



Aaron Brown

Whose Life is it Anyway?

A discussion of Derman's biography leads to an answer to the question 'just what do you think you are doing?'

If you have not read Emanuel Derman's *My Life as a Quant: Reflections on Physics and Finance*, put this magazine down now, buy a copy and read it. It is an inspiring autobiography by one of the great financial quants, and it's filled with insightful essays on physics, finance, art, philosophy and life. It is the best available account of what a practicing Wall Street quant actually does. If you are a quant, it will make you a better one. If you are not a quant, it will help you decide if you want to be one, and give you pointers on how to become one.

For all of my admiration of this book, and immense respect for Derman and his work, I think he missed half the soul of quantitative finance. The mood of the book is of a man who knew passion for a tempestuous woman (physics) in his twenties, but after discouragement and loneliness, traded it for comfortable affection and domesticity (finance) in later life. I say his wife is at least as sexy as his mistress, but he takes her for granted (I am speaking metaphorically here, I have never met his real wife, molecular biologist Dr. Eva Derman).



Derman's attitude is common among physicists on Wall Street, as well as other people who had successful academic careers in mathematics or hard sciences. I sometimes see an unpleasantly virulent form of it in unsuccessful mathematicians and scientists. I was trained in finance, and applied mathematics before that. Things look different from my perspective, which I think is shared by most others of my background, as well

as people who started in the front office before learning quant skills.

Nice view from here ...

Naturally, my disagreements start on page one, where Derman gives a history of physics that begins with Newton. What about Copernicus, Brahe, Galileo and Kepler? Newton's IQ might have equaled the four of these guys combined,

but they were the ones who shattered the conventional wisdom, gathered the data and took on the critics. Newton stood on the shoulders of these giants and gained universal (okay, Leibniz didn't like him) admiration. I am amazed at how hard it is to duplicate some of the results in *Principia*, despite knowing the answer and having modern mathematical tools, but I identify with the earlier workers.

Finance does not yet have its Newton. Markowitz is our Copernicus, Fama and Ibbotson our Brahes, Miller, Sinquefeld and Sharpe our Galileos. Black, Scholes and Merton, between them, have come up with one of Kepler's laws, in the form of the Black-Scholes model. We need two more laws, an interest rate model that gives realistic short rate evolution and yield curve shape simultaneously and a model of price dynamics consistent with both implied and actual volatility. Only when Kepler's work is done can we hope (to paraphrase Pope) "Finance and Finance's laws lay hid in night: God said, Let Newton be! and all was light."

Overlooking pioneers in both physics and finance relates to a point on page 51 "I was horrified to notice quant resumes listing the nonexistent degree A.B.D. ..., Since a PhD is by definition a research degree, the main achievement of which is the completion of a piece of original research described in a dissertation, I looked at A.B.D. as a kind of 'Wayne's World' Phd (not!)."

I don't defend A.B.D. as a real degree, and certainly not as an almost-PhD. It is a failure. But all failures are not worthless. Since there are no standards for a nonexistent degree, it can be claimed by people who flunked out their first year of a PhD program, people who were accepted but never showed up and even people who thought about applying once. But a true A.B.D., *in finance*, is a meaningful qualification for a quant. Derman's PhD meant three years of coursework, qualifying exams and four years of original research. In addition, it introduced him to many of the greatest living people in his field, and students of many of those no longer living.

A high-quality finance A.B.D. would have the coursework and qualifying exams, plus some useful research never brought to the level of a dissertation, plus the contact with the leading

lights of the field. Had Derman's PhD been in finance at the University of Chicago, instead of physics at Columbia, I think he would not have written (as he does on page 266), "[P]hysicists turned quants don't expect too much from their theories, though many economists naively do. Perhaps this is because physicists, raised on theories capable of superb divination, know the difference between a fundamental theory and a phenomenological toy ...," He would not need to complete his finance dissertation to absorb the

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tremendous effort and intellectual brilliance that went into making the toy, and the amount of abuse and criticism that had to be endured. That would encourage him to see it as a scale model, not a toy; the first step toward a fundamental theory, not a temporarily useful money-making device.

When things got physical ...

As a PhD student ten years later than Derman, I remember hearing Roger Ibbotson presenting his first (1979) *Stocks, Bonds, Bills and Inflation Yearbook*. Almost the entire talk was devoted to defending the thesis that stocks could go up and interest rates could go down. The argument was basic statistics, applied to a 53-year time period, made to an audience mostly incapable extrapolating over a tenth that period. Since then I have had the pleasure of seeing Ibbotson's numbers confirmed almost exactly, as expert opinion fluctuated wildly year-by-year, and always underestimated the uncertainty in the future.

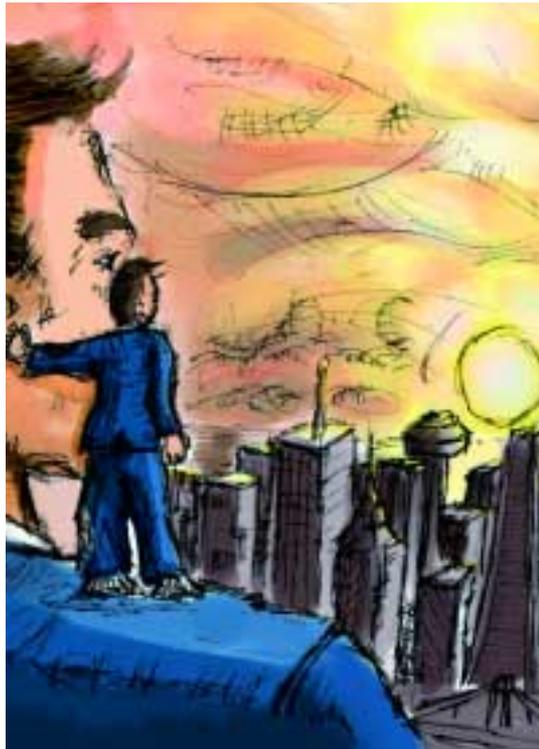
This, and many similar experiences both in academics and practice, give me a much more optimistic view of quantitative financial progress than Derman's. I still thrill to the boldness of the Efficient Market Hypothesis, the

refusal to explain security prices by irrationality, not because irrationality does not exist or is not important, but because irrationality can explain anything, and hypotheses that explain anything cannot be tested. I remember the scorn that was poured on the idea, and the painstaking, thankless work necessary to defend it. I have always struggled to teach students that Modern Portfolio Theory, the proposition that investors care about the statistical properties of their portfolios, was very far from obvious when

Markowitz first wrote it down. I treasure the perfection in the Capital Asset Pricing Model, a necessary advance, even if expected return is non-measurable so the model cannot be tested.

Of course, all this happened before Derman got interested in finance. But he was employed at Goldman Sachs and Salomon Brothers during the entire period from 1988 to 1994 in which VaR was invented. Yet he describes it on page 256 as "[i]nvented in 1994 by the J. P. Morgan bank ..." Were there no people who gathered data, developed theories and fought battles for six years? Moreover, like many less-well-informed people, he criticizes it as a metric, only briefly alluding to the much more important concept of VaR as a risk measure: that risk should be measured by the statistical distribution of value over a fixed time horizon. Even more important than that idea is the synthesis of risk measurement and financial control. Yes, VaR is not physics, but it gave birth to modern quantitative financial risk management and represents an impressive intellectual achievement by a large group of contributors. Derman finds VaR useful, but writes mostly of its many flaws, and seems to miss entirely the adventure in the idea.

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modern-day physicists stand on top of a large pyramid of giants. In finance, we have to make do with a lower vantage point. When Derman was a physicist, I think there was a reasonable hope that he could have seen in his lifetime, or even created himself, a Grand Unified Theory that explained all physical observations from a small set of principles. Despite enormous progress since then, I think the mood of the profession is that we're farther away from that goal than it seemed in the 1970's. New discoveries raise more questions than they give answers. We may reach the limits of testability before we get satisfyingly final answers. Physics looked like the king of sciences (as Derman crowns it on page 1) when it seemed likely to be the first one to be finished, today I think there are better-looking claimants to the throne.

Not all physicists are reductionists, but all reductionists with enough talent should be physicists. If you believe finance can be explained by psychology, psychology by biology, biology by chemistry and chemistry by physics; then physics is king. Work in other sciences is just temporary scaffolding, erected to serve until physicists can get around to computing the deeper truth. Derman never uses the reductionist argument, but I can't help suspecting someone who clearly preferred being among the top few hundred physicists in the world to being among the top few practicing financial quants (for a lot more money).

I don't have philosophical arguments to make against reductionism, but it never appealed to me. I haven't met many reductionists with any better reason for their belief. I think that interesting and beautiful mathematical patterns arise in all sciences. I see no reason that such patterns need reduce to anything else. You can explain a mechanical watch in terms of springs and gears. It's true the properties of the springs and gears derive from chemistry, and ultimately from physics. But if those properties had been different, the watch would have been built from different materials. Gears and springs are important, atoms are not. I think it's entirely conceivable that chemistry could be supported by entirely different physics, biology by different chemistry, psychology by different biology and

finance by different psychology. Perhaps the laws of some of these sciences apply to virtual worlds with no physics at all. At the core I believe in patterns, and mathematics to describe them.

I'd like to buy a quant a coke ...

On page 2, Derman says financial quants used to be called "rocket scientists" by those who mistakenly thought that rocketry was the most advanced branch of science... He may be right about the etymology, but I always understood the term differently. Putting a human on the moon was a triumph of science in an entirely different way than conceiving and verifying the Standard Model. Perhaps Robert Goddard was not as smart as Albert Einstein, but he is a better role model for a financial quant. I'm not sure if Derman really loved physics, or the excitement of working in theoretical particle physics at a time of easy funding, rapid progress and seamless cooperation between enormously talented theorists and experimentalists.

Finance, and other fields including molecular biology, have something that theoretical particle physics does not, the clear short-term possibility of doing immense practical good. Solving economic problems is enormously beneficial in its own right, and helps directly to reduce problems like violence and ignorance. Indirectly, it frees resources to improve life through medicine and other technologies, and to create things of beauty and long-term value, like theoretical physics. I'm an engineer at heart, I like to help build well-designed things, and I want them to do some good.

Pricing an exotic swaption a basis point better than the competition is a long way from ending poverty, but noticing that some stars move relative to others is a long way from writing Principia. It's not the pricing that defines a financial quant, but the sharing of knowledge and experience. With enough sharing, and enough talent, we can move the world.

I hope no one interprets this as criticism of the book. It is a great thesis that everyone should read. I intend this essay as antithesis, not complaint. It's up to you to reach synthesis yourself.