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THE TOP ACHIEVEMENTS, CHALLENGES, AND FAILURES OF FINANCE

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Introduction

Having seen one too many David Letterman show, I decided that it was time for me to put together my own list for the real achievements of my discipline, *Finance*. There is much subjectivity in my particular selection of subjects. Still, I would guess that most finance professors would agree that most of my choices below represent important progress in the development of finance. Likewise, I would expect none to agree with my specific rankings. Yet, my hope is that the list below is of interest to many practitioners and academics.

I then went overboard and decided that it would also be useful to put on paper what I consider to be the most important challenges for Finance to work on, as well as its failures to date. Necessarily, the target audience for these is primarily academic researchers, not practitioners. And, naturally, however subjective the list of accomplishments, the list of challenges and failures is substantially more debatable. If this list manages to direct the attention of one talented PhD student towards these research issues, writing it up will have been a worthwhile exercise.

If you do not like the choices on my list or have better ideas of subjects for the lists or ordering/priority, feel free to send me an email. Ivo.Welch@Yale.Edu. Do not expect a response, even though I promise to read your email. If your email can help me look less foolish in the next draft, I would be especially grateful.

The Achievements of Finance

Each idea starts only with a very short introduction. For a real introduction, please consult a Finance textbook (e.g. Brealey-Myers, Grinblatt-Titman, or Ross-Westerfield-Jaffe).

1. **No Arbitrage**

The idea that there is no risk-free way to get rich quick, and that this has implications for the prices of assets. This is similar to the "law of one price": if your neighbor sells gas for $2 a gallon, you will not be able to sell it for $3. The absence of arbitrage was first prominently used by Modigliani-Miller (1958, 1961) in their famous capital structure propositions. Later, and with equal force, it lay the basis for Ross' (1976) APT and for the pricing of derivatives.

2. **Efficient Markets (EM)**

The idea that the market uses all available information in its setting of an asset's price (according to some tradeoff between risk and returns). Bachelier (1900) and Cootner (1964, 1961, 1960) may have pioneered EM, but it was probably Eugene Fama's (1970) article that sorted it all out and sparked the revolution in the asset-pricing component of modern Finance. As well, it sparked the invention of event-studies.

3. **Net Present Value (NPV)**

The idea that one can compute a today's equivalent for future payoffs based on expected cash flows in the numerator and a risk/time adjustment in the denominator. This allows corporations and individuals to perform "capital budgeting," the process of comparing dissimilar projects on a "profitability" metric to decide where to deploy capital. Although practically *everything* can be valued using NPV, its purest application may be the valuation of bonds. A second noteworthy application of NPV principles is the Gordon (dividend) growth model, which is a special of NPV. A third noteworthy application is "EVA" (Economic Value Added) and its variants, heavily promoted by corporate consultants. The contributions to the development of NPV may go back a long time, but Irving Fisher (1908) and Jack Hirshleifer (1964) put the subject on sounder empirical footing than it was before, and explained the relation and differences between IRR (Internal Rate of Return) and NPV.

4. **Derivatives Valuation Techniques**

A derivative is a financial instrument whose payoff will depend on yet another financial instrument in a specified manner. Naturally, the value of the base and derivative asset should be related, and this relations has become the perhaps most successful branch of knowledge (prediction) in the social sciences. Derivatives valuation started with techniques to value equity options. Black and Scholes (1973) and Merton (1973), and perhaps Harrison-Kreps (1979), are usually cited here. But I personally believe that the Sharpe (1978) and Cox, Ross, and Rubinstein (1979) binomial approach was as important, because it helped even MBA students understand the basic insights behind derivatives valuation and opened up a large venue of simulation methods for all sorts of complex instruments. *The vast majority of research departments' proprietary trading on Wall Street was made possible by the wide teaching and understanding of derivative methods, usually based on binomial trees.* Finally, applications of derivatives techniques have migrated into Corporate Finance, where "real options" are giving us new insights into the value of such concepts as flexibility and timing.

5. **Mean Variance Analysis**

Mean variance analysis maps the attainable tradeoff between expected future returns and their standard deviation (risk), which has many surprising and parsimonious properties. For example, a security's covariance with other securities is typically more important than its own variance risk. Invented by Tobin (1958) and Markowitz (1952), this is still the foundation of all investor choice analysis problems, and the basis for the CAPM (see #7 below). This is the basis for the ubiquitous concept of *diversification* and perhaps for the growth of the mutual fund industry.

6. **Capital Structure and Dividend Irrelevance**

The deservedly famous Nobel-prize winning insights by Modigliani and Miller (1958, 1961; often abbreviated as "MM") that capital structure and dividend policy do *not* matter *if* markets are perfect. It was an early pioneer in the use of Perfect Markets as a concept for analysis. The MM propositions are often misinterpreted: capital structure and dividend policy is indeed relevant (e.g., see <u>capital market imperfections</u> below); MM explained to us why and when. There may be an interesting predecessor: John Burr Williams, "The Theory of Investment Value," 1938, contains a page of text with similar arguments.

7. **Capital Asset Pricing Model (CAPM)**

Built on *mean variance analysis* (see #5 above), the CAPM was the first modern model of appropriate security pricing in equilibrium (i.e. how much expected return is a reasonable tradeoff for a given risk profile). Credit Lintner (1965), Mossin (1966), Sharpe (1964), and Black (1972) for the development, and Roll (1977) for helping us better understand its limitations. Still the most widely used model to obtain "discount rates" (see #3 above), even though it suffers from many empirical shortcomings.

8. **Understanding of Yield Curve and Fixed Income Instruments**

Unlike many of the other ideas in this list, our understanding of how bonds are priced has come gradually and from many different contributors. Noteworthy are Fisher's (1908) work on interest rates, the Macaulay (1938) duration measure, and the Vasicek (1977) and Cox, Ingersoll, and Ross (1985) equilibrium valuation models.

9. **Event Studies**

Event studies assume a (reasonably) efficient capital market, and ask the question of how the market reacts to the release of new information. This reaction provides a good measure of the value impact of the (unanticipated) news. The event study was originally invented by Fama, Fisher, Jensen and Roll (1969) and improved by Ball and Brown (1968). The power and simplicity of event study techniques is enormous. (To illustrate its flexibility, note that it has even been used to measure such phenomena as the impact of political pressure on the South-African apartheid regime.) Event studies have suffered some academic disdain in recent years, primarily because it is too easy to run an event study without putting much thought into it. In my view, this is not a fault of the technique, but a sign of its power.

10. **Factor Pricing Models (APT, ICAPM, CCAPM)**

Breeden (1979), Merton (1973), and Ross (1976) deserve credit for methods different from the CAPM that allow pricing securities. Unfortunately, they suffer from poor empirical results or poor factor identification. Still, this gave us an insight into how rational pricing works, and papers by Roll and Ross (1980), Chen, Roll and Ross (1986) and Hansen-Singleton (1982) gave us a first set of tests.

11. **Corporate Structure Information Imperfections**

We do not live in an Modigliani and Miller world. We should give Holmstrom (1979) and Meckling-Jensen (1976) credit for analyzing the role of agency problems, and Ross (1977) and Leland and Pyle (1977) credit for analyzing the role of information (signaling) problems. Despite constant claims to the contrary in many an empirical paper, practically by definition, neither of these issues lend themselves to easy empirical tests. That is, by definition, most of these effects are hidden (or they would not be effects). Thus, and perhaps necessarily, this is an area in which the empirical relevance remains difficult to assess, not only quantitatively but also qualitatively.

12. **"Anomalies"**

Are there financial instruments that offer much higher returns than appropriate for their risk profile (contribution)? Although Graham and Dodd (1934) started "value-investing" as early as 1934(!), a rigorous search for such "alien life" originated with Banz (1981), Keim (1983), Reinganum (1981), Roll (1983), DeBondt and Thaler (1985), and others, in the early eighties. These anomaly studies all pointed to various factors that seem to offer higher returns in equilibrium.

13. **Long-Term Market Timing**

Can we predict what the overall stock market return will be over the next year or over the next decade? Shiller (1981) and Campbell and Shiller's (1988) path breaking work was significantly extended and perhaps transformed by Fama and French (1988) in the early nineties. There is an ongoing debate about the validity and significance of the long-term timing findings, but forecasting the equity premium is such an important and ubiquitous issue in all sectors of Finance, that it just had to be included. At the very least, these papers opened up a whole new strand for research.

14. **Certain Modeling Setups**

In some sense, everything from algebra to Bayes theorem qualifies as important tools used in finance. However, as a unique way of building models in finance, it is worthwhile to mention at least two modeling setups that are very common in finance and rather uncommon elsewhere: First, there is the "exponential utility+normal-distribution" modeling technique, used in a whole class of mathematical models. This technique was first brought to Finance by Diamond and Verrecchia (1981), solving a famous problem pointed out by Grossman (1977), and Kyle (1985) in different contexts. (PS: Hellwig wrote a similar paper in economics contemporaneously.) Kyle's application created an entirely new field, the analysis of market microstructure. The second is Merton's (1969) stochastic calculus technique, which was used to address many problems in derivatives (mentioned in #4 above).

15. **CRSP**

The first major ongoing stock pricing database for research use, and still the gold standard for historical pricing data. Prior to CRSP, Cowles (at Yale) was the prime source for financial data.

Other Contenders

I had to omit many worthwhile great contributions, some of which are more impressive in their body of knowledge than in the path breaking contributions of single individuals. Among the contenders here were: § Lintner's (1956) behavioral dividend model. § The long-term return performance event studies initiated by Ritter (1991). § Work by Sharpe (1966), Jensen (1968), Elton and Gruber (19**??**), Grinblatt and Titman (1989), Brown and Goetzmann (1997). and many others on mutual funds and lack of general predictive ability by fund managers. § Work by Glosten and Harris (1988), Stoll (1976), Ananth Madhavan, and many others on empirical market microstructure § Work by Thaler and others on Behavioral Finance. § Much nice IPO, M&A, dividend work.§ Method of Moments. § Risk Assessment: VAR, Ledoit. § International Finance. § Banking and Investment Banking. § Macroeconomics and finance relations and inflation. § Black's and Roll's presidential addresses on noise and R^2; § Bank runs (Diamond and Dybvig [1983]); § Herding/cascade effects in financial markets.

Sincere apologies to all those whose work has been omitted here.

The Challenges of Finance

This part is directed more towards academic Finance researchers and Ph.D. students in search of a good research topic. Clearly, these are my personal choices for important unsolved problems and/or future areas for Finance. Almost all of these are primarily empirical questions, and many of these questions will require the time-intensive creation of new data sets. There are few modeling or modeling methodology questions (as I am primarily an applied theorist). This is partly because I believe that our understanding of Finance will improve more from this empirical knowledge, partly because I believe that the empirical questions to be solved are easier to lay out than their more serendipitous counterparts in theory.

And, naturally, I am trying to work on some of these issues myself. You might thus consider these research questions my own personal pet peeves. When I criticize some of our existing literature, please keep in mind that I include my own research in this criticism as well. I am just as guilty. You may also want to ignore some of my diatribes here: the list and my description of existing research is intentionally critical and controversial.

And finally a plea: if this list prompts you to write a specific paper, please let me (and the readers of your paper!) know.

In No Particular Order:

Meaningful **Behavioral Finance**

In some sense, this was originated by Graham and Dodd (1934) and Keynes (1936) famous "beauty contest" analogy.

This is worth a digression. I have mixed feelings about "behavioral finance," as it is most commonly practiced and marketed:

First, "Behavioral Finance" is a misnomer. It implies that the rest of Finance is not about models of economic behavior. But, there is behavior in all financial models (including perfect competition models). So, what is "behavioral finance"? The usual meaning is that "behavioral finance" is really "imperfect rationality" finance. Although this phrasing is less sexy, it is more truthful. And it is also less derogatory to the rest of finance, which studies "rational behavior" models.

Second, it is not enough for imperfect rationality finance to detect a regression coefficient that cannot be immediately explained by the most naive model of rationality. Instead, much of "imperfect rationality" finance needs to improve its own specific predictions (alternatives). (This critique does not apply to all "imperfect rationality" papers. My favorite paper is Benartzi-Thaler (AER 2000): it offers a specific simple, believable imperfect heuristic [1/n] and tests its validity not on aggregate returns, but on individual behavior.)

Third, simple and mild price pattern aberrations are not evidence of irrationality (or rationality for this matter). I find the claims of victory for behavioral finance in such contexts akin to claiming the existence of alien life because we found heat. Yes, alien life might generate heat, but so can many other phenomena.

Having said all of this, I think Finance could gain tremendously from a better understanding and acceptance of imperfect rationality. It is no accident that this topic is at the top of my list. There is no doubt that imperfect rationality can play a role in financial markets, especially in the non-arbitrageable link from real values to financial values:

So, there are at least a couple of issues where I would love to see progress:

1. <u>Specific Predictions:</u> Exactly what are the refutable predictions of irrational behavior (other than 'everything goes')? Can it do better than just be the null hypothesis when other theories have been rejected?
2. <u>Aggregation:</u> When do behavioral mistakes aggregate up to have a meaningful influence on prices? Example: I do not see how overconfidence, myopia, or prospect theory would necessarily survive aggregation in meaningful amounts in asset pricing contexts. Now, this is not to say that aggregation (the behavior of a "representative agent") is flawless in "perfect rationality" models; but solid arguments about why individually distorted decision-making does not wash out would help "behavioral" research significantly. (I have at least an intuitive notion that the "lack of arbitrage" assumption holds better as we aggregate and that the "individual mistakes" [e.g., habit, reference points] wash out. Please, prove me wrong!)
3. <u>Trading:</u> Why do people trade so much? Is it perceived information?
4. <u>Perceived Information:</u> Rational Finance is wrong when it assumes that people recognize that markets are better informed than they are. I personally believe that "perceived information" is the prime driver for high trading volume, even though I admit that I have no formal evidence.

We need to understand when and why individuals are overconfident. (PS: Bernardo and Welch (2000) tried to make a little bit of theoretical headway here, but it was not well-received by either camp. I also very much like the Barber-Odean QJE paper: there are well-known evolutionary arguments for a more risk-seeking programming of males relative to females. Documenting this in their relative immediate trading patterns combines a sound theory with good empirical evidence.)

5. <u>Expectations</u>: Closely related to the above: Exactly how do expectations form?
6. <u>Assessments:</u> How much money is wasted by irrational behavior? Not "are people rational or irrational," but where on the scale do they sit? Related: who is more rational and who is less rational?

The Equity Premium

Why has it been so high, will it last, and can it be predicted? Why are the U.S. and Japanese experience so different? [PS: In 2001, it looks no longer as high as it did in

1999 when I first wrote this piece. A recent paper of mine shows that economists now believe the equity premium is lower than historical levels, too.]

Transaction Costs

No, I do not mean papers showing that a theoretically hypothesized coefficient has a significant T statistic. There is nothing wrong with such papers, but I would definitely like to know more about quantities, not qualities here. I would like to use the transaction cost equation to help me estimate the profitability of an asset-pricing anomaly.

Liquidity

Often talked about, rarely understood. Why was the summer of 1998 (when long-term capital management went out of business and spreads relative to treasury increased in almost all other fixed income instruments) so different?

Crashes

Because we have not had any recently, this has somewhat fallen out of favor. But, what is different about banking crashes, stock market crashes and other crashes? Are there common factors? Here we can use both theoretical and empirical insights.

(Anonymous Uncoordinated) Frenzies

What happened with Internet stocks in 1999? Why then and why for 18 months?

Factor Identification and Stability

In cross-section, which factors can reliably and stably *explain variance* and which factors can reliably and stably *explain mean*? (Momentum, book-market, size, earnings-price ratio, and beta: which?)

Taxes

Way too neglected. Perhaps more important than the more popular agency and information issues, but tax research requires in-depth understanding of the ever-changing tax code---and tax research is academically just not as "sexy" as Information Economics. And, no, I do not mean eternal rollover strategies which are not implementable due to transaction costs.

Influence

(or, the politics of organizations). Pervasive. For one, influence costs (and commitment inability) prevent organizations from writing many "revelation principle contracts." Yes, agency and information problems intersect the issue of influence, but even if organization-political problems can be traced back to information, the latter does not stand a chance of realistic modeling of firms. (Perhaps the best analogy is an attempt to model sports car performance handling with quantum mechanics.) But, direct influence models are surprisingly tractable, even if they meet disdain from first-principle purists.

Employees

They are not an input factor just like capital. Most of the time, corporate performance has more to do with employees and "management of assets" than it has to do with capital structure. (We have many models in which capital structure has influence on real output, not just vica-versa. But, except in times of/near to firms' financial distress, it is my view that capital structure has very little influence on real product markets.
Other often neglected factors here might be sales, marketing, government relations; all are widely under-researched, either because Finance believes that those other areas within business schools are different domains, or because it is simply too difficult to produce meaningful data sets.

(Global) Capital Flows

Maybe this is just my ignorance, but I know way too little about the capital redeployment process. This intersects demographics and institutions as a worthwhile area of research.

Capital Markets and Economic Well Being

Are capital markets forces of good or evil? Are they created by good or evil? Are they efficient at allocating capital to its best use? Please: not just another clever counterintuitive model, but first-order insights and evidence. Some of the work by Lopez-de-Silanes, Vishny, and Shleifer has made some great progress, but I would like to see this work extended into time-series differences to reduce spurious correlation issues. (Sometimes I wonder if their variables could explain soil quality in simple correlations…)

Evolutionary Competition

What are the processes by which efficient and inefficient decision-making gets weeded out of participation in the capital markets?

Closer Theory-Empirics Linkage (or "Empirical Optimality in Policy")

Can we build a good model of, e.g., optimal corporate leverage decisions based on observable firm characteristics, and measure the effects of moving *towards/away* from this optimum? I want to see more than just "on average, leverage increases firm value; thus the evidence supports principal agent hypotheses."
I am dreaming of a research methodology that is very different from the more common empirical work where any theory is used only to ex-post explain significance in T statistics, or theory that relies on so many unobservable variables or is so complex that no good empiricist would ever use it. I dream of models whose predictions are more quantitative than qualitative; models which are of direct use to empiricists.
In fairness, there is work that does this, but it is somewhat rare. I also want to mention Hayne Leland who has been making some progress on the theory side here, but without the direct empirical application, it is just that and a wide gap away from what I would like to see.

Empirical Herding/Cascades

All right, I admit that this topic is definitely here primarily due to my very personal interest. I would like to see some more direct evidence of how links between specific decision-makers influence decision-making. For example, do your friends and colleagues influence your choice of financial assets? To write a good paper here, a researcher should really have a data set of who is friends with who. (See also my example of alien life and heat above; this criticism applies to my some of my own personal work [Welch, JFE 2000] as well.)

The Failures of Finance

The following are definitely personal opinions, and unlikely to be shared by anybody else. Also, by making a case, I almost surely overstate it.

The Empirical Applicability of the CCAPM

No one is deliberately (and unlikely accidentally) using the cross-section of financial instruments to hedge future consumption flows. Even finance professors have not given much thought about *which* stocks help them hedge their future consumption risk. Instead, investors likely compartmentalize their financial investments. Now, when people receive more money, they tend to save some. This is not the CCAPM, but the permanent income hypothesis. Further, some individuals may try to reduce risk in their "investment compartment" or invest in the stock market when they g**o**t wealthier (backward-looking!). But noone, and I mean noone, tries to use the *cross-section* of investment choices in the way the CCAPM suggest—and there is no mechanism by which investors would happen to choose the right investments even by accident.
PS: The CCAPM is a beautiful idea, contains a nice normative theory, and is a great theoretical insight---which is why the CCAPM makes my list of the most important ideas in finance. But it does *not* describe economic behavior in positive terms.

Mathematical Sophistication with Lack of Tests

There seems to be ever increasing mathematical sophistication in models that are exceedingly unlikely to ever be tested (or perhaps intrinsically untestable) and/or indistinguishable from dozens of other, similar papers. Information economics comes to mind: the theorists are just happy to exercise their own sophistication or just happy with the beauty of their structure. But, truly, there is nothing new and profound about showing that, depending on where there is an information asymmetry, all sorts of things can happen---even if it is difficult to solve and does signal high IQ by the modeler. Now, I am not entirely unsympathetic to the need for conceptual models without any empirical applicability---*but* the bar on pure "generic insight" work must be much higher, with a big emphasis on *generic*.

Nice contrasts are models of derivatives pricing (and even many asset-pricing papers) which have remained close to the data. In this case, mathematical sophistication is a winner!

Out of Sample Prediction

Most phenomena in finance are non-stationary. This could be seen as an omitted variables problem. Let’s have more truth in advertising about what we really know.

Fixation on Statistical Rather than Economic Significance

There is much empirical work that focuses on T-statistics rather than on economic questions and economic relevance.

Academic Rewards For IQ Instead of Relevance

The mindless counting of articles for promotion, which discourages long-term projects into important questions that would require large-scale data collection efforts---aside from a common attitude that when work fails to signal researcher IQ, it is not worthy of academic promotion.

Journal Publication Tests

The common journal referee fails to publish insignificant results, even if the tests are powerful and interesting. Now, the fact that results turn out insignificant does not by itself mean that the paper is interesting. It must be that the test is powerful, had a chance to uncover an effect, and that the question is interesting.!

A Consistent Journal Refereeing Process

At least one journal in each discipline needs to develop a more consistent evaluation mechanism: The top finance journals have rejection rates around 90%. The best 25% of these submissions probably meet the quality threshold, and the selected 10% probably reflect more the draw of referee than the quality of the paper. There are issues of variability in taste *and thresholds* across different referees. The *only* consistent element across submissions is the editor---and luckily some editors have taken a more activist stance.

Revolution Rewards Rather Than Evolution Rewards

Journals could benefit more from actively developing evaluation mechanisms that favor risk and innovation over incremental progress. For example, many of the challenges described above are very difficult to treat in the context of established literature strands. They require not simply a small extension to someone else's work (who, as referee, is likely to be more favorable disposed towards an extension of his own work), or the unassailable solving of a mathematical theorem, or the running of the same regressions with the addition of one additional variable that happens to come out significant. By nature, novelty typically has weaknesses different from those of the existing literature, and referees are typically *correct* when they point out how established literature does not share these shortcomings. Still, it is better to publish "major novelty" papers even if nine out of ten end up ex-post dead-wrong, than it is to just publish the incremental over and over and over again.

An interesting paper was "How the Mighty Have Fallen" (Gans and Shepherd, JEP 1994), which described that the true innovating papers in our profession had unusual difficulties in the refereeing process. Many only got published because an editor said "I know this paper and to h… with the referees.

My own suggested solution for the last two issues is a journal that has the same 3 editors first decide by majority vote whether they believe a particular submission will have impact. If the answer is affirmative, the paper is sent onto a referee, whose sole job is to make sure there are not obvious errors. After 1 or 2 years, different editors replace the editors. Not all journals should be run this way, but one journal in finance should be!

Ivo Welch

Delusion. Perhaps the biggest megalomania is my presumption that I am qualified to write this and render judgment. However, be aware that just because I can criticize other work does not mean that I believe that my own work is blameless.

If you have a reaction to this list that you would like to share, please send email to Ivo.Welch@Yale.Edu and indicate whether you would like your view to be posted (anonymously or with a name), or whether you just want to nudge me towards changes in this list. I thank Andreas Gruenbichler, David Hirshleifer, Eric Rasmusen, Dick Thaler, and my colleagues at Yale (especially Will Goetzmann, Roger Ibbotson, Jon Ingersoll, Matt Spiegel, and Jeff Wurgler) for helpful comments and suggestions.

Although this list was created without knowledge of the last chapter in Brealey and Myer's Principles of Corporate Finance, their list of top accomplishments and top challenges is also highly recommended.

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