Investment Insight

Nobel Prize in Economics, Physics Envy, and Quant Investing

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The 2013 Nobel Prize in Economics[1] was awarded to three financial economists for their contribution to the understanding asset prices. The winning economists were Eugene Fama and Lars Peter Hansen from the University of Chicago and Robert Shiller from Yale University. Quite understandably, there has been considerable reaction from the financial press on the seemingly contradictory choice of this year's laureates. This is because Fama is the father of efficient-market theory while Shiller made pioneering empirical research to debunk market efficiency. Both Fama and Hansen are fervent disciples of the school of rational expectations while Shiller is the famous author of the book "Irrational Exuberance" with his prediction of stock market and housing bubbles. This is quite a contrast in views.

Here are some of the analogies appearing in the media. "It's like giving a prize to the Yankees and the Red Sox,[2]" says one. "It is like awarding the physics prize jointly to Ptolemy for his theory that the Earth is the centre of the universe, and to Copernicus for showing it is not,[3]" says another. I am not sure the baseball analogy quite applies to this case except for die-hard fans of the two archrivals. But it would be indeed unthinkable for the Nobel Prize in Physics to go to both Ptolemy and Copernicus.

Over the years, there has been an ongoing debate about whether economics is a *hard science* compared to physics or mathematics. This year's Nobel Prize clearly demonstrates that it is not.

Theory and Empirical Evidence

Another perspective on this year's Nobel Prize in Economics is that they were given to theorists and an empiricist, with the latter turning up evidence against the former. It is perhaps an acknowledgement that both sides have merit. It is also a clear admission that it is impossible to have a "correct" theory in economics.

This tug-of-war between theory and empirical (or experimental) evidence has long existed in physics. However, in physics or other disciplines of natural science, there is usually a final judgment – empirical evidence either validates or rejects a theory. There is little room for a middle ground, or "hedging" in the "economic sense." Consider for example, this year's Nobel Prize in Physics being jointly awarded to two theorists, François Englert and Peter W. Higgs, who proposed the Higgs particle in 1964. They have waited almost 50 years to claim the Prize because only in 2012 was the existence of the Higgs particle confirmed at the CERN laboratory outside Geneva in Switzerland. Unlike this year's winners in economics, Englert and Higgs didn't have opposing ideas on the existence of the Higgs particle.

While we are on the subject of physics, let's go back in history and ponder who some other winners might have been if physics were more like economics. In 1907, Albert Michelson became the first American to win a

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[1] The official title of the prize is "the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel."

[2] Robert Solow, winner of the Nobel economics prize in 1987, Bloomberg News.

[3] John Kay, Financial Times.

Nobel Prize in Physics. He was an experimentalist and won, to a large extent, for the Michelson-Morley experiment, which provided strong evidence against the existence of ether – a fictitious substance postulated as a medium in space for propagation of star lights or electromagnetic waves. Imagine the Nobel Committee also had given a prize to the proponents of ether while giving it to Michelson! Of course, Einstein's theory of special relativity rendered the concept of ether obsolete.

Ironically, Einstein never won a Nobel Prize for his theory of relativity, because the Nobel Committee considered the theory of special and general relativity too speculative. This was true even after an empirical verification of one of the more fascinating predictions of general relativity – the bending of light by the sun.

In one of the most famous scientific events, British astronomer Sir Arthur Eddington conducted an expedition to the island of Príncipe near Africa to observe the solar eclipse of May 29, 1919. The pictures taken during the eclipse showed stars with light rays that passed near the Sun had been slightly shifted because their light had been curved by the gravitational field. The magnitude of the shift was consistent with Einstein's prediction.

Let's imagine that Eddington's observation, rather than confirming Einstein's prediction, showed instead that he was wrong and the Newtonian model of gravitation was right all along. Would the Nobel Prize be awarded to both Einstein and Eddington for their contribution to the understanding of gravitation?

Physics Envy

Mathematics is a powerful tool in many disciplines of natural science, especially in physics. *Physics envy* refers to the desire of using a similar mathematical approach in social sciences, such as economics and finance. The so-called *mathematization* of economics and finance has led to the development and advancement of theoretical ideas, such as game theory, general equilibrium theory, portfolio theory, capital-asset pricing, option-pricing theory, and rational expectations[4], to name a few.

Many of these ideas are strong and insightful, resulting in their originators being awarded with the Nobel Prize in Economics. However, these theories are based on numerous assumptions that are far removed from reality. Although mathematical analysis and international recognition from the Nobel Committee can mistakenly create the perception of precision, the reality is that even the most astute economic research is mired in uncertainty. Consequently, physics envy can bring unwarranted confidence in economic analysis, which can cause serious economic and financial damages if one applies these theories blindly in the real world.

Perhaps, the most important reason for physics envy is the creation of the Noble Prize in Economics itself. The Prize in Economics is the only Prize that was added after the original Prizes in Physics, Chemistry, Physiology or Medicine, Literature, and Peace. By placing economics among physics, chemistry, and medicine, it certainly can give the wrong impression that economics is also a hard science.

For this and other reasons, many Noble Prizes in Economics could be mistaken as prizes in Mathematics. Of course, mathematicians and physicists welcome this quantification of finance and economics in general because of the research and employment opportunities afforded to them in these fields. But I guess true mathematicians might object to the notion that they could be called Nobel Prizes in Mathematics because the mathematics typically involved in economics and finance are neither new nor are they breakthroughs, but rather applications of existing mathematics. Put mildly, they don't compare to the Fields Medal.

Physics envy should not be about the use of mathematics in physics. The true envy lies in the fact that there is a complete lack of controlled experiments in economics while in physics, experimental physicists and theoretical physicists work together to advance the

[4] "WARNING: Physics Envy May Be Hazardous To Your Wealth!" Andrew W. Lo and Mark T. Mueller

frontier of their science. Experimental physicists can prove or disprove theoretical ideas and in return theoretical physicists can theorize based on experimental findings. The theory of general relativity is one case already mentioned. Another case of this joint effort is the development of quantum physics. Unfortunately, this kind of collaboration or cross validation has been rare and may never be the norm in economics.

It is both interesting and ironic to note that the physics community used to be far less enthusiastic about theoretical ideas, and by extension, the use of mathematics in physics. At the beginning of 20th century, before the development of quantum mechanics and the theory of relativity, Nobel Prize laureates in physics were dominated by experimental physicists. Albert Michelson won in 1907 for the Michelson-Morley experiment. Other examples include Madame Curie (with her husband) for the discovery of new radioactive elements. In fact, the Nobel Committee was so averse to pure theoretical physics that they could not bring themselves to award Einstein a Nobel Prize on relativity. While this biased emphasis on experiments was proven wrong for physics, it is probably right for economics.

From this perspective, it is amazing to note that after the dot-com bubble in the late 90's, the housing bubble and the global financial crisis in the recent years – evidence against efficient-market theory – the Nobel Prize in Economics was awarded to two proponents of such a theory[5]. Maybe physicists should be envious of economists instead!

Quant Investing

Ideas in economics and finance have had a profound impact on the asset management industry in general and on quantitative investing in particular. Some pioneers in quant investing were trained physicists and mathematicians. The mathematical and statistical analysis allows quant investing to be more systematic and less subjective to behavioral biases. However, it could also be prone to physics envy with too much confidence in mathematical models and techniques. For some, the fact that some of these models and techniques were recognized with Nobel Prizes could only make matters worse. Here is a short list of cautionary tales.

Mean-Variance Optimization

Mean-variance optimization, which was introduced by Harry Markowitz, constructs portfolios by maximizing expected returns for a given level of risk. In addition to being a portfolio construction tool, it is also one of the core assumptions for CAPM – Capital Asset Pricing Model. Mathematically, it is an application of quadratic programming to portfolio theory. Even though it is often referred to as Modern Portfolio Theory, the 1990 Nobel Prize winner himself quipped that there is nothing modern about it. This is certainly true in terms of mathematics.

As practitioners soon discovered, it is very hard to use mean-variance optimization to directly construct portfolios. There are at least two reasons. First, it turns out that the derived optimal portfolio weights are extremely sensitive to various inputs, whether they are expected returns or risk estimates, such as volatility and correlation. In practice, these inputs, especially the expected returns, are far from being precise and they are mostly educated guesses. Failure to account for these estimation errors results in portfolios that are not as optimal as the theory would otherwise suggest. Second, optimal portfolios coming out of optimization are often non-intuitive with highly concentrated weights. For these reasons, mean-variance optimization is often called an error-maximization black-box.

There is no doubt that risk-return tradeoffs and asset correlations are important ideas in financial economics and in particular portfolio theory. But using mean-variance optimization as a mathematical tool to solve practical asset allocation problems seems to carry things too far.

Today, very few practitioners use mean-variance optimization at face value. Other techniques, such as risk budgeting, are more robust and intuitive, while still preserving the important ideas of risk-return trade-offs.

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[5] It would be more amazing if Fama had won in 2009. On the other hand, why wait four years to give Shiller the Prize?

CAPM

The theory of CAPM has also had a tremendous impact on the asset management industry. The theory postulates the optimal portfolio that *every* investor should hold is a market portfolio. Well, this is certainly not true. William Sharpe, who won the 1990 Nobel Prize together with Harry Markowitz for the development of CAPM, couldn't believe it either. In an interview with the author and financial sociologist Donald MacKenzie, he said "I thought, well, nobody will believe this. This can't be right.[6]"

Apparently, many investors, perhaps a majority of investors, now believe in its truth, judging by the total assets invested in various capitalization-weighted indices. Among many factors that caused the seismic shift to this type of "passive" investing, one wonders if "the seal of approval" given by the Nobel Prize for CAPM played a significant role.

Admittedly, the idea of the possible relationship between investment risk and expected returns is very insightful. However, to go from this notion to CAPM requires several leaps of faith. One of the underlying assumptions of CAPM is that every investor uses mean-variance optimization based on the same expected returns. The reality is close to the opposite: few investors use it[7]. At least, investors in capitalization indices don't use it at all – or according to CAPM, they are counting on everybody else using it.

One of the consequences of investing in traditional market indices is that investors become oblivious to the embedded risks in these indices. Past winners and structural reasons such as increased issuance lead to the buildup of risk concentrations in these indices. At times, these risk concentrations could be quite extreme and can subsequently lead to large drawdowns and high volatilities in index returns. Some of these painful episodes experienced by equity indices include the technology concentration during the dot-com bubble and the financial concentration during the more recent credit bubble.

Viewed from the perspective of risk contribution, capitalization-weighted indices are not truly passive investments. They have implicit return assumptions that are quite uneven for their constituents. They should be called inactive strategies instead, since they require no portfolio rebalancing absent any corporate actions or changes in their constituents.

Truly passive investments, on the other hand, should have little bias toward any particular sections of portfolios. Risk Parity portfolios, viewed this way, are passive investments. Even the regular portfolio rebalancing that brings a portfolio back to risk parity is an action of passive intention.

The Fundamental Law of Active Management

These cases might serve as a prime example of physics envy. Physics is famous for its principles or laws – laws of conservation, Newton's laws in mechanics, laws of gravitation and relativity, laws of thermodynamics, laws of quantum mechanics, and the list goes on. It is tempting to have laws in economics and finance, such as the law of one price and the law of supply and demand. But the laws in physics and laws in economics and finance are vastly different. Physical laws are a close approximation of reality and they are invariant under different circumstances or transformations. On the other hand, laws in economics and finance often are merely intellectual ideas, which could share little resemblance with reality. In addition, their validity is often limited by circumstances and changing times.

So what is the fundamental law of active management[8]? It states that the information ratio (IR), a ratio of active return to active risk, both measured against a benchmark, is given by the information coefficient (IC) multiplied by the square root of *N*, where the information coefficient is the correlation coefficient between forecasts and actual returns and *N* denotes the number of independent bets. Or

[6] Donald MacKenzie, An Engine, Not a Camera: How Financial Models Shape Markets (MIT Press, 2006)

[7] As he professed in a Journal of Investment Management conference, Harry Markowitz doesn't use it either, not in a manner assumed by CAPM.

[8] Richard Grinold, The Fundamental Law of Active Management, Journal of Portfolio Management, Vol. 15, No. 3, Spring 1989

$$IR = IC\sqrt{N}.$$

Again, both information ratio and information coefficient are useful concepts in quantitative portfolio management. But does this formula, albeit simple and elegant, give a good description of how active management works?

We have found that this description is not accurate. First, the information coefficient is not clearly defined. Is it a time series correlation between forecasts and returns of individual assets, or a cross sectional correlation between forecasts and actual returns of multiple assets? Second, it is hard or next to impossible to know what *N* is in practice. Is it the number of assets or number of time periods or both? Third, the formula is about the expected IR of a single period. In practice, investors care about a multi-period IR. Finally, it totally neglects the risk of factor returns.

Quantitative equity portfolios are usually built with factor exposures, such as value, momentum, quality, and size. One of Fama's contributions was to formally add value and size factors to the single factor CAPM with just the market factor[9]. One can then build factor portfolios with specific factor exposures while neutralizing other factor exposures. The excess returns of these factor portfolios are then proportional to the cross-sectional correlations between the factors and realized risk-adjusted returns, or IC. As a result, the multi-period IR of such factor portfolios is related to the ratio of the time-series average and standard deviation of IC[10], i.e.

$$IR = \frac{\mathrm{avg}(IC)}{\mathrm{std}(IC)}.$$

This result gives a much better description of how a single factor portfolio adds value and in addition, it provides a useful framework for combining multiple factors together into composite factor models. But still, there are many practical issues that would impede its exactness in practice. They include portfolio constraints, transaction costs, and errors of a given risk model. Therefore, it is a much improved result over the "fundamental law" of active management; however, both substance matter and personal modesty have prevented it to be labeled as a law.

Summary

Modern economics and finance often have a strong quantitative and mathematical bent. While the use of mathematics is crucial in forming and exploring ideas in economics and finance, too much emphasis on precision or analytical rigor could lead investors astray, resulting in a negative impact on investment returns. I have listed some examples in quant investing to illustrate this point.

Even more broadly, mathematical and statistical models, while useful in analyzing returns and risks, are not capable of capturing true uncertainty in the fields of social science. There is uncertainty in models, in forecasting future returns, and in human elements of economic activities. There is also uncertainty or bias in interpreting theories and empirical evidence related to economics and finance. Nobel Prizes in Economics wouldn't change all that. Given this year's winners, there is uncertainty as to who is right about market efficiency and I am quite concerned that for some, this uncertainty will never be resolved.

[9] Investors who are clamoring for "smart beta" or factor portfolios will no doubt be more convinced in its truth now that Fama's three factor models are recognized by the Nobel Prize.

[10] Qian, Edward, Ronald Hua, and Eric Sorensen, Quantitative Equity Portfolio Management, Modern Techniques and Applications, Chapman & Hall, 2007

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