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When Finance Met Physics

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James Owen Weatherall has embarked upon a futile mission, but it is a futile mission to be admired. *The Physics of Wall Street* is in part an intellectual history, examining the rise of the financial models that have received so much attention over the last half decade, after they were, perhaps unfairly, pinpointed as a central cause of the financial crisis. But the book is also a work of mathematical advocacy: Weatherall's stated goal is to revive the ailing practice of financial modeling by basing models upon better assumptions and making them more reliable.

The story of mathematical finance is as much about its practical implications as it is about the evolution of the field itself. While Adam Smith and the other progenitors of modern economics viewed exchange as a component of moral philosophy and so were not disposed to create mathematical models, the neoclassical economists of the early and mid-twentieth century embraced the methods of the physical scientists they styled themselves after. The economist Don Patinkin depicted the new face of his field in his 1956 book *Money, Interest, and Prices*: "We can consider the individual—with his given indifference map and initial endowment—to be a 'utility computer' into whom we 'feed' a sequence of market prices and from whom we obtain a corresponding sequence of 'solutions' in the form of specified optimum positions." This mechanistic view of human exchange would have been unrecognizable to Smith, but was emblematic of the neoclassical school's new reliance on models and mathematics.

The Physics of Wall Street: A Brief History of Predicting the Unpredictable
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As neoclassical economics progressed and financial economics formed its own sub-discipline, the emphasis on making economics conform to the methodology and rigor of the physical sciences grew. M.I.T. economist Robert Solow averred in 1985 that "if the project of turning economics into a hard science could succeed, it would surely be worth doing." The rise of massive computing power in the late twentieth century, and the consequent turn in finance away from stock-picking and toward algorithmic trading, seemed to make the neoclassical dream a reality. Complex models imported from physics and stochastic calculus helped earn fortunes for their creators, and they seemed at long last to bring that most elusive of investor dreams—certainty—within reach.

Financial models are now extraordinarily complex things, and they underpin much of what we see and do not see in the world of finance. Not only have models totally colonized the world of investments—estimates usually credit algorithmic trading with 60 to 70 percent of all stock trades—but they are used to price everything from options to bond market movements. To Weatherall, this is undoubtedly a positive development. Models create better efficiency for markets, better profits for investors, and better intellectual progress in the field. Not only are models "not to blame for our current economic ills," he says, but more sophisticated and more complex models will help us avoid such ills in the future.

Much of this story has been told before in pre-recession books like Peter L. Bernstein's *Against the Gods* (1996) and Roger Lowenstein's *When Genius Failed* (2000), but Weatherall's version of events is nonetheless enjoyable. And Weatherall is well equipped to tell it: an assistant professor at UC Irvine, he has one Ph.D. in physics, another in the philosophy of physics, and an M.F.A. in creative writing to boot (all earned by the age of 30). Rarely has such arcane material been presented in such lucid and readable prose. But the book is, in the end, an unsuccessful attempt to recuperate a failed project.

One of the weaknesses of the book is Weatherall's proclivity for tangential anecdotes—the chapter on physicists and mathematicians trying to beat the casino can be a slog—but he redeems himself with his sympathetic portraits of a wide range of fascinating eccentrics. Take, for example, the almost comically tragic story of Louis Bachelier, the now-famous mathematician who essentially invented mathematical finance. This woebegone Frenchman wrote an astonishingly original dissertation that challenged the dominant approaches of his field, inevitably resulting in professional banishment. Even with Henri Poincaré, the famous mathematician and theoretical physicist, to advise and support him, Bachelier earned no respect from his peers or his evaluators. After several wanderlust years, he finally secured a lecturing appointment at the University of Paris—but without a salary. Finally, after five years of academic penury, he was to be offered a permanent position. But the small matter of Germany's invasion of France intervened, and Bachelier was drafted to fight in World War I. He survived, but his career never reached the altitudes that his brilliance merited.

The book is full of stories like these: tales of misunderstood geniuses and stymied academics who challenged prevailing standards and were ostracized for their trouble. Weatherall presents the story of mathematical finance as a sort of Whig history, in which refinements are continually made by successive generations of brilliant misanthropes and hippie physicists, getting closer to the goal of making finance more like physics, until eventually we achieve the ability to predict the ostensibly unpredictable.

Though it is a story of scientific advancement, the later thinkers' penchant for turning their scientific discoveries into proprietary investment tools and untold riches mars the image of pristine intellectual pursuit. Weatherall concludes the story with a call to import *more* lessons from physics into finance—to perfect mathematical finance by further refining models, and to regard as symbiotic the psychological study of human behavior and the mathematical modelling of financial markets. It's a compelling argument, but ultimately an unpersuasive one.

There are several reasons not to readily accept Weatherall's arguments. The first is the most obvious one, and has been circulated in these pages ("The Financial Crisis and the Scientific Mindset," Fall 2009/Winter 2010) and elsewhere since the financial crisis of 2008: human beings are decidedly not quarks. Whereas the particles and atoms studied by physicists generally behave according to more or less predictable patterns, human beings are foolish and irrational and often do unexpected things. The best model in the world cannot fully predict when a group of human beings will engage in collective irrationality: consider, as Weatherall does, the bank-run scene in *It's a Wonderful Life*, or the Dutch tulipomania.

Weatherall is canny enough to address this objection directly. He is not of the scientistic variety of thinkers who promote math, method, and models as the ultimate explanations of all phenomena; he merely argues that financial models should take psychology and sociology into account. This, he says, is the approach of behavioral economics, which offers a more complete view of human behavior than physics or purely utilitarian economics.

But do they? The biggest elephant in the modern academic room is the questionable methodology of the social sciences. Psychology, for example, is in the midst of a major rethinking of the validity and reproducibility of many of the fundamental findings of the discipline. The Open Science Framework's Reproducibility Project is attempting to replicate major studies in the field because of widespread, longstanding skepticism of the conclusions of psychology studies in general—most of which are never subjected to this basic test.

The best research in psychology and social science draws on large, longitudinal samples, or is repeatedly verified across multiple studies and by multiple researchers. The famous work of Daniel Kahneman and Amos Tversky on cognitive biases, and the more recent work of Richard Thaler and Cass Sunstein, are examples of this kind of impressive and methodologically robust research. But too many social science studies rely for their research subjects upon a small pool of college students compelled or strongly induced to participate, and the results are too often reported as solid fact.

The flaws of behavioral economics studies are more troubling because of their use in public policy. Andrew Ferguson, in a 2011 article for the *Weekly Standard*, notes that the rationale for the tax-cut component of the 2009 stimulus bill rested largely on a single behavioral economics study. Published by two researchers at Texas A&M–Corpus Christi, the study simply asked a group of 141 students to imagine receiving a refund, and found that they were more likely to spend it if it came in monthly increments. But the study cast this weak tea as bold scientific fact: "results…confirm that monthly refunds stimulate current spending significantly more than yearly refunds."

Weatherall himself locates several areas of social science research that he claims will be greatly beneficial to mathematical finance. For example, he praises the French physicist-turned-economist Didier Sornette's research on herding effects—the ways a crowd, based on the desire to imitate others, can create a feedback loop and magnify a price drop or rise—and says this work can help make finance more predictable. But, as Weatherall points out, though Sornette's quantitative methods have allowed him to discern when these herding effects have taken over, Sornette "doesn't have an answer" to the more important question of why the ordinary tendency to imitate leads to herding effects at some times and not others.

Weatherall emphasizes that models are only as good as their assumptions. But he nonetheless makes his argument by drawing on lessons from fields that pile assumption on assumption, so deep that their practitioners typically do not even realize that they can't see the bottom. Weatherall aims to strengthen the scientific bases of financial models, but by incorporating research from areas that are very much not scientific—at least not in the way his own field of physics is.

There have been some figures in mathematical finance who have recognized that it does not yet amount to a science—but they are rare. Weatherall spends a chapter with Fischer Black, the polymath who helped form the famous Black-Scholes-Merton options pricing formula. Black-Scholes-Merton, perhaps the most influential financial formula of the past few decades, offered its users a way to price options accurately even in the face of uncertain information regarding price, volatility, and market conditions. Emanuel Derman, a former quantitative risk analyst, described it thus in his 2011 book *Models Behaving Badly*: "It's like a recipe that tells you how to make fruit salad (an option) out of fruit (stocks and bonds)" but also tells you "what the fruit salad is worth." The formula revolutionized options and indeed much of modern trading, and its central idea of fully hedging risk helped underpin some spectacular successes and disasters on Wall Street.

Black was never as certain as Myron Scholes and Robert Merton of the seamlessness of the model. Weatherall does not consult Black's "Noise," a speech delivered to the American Finance Association in 1985. Whereas other efficient market theorists saw all information as helping create an accurate price, Black distinguished between "information" traders and "noise" traders. The latter, he thought, trade on behaviors arising from speculation and uncertainty as if it were real information, and thus create inefficiencies and unnecessary volatilities in pricing. Because modern markets are so unfathomably complex, there is no pricing model—Black-Scholes-Merton very much included—that can fully block out the noise.

Separating the information from the noise is precisely the problem that Weatherall is trying to solve—and yet it remains as much a problem today as when Black gave his speech, despite a quarter century of technological progress. The trouble has as much to do with inherent limits on our predictive power as with our constant failure to recognize those limits. As Weatherall notes, "one can convince oneself that a model that has worked before is a kind of magical device that will continue to work, come what may." And one may gain "a false sense of confidence that, because you have some theoretical justification for a model, the model must be right. Unfortunately science doesn't work this way." But the most glaring truth Weatherall overlooks is that, although science doesn't work in this magical way, our minds still do.

A model is at heart a simplification, a reduction, and a metaphor that relies upon assumptions. As Weatherall says, "a few simplifying assumptions can go a long way toward making an otherwise intractable problem solvable—and once you solve the simplified problem, you can begin to ask how much damage your simplifying assumptions do." The trouble arises when we fail to make the move from our abstract sketch to a whole picture. We let models stand in for reality in finance because it's more efficient to base everything on simplified assumptions and reduced data.

The central shortcoming of Weatherall's book is its failure to emphasize just how persuasive metaphors—and thus financial models—really are. Analysts choose one portion of reality and operate from it. But the problem is not just that they fail to accurately describe the entire picture, but that they fail to realize their own failure. Ushered along by the thought that yet-uncertain developments will eventually become known, once investors have some convincing model they can easily begin to operate as if they have already achieved total certainty. Weatherall is not unaware of this problem. While promoting his book, he told the science writer John Horgan in an interview, "I think that some economists have been blinded by the rigor of their work: if the mathematics is right, the theories must be true. But the relationship between mathematical theories and the world is more complicated than that." It would have been nice if he had struck that cautionary tone more often in his book.

One result of false certainty in models is that the models wind up actively molding the very thing they purport to passively describe. Though Weatherall mentions this only briefly—and seems to see it as a feature, not a bug—the sociologist Donald MacKenzie convincingly demonstrated the molding effect of financial models in his aptly titled 2006 book *An Engine, Not a Camera.* In one example from the 1990s, theorists at the hedge fund Long-Term Capital Management (LTCM) based their models on the efficient-market hypothesis, which holds that markets are good at reflecting available information about pricing. Yet they also exploited arbitrage profits, in which goods are purchased from one market and resold in another where they are priced higher, in contradiction to the efficient-market hypothesis. MacKenzie argues that LTCM's models forced a certain set of behaviors in other market actors that altered the very conditions upon which the model was based.

A second fundamental problem in the LTCM case relates to its use of the Black-Scholes-Merton formula. The influence of the formula in finance even today would be difficult to overstate; in the 1990s, it was central to LTCM's entire system, which is understandable insofar as Scholes and Merton were both on the LTCM board. In order to make the formula work smoothly, its creators had to assume so-called "continuous time" in share prices. As Roger Lowenstein puts it in *When Genius Failed*, Black-Scholes-Merton assumed that "the price of a share of IBM would never plunge directly from 80 to 60 but would always stop at 79¾, 79½, and 79¼ along the way." Black-Scholes-Merton not only failed to foresee huge price jumps but also minimized their importance, diverting attention away from consideration of what might happen if they actually did occur. Finance failed to see where the assumptions could *not* be applied, because that would have disrupted the model. Irrationality on a large scale, creating the conditions of a feedback loop, did not fit neatly inside a model. So when irrational behavior actually did occur, its effects were amplified by the very rationalist models that had assumed it wouldn't—and this combined effect contributed not only to the collapse of LTCM in 1998 but to the financial crisis of 2008.

So why do these financial models, which are really just elaborate, reductive metaphors, continue to persuade financial professionals? Because they are elegant forms, and the field has been expressly searching for elegant forms that boast explanatory power. Irving Fisher, the famous Yale economist, referred bluntly in the 1940s to "the goal on which my heart has been most set, the goal of economics becoming a true science comparable with physics." Finance and economics took up this challenge, reducing a huge swath of human interaction to simplified formulas reflecting utility maximization.

Economics and finance did not simply evolve to become mathematical: its practitioners set out to make it so. And as in the physical sciences, elegant formulas that explain complex subjects have great persuasive currency among their fallible human users. As the literary theorist Kenneth Burke put it, "a yielding to the form prepares for assent to the matter identified with it." Once economics and finance made it their goal to boast the elegant model form, they assented to the simplified assumptions of that form. They mistook the map for the territory, and still do.

This mistake pervades finance and economics. We let the Case-Schiller index, a metric of housing prices, stand in for the state of the housing market as a whole, at the expense of paying attention to the plight of defaulting homeowners. We let a narrow index of thirty stocks, the Dow Jones Industrial Average, stand in for the health of financial markets. We let figures like the gross domestic product stand in as true measures of the nation's economic health despite being rife with well-known flaws. We simplify assumptions because it's easy, and we don't expand our descriptions because it's difficult. The trouble is not so much that we cut corners as how steadfastly we fail to realize that we do so.

These problems with financial models undermine Weatherall's basic idea of what science is, what it does, and what it should be. In his telling, science gradually moves us from the apparent chaotic disarray of the world to perceiving the world's underlying order. This is a reasonable description of the physical sciences. But human affairs are messier and involve complicated moral dilemmas. Weatherall, though, seems to think that virtually anything the scientist does is good, and any transformations resulting from scientific work are even better. He sees advancement and progress virtually wherever physicists and mathematicians put pen to stock purchase. His story is one of slow and beneficial infiltration: "quants" (the mathematicians who work on Wall Street) move in to profit from inefficiencies that the old guard is too dim to spot. The scientists are the crusading heroes bringing order to an unruly world.

Weatherall is also all too hasty to credit science for developments he deems good. In one example, the "highly secretive Chicago firm" O'Connor and Associates spotted a flaw in Black-Scholes-Merton and adjusted its models accordingly, giving it a competitive advantage while largely sparing it from the 1987 crash. In the book's endnotes, Weatherall says the firm's success proves that the flaw "didn't appear so suddenly after all, if you knew to look for it!" For him, this case constitutes proof that continual scientific refinement and hypothesis-testing can save the day, averting the problems that models produce. But perhaps there's another explanation: luck. In O'Connor's case, the firm hired a man who had once worked for Black and so had some inkling of the formula's flaws. Depending on one's perspective, this is either a case of scientific refinement or of fortuitous insider information.

The biggest question that goes unasked in this book is whether this is what we *want* finance to look like. Weatherall emphasizes the good that finance has done for the U.S. economy while giving short shrift to the ills it has created, and claiming that those failures show that finance needs to be *more* like physics rather than less. In the book's epilogue, Weatherall notes that models underlie all science and engineering, and if we doubt their basic validity, we "should never drive over the George Washington Bridge or the Hoover Dam." It's "hard to see why," he says, finance is "a different kettle of fish from civil engineering or rocket science." But this is a specious analogy. The engineers who perfect load-bearing formulas for bridges don't profit from them in the same way that Wall Street quants do. The incentive structure is different. The model underlying the bridge has one purpose: to provide a conveyance for transportation. A trading algorithm has a very different purpose: to earn money for the designer.

As Adam Smith realized long ago, economics and finance are not context-free mathematical activities in the way an engineering model could be, for they are inescapably bound within human relations. If I am the chief engineer of the Hoover Dam, I might be able to make some illicit money with my model—perhaps I shift the flow a bit to favor a real estate investment downriver—but I can't design the model to exploit an inefficiency in structural dynamics that will enrich me thanks to my hugely leveraged options contracts. The purpose of the model is still to regulate the flow of water and provide a way for traffic to cross. I make money from how well the model conforms to these physical requirements. No matter how much the financial engineers borrow from physics and calculus, their motives will always be fundamentally different from those of physicists and mathematicians. You don't design models that exploit market inefficiencies for the sake of building an accurate representation of the world; you do it to make money.

And having made that money, the foundations of your model, and others' models, are liable to shift. This is the ultimate trouble with pretending we can make finance as robust as physics: while matter is manipulable, the laws governing that manipulation remain absolute. But in finance, the stuff we are manipulating with our abstractions is itself an abstraction, and the laws governing it can themselves be manipulated, accidentally or deliberately. Tweaking the very rules of the game is in fact one of the best ways to win it—and to wind up destroying it.

Financial models will forever lack the solid foundations of bridges, but their failure can be even more catastrophic. As we saw in 2008, pensions are dissolved. Jobs are lost. Lives are ruined. Models are not contrivances in a social vacuum; they are simplifications with consequences, hopelessly intertwined with the motives of the designer and existing in a world of greed, dishonesty, and irrationality.

In a limited sense, Weatherall's basic mission—to make our models work better, and to use them to develop new economic tools—is admirable. But in practice, financial models can leave us unable to see many of the most important aspects of financial markets. And in that blindness lie the roots of catastrophe. Weatherall's endorsement of "an economic Manhattan Project"—a massive project of "collaboration between economists and researchers from physics and other fields"—amounts to a utopian dream to create the heaven of an orderly model in our disorderly world. On the last page of the last chapter of his book, Weatherall claims that, before the 2008 financial crisis, "there was no one there to point out that the shadow banking system was built on a house of cards." To Weatherall, the failure was merely an empirical one, for which "mathematical sophistication is the remedy, not the disease." He does not seem to wonder why the people who couldn't see the house of cards for what it was were the ones who built it in the first place.

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