The fact that economists as preeminent as Cochrane and Samuelson adopt the same stance toward what constitutes a successful theory (ephemeral popularity
Undiminished Praise of a Vacuous Theory

among scientists), highlights the strange power of any idea that spawns a large literature.

Peter Bernstein wrote the best-seller *Capital Ideas*, which presents the modern finance architects as heroes and mavericks; one can almost hear the "Ride of the Valkyries" as one reads his firsthand account of their achievements. Even books that note the CAPM, or the APT, were empirically vacuous, paying homage to the standard theory in obligatory fashion; harping on its flaws was like criticizing Newton for not accounting for special relativity. Derivatives pioneer Mark Rubinstein's 2002 homage to Markowitz noted that

*Near the end of his reign in 14 AD, the Roman emperor Augustus could boast that he had found Rome a city of brick and left it a city of marble. Markowitz can boast that he found the field of finance awash in the imprecision of English and left it with the scientific precision and insight made possible only by mathematics.*

Subsequent honors on the creators of the current theory have never been moderated, which is often the case for findings that create a field. For example, Carson's *Silent Spring*, Mead's *Coming of Age in Samoa*, or Kinsey's *Sexual Behavior and the Human Male* are now found to be seriously flawed to the point of being really manifestations of the author's prejudices, yet, they remain canonical works nonetheless. A recent survey by Ivo Welch of finance professors noted that 75 percent would recommend using the CAPM for capital budgeting, 10 percent the Fama-French model, and 5 percent some unspecified APT. So, the CAPM is holding up pretty well in spite of its failures, if only because the alternatives have little intuition and no consensus. A set of video lectures on the history of finance had four videos as of 2007, and two were from Sharpe and Markowitz. In short, they are still the fathers of academic finance for their work on what became the CAPM, which is the basis for current asset pricing theory. But it strikes me as odd, because I don't see it as merely patronizing niceness, or being nice to old men because it's better to be polite than honest; I see it as a genuine belief by this cohort that the insights created in the development of the CAPM will survive like the insights of quantum theory from the 1920s. They praise the developers of the CAPM, not because their theory works, not because its refinements work, but because there is faith that a solution will be found that is a consistent extension of the CAPM.

As the mind is very good at explaining reality, if not predicting it, financial economists are quick to make sloppy analogies to support the idea that risk, as economists understand it, is important. But there is a problem, which is that the risk that is important is very unlike the risk
relevant to expected return in theory. For example, Cecchetti’s textbook *Money, Banking, and Financial Markets*, immediately presents the seemingly straightforward example of how bonds with higher default rates have higher yields: Risk and return go together. Yet, this is purely an anticipation of the default rates, and so is not risk in the sense of something priced. As noted, BBB bonds have, over time, about the same total return as B rated bonds. One must subtract the expected defaults and the resulting losses from a stated yield regardless of one’s risk tolerance. The essential, and largely successful and ubiquitous usage of one flavor of risk—the mere statistical volatility and loss estimation—does not imply the second flavor of risk relating to a priced factor affecting future returns as also ubiquitous and essential. As the distinction between or default risk by itself and priced risk is a fundamental distinction in modern risk-return theory, the common usage of default risks when it generates an intuitive support at 30,000 feet suggests that financial professionals have a strong, perhaps unconscious, bias toward the big idea: Risk begets higher returns.

Another misplaced example of success is derivatives, which are obviously very important and useful. The really innovative portion of the Black-Scholes-Merton discovery about options was not the general formula itself, which had been published 10 years earlier by several authors. They proved that, because of arbitrage, an option can be priced as if it were a claim on a risk-neutral asset. The risk premium was irrelevant because of a dynamic hedging argument. With the price of risk out of the derivatives picture, you could generate derivative pricing models like Black-Scholes using just the probability distribution, because in this case, in a state-contingent pricing perspective, the risk premiums are all zero. This leaves a classic engineering problem, complex but soluble, and no different from the naïve expected value problem using risk-free interest rates.

Derivative pricing models are the crowning success of finance, which is the most successful field within economics, which is the most successful social science. Yet the success of the risk modeling in derivatives is really just the success of using expected value, assuming no price of risk. That is, though theoretically the SDF approach encompasses a really complex set of DFs, in practice all the DFs were the same because arbitrage implies the price of risk is irrelevant. The DFs were indexed only by time, not by states of nature. Nevertheless, SDF proponents never fail note the spectacular success of their approach in the real world of derivatives, even though the price of risk is turned off, and all it is is payoffs weighted by their probability. It’s like ascribing Watson and Crick’s double helix modeling success to Darwin, plausible at a superficial level—isn’t everything in biology related to Darwin at some level?—but really misleading.
Around 1840, Macaulay wrote a grand history of England, and noted that doctors had historically recounted their field’s successes with an obvious lack of detachment:

*The history of our country during the last hundred and sixty years is eminently the history of physical, moral, and intellectual improvement. And this is the way the history of medicine used to be written, principally by doctors in their retirement, as a form of ancestor-worship (no doubt in the hope that they, too, would become ancestors worthy of worship). In this version, the history of medicine was that of the smooth and triumphant ascent of knowledge and technique, to our current state of unprecedented enlightenment... [but] it is clear that for centuries it possessed no knowledge or skill that could have helped its patients, rather the reverse.*

This was before anesthesia and the theory of germs, a time when visiting a medical doctor was about as useful as visiting a witch doctor. Just as macroeconomics bestowed many Nobel Prizes on its macroeconomists before acknowledging they really had no clue what caused growth or business cycles, so too has the Nobel academy bestowed prizes on economists for truly impressive work, yet work that is hardly useful for an investor. The financial academy presents itself as having a strong foundation, seemingly measuring progress by the amount of papers allocated to its issues. But the fundamental point, that no one can define a risk metric that is intuitive, and explains returns, is not a deficiency that history will remember well about these last 50 years.

A field will be considered fruitful regardless of empirical evidence if there is hope that a solution is possible within the paradigm. When a field’s empirical success stagnates, a framework replaces a theory, and it becomes increasingly mathematical, it is on the wrong path. In this way, modern theorists are like lottery ticket buyers, or people who invest in volatile stocks, operating on hope. The key is the beauty, the mathematical elegance, of the framework. For example, in 1885, Johann Balmer found a straightforward formula that contained all the known frequencies of the spectral lines of hydrogen: each frequency could be written as one fundamental frequency multiplied by the difference between the inverse squares of two whole numbers. The formula’s simplicity in accounting for so varied a set of numbers was intriguing. Then, in 1913, Niels Bohr presented a model that explained the stability of the atom by applying Balmer’s formula for the frequencies of light from hydrogen by adding the notion of quantum leaps from photons hitting or leaving electrons in orbit, altering the mechanical energy of
electrons going around the nucleus. An abstract, elegant (no improper fractions!) mathematical solution was found not merely descriptive, but real.

The lesson of twentieth-century physics, which guides so much mathematical theorizing as a template, is that common sense and intuition is secondary to mathematical beauty and consistency in modeling the real world. Beauty implies truth, and truth, beauty. String theory currently dominates high-level physics research, but has been criticized because it is untestable.

Nevertheless, string theorists in academia keep flourishing. Juan Maldacena is an accomplished young string theorist, currently at the famous Institute for Advanced Study in Princeton, New Jersey. In an interview in Big Ideas, he was asked, “Have you ever thought your ideas may be wrong?” He replied,

Yeah, this is possible. However, the mathematical structure is probably going to be useful for whatever theory is the correct theory. And, what we do ... at least is generate good interesting mathematics that is useful for other things in physics, and I think if it’s not string theory, it will be probably something similar to it.\textsuperscript{11}

And so it is with modern finance, which fully expects the yet unknown risk solution to be built out of the mathematical edifice already created, because the elegance and power of what has been created seems not just capable, but necessary to be any reasonable solution. In the words of leading researcher John Campbell, giving his overview of the state of finance at the millennium, “Precisely because the conditions for the existence of a stochastic discount factor are so general, they place almost no restrictions on financial data.”\textsuperscript{12} The effect of a good theory is to make an accurate view of the world less complicated, not more, but instead modern researchers focus on the framework’s potential as opposed to its predictive power. The stochastic discount factor approach encapsulating CAPM is surely not a coincidence to these researchers, it merely highlights that while currently wrong, it will be able to encompass the ultimate true theory. As Mark Rubinstein says about the CAPM:

\textit{More empirical effort may have been put into testing the CAPM equation than any other result in finance. The results are quite mixed and in many ways discouraging. ... At bottom ... the central message of the CAPM is this: Ceteris paribus, the prices of securities should be higher (or lower) to the extent their payoffs are slanted toward states in which aggregate consumption or aggregate wealth is low (or high). ... The true pricing equation may not take the
exact form of the CAPM, but the enduring belief of many financial economists is that, whatever form it takes, it will at least embody this principle.¹³

The problem is, as the data have become clearer, the theory has become less clear. This is not a sign of a successful theory. Risk started out as merely volatility, and now volatile assets presumably have low risk through some spooky risk factor, and behavioral biases are applied piecemeal to various anomalies.