New Directions in Finance Research: A Panel Discussion in Honor of David Mayers

Moderator: Richard Smith
Panelists: Thomas Copeland, Edward Rice, Wayne Ferson, and Clifford Smith

In April 2013, a colloquium was held in Riverside, California to recognize the lifetime contributions of David Mayers to the field of finance. Dave is currently Professor Emeritus of Finance at the University of California, Riverside’s Anderson Graduate School of Management. He received his Ph.D. in business administration from the University of Rochester in 1972 and has previously served on the faculties of UCLA and Ohio State University. Throughout his academic career, Dave has authored or coauthored a number of highly impactful academic studies on asset pricing, market efficiency, insurance markets, governance, practices, and convertible debt. He served for seven years as co-editor of the Journal of Finance. Since his formal retirement in 2007, Dave has remained an active scholar and committed research colleague. The conference in his honor featured paper presentations by René Stulz, Harry DeAngelo, Richard Roll, Thomas Copeland, and Cliff Smith. It concluded with a panel session on future directions in finance research. What follows is a transcript of the panel session. Including discussion by Harry DeAngelo, Richard Roll, and Rene Stulz, who were among the attendees.

Richard Smith (Moderator) is the Philip L. Boyd Chair and Professor of Finance at the University of California – Riverside in Riverside, CA. Edward Rice is an Associate Professor of Finance at the University of Washington in Seattle, WA. Thomas Copeland is the Distinguished Professor of Finance at the University of San Diego in San Diego, CA. Wayne Ferson is a Professor of Finance at the University of Southern California in Los Angeles, California. Clifford Smith is the Louise and Henry Epstein Professor of Business Administration and Professor of Finance and Business at the Simon Business School at the University of Rochester in Rochester, NY.
Moderator: Our panelists are Ed Rice from University of Washington, Tom Copeland from the University of San Diego, Wayne Ferson from the University of Southern California, and Cliff Smith from the University of Rochester. I am Rick Smith, from the University of California Riverside and will be moderating. We have asked each panelist to think broadly about finance research directions for the future. As chair, I really have just two rules. One is that I’m supposed to try to maintain order so I’ll try to do that and the other is that I’m supposed to keep quiet and let the panel members do the talking.

I will start by violating the second rule – just briefly, though. When I was thinking about this session and thinking back about my experience with Dave, I was recalling the papers he wrote between 1972 and 1974 (Mayers 1972, 1973, and 1974). There is a set of three on how asset pricing changes when people have undiversified positions in their portfolios. It has been awhile since I’ve thought about those papers; but when I started working in entrepreneurial finance, it became the forefront of concern for me, because entrepreneurs can easily commit maybe 30% of their human and financial capital to a single project that pays out over a long period of time.

I continue to think this is an important area. I struggled with the valuation issues for entrepreneurs who must be underdiversified. I didn’t really get very far, ended up taking some shortcuts, and kind of left things the way they were. I think that’s the case for others, as well. There is, for example, a recent American Economic Review paper by Bob Hall and Susan Woodward (2010) where they look at returns to venture-backed entrepreneurs and find an average cash return of $5.8 million. The approach they use to value the entrepreneur’s underdiversified position is similar to one that I use, which is, I create a hypothetical two-asset portfolio where one is the market and the other is the venture. The entrepreneur cares about total risk, values the overall portfolio in light of its risk, and then backs out the value and required return for the entrepreneurial venture (Kerins, Kiholm Smith, and Smith, 2004). The problem is that, in their analysis, assuming normal risk aversion, the $5.8 million is worth a negative number, even though the distribution of possible returns is strictly non-negative.

So, just using standard asset pricing models, because of the long holding period and the high risk we don’t get very good estimates of value. I continue to believe this is a promising area for future research, even though it is something we were exposed to a long time ago. The questions are important and not much progress has been made beyond Dave’s original research.

With that, I will turn the floor over to Ed Rice.

Important, but Overlooked, Sectors of the Economy

Edward Rice: Okay, first I’d like to thank the organizers for having me participate in this. There’s nobody I’d rather honor than Dave Mayers. Now, as far as directions in finance research, what has struck me recently is how much we know about corporate finance for industrial firms and how little we know about corporate finance for everybody else. What does everybody else include? It includes, for example, insurance companies. We have René’s paper from earlier today – for insurance companies there are rules that affect investment practices, but because we’re not experienced with them René had to go through them in detail and explain the complexities.

These rules have dramatic effects. In a recent paper, Kojen and Yogo (2013) look at the effect of these regulatory requirements on the sale of annuities by life insurance companies. Annuity sales by life insurance companies are a lot like selling simple bonds. But, because of NAIC (National Association of Insurance Commissioners) accounting, the discount rate used for reporting these annuities, instead of being the risk-free rate or a long-term risk-free rate that reflects the term structure of interest rates, is a weighted average of 3% and the current rate on corporate bonds of all kinds of maturities. This rate is used to discount all cash flows regardless of term. Now why is that so? Who would ever come up with a number like that and do this discounting in that way.

I think one of the reasons this is happening is because there’s an insurance community that thinks this is normal or something and then there’s a corporate finance community that thinks it is not. We (the corporate finance community) have basically abdicated our responsibility to assist in this kind of accounting. This abdication has created a situation where annuities have been terrifically mispriced, according to their paper at least, something like 50%. The annuities have been sold at 50 percent discounts because, according to the accounting, it looks like they were selling at a premium and therefore helping to satisfy regulatory capital requirements.

If you think of what happened in January 2009, interest rates had gone way down. Therefore, the discount rates being used by this weighted average formula were much too high and the effect of that is it looked like the liability from these insurance claims was really low. So selling an underpriced annuity looked like a profitable transaction when in fact, it was wildly unprofitable in this very long-term world.

This is just one example of a situation where we don’t know the rules. Nobody pays attention to this in corporate finance, and I think there are big inroads to be made by understanding the institutions of insurance companies and then using those to say some things of value. Rene’s paper is another example of where understanding those institutions can help us quite a bit.

One other area where I think we don’t know what is going on almost at all is in nonprofits. If we look at hospitals, for example, there is a recent paper by Katharina Lewellen
I used to teach a course at UCLA. You know there is this literature in public not-for-profit finance and two of the topics you mentioned were in the course. One is basically what rate on public not-for-profit finance and two of the topics you can talk about are how local tax rates influence the interest rate. I can incorporate literature about how state and local governments make decisions about how much debt they should have, and I think there is a lot more to be done about nonprofits.

One other example is there are two people at the University of Washington, Thomas Gilbert and Chris Hrdlicka, who are investigating university endowments - even more embarrassing, these are universities where we work! What do we know about how those monies should be invested? What do we know about what kind of investment decisions universities should make? Why do we have endowments like Harvard’s, which is so enormous that it would take a million years before they could spend it all? But it looks like the flow of funds is coming in and they’re not spending it on anything. Why are people donating to something where their money doesn’t go anywhere, just into the endowment? And why is Harvard investing that money in risky assets? Actually, Hrdlicka and Gilbert have a model where there’s an agency problem between people trying to grab rents like self-interested faculty.

Last, I want to mention government. I’m working on a paper with Atsushi Chino and Hae Mi Choi on state and municipal debt. In writing this paper, I wanted to incorporate literature about how state and local governments ought to be funded, how much debt they should have, and what kind of rules of thumb they ought to use. I cannot find any literature in finance that addresses the decision by states and municipal governments to raise debt. I can find papers about how local tax rates influence the interest rate. I can find little pieces of things about the municipal bond market. I hope this is wrong, but as of now I know of no papers that talk about the optimal way for state and local governments to be financed, and I’ve talked to state and local government officials who also say things like “we really worry about our bond rating” and that’s basically what you get.

These are really big markets - insurance is a multi-trillion dollar market, state and local finance is a multi-trillion dollar market. You know it’s not like these are tiny things. These are big things and we ought to know something about them.

Richard Roll: You know there is this literature in public finance that has to do with nonprofit. There’s a question like what discount rate do you use if you build a tomato paste factory in Indonesia [n.b., without systematic risk] and the answer is the risk free rate, of course. That’s their answer you know, you’ve seen that literature.

David Mayers: I just wanted to add there is a literature on the insurance industry that you might want to look at.

Edward Rice: Well, I did want to say I think you and Cliff Smith opened some of the windows on insurance but there are a lot more windows to open.

Moderator: This is not just about insurance. The defined benefit pension plan earnings assumptions - who made those up? They give you a risky rate as an investment return assumption and then the plans have an obligation that pays out like it’s riskless. Also, the discount for people’s investments in their own companies, in 401K plans, is traditionally 10%. I don’t think it has anything to do with the real exposure.

Clifford Smith: But you know if you take a look at how the government generally operates, the government makes up their own rules for their own purposes, and if private companies were to account for pensions the same way that governments do, they’d be in jail. I wish I could tell you that I thought that individually or collectively the people in this room are smart enough to do both the finance and the politics. The politics part of this is just a lot harder I think.

Edward Rice: I want to add one thing. I think a lot of these areas are kind of where behavioral finance was 20 – 25 years ago. We had individuals making strange decisions, we had models that said everything was optimal in their decision making, and we’ve had something like 1,000 papers over the last 20 years about this disconnect. I’m a little disappointed as to how few principles we’ve learned. Still, I think there are opportunities here.

Moderator: Why don’t we switch topics? Tom, you want to go next.
Valuation and Investment Performance Evaluation

Thomas Copeland: I have three topics that come from talking to CFOs (Chief Financial Officers) of companies. The first one is simply the valuation of companies. We think we know how to value companies, but there are very few academic publications on valuation that report any empirical evidence at all. I have this chart which is 22,000 – 27,000 valuations, full discounted cash flow valuations of companies done by an expert system that I designed. For firms with high projected growth and high volatility, the discounted cash flow value is less than the market value in about 5,000 observations. For those with low projected growth and low volatility there is just the opposite type of bias. So it appears to me that there is the standard discounted cash valuation approach that we have in textbooks is badly biased and we need a new model.

Another thing that comes up a lot is performance measurement. The typical performance measurements are earnings growth, return on invested capital, return on invested capital minus the weight of the average cost of capital or EVA (estimated value added), which is return on invested capital minus the cost of capital times the amount of invested capital. I think that none of those measures is correct. How about this measure for performance? The actual return on investment capital minus the expected return on invested capital times the amount of invested capital. What is missing in the traditional measures is expectations and so that’s some food for thought, also.

And another area where work has been done is: for what do we use value? We used it for restructuring companies and I think there needs to be more work done on how spinoffs, for example, affect value. I have come across several situations where the announcement effect for the spinoff was greater than the value of the entire division that was being spun off. There are some papers on this spinoff topic that Dave [Mayers] and I wrote, one with Eduardo Lemgruber, but I think more work along this line would also help.

Edward Rice: I think it’s remarkable that most of what our students, or a large fraction of our students, do has to do with calculating net present value and estimating cash flows. In fact, however, there’s almost no research connecting fundamentals of net present value analysis to practice, and it seems like if we can figure out a way to do valuable research about this, it would have a huge impact on what we teach.

Rene Stulz: I am surprised, Tom, that you ignored risk in your valuation analysis, in the sense of the total risk of the firm, but then you think we should take it into account when assessing performance.

Thomas Copeland: This morning I said, when I started out my talk, that net present value should not be expected cash flows discounted at a CAPM (capital asset pricing model) rate and it really should be a value tree analogy. This – the device will help you to change your path and to make safe, contingent decisions better, but that means that the cost of capital may not be constant for a project, because the optionality involved in the analysis has a beta that is changing across time.

Moderator: All right, so why don’t we move ahead to, Wayne Ferson?

On the Divorce of Empirical Research From Theory-Based Foundations

Wayne Ferson: Tom mentioned performance evaluation and I wanted to raise just one question that I have about investment performance evaluation. This is a fairly narrow question but I want to argue briefly that there are two contexts in the literature on investment performance evaluation where we used to have some discipline and we’ve lost our discipline. Of course, discipline is provided by models and theory, and my question is going to be “should I worry about this for the literature going forward or is this in some sense a natural evolution?” So let me flesh that out a little bit.

So the first context is returns-based performance measurement. We run regressions of portfolio returns on some factors, and we’re going to look at the alpha as a performance measure. This literature used to have a discipline about it. I mean, Dick (Roll, 1977) worried about this in the context of mean variance analyses and Dave [Mayers] with Ed (Rice, 1978) worried about this back in their UCLA days and they asked the question, “Under what conditions can I show that a portfolio manager with information actually generates a higher alpha or a high return?” They wrote down some assumptions under which they could claim that you would get that and, of course, then they were criticized for their assumptions. That’s how I interpret Verrechia (1980) and some of the other papers that followed. There were a number of papers on this question of what is the discipline, what is the theory behind these measures of performance and that continued in various forms. When we were doing Arbitrage Pricing Theory, Connor, and Korajczyk (e.g. 1986) were working on that and Mark Grinblatt and Sheridan Titman (e.g. 1983) had a lot to say about that again in a mean variance setting, so we had a lot of discipline that helped us to think about what we were doing in performance measurement and what it meant.

Well, at a certain point in history, early 90s, mid-90s, the Fama-French factors took over the world and then everyone would just regress returns on the Fama-French factors and maybe a momentum factor and then look at that alpha. So we lost our discipline. We didn’t even have a theory in
which we could ask simple questions like, “would a manager generate alpha if they have information?” So we’ve lost our discipline. So that’s the first context and my question is, “should I worry about that or is that just a natural evolution and it’s okay to just go ahead and proceed and estimate these alphas and not really worry too much about the absence of any theory?”

Now, the second context is where I think the literature on performance measurement is going and that is holdings-based measures of performance. Tom [Copeland] and Dave [Mayers] get cited for doing this in their Value Line paper [1982], where the holdings, the weights of the portfolio being evaluated, were assigned to them based on Value Line rankings, and they thought a lot about benchmarks against which we would evaluate these portfolios. They did an earlier version of what is essentially the covariance between the portfolio weight of the strategy and the return’s excess over a benchmark. Now that kind of measure actually had a discipline to it. They actually wrote down a model, made assumptions under which they could show that a manager with information would have a positive covariance, summed up over all the securities, in the portfolio between their holdings and the subsequent return. The weights would predict the returns in that sense.

So what has happened in the literature since then? Well, that measure has been decomposed I’d say most famously by Daniel, Grinblatt, Titman, and Wermers (DGTW, 1997) by introducing a security-specific benchmark, essentially a security characteristics-based version of the Fama-French and momentum factors, assigning a benchmark based on classifying stocks into cells based on those characteristics. Each stock gets a benchmark, and then they would break the original Grinblatt and Titman measure up into various pieces – one they call characteristic selectivity and one they call characteristic timing and one they call average style. The three of those sum to the Grinblatt and Titman measure and so have the same theoretical justification as the original measure.

But what has happened in the literature - and I don’t know if this is a case of where somebody looked under the streetlight and saw something and so now we’re all just looking there - but they found that really the only one of those pieces that empirically seemed to be very interesting is the DGTW characteristic selectivity measure. And now as I look at the literature, most people are looking at only that measure. It’s intuitive - just like a Fama-French alpha - it has some intuition to it, but we’ve lost that discipline now. We’ve lost that theoretical justification for what it means. We can’t really claim that we have assumptions, as far as I know, under which it really measures anything. So, you know, should I be worried about that? Is this just a natural evolution of an empirical literature or should we be concerned that we’ve lost our discipline in this area?

Harry DeAngelo: I think that three people on the panel have identified problems with a common cause:

- I’ll start with Rick talking about entrepreneurial finance and investments in companies that are closely held,
- Ed speaking about social investment criteria, and
- Wayne talking about performance evaluation of abnormal returns in a portfolio context.

I think that in each case we’re trying to shoehorn a problem into a form that can be analyzed using value as an index of performance or welfare. When is it appropriate to do that? Strictly speaking, the idea of using wealth as a welfare index is rooted in basic Fisherian intuition. If you go back to that basic analysis, and ask under what conditions you can legitimately use wealth as an index of welfare, you’ll find it’s basically a strong form efficient market where there are perfect exchange opportunities, with no impediments to trade. Markets don’t have to be complete in the Arrow-Debreu sense, but friction-free exchange is critical for getting a utility-free performance measure: Wealth. More realistic conditions – any sort of asymmetric information problem, constraints on portfolio holdings, wealth limitations where you don’t have diversification at work, or ex-post performance measures that are a function of realizations – do not meet the basic Fisherian criteria to justify having wealth be the appropriate index of individual welfare. But in this discussion we’re operating with intuition grounded in the Fisher model that we ought to be able to have some simple performance index that would let us solve the problem for the entrepreneur, solve the problem for social investment, and solve the problem for evaluating ex post investment performance. So, if we apply Wayne’s point about the need to go back to the fundamentals, what should be, in each unique context, the right measure? My sense is that you’re not going to find a utility-free measure because the only times you ever find them are situations where you’ve got strong form efficiency and so on, as with the Fisher separation theorem.

Moderator: Thank you.

I was just thinking of your comments, Wayne, in the context of looking at institutional investor performance. There are several papers that claim to find positive performance by financial institutions, but it looks to me that, at least in many cases, if you just assume that institutions behave in a reactive fashion of selling companies that have underperformed or that have violated some of the conditions under which certain institutions are permitted to hold those companies, you can account for all of their superior performance [Erenburg, Kiholm Smith, and Smith, 2012]. This conclusion is not sensitive to the choice of asset pricing models. So, in this case, it doesn’t take any analytical rigor to get that result. So I think this is in the same genre as some
of your comments.

Okay, so why don’t we then turn to Cliff Smith?

Governance and Organizational Architecture – The Need for a Broad Perspective

Clifford Smith: I will briefly go through some things that we’ve been thinking about. One of the things that is worth giving some additional thought to is just broadening the scope of some of the things that we look at and think about. When I start looking back at corporate governance issues, I really think about it as the partitioning of decision rights among shareholders, board members, management, and various other groups. There is an array of systems to monitor and evaluate performance and the methods by which these groups are rewarded. Now, I will admit that part of the reason that I think about things this way goes back to this book that Jim Brickley, Jerry Zimmerman and I are working on the sixth edition of now [5th ed., 2008]. But a lot of this goes back to what Bill Meckling and Mike Jensen [1976] taught in a managerial economics class in Rochester back when Dave [Mayers] was a Ph.D. student. They were certainly working on some of these issues before I joined the faculty. When we discuss, in the book, what we call organization architecture, there are three things that we’d like people to think about: the assignment of decision rights, the control systems within the organization, and the reward system that is being applied. When I think about corporate governance, it’s basically those sorts of organizational architecture issues at the very top of the organization.

Some of the facets that get wrapped up in these issues are largely internal policy choices of the organization – the corporate charter, the bylaws, the organizational structure, the accounting system, the budgeting system, the HR system, the firm’s financial architecture. Others, like oversight agencies, input and output markets, legal regulatory tax regimes are largely external to the firm. My point is these factors are interrelated.

One way to think about this is that there is a continuum - some things are more internal, some are more external. If we look at provisions in the charter and the bylaws, there are considerations like ownership structure, voting rules and takeover provisions. If we talk about board structure, there are issues about board composition, board compensation, and committee structure within the board. When we think about organization structure, there are issues about centralization versus decentralization. Business units – do I organize inside the firm by function? Do I organize by product? Do I organize by geography? When you think about the accounting system, there is financial reporting, but also the internal management accounting or cost accounting system. There is an internal audit function within the firm. And with your HR system – there is this question about fixed versus incentive compensation. If I’m going to provide incentive compensation, is it incentive compensation that tries to measure performance using accounting numbers or stock price-based measures of performance?

When we talk about financial architecture, there are leverage questions but there is a lot more texture in what’s going on here. There are the debt covenants. If I raise debt, is it from public markets or private lenders? Do I go to banks, insurance companies, or pension funds? There are risk management issues so, for example, do I borrow at a fixed rate or a floating rate? Do I use interest rate swaps, currency swaps, or similar instruments? Do I use structured instruments like dual currency bonds or oil index notes? If I am acquiring the use of an asset, do I buy it and finance the purchase or do I lease the asset?

There are external monitors like oversight agencies and public accounting firms. There is some amount of monitoring that is done by the directors and officer’s insurance companies. Capital markets have an important set of institutional arrangements here, such as exchange listings, the use of investment banks, institutional investor, block holder, stock analyst, and bond rating agencies. There are transactions in the external market for corporate control - mergers, tender offers, and proxy fights. Labor markets can be divided into managerial labor markets and board level labor markets. For legal and regulatory considerations, there are state of incorporation decisions, federal and state securities laws like Sarbanes Oxley, Dodd-Frank, and the Williams Act.

If we think about this whole array of institutional arrangements when we start looking at some transactions that, as a profession, we have focused on a lot, such as hostile takeover, the organizational architecture should be taken into account. Are there anti-takeover provisions that are part of the structure? In our HR system, does the management team have a golden parachute? In terms of financial architecture, do debt agreements contain anti-takeover provisions? So much of our strategy with respect to these kinds of issues is to take out a microscope and take one facet of this structure and look at it in a tremendous amount of detail with a tremendous amount of care, but I’m not sure that we’re getting as much insight as we ultimately would like to have because these things interact. So trying to stretch, go to a little broader focus and think about these things not just on a one-off basis, but how packages of these organizational architecture choices wind up interacting seems like an interesting, important and worthwhile exercise.

I think a couple of quick conclusions are merited by going down this kind of path. We saw things blow up in certain organizations a few years ago that prompted regulatory responses like Dodd-Frank or Sarbanes Oxley if you go back to the Enron mess. I want to assert that in every organization, every public company that I’m familiar with after that
wound up on the front page of the Wall Street Journal, at the very next board meeting, there was an internal discussion of: “Is there anything remotely like this that is going on in our firm, and can we do anything to reduce the likelihood of a similar problem in the future, and is there a way that we can credibly convince external parties that the kinds of things that happened at Enron aren’t going to happen here?”

You can’t make an investor buy your stock. You can’t make a lender make you a loan or buy your debt. All you can do is offer securities and terms that, given the alternatives they face, investors deem to be attractive investment opportunities. The more uncertainty there is around the edges of these kinds of potential problems, the lower the demand price for those securities.

I’ve already mentioned that some of the most public responses to that kind of debacle were responses that came out of Washington, and what bothers me, and bothers me a lot, is the costs associated with things like Sarbanes Oxley (SOX) and Dodd-Frank. A lot of the discussion has focused on direct compliance cost. Unfortunately, although those numbers are shockingly large, I fear that they are the smaller part of the relevant costs. I believe there are material opportunity costs associated with this kind of regulation and my biggest fear is that those opportunity costs are simply going to grow over time. Things like the Williams Act and anti-takeover statutes reduce the effectiveness of the corporate control process and put a bigger potential wedge between the opportunity cost of an array of business opportunities that a firm faces and how far short of that an existing management team can fall without being taken out by some competing offer.

When I think about things like SOX and Dodd-Frank, one material concern is that a huge array of what we know about the right way to organize a firm in general, the right way to structure the liability side of a firm’s balance sheet comes through a process that Armen Alchian refers to as economic Darwinism - that there is a tremendous amount of experimentation that goes on in the marketplace and the result of that kind of trial and error is that things that seem to work get copied and things that blow up in people’s faces get discarded. One of my biggest concerns about process regulation, like Dodd-Frank and, Sarbanes Oxley, is that it regulatorally proscribes process and what that means is it prohibits experimentation. If we’ve got regulation on the book that says you can’t experiment, all of a sudden an incredibly powerful efficiency-enhancing mechanism that has given US businesses a huge leg up on businesses in Western Europe and most of the rest of the world - this opportunity to engage in experimentation to use the power of this Darwinian mechanism - we are regulatorally shutting down.

To me, the scariest part of that whole discussion is that trying to assess the magnitude of that opportunity cost is something that is just beyond my capability. Trying to guess what experimentation might have produced in terms of innovative organizational structures and innovative financial structures - that, because we passed a law in 2002, cannot be tried - there’s an opportunity cost there and it’s just conceptually, in my mind, impossible to gauge the order of magnitude of that opportunity cost. To me, this is something that has the potential to materially adversely impact the wealth that can be created in the business community.

**Moderator:** So, Cliff, are you suggesting that a good research prospect would be to explore the opportunity costs associated with those types of regulatory impacts?

**Clifford Smith:** I have great difficulty understanding how anybody in the academic community can get their arms around that number. In terms of research opportunities, the serious point that I tried to make earlier, I’ll stick with that, is, rather than thinking about some of these institutions on a one-off basis, focusing on them in isolation, thinking about the interactions among them is something that we’ve done less of than we are in a position to do now. Understanding the extent to which some of these things are compliments, not just substitutes, is a potentially fruitful source of academic inquiry.

**The Research Dichotomy – What is Measureable V. What is Important: An Application to Governance**

**Harry DeAngelo:** My thought doesn’t relate specifically to anything Cliff said but more generally to the governance area. I used to find the governance literature really interesting, but these days it has little that catches my attention. I worked in the area in the ‘80s, but I have become very discouraged with it over time. The reason – and I’ve thought about it a lot – the reason is that, as with all things that we study in finance, there are things that you can quantify and things that you can’t quantify that are important. The governance literature more than any other field and sub-field in finance that I can think of has evolved into one where the questions that people look at are the ones that are easily quantified, while the issues that are most important are the ones that can’t be quantified.

I think it is going to take a different style of research to come to grips with the aspects of governance that are really important. I’ll give one simple example of what I think is wrong. I’ve been doing a lot of reading on what happened with the auto companies over time. There’s a journalist – I forget who it was but his analysis was more insightful than any governance paper I’ve read in very long time. He juxtaposed Ford and GM and noted that GM won awards for its corporate governance principles. You can find Business Week stories in the ‘90s about the wonderful governance
principles of GM. GM was credited with creating the Magna Carta of Corporate Governance. As it turns out, GM had a board of directors that did not hold Rick Wagoner and other people’s feet to the fire, and we know what happened as a result. In fact, we all helped pay for what happened, even if we didn’t own GM stock. Over at Ford you had a dual class structure with the Ford family in control with a huge share of the voting rights and trivial cash flow rights. The CEO (Chief Executive Officer) of the firm was a member of the controlling family. On paper and by all the analytical measures of the governance literature, Ford’s governance structure was a nightmare. What happened at Ford? Well, the family member who was CEO fired himself because he saw that he wasn’t getting the job done. He undertook a serious search for a replacement and hired an outsider as CEO. The new CEO performed wonderfully and, unlike GM, Ford avoided the need to take a government bailout. What the journalist said about GM versus Ford: “judgment trumps structure.” Absolutely right - GM had the Magna Carta of Corporate Governance, but the board failed in its judgment. Ford had (and has) a terrible governance structure by the standards of the academic literature, but its CEO used good judgment and that carried the day.

It’s a hugely important general principle: Judgment Trumps Structure. In the governance literature, it’s exactly the reverse: Structure Trumps Judgment. Why? Because structure is something you can measure with governance indices and drop them into regressions and calculate test statistics. Judgment is really hard to measure in ways that referees find credible, and so it goes by the wayside in the literature. In so many governance studies, the focus is on what one can measure or quantify for a regression, not on what’s really important.

Is There More Work To Be Done On Understanding The Financial Crisis?

Rene Stulz: I do have governance papers and I’m very proud of them. But my comment doesn’t have to do with that. It has to do with I would say the uniqueness of the panel in the sense that over the last three or four years in any panel that I either have been on or that I have listened to the centerpiece was we have had this dramatic crisis and it was the most dramatic since the 1930s. The theme in these other panels was that we can’t be thinking like we used to think. We have to change, so there is a lot of work to do. We heard none of that today. So is it because you think that we don’t need change?

Moderator: I want to give my answer to that question. I think it’s partly because on the panel, and among the participants, we have several centuries of academic research experience, each of us with a perspective spanning decades, and not just the last few years. I think mature scholars may think about bigger questions or they think about them with more context. Maybe it’s just going back to the roots, but to me, it is much more enriching than a lot of the very narrow sort of gap-filling research that seems to be going on now.

Ed Rice: I would say, first of all, the structure we were given was pick an area. Now, I suppose we could’ve picked the effect of the crisis on finance research, but absent that you’re talking about a situation over the last few years. Over the last few years, we’ve seen a lot of papers on the crisis, and so we’re learning more about the crisis and maybe a little too much. Not every paper has to be about the crisis and there are longer time spans to be thinking about.

Harry DeAngelo: I’d like to follow up on Rene’s point because I think it’s an important one. When Ed started talking about insurance companies, I thought he was going to take the discussion broadly into the area of intermediation, and not limit it to insurance companies but to consider banks, shadow banks and so forth and so on. Financial intermediation seems to me to be a critical area, something that we don’t really understand very well that we’ve shuffled off onto the side. So that would be one thing I’d say is important and deserves serious attention.

René, I’d like to hear your thoughts. If you were up there on the panel, what dimensions of the crisis do you think people should be looking at?

René Stulz: I think there are lots of issues that we have to investigate. We have to understand financial intermediation much better. We have to understand cross-border financial intermediation much better. We understand very little of it. We have to understand the role of regulation and how people form their beliefs. There are clearly lots of beliefs held before the crisis about risk that seem to be hard to justify now. We don’t really understand how those beliefs were formed. I think we have a lot of work to do: especially on the issue of how people think about risk, how they form probabilities, and how they know what biases there are. That’s why I think there’s a ton of work to be done.

Clifford Harris: One of the things that I worry about more and more is: How much of the problems that we’re talking about are things that we think about as failures of private market institutions versus how much of it is a failure of the legal and regulatory system that these private institutions have to operate inside of? I’m afraid that when I look at something and see it as a regulatory failure, some other well-respected people could look at that same set of facts and say no, this is a failure in capital markets.
Moderator: I’m enjoying this discussion a lot, but we’re actually at the end of our time. I’d like to thank all the members of the panel. Thank you all for participating with your papers and comments throughout the day.

References


